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

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Abstract: I implement an experiment among 1,300 Salvadoran migrants and their family members (recipients) in El Salvador that examines the impact that changing the ability of the participants to monitor each other and reveal their preferences has on remittance decisions. Migrants decide how much of a \$600 lottery prize to keep and how much to send as a remittance. Recipients decide how to spend a \$300 remittance prize. To test for moral hazard participants are randomly allocated into two groups: either told their partner will be informed of their choice or that it will be a secret. To test for communication barriers half the recipients are also informed of the migrants' preferences for their choice. Migrants keep significantly more when their choice is secret. Participants make choices closer to the migrants' preferences when their choice will be revealed and they are informed of the migrants' preferences. The effects of the communication treatment are present in all pairs but the effects of the moral hazard treatment are only evident when a proxy for the likelihood of contract enforcement is high.

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I. Introduction and Motivation

Secret keeping in families has long been featured in stories told in literature, film and television. However, despite the important role that information asymmetries have played in the study of other important economic institutions such as labor and financial markets, only recently has attention been given to their role in the economics of household decision making. The extensive literature addressing the allocation of resources within households has instead focused on the bargaining power held by household members that makes them more or less able to negotiate agreements that match their preferences and the theory behind how these agreements are negotiated.¹ Although the empirical evidence is conclusive that bargaining power matters for resource allocation, the theory for the most part does not consider how the bargaining process would change if resources or spending decisions are kept secret. Understanding household resource allocation is particularly important in the developing world because when resources are scarce small changes in the way in which they are distributed among household members can have large effects on household outcomes.

Recent experimental work shows that information asymmetries do matter for choices made in the household. Specifically, Ashraf (2009) shows that, in the Philippines, men whose wives are the household financial managers will hide income from their spouse when that decision is private. When their decision is public men choose to divert income to committed consumption that cannot be undone when the spouse is informed. Only when spouses communicate about their choices before they make them do men choose to share the income with their spouses. In another experiment in Zambia, women are more likely to take advantage of

¹ Empirical studies include Lundberg, Pollack and Wales, 1996; Duflo, 2000, 2003; Qian, 2008; and Ambler, 2012. For theoretical models see Manser and Brown, 1980; McElroy and Horney, 1981; and Lundberg and Pollack, 1993. Another class of models puts less structure on the decision process and assumes only individual utility functions and that the outcome will be Pareto efficient. See Chiappori, 1988, 1992.

vouchers for contraception and use concealable forms of contraception when these vouchers are given to them outside of the presence of their husbands, showing that information issues extend beyond simply the allocation of funds in the household (Ashraf, Field and Lee, 2010).

Motivated by this growing literature addressing the importance of information, this paper brings the question of how information asymmetries affect the allocation of resources in the household to a new setting, that of international migrants and their family members in the home country, in this case Salvadoran migrants in the Washington, DC area and their family members in El Salvador. Leveraging matched surveys done in both countries I conduct an experiment that studies how moral hazard and barriers to communication can affect the sending and spending of remittances sent from the United States to El Salvador. The migrant context is important and relevant for several reasons. First it allows for an examination of the role of information asymmetries outside of the husband-wife pair, the setting that has been the context of almost all the previous work in this area. People in developing countries often live in extended households and decisions about resource allocation consequently are likely to involve people beyond just the husband and wife. It is precisely these households where information problems may be more acute.

In addition to looking into the extended family, studying migrants is interesting because of the importance that migration and remittances hold for development. In El Salvador, for example, remittances received were 16 percent of GDP in 2009 (Yang, 2011). 22 percent of people lived in households that received remittances and average monthly remittances were \$171 for families that received them, a figure that is almost 50 percent of average monthly household expenditures (DIGESTYC, 2010). Clearly remittances can constitute an important stream of income for families that receive them. Even in countries where international migration is not as

important, domestic migration particularly from rural to urban areas can result in large amounts of internal remittance flows.

An extensive previous literature on remittances focuses on the motivations of migrants to send remittances. Potential motives include altruism, exchange, investment, insurance and loan repayment (Lucas and Stark, 1985; de la Brière et al., 2002; Rapoport and Docquier, 2006; Yang and Choi, 2007 among others). These motives may operate simultaneously, and while there is empirical evidence to support the existence of them all, the literature has been less successful in defining their relative importance. Because remittances sent for certain motives such as altruism are less likely to be affected by information asymmetries than those sent for other motives such as loan repayment, the existing literature on motivations to send remittances does little to suggest how important information asymmetries might be in remittance decisions.

Yet it is clear that any agreement struck between migrants and family members regarding the sending and spending of remittances may be affected by the information void between the two parties. Because of the distance between them, migrants cannot fully monitor the expenditures of remittances that they send and recipients cannot fully observe the migrant's income. This information gap does appear to impact behavior. De Laat (2008) finds that migrants in Kenya spend resources on costly monitoring of their rural wives. Chen (2006, 2012) finds that in China wives with migrant husbands exhibit non-cooperative behavior more often for activities that are more difficult to monitor.

Another strain of literature considers whether migrants send less than their optimally desired level of remittances because of concerns over how those remittances will be used. In a field experiment, Ashraf, et al. (2011) show that Salvadoran migrants in Washington, DC save more when they are given greater control over bank accounts that they share with family

members in El Salvador. In another experiment among Salvadoran migrants Torero and Viceisza (2011) find little evidence that migrants send more when they are able to control how remittances are spent, but attribute this to the fact that the control offered by their experiment (vouchers for groceries) was too limiting. The idea behind both of these experiments is that offering control to migrants will mitigate a moral hazard problem in how recipients spend remittances. Yet these papers are limited not only because the control they offer is restricted to one specific category, but also because they address only the migrants' decision, not considering that recipients may have just as much influence in the development of a remittance agreement as the migrant.

I view the decisions made by migrants and remittance recipients as the result of an informal, implicit contract that dictates how much of his or her income the migrant should send to the recipient and what that money should be spent on when received by the recipient. The ability of families to create this contract depends on the enforceability of the contract which will vary across migrant-recipient pairs. However, even when the contract is in place, moral hazard plays a role as the contract cannot be enforced if deviation from its terms is not detected. Because the probability of detection may vary across time or be over-estimated by the enforcing party, sporadic or consistent strategic deviation from the contract terms may be a characteristic feature of many migrant-recipient remittance relationships. Additionally, communication barriers, particularly in regards to the migrant's preferences over the recipient's spending habits, may lead to inadvertent deviation from the migrant's preferences by recipients.

I implement an experiment to test the importance of these moral hazard and communication issues. The experiment is designed to mimic real life decisions about remittances made by migrants and their family members and the random assignment of treatment

will allow for causal identification of the impacts of the informational conditions being tested. Salvadoran migrants recruited in the Washington, DC area are given the opportunity to participate in a lottery to win \$600 and choose how much of that \$600 they wish to keep and how much they wish to send to a family member in El Salvador. To test how migrants react to changes in the observability of their choices they are randomly allocated into two treatment groups: those who are told their decision will be kept a secret and those who are told that their family members will be informed of their choice. These same family members participate in a companion experiment where they are given the opportunity to win a remittance worth \$300 and are asked what they would like to spend it on. As in the migrant experiment, half of the recipients are told that their choice will be a secret and the other half are told that the migrant will be informed of their decision. In a second randomization addressing the importance of barriers to communication, half of the recipients are informed of the migrant's preferences for how they should spend the money, and the other half are not.

This experiment is closest in spirit to the Ashraf (2009) study of spousal decision making in the Philippines. However, this paper makes a distinct and important contribution to the literature beyond just the application to the migrant context and extended families described above. The Ashraf study considers just one choice: whether to hide, spend, or share income. The present experiment views this choice as only the first step in the final allocation of resources. Once income is shared it must be spent and family members may also make strategic decisions in the face of information asymmetries in this second stage. It would be, for example, interesting to know how the wives in the Philippines experiment would react to monitoring of their spending decisions by their husbands. This experiment documents the impact of information on both stages of the allocation process. An additional contribution is that migrants, when choosing to

keep or send the funds, can choose to split the funds as they wish instead of keeping all or sending all. This allows for a more detailed understanding of the complex ways in which information asymmetries may affect families.

I find that migrants keep less for themselves when their decisions are not secret, but only in families where a proxy for contract enforceability is high. I use closeness of migrant-recipient ties as a proxy for contract enforceability. The effect is up to 30 percent of the mean amount kept for the group with close ties where choices are secret. Recipients are similarly affected by whether or not the migrant is told of their choices only in cases where ties are close. Revealing recipient choices to migrants results in a 10 percent reduction in the difference between recipient choices and migrant preferences in the high enforceability group. Reducing communication costs by revealing migrant preferences to the recipient results in a lower migrant-recipient difference across all pairs as predicted by a situation where some recipients simply value the migrant's opinion but may not have good information on what that opinion is.

The paper proceeds as follows. Section II describes a framework for understanding how moral hazard and communication costs may impact decisions about remittances. Section III details the experiment. Section IV describes the data and the empirical strategy. Section V presents the results and Section VI concludes.

II. Framework

As discussed in the previous section a long literature in economics investigates the motives for why migrants send remittances. The determinants of whether or not a migrant will remit and how much he or she will send are varied and certainly not mutually exclusive. The goal of this paper is not to characterize these motives explicitly, but rather to understand how information asymmetries interact with these factors to determine the final remittance.

Importantly, I also consider how subsequent decisions about how to spend remittances are impacted by information problems. Specifically the framework described below considers the extent to which moral hazard affects the decisions made by migrants and recipients and further how recipients are additionally affected by barriers to communication.

Each remittance payment R is composed of two elements such that $R = x + y$ where x is the component of the remittance payment that is determined by the migrant's own preferences. I will refer to this as the altruistic component of the remittance payment, but it could also be sent for other purposes, such as if the migrant is expecting the recipient to invest the funds in a way that will benefit the migrant. For these purposes the specific motive is not important, x can simply be thought of as the amount that the migrant would send independent of any negotiation with the recipient. The second component of the payment, y , is the result of the negotiation of an informal remittances contract between the migrant and recipient. In other words, y is the amount over what the migrant would send if they were not influenced by the recipient in any way.

The remittance contract struck between the migrant and the recipient is only as good as the degree to which it can be enforced. If contracts are enforced through the threat of some sort of punishment for non-compliance then the distance between migrants and their family members clearly causes an enforcement problem as punishments may be more difficult to implement from afar. Moreover, there is likely to be heterogeneity in enforcement capacity across migrant-recipient pairs because the characteristics of each relationship will determine the extent to which punishment is possible. For example, because of the cultural norms that exist around the sending of remittances, an effective form of punishment could be damage to the migrant's reputation through public discussion of his or her failure to remit. However, this punishment should also be

more effective for migrants who care about this reputation and have close ties to their home community, or who plan to return.

I characterize this heterogeneous enforcement ability as the cost c that the migrant will incur if she deviates from the negotiated contract. Consequently, the negotiated portion of the remittance payment will depend on c . In cases where recipients are unable to make credible threats of punishment c will be zero and the migrant will send only x . In other words, when no enforcement is possible, there is no remittance contract between the migrant and the recipient and the total remittance payment is equal to what the migrant would send altruistically.

Logically, y will also vary with c when c is positive as the greater the potential punishment, the more negotiating power the recipient has. Writing the negotiated remittance as $y(c)$, $y(c)$ will always be less than but close to c so as to induce compliance while allowing the recipient to extract as much utility as possible.

Although the remittance contract takes enforcement into consideration, because the migrant's income is not directly observable by the recipient the migrant still has the opportunity for strategic deviation from the contract if she thinks she will not be discovered. When deciding whether or not to comply with the remittance contract, the migrant considers not the cost of deviating, but rather the expected cost of deviating which is determined by the probability that the recipient will discover the deviation. Denoting the probability of detection as p the migrant will choose to deviate and not send the negotiated remittance if

$$y(c) > p * c$$

that is if the cost of sending the remittance is greater than the expected cost of deviating.

Certainly recipients may be aware that migrant income is not perfectly observable and may take this into consideration in the implicit contract, but they may overestimate p or be unaware of the

extent to which p can vary across time even for the same migrant. Migrants may also send part but not all of the negotiated remittance as hiding some income from the recipient may be more plausible than hiding all income.

The decision made by the migrant about how much money to send in remittances must clearly be followed by a decision by the recipient about how to spend the money sent to them. The remittances contract will consist of agreements both about how much to send and how that money should spent, but the migrant and recipient will each make an individual decision about whether or not to deviate from that agreement. The recipient's decision can be formulated in a similar manner to that of the migrant.

The migrant has certain preferences over how the recipient should spend the money received from the migrant. The recipient can follow these preferences not at all, in part or in whole up to a maximum compliance of 100 percent. The extent to which he follows these preferences comes in two parts. The first component, denoted a , is analogous to the altruistic component of remittances sent by the migrant. This is the percent of the migrant's preferences that the recipient complies with because he wants to, possibly because he and the migrant have the same preferences or because he simply values the migrant's opinion. The second component, denoted $b(c)$ is the percent of the migrant's preferences the recipient follows as a result of the negotiation over the remittance contract. c is again defined as the cost the recipient would incur as punishment for deviating from the agreed upon contract. As in the case of the migrant decision, $b(0) = 0$, so these contracts are only in place in migrant-recipient pairs where enforcement is possible, and b is increasing in c .

If the recipient complies with the contract he will get the utility he derives from that consumption $u(b(c))$. If the recipient deviates the probability that a recipient's deviation will be

detected is p , therefore the expected cost of deviation is $p * c$. The utility that the recipient will receive from deviating will therefore be equal to the utility he gets from spending the remittance money on something he prefers $u(d)$ minus the expected cost of the deviation. Therefore the recipient will deviate if

$$u(b(c)) > u(d) - p * c.$$

Although as in the negotiation for how much to send in remittances $b(c)$ will be set so that the recipient should just prefer to comply, because p can vary without the knowledge of the migrant or be overestimated by the migrant, the recipient may have opportunities to deviate that the migrant does not expect.

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

The main point of this discussion is that both migrants and recipients will be more likely to strategically deviate when the probability of detection is low. Because communication costs may obscure migrant's real preferences over remittance spending, recipients may also deviate inadvertently when they are misinformed. One important point to note is that the important

determining factor of the negotiated remittances in this setup is the enforceability of the contract. While the bargaining power that is theorized to determine household allocations in standard household decision making models is a relative concept, that is not necessarily true of the ability to enforce a contract. In fact, many of the characteristics that would predict ability to punish, mostly related to closeness of migrant-recipient ties, will be the same on both sides. Certainly the punishment may be more costly for one or the other member of the pair, but we should expect to see impacts of information asymmetries in migrants and recipients with similar characteristics. Although bargaining power may also play a role in the formation of the contract between migrant and recipient, this framework abstracts away from it because enforceability concerns are so important in the migrant context.

III. Project Design

Testing the importance of the information asymmetries described above for choices made by migrants and their family members is difficult for two reasons. First, the probability of detection in any remittance choice and the communication costs may be correlated with unobserved characteristics of the migrant-recipient pair, making it difficult to causally identify the impacts. Second, observing either of these parameters (the probability of detection and communication costs) is difficult, if not impossible, in a standard survey context. To solve these problems I implement an experiment to test the framework described above within the context of a large field experiment on remittances and education among Salvadoran migrants in Washington, DC and their families in El Salvador.² Specifically, I exploit an unusual feature of this project; it involves surveys with matched pairs of migrants and family members (remittance recipients), allowing me to investigate the preferences and choices of both. In the experiment I

² Joint with Diego Aycinena and Dean Yang.

randomly vary the probability of detection and communication costs, allowing me to identify the causal impact of both of these on migrant and remittance recipient behavior.

Migrants were recruited in locations in Washington, DC metro area, primarily at the two area locations of the Salvadoran consulate.³ Survey work was conducted between late September 2011 and February 2012. They were interviewed while they were waiting for consular services, primarily passport renewal. Because the focus of the companion experiment was remittances and education participants were required to have a high school or college aged relative in El Salvador. Those who qualified and agreed to participate were administered a baseline survey followed by the offer of a product designed to facilitate the sending of remittances for education to El Salvador.⁴ The experimental questions were asked at the end of the baseline survey but before the randomized marketing treatment. Over the course of the baseline survey migrants identified a high school to college aged student in El Salvador whom they were interested in supporting. Interviews were subsequently conducted with the household of that student. If the student was 18 years of age or older the student himself was to be interviewed, for those students under 18 a guardian was identified to be interviewed. The El Salvador survey was conducted by phone in the days following the migrant survey in the United States;⁵ 82 percent of El Salvador families completed the survey. The experimental questions in the El Salvador survey were also conducted at the end of the interview.

The interventions were designed to mirror decisions that the respondents make on a regular basis. Migrants were asked to make a remittance decision: given \$600 how much would

³ The consulates are located in the Georgetown area of Washington, DC and Woodbridge, VA. 96% of migrants live in Washington, DC, Maryland or Virginia. The others live in states served by these consulate locations.

⁴ This was a randomized intervention and migrants received offers of different versions of the product depending on their assigned treatment group. Migrants in a control group received only information and no product offer.

⁵ Median number of days between US and El Salvador survey is 8. The El Salvador surveys concluded in mid March 2012 roughly two weeks after the conclusion of field work in the United States.

they like to send and how much would they like to keep. Family members were asked to decide how to spend a \$300 remittance that they receive. The amounts involved in the decisions (\$600 and \$300) were designed to be realistic as well; consequently the respondents were told that a lottery would be held to decide whose decision would be implemented. This allowed for the project to be financially feasible, while still incentivizing respondents to consider this to be a “real” decision.

In the study, migrants were told that they were being given the chance to win \$600 but would have to decide what to do with it. The choice was between keeping it for themselves and sending it to their family member in El Salvador. Migrants were randomly allocated into two groups: those who were told that their family members would be informed of their decision, and those who were told that their family member would not be informed of their decision. In all cases the family member referred to in the question was the person to be surveyed in El Salvador: that is the student if the student was 18 or over, and the student’s guardian if the student was under 18.

The respondent in the El Salvador phone survey was told that because his family member in the United States participated in the study, he (the respondent) now has the chance to win a remittance worth \$300. He had to decide what to spend the remittance on and was asked to choose among four spending categories: restaurant meals, education, daily expenses, and health expenses. Four categories were chosen for simplicity of implementation in the context of a phone survey and were split between investment type expenditures (education and health) and consumption type expenditures (restaurant meals and daily expenses). Two separate treatments were administered, the moral hazard experiment and the communication experiment.

Moral hazard experiment: Recipients were randomly allocated into two groups: those who were told that the migrant would be informed of their decision, and those who were told that the migrant would not be informed of their decision.

Communication experiment: During the US baseline the migrant was told about the lottery for recipients and asked what his preference was for how the recipient would spend the money. Again, recipients were randomly allocated into two groups: those who were told about the migrant's preference and those who were not.

These two treatments result in four treatment groups (the four possible combinations of the two treatments), allowing for analysis of the interaction of the two treatments. The four treatment groups are defined as follows:

- T1: Migrant **not told** recipient choice, recipient **not told** migrant preference
- T2: Migrant **not told** recipient choice, recipient **told** migrant preference
- T3: Migrant **told** recipient choice, recipient **not told** migrant preference
- T4: Migrant **told** recipient choice, recipient **told** migrant preference

The experimental questions were designed to be clear and focused. Importantly, the experimental component of the question was read to the respondent immediately before she made her choice to ensure that the information was salient in the moment of the decision. The question text can be found in Appendix 1. Appendix 1 shows the text used for all different treatments, however only the assigned text was provided to the interviewer to ensure that the correct treatment was applied. For all respondents in the told treatment group of the moral hazard experiment an effort was made to inform the recipient or migrant of the family member's choice. More important however is what the respondent believed would happen, and whether the threat of informing his family member was credible. Because the interviewer spent a substantial

amount of time at the beginning of the migrant survey collecting contact information for the recipient family and allowed the migrant to use a project phone during the interview to call the family member and tell them about the study, migrants were aware that their family members could indeed be contacted. Similarly, because recipients being interviewed knew that they had been contacted through the migrant they knew that their migrant family members could be contacted.

The randomization in this study was performed at the individual level. Surveys were pre-assigned unique identifiers before being sent into the field and this unique identifier was used for the migrant and the recipient surveys. Before implementation began both the migrant and recipient treatments were assigned to all possible identifiers to be used in the study. The randomization for the migrant treatment was stratified by groups of 12 unique identifiers in order to ensure that the treatment groups would be evenly spread across time. The recipient treatments were stratified both by the groups of unique identifiers and the migrant treatment group. Because the experiment was conducted in conjunction with the baseline survey it was not possible to stratify on individual baseline characteristics.

The moral hazard experiments (the migrant experiment and the first randomization in the recipient experiment) exogenously vary the probability of detection if the respondent chooses to deviate from the agreed upon contract, therefore measuring the extent to which respondents strategically take advantage of asymmetric information. The communication experiment (the second randomization in the recipient experiment) measures the importance of reducing communication costs for recipients' decisions and is a measure of the extent to which inadvertent information asymmetries are important.

Viewing the experimental setup in the context of the framework described in Section II results in the following hypotheses for respondent behavior.

Hypothesis 1:

Migrants in the treatment group where the recipients are informed of their choice should keep less than migrants whose choice is a secret. However, this effect should vary by migrant-recipient pair. Only in pairs where proxies for contract enforceability are high should an effect of the treatment be evident. In migrant-recipient pairs where enforceability is low, migrants will not be affected by the moral hazard treatment when they make the decision about how much money to keep and how much to send to the recipient. Their entire remittance is made up of the altruistic component.

Hypothesis 2:

Recipients in the treatment group where the migrants are informed of their choices should make choices that are closer to the migrants' preferences. However, again this effect should only be evident when enforceability is high. When enforceability is low, recipients will not be affected by the moral hazard treatment. The extent to which their choice matches the migrant's preferences is determined entirely through the non-negotiated portion of their decision.

Hypothesis 3:

The communication treatment should decrease the difference between the recipients' choices and the migrants' preferences regardless of enforceability, because lowering communication costs can impact the non-negotiated portion of the recipient's choice.

IV. Data and estimation strategy

A. Data

The migrant baseline survey collected extensive information on migrant and recipient demographics and family relationships both in the United States and in El Salvador. It contained detailed information on remittances both to the recipient household and other households and a set of questions to assess the quality of the migrant's relationship with the recipient household and the migrant's involvement in household affairs. The recipient survey was shorter and contained demographic information and some limited questions on remittances received from the migrant, as well as an extensive module on the education of children in the household. Table 1 shows summary statistics from both the migrant and the recipient baseline surveys. For the migrant survey statistics are shown for the full sample and the sample with a completed recipient survey. No obvious differences are apparent between the two samples; consequently the analysis sample is composed of 1,298 migrant-recipient pairs with completed El Salvador surveys and non-missing responses to the experimental questions.⁶ Results from the migrant experiment do not change when limiting the sample in this way. For the migrant experiment there are 648 migrants in the not told treatment group and 650 in the told treatment. In the recipient experiment there are 314, 324, 327 and 333 recipients in T1, T2, T3 and T4 respectively.

Despite the fact that the participation in the study is conditioned on having a high school or college aged relative in El Salvador the sample is fairly diverse. The migrants are half male and half female with an average age of 38. The mean number of years in the United States is 11, so the sample is composed largely of migrants who are established in the United States, but the extent of that establishment varies; the 10th percentile of the distribution is 5 years and 90th percentile is 21 years. 32 percent of migrants report having a son or daughter under 23 in El Salvador. 85 percent have sent remittances to the recipient household in the last 12 months and

⁶ Only 10 observations are lost to missing experimental data. I also drop 57 observations that were randomly assigned to receive a condensed version of the baseline survey during the first month of surveying.

69% report communicating with the recipient household at least weekly. The sample is also low income; half of the migrants report earning \$400 a week or less.⁷ Because of the structure of the project, the interviewed recipients are either the target student identified by the migrant (45 percent) or the student's guardian if the student is under 18 (40 percent). The remaining 15 percent of interviews were done with a different adult in the household if the indicated student or guardian could not be reached. The recipient sample is heavily female (68 percent) because identified student guardians tend to be female. Interestingly, average annual remittances reported by the recipient (\$1,522) are substantially lower than those reported by the migrant (\$2,629).

Table 1 also provides motivation for conducting the experiment. In order to judge the extent to which migrants and recipients have similar preferences over budgeting in the recipient household, both were asked to list the three most important budget priorities from a set list of seven categories: food and other basic expenses, health, education, savings, entertainment, home improvement and transport. Despite significant bunching of responses in the first three categories, only 48 percent of pairs report the same three priorities, suggesting that there is indeed room for negotiation over remittance spending. Three additional questions were included to test the migrant's knowledge of the recipient household. Only 24 percent of migrants could correctly report the student's GPA, 43 percent the mode of transport a student uses to get to school, and 43 percent the student's health status.⁸ Although this is not the same thing as recipients not understanding how migrants want them to spend remittances, it is evidence that knowledge does not necessarily flow freely between countries.

⁷ Respondents were asked to classify their income and that of a co-resident spouse into one of four categories: \$400 weekly or less, \$401 - \$600 weekly, \$601 to \$800 weekly, \$801 or more weekly.

⁸ The questions about student GPA and transport to school are only asked when the student is reported to be in school.

The random assignment of the treatments in this experiment allows for the causal identification of their impacts. Randomization should provide treatment groups that are the same on average so that any difference between the groups can be attributed to the treatment and not some pre-existing difference between groups. Tables 2 and 3 test whether the treatment groups are balanced on observed characteristics from the baseline survey for the treatment groups for the migrant experiment and the recipient experiment respectively. In Table 2 the means for both treatment groups are presented in the first two columns and the p-value from the test of whether or not those means are equal is in the third column. Overall the treatment groups are well balanced: only three of 38 differences are significantly different from zero at the 10 percent level. Table 3 shows the means by the four unique treatment groups in the recipient experiment. P-values for differences in means are presented in the last four columns of the table. The first column of p-values is from the F test for joint equality of the four treatment groups. Again the groups are well balanced, only two of the 38 p-values are less than 0.10. Because treatment group 4 (migrant told recipient choice, recipient told migrant preference) is predicted to have the largest impacts on recipients' decisions, I also show tests for the equality of means between group 4 and each of the other three groups. Again there are no differences that might be cause for concern.

Both Tables 2 and 3 also test whether attrition from the full sample of migrants to the estimation sample of migrant-recipient pairs with completed recipient surveys, non-missing experimental data and long format migrant questionnaire is related to treatment. Attrition is not significantly related to treatment for migrants or recipients.

B. Estimation strategy

The results of the migrant moral hazard experiment can be analyzed by estimating the simple regression:

$$kept_i = \delta + \alpha Told_i + X_i' \gamma + \varepsilon_i$$

where $kept_i$ is the dependent variable indicating how much of the \$600 the migrant chose to keep for themselves, or, alternatively, an indicator for whether or not the migrant chose to keep a positive amount. $Told_i$ is the treatment indicator for the moral hazard experiment, and it equal to one when the recipient is told of the migrant's choice, i.e. when the decision is not a secret. The coefficient α is the average difference between how much the migrant chooses to keep when the decision is a secret and when it is not. If α is negative, the migrant keeps less money for himself when $Told_i$ equals one, i.e. when the decision is not a secret. X_i is a vector of control variables and ε_i is the error term.

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

recipient's choices matched the migrant's preferences on 200 of the 300 dollars, but that they allocated the remaining 100 dollars to different categories.

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

$$diff_i = \delta + \beta_1 Told_i + \beta_2 Preference_i + X_i' \theta + \varepsilon_i$$

where $diff_i$ is the difference between migrant preferences and recipient choices in one of the four spending categories or the total difference. $Told_i$ is the treatment indicator for the moral hazard experiment and is equal to one when the migrant is told of the recipient's choice.

$Preference_i$ is the treatment indicator for the communication experiment and is equal to one when the migrant's preferences are communicated to the recipient before the recipient decides how to allocate the remittance funds. The coefficient β_1 is the difference in the difference between migrant preferences and recipient choices when the recipient choice is not a secret. β_2 is the difference in the difference between migrant preferences and recipient choices when the migrant's preferences are communicated to the recipient. If, as predicted, telling the migrant about the recipient's choice and communicating the migrant's preference cause the recipients to make choices more similar to the migrant's, then the differences will be smaller and β_1 and β_2 should be negative. X_i and ε_i are as in the migrant experiment.

An alternative specification that considers the interactions between the moral hazard and the communication experiments is as follows:

$$diff_i = \delta + \gamma_2 T2_i + \gamma_3 T3_i + \gamma_4 T4_i + X_i' \theta + \varepsilon_i$$

where $T2_i$, $T3_i$ and $T4_i$ are indicator variables for three of the four treatments and T1 is the omitted category.⁹ Because in T1 the recipients are told their decision will be a secret and are not

⁹ As a reminder the treatment group definitions are as follows:

informed of the migrant preference, the framework predicts that the average difference in T1 will be the highest of all the treatment groups. Consequently, γ_2 , γ_3 , and γ_4 should all be negative. Similarly, the average difference in T4 should be the smallest of the four groups so γ_4 should be more negative than γ_2 and γ_3 . The relationship between γ_2 and γ_3 will depend on the relative importance of the moral hazard treatment and the communication treatment.

The last component of the empirical strategy is the development of a strategy for measuring contract enforceability. Although it is not possible to directly measure the ability to punish in a migrant-recipient pair, it can be proxied through a measure of the closeness of migrant-recipient ties. The logic is that in pairs with close ties the obligations to one another are likely to be greater and the social norms of remittance sending are likely to be higher. Additionally, pairs with close ties may simply care more about what the other thinks giving them the added tool of emotional punishment. I create an index for the closeness of migrant-recipient ties to proxy for enforceability by leveraging data collected in the baseline survey. I select three variables to proxy for closeness: whether the migrant has been in the US for fewer years than the sample median, whether the migrant has a child under 23 in El Salvador, and whether or not the migrant reports communicating with the recipient household at least once a week. I create a closeness score using the first principal component of these three variables.

Each of these three variables characterizes a different aspect of ways in which migrants and recipients can have close ties. Migrants who have been gone less time are more likely to have debts to repay and are more likely to have plans to return home. Additionally, ties simply deteriorate with time away from the home country. Migrants with non-adult children in El

- T1: Migrant **not told** recipient choice, recipient **not told** migrant preference
- T2: Migrant **not told** recipient choice, recipient **told** migrant preference
- T3: Migrant **told** recipient choice, recipient **not told** migrant preference
- T4: Migrant **told** recipient choice, recipient **told** migrant preference

Salvador are more likely to have close family in El Salvador which implies stronger social norms regarding remittance behavior and enhances the possibility of emotional punishment. Frequent communication is a sign of closeness and also provides an opportunity for monitoring and punishment. In order to examine how the treatment effects vary by enforceability I split the sample into high and low closeness groups using the median of the closeness index and rerun the regressions described above.¹⁰

V. Results

A. Migrant experiment

Figure 1 shows the distribution of the amount kept by migrants, separately by treatment group. Choices for migrants for whom the choice was secret are in panel A and choices for migrants for whom the choice was shared with the recipient are in panel B. Because the experimental protocol limited migrants to splitting the money in 100 dollar increments the distributions are discrete. The first observation to be made from these figures is that the migrants send large amounts: over half of the migrants in both treatment groups choose to send the entire \$600 to the recipient. The other smaller spike in both distributions is at \$300 where migrants decide to split the money equally between themselves and the recipient. Despite the fact that the two distributions follow the same basic shape, differences are evident. Specifically the spike at zero is smaller in Panel A and the percent of migrants selecting to keep \$200 and above is higher. Although the large percentage of migrants who send everything even in the recipients not told treatment group suggests that the altruistic component of remittances is high, remittance contracts also seem to play a role. Migrants who can keep their choice a secret are choosing to send less home.

¹⁰ The index is not particularly sensitive to choice of variable. Usage of other variables that logically approximate closeness give similar results. A simple index created by summing across the indicator variables also does not change the results.

The difference between treatment groups is made more explicit in Table 4 which gives the mean of the amount kept by the migrant by treatment group. Because of the large number of migrants who send everything, I also consider an indicator variable that is equal to one if the migrant chose to keep any of the money for himself. Migrants whose choice is kept secret keep \$24 more for themselves on average and are 5 percentage points more likely to keep anything for themselves. The table also shows the p-values from a t-test for difference in means; both differences are statistically significant.

These results are replicated in Table 5 using a regression framework that estimates regression equation 1 from Section IV of this paper. Columns 1 and 2 show results for amount kept by the migrant and columns 3 and 4 for whether or not the migrant kept anything. Columns 1 and 3 are a simple regression of the dependent variable on treatment and columns 2 and 4 include demographic control variables and survey month fixed effects. The results are robust to the inclusion of control variables, although the impact of treatment on the migrant keeping anything is no longer significant when controls are included. Migrants keep \$20 to \$24 less when their choice is not a secret, a result that is 13 to 15 percent of the mean of the not told group. Additionally, migrants are 4 to 5 percentage less likely to keep anything when their decision will be communicated to the recipient. This is approximately 10 percent of the not told mean, but is only marginally significant.

Table 5 also reports the coefficients on the demographic control variables included in columns 2 and 4. Only three characteristics predict the migrant's choice, the most important of which is gender of the migrant. Female migrants keep on average \$35 more than male migrants.¹¹ Migrants who have been in the United States for longer keep less for themselves, possibly they are more established and therefore have higher income. Finally, migrants who live

¹¹ Although women keep more on average than men the effect of the treatment does not vary by gender.

with their spouses keep more than those who don't. This is likely because they have greater financial obligations in the United States and are more likely to have their whole immediate family with them in the United States.

The results in Table 5 show that information asymmetries do matter at least somewhat for migrants making remittance decisions, however they do not shed light on whether this effect varies by contract enforceability. Table 6 presents results to address this question. The sample is split into two subsamples at the median of the closeness index discussed in Section IV. Columns 1 and 2 present results from the subsample below the median of the index and columns 3 and 4 from the subsample at or above the median. Columns 2 and 4 include control variables and survey month fixed effects. Panel 1 presents the results for amount kept by the migrant and panel 2 the results for whether or not the migrant kept anything.

As predicted, there is no effect of the treatment present for migrant-recipient pairs without strong ties. The point estimates in columns 1 and 2 are small, positive and do not approach statistical significance. In contrast, the results for the high closeness subsample are large, negative and highly statistically significant. When the recipient is told of the migrant's choice, migrants keep \$49 to \$54 less than when the recipient is not told; migrants are keeping 30 percent less than in the not told treatment group. Another way to understand the magnitude of the coefficient is to think of the amount sent to the recipient in the not told treatment group as the altruistic portion of the remittance payment and the increase in the remittance from the not told to the told group as the negotiated portion of the remittance. These numbers are \$422¹² and \$54 respectively, meaning that, in this setting, on average the negotiated portion of the remittance makes up 11 percent of the total remittance.

¹² \$600 minus the average amount kept by the migrant.

Interestingly, the strength of the treatment effect increases with the closeness index. Figure 2 graphs the mean amount kept by treatment group and quartiles of the closeness index. There are no significant differences between the treatment groups in quartiles one or two and the difference between the not told and told treatment groups doubles from quartile three to quartile four. This observation lends credence to the use of the closeness index as a measure of enforceability. Figure 2 also allows for the comparison of the levels (not just the treatment effects) of the amount kept across different values of the closeness index. One might expect that close ties would lead to lower average amounts kept by the migrant and greater altruism towards their family members. However, the averages in the top two quartiles are not significantly lower than in the bottom two quartiles and are actually higher in the case of the not told treatment group. One possible explanation of this is that because migrants in the upper half of the distribution have stronger remittance relationships with the recipients¹³ they feel more day to day to pressure to send money home and therefore are more likely to approach the experimental decision as they would their regular decision about how much money to send.

B. Recipient experiment

Mean amounts allocated to different spending categories by recipients and migrants are presented in Table 7. The first four columns show the mean amounts by the four recipient treatment groups and the fifth column shows the means of the preferences reported by the migrant. Across both recipients and migrants education is the most popular choice, an interesting pattern given that education is not usually thought to be the number one destination of remittance funds. However, it is not unsurprising in this context given that participants answered this question at the conclusion of a survey that was rather heavily focused on questions about

¹³ Average annual remittances are \$1,696 for the bottom half of the closeness distribution and \$3,782 for the top half of the closeness distribution.

education, meaning that they may have been primed to consider education. This is not necessarily a problem as there is no reason to believe that either migrants or recipients were more primed than the other. Daily expenses are the next most popular category, followed closely by health and finally restaurant meals. As discussed previously, unless clear differences between migrant and recipient preferences are evident on average an analysis of the impact of treatment on amounts allocated to different categories will not be interesting. That is largely true here. Although migrants allocate less to education than recipients and more to daily expenses, health expenses and restaurant meals, regressions of treatment on recipient choices over amounts do not reveal any interesting patterns (results not shown, available from the author on request).

A more powerful analysis utilizes the data collected from both the migrant and the recipient to analyze how the treatments affect the pair-level difference between their choices. Table 8 displays the mean differences by recipient treatment. In order to more easily understand if significant differences are present the results are displayed separately for the moral hazard experiment and the communication experiment; the means from the moral hazard experiment and p-values testing the equality of those means are in the first three columns and the corresponding information for the communication experiment is in the last three columns. Means of the differences for the four spending categories as well as the total difference are shown. In both experiments the prediction is that the difference will be smaller in the “told” treatment group. When probability of detection is high or when recipients are well informed they will more greatly adhere to the migrant’s preferences.

This prediction is not borne out for the moral hazard experiment in the full sample. For all spending categories and the total difference, the means across the two treatment groups are essentially equal. The same is not true for the communication experiment. The difference when

recipients are told the migrant's preferences is smaller in all categories. Although of the spending categories only the difference for education is significant, importantly so is the total difference, implying that migrants and recipients are getting closer together overall. The \$14 reduction in the total difference is driven by the difference in education spending with the corresponding reductions in differences in other categories being split between daily and health expenses and, to a lesser extent, spending on restaurant meals.¹⁴

Table 9 shows these results in regression format and adds control variables. Panel 1 shows the results from estimating regression equation 2 that estimates the result of each experiment separately and panel 2 presents the results of estimating regression equation 3 which considers the separate effects of the four distinct treatment combinations. T1 is the omitted category. The dependent variables in columns 1 through 4 are the recipient-migrant differences in restaurant spending, education spending, spending on daily expenses, and health spending respectively. The dependent variable in columns 5 and 6 is the total recipient-migrant difference. Column 6 adds control variables and survey month fixed effects. The control variables are the same as those presented in Table 5 with the addition of a control for number of days between the migrant and the recipient survey. Survey month fixed effects for the month in which the recipient survey was conducted are also included.

The results in panel 1 replicate the results from Table 8 almost exactly; as expected given the randomization controlling for the other treatment does not change either estimate. In addition, the results are robust to the addition of control variables (results for individual spending categories not shown but available upon request). The results in panel 2 show that the same conclusion is drawn when considering the separate impacts of the four groups. Focusing on the

¹⁴ Mechanically the sum of the differences between the not told and told groups over the four categories must be equal to twice the difference in the total difference.

total difference results in columns 5 and 6, the coefficients on the T2 and T4 are both negative and significant meaning that the difference in these groups is smaller than in T1, which is the omitted category.¹⁵ T2 and T4 are the groups where recipients are informed of migrant preferences and the estimated coefficients are quite similar in magnitude to the coefficient on the communication treatment in panel 1. The coefficient on T3 is small and statistically indistinguishable from zero. Essentially this specification that considers the separate effects of the interacted treatment groups shows the same pattern as the specification that considers the treatments separately. Revealing the migrant's preferences lessens the total difference between recipient choices and migrant preferences by \$14 or approximately ten percent of the T1 mean. In the full sample displayed here, the moral hazard treatment appears to have no effect.

Before analyzing the meaning of these results it is important to examine how the treatment varies by closeness. Table 10 shows the results of the recipient experiment for the subsamples below the median of the closeness index and at or above the median of the closeness index. This table focuses only on the total recipient-migrant difference. Columns 1 and 2 show results for the low closeness subsample and columns 3 and 4 for the high closeness subsample. Columns 2 and 4 include control variables and survey month fixed effects. As in Table 9 panel 1 examines the effects of the two experiments separately and panel 2 looks at the impact of each treatment combination separately.

The coefficient on the moral hazard treatment variable in the low closeness subsample is 17, notably significant and positive, indicating that when migrants are told of the recipient choice, the difference increases by \$17. This is a puzzling result as it is counter-intuitive for an

¹⁵ As a reminder the treatment group definitions are as follows:

T1: Migrant **not told** recipient choice, recipient **not told** migrant preference

T2: Migrant **not told** recipient choice, recipient **told** migrant preference

T3: Migrant **told** recipient choice, recipient **not told** migrant preference

T4: Migrant **told** recipient choice, recipient **told** migrant preference

increased probability of detection to result in increased deviation from migrant preferences. The coefficient on the communication treatment is negative as predicted; even when a contract cannot be enforced, recipients who value the migrant's opinion but have bad information about the migrant's preferences will react to the communication treatment. The coefficients are just short of significant (the p-value in columns 1 and 2 are 0.106 and 0.100 respectively).

Understanding how these patterns vary by each unique treatment cell might help to understand the perplexing positive effects of the moral hazard treatment. The coefficient on T2 is -12, negative and similar in magnitude to the coefficient on the communication treatment variable in panel 1. The positive impact of the moral hazard treatment is evident in the coefficient on T3 and is essentially equal to the coefficient on the moral hazard treatment in panel 1. The coefficient on T4 is 6, which is close in magnitude to the sum of T2 and T3. In fact, if the specification in Panel 1 is re-estimated to include an interaction term between the two treatments, that term is indistinguishable from zero.

The results in the high closeness subsample are consistent with the hypotheses formed by the framework. The results in Panel 1 show that both the moral hazard experiment and the communication experiment have negative and significant impacts on the difference between recipient choices and migrant preferences. In the specification with covariates the coefficient on the moral hazard treatment is -14.5 or approximately 10% of the T1 mean difference. The coefficient on the communication treatment is -19.8 or approximately 13% of the T1 mean difference. The coefficients are not however significantly different from each other; it does not appear that one has a greater effect than the other. When examining the effects separately by treatment combination in Panel 2 the same pattern is visible. The effect of T4 is very large (-34.3 in the specification with covariates) but is close the sum of the coefficients on T2 and T3. As in

the low enforceability sample the coefficient on a treatment interaction term in the Panel 1 specifications is close to zero and insignificant. The effects of the two treatments are operating independently from one another. This suggests that communication issues do not affect the negotiated portion of the recipient's compliance with the migrant's wishes; if recipients were deviating inadvertently then the interaction term on the treatments should be negative. Instead the communication treatment appears to be operating on the non-negotiated portion of the recipient's decision.

Another interesting dimension along which to examine the results of the recipient experiment is similarity in migrant and recipient preferences. Logically, if migrants and recipients have the same preferences than neither treatment should have a large effect as recipients will already be doing what migrants want since it is what they want as well. A question on the baseline survey for both migrants and recipients helps to differentiate between migrant-recipient pairs that have similar preferences for spending and those who do not. As described in Section III migrants and recipients are both asked to choose the three most important budgetary priorities for the recipient household from a list of seven categories: food and other basic expenditures, healthcare, education, savings, entertainment, house improvements and transportation. I view migrants and recipients as having similar preferences if they list the same three categories as their top budgetary priorities.

Figure 3 shows the averages of the total difference by similarity in preferences, closeness of ties and treatment group. Panel A shows means for the moral hazard treatment and panel B shows means for the communication experiment. For the moral hazard experiment, in the high closeness group, the effects of the treatment are only evident for pairs who do not have similar preferences. The effect for the low closeness group is positive regardless of preference similarity.

For the communication treatment, the effect of revealing preferences is also strongest amongst those pairs who do not have similar preferences, and this is true across values of the closeness index. Although this preference measure is not perfect and it is surprising that, for example, the mean differences are not lower in pairs with similar preferences, the way that the treatment effect varies with this variable is supportive of the interpretation given to the results in this paper.

C. Comparison of experiments

One important aspect of this experiment is that it allows for the examination of the presence of information asymmetries in matched pairs of migrants and recipients, and it is therefore interesting to ask whether the results of the two experiments can be compared. Certainly the results show that information conditions are important for both migrants and recipients, but is one group more responsive than the other? Because the experiments were designed to mimic the real life decisions made by migrants and recipients the experiments are not directly comparable. However there is some suggestive evidence that migrants, at least in the limited context of the decisions made in this experiment, may be more responsive to pressure from recipients than recipients are to pressure from migrants.

First, migrants send much more to recipients than recipients comply with migrant preferences. 58.5 percent of migrants in the told treatment group in the full sample send everything, while only 15 percent of recipients in T4 (told, told) match the migrants' preferences exactly. Keep in mind that recipients in this group are told exactly what the migrant preferences are so it would be very easy for them to simply agree with what the migrant had selected. Second, as shown in Figure 2, migrant responsiveness to the treatment increases sharply with the closeness index, while recipient responsiveness to the moral hazard treatment does not behave as monotonically. This suggests that at high levels of closeness recipients have extensive power to

pressure migrants to send remittances. Although not conclusive, this implication that recipient power to negotiate remittance contracts is substantial suggests that policy solutions that offer migrants the ability to control remittance spending should be complemented by tools that mitigate moral hazard on the migrant side as well. Additionally, the results from the communication experiment imply that improved communication could be an inexpensive method of reducing the difference between recipient choices and migrant preferences.

VI. Conclusion

This paper analyzes a set of experiments designed to test for the existence and the importance of information problems in migrant households. Specifically, an experiment among Salvadoran migrants in the Washington, DC area examines the extent to which moral hazard is a factor in remittance decisions. When choosing how much of a potential prize of \$600 to keep and how much of it to send to family in El Salvador, migrants keep more for themselves when the probability that their family member will be made aware of their choice is low. Consistent with a framework in which migrants and recipients negotiate over remittance amounts the effects are only present when an index that proxies for the enforceability of the remittance contract with closeness of migrant recipient ties is high.

A second experiment conducted among the family members of the migrant sample examines the role of moral hazard in the decisions remittance recipients make about how to spend the transfers that they receive by varying whether or not the migrant will be informed of how the recipient chose to allocate a potential prize of \$300. A simultaneous intervention tests whether lowering communication costs by revealing the migrant's specific preferences over the spending decision causes recipients to more closely adhere to these preferences. Recipients also strategically deviate when the chance they will be discovered is low; the difference between the

recipient's choice and the migrant's preference is higher when the choice is secret, but again this effect is only present when the closeness index is high. Lowering communication costs by revealing migrant preferences brings recipient choices closer to migrant preferences for both low and high closeness groups. Although communication barriers do impact recipient choices, there is no impact of the interaction of the communication and the moral hazard treatment, suggesting that there is little misinformation about the negotiated portion of the recipient choice. Rather some recipients who want to follow the migrant's preferences are under or misinformed about what the migrant would want.

These results are the first that show that moral hazard can impact the allocation of resources within the family at various stages in the resource allocation process, in this case both the sharing and the spending phases. They also show that moral hazard can affect different decision makers in the family, the migrant and the recipient. This differs from the finding in Ashraf (2009) where results are concentrated on one stage of resource allocation and one decision maker, specifically the household financial manager. The paper also takes the question of how information asymmetries can affect the family outside the spousal relationship to a more extended family network, a context that is very relevant to many households in developing countries. The results provide support for the view of household resource allocation as an informal contract that can be effective only when the ability to enforce that contract exists. This is an important result for policy makers because it suggests that while information asymmetries are important, they may not be relevant for all families where resources are shared.

This is also the first study to specifically manipulate information asymmetries in migrant households. The study of how moral hazard and communication barriers affect migrant households is important because the magnitude of remittance flows and their economic

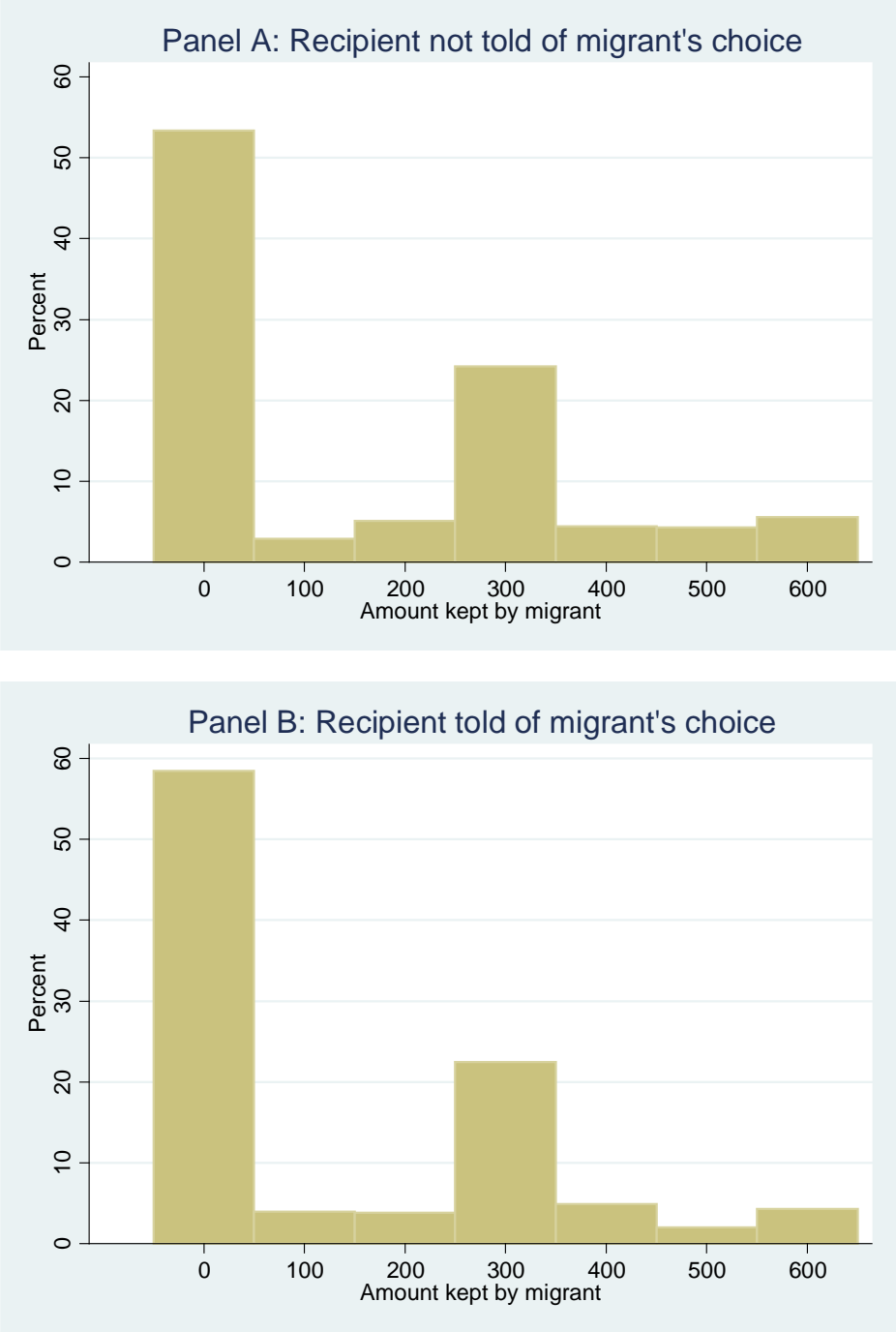
importance is large while decisions about them are made in a context where information problems are especially acute. Although previous work in this area has focused on how migrants monitor the actions of recipients or seek to increase control over the remittances they send, this study additionally recognizes that recipients have influence over how much is sent home by the migrant. In the experimental context migrants respond to monitoring by the recipient as much as if not more than recipients respond to monitoring by migrants.

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Figure 1: Distribution of amount kept by migrant by treatment group



Notes: Sample is observations with long form migrant survey, non-missing values for experimental questions, and completed recipient survey.

Figure 2: Amount kept by migrant, by quartile of closeness index

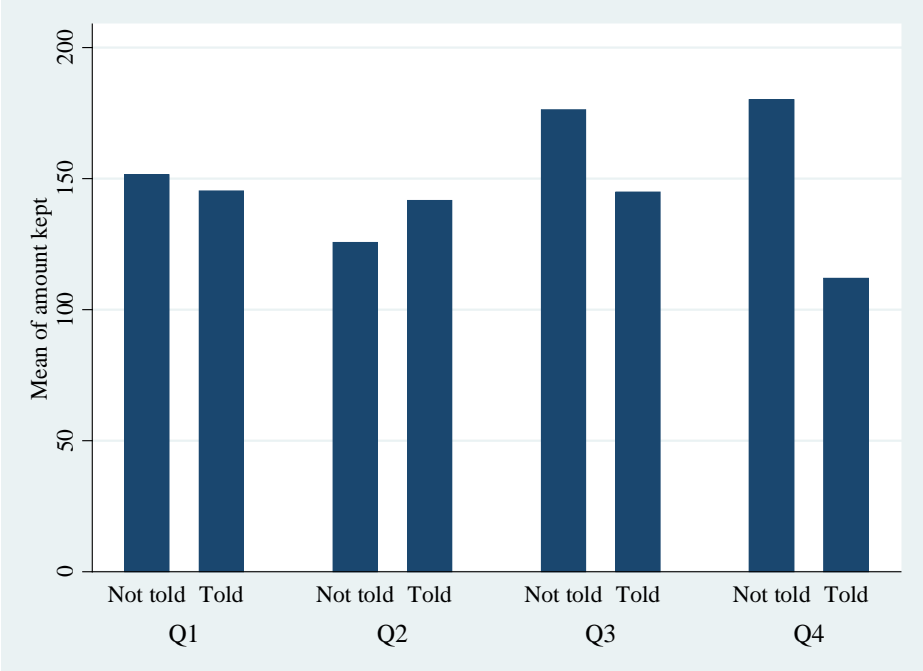
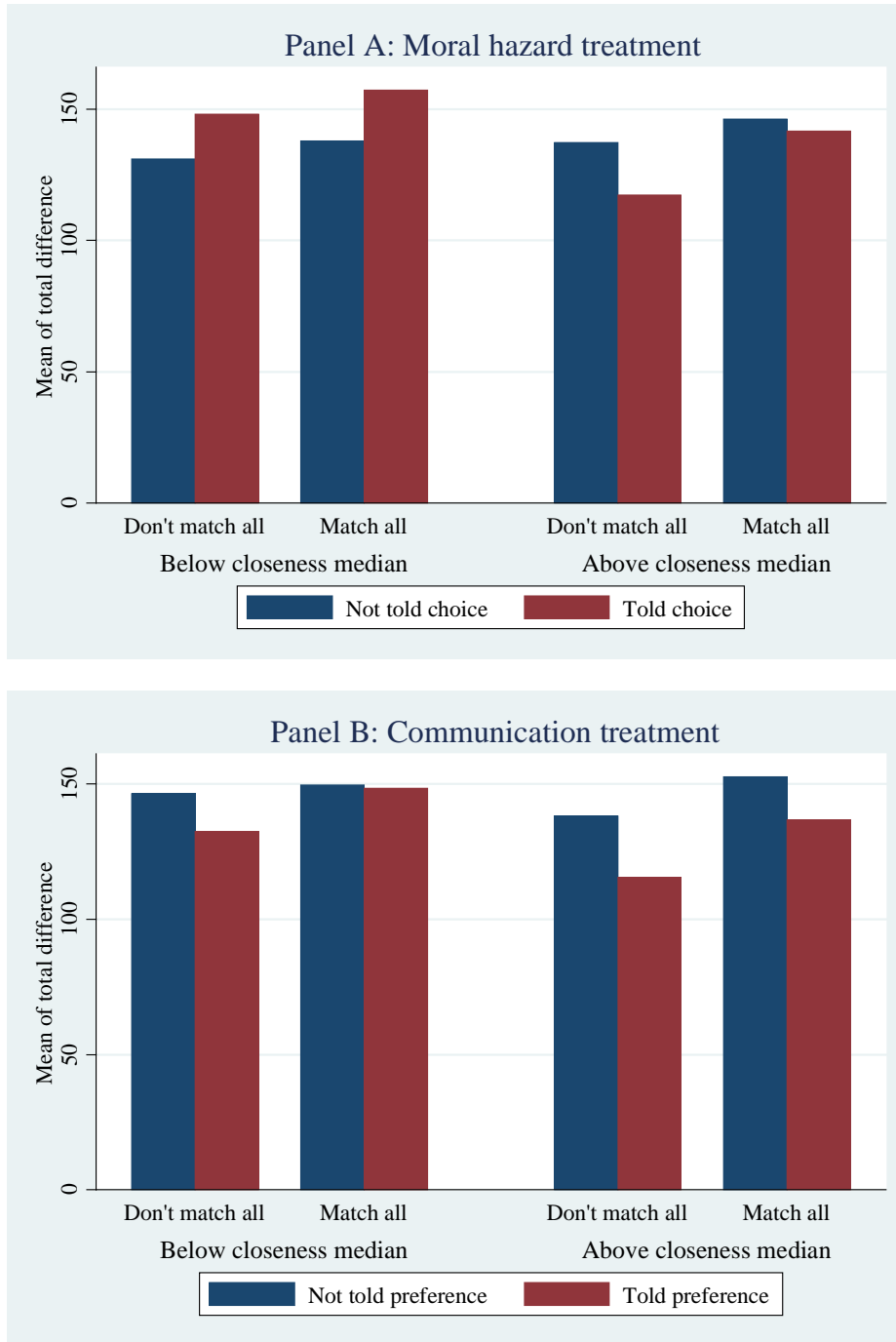


Figure 3: Total difference by closeness index and preference matching



Notes: Sample is observations with long form migrant survey, non-missing values for experimental questions, completed recipient survey, and non-missing closeness index and budget preference variable. Closeness index is constructed from the first principal component of whether or not migrant has been in the US fewer years than the sample median, whether or not the migrant has a child under 23 in El Salvador, and whether or not the migrant communicates at least weekly with the recipient hh. Migrant-recipient pairs are in the match all category if they list the same three categories as their top three priorities for household budgeting.

Table 1: Baseline summary statistics

	<i>All Observations</i>			<i>Observations with completed recipient survey</i>		
	Mean	SD	N	Mean	SD	N
<i>Baseline variables from migrant survey</i>						
Migrant is female	0.50	0.50	1,581	0.51	0.50	1,298
Age of migrant	36.83	9.41	1,538	36.92	9.29	1,264
Migrant is married	0.62	0.48	1,575	0.63	0.48	1,294
Migrant can read and write	0.96	0.20	1,554	0.96	0.20	1,275
Migrant's years of education	9.08	4.67	1,560	9.01	4.67	1,282
Migrant's years in the US	11.31	6.38	1,577	11.13	6.27	1,295
Migrant's total number of children	2.28	1.69	1,579	2.34	1.69	1,296
Migrant's children in El Salvador	1.01	1.43	1,577	1.07	1.47	1,294
Migrant's children in US	1.26	1.32	1,575	1.25	1.29	1,293
Migrant's hh size in US	4.32	1.98	1,581	4.36	1.96	1,298
Migrant lives with spouse	0.49	0.50	1,579	0.50	0.50	1,296
Migrant has child under 23 in El Salvador	0.32	0.47	1,581	0.34	0.47	1,298
Migrant has worked in last 12 months	0.89	0.31	1,581	0.89	0.31	1,298
Migrant in lowest income bracket	0.52	0.50	1,429	0.53	0.50	1,181
Migrant sent remittances to recipient hh	0.85	0.36	1,580	0.87	0.34	1,297
Migrant's annual regular remittances to recipient hh (\$)	2,298	2,907	1,565	2,440	2,998	1,283
Migrant's annual irregular remittances to recipient hh (\$)	337	706	1,575	344	707	1,293
Migrant's annual remittances to recipient hh (\$)	2,629	3,199	1,563	2,777	3,284	1,281
Migrant's annual total remittances to other hhs (\$)	1,096	1,905	1,568	1,122	1,944	1,285
Migrant has preference for how remittances are spent	0.27	0.44	1,542	0.28	0.45	1,268
Migrant communicates with surveyed hh at least weekly	0.69	0.46	1,578	0.71	0.45	1,295
Migrant is "very well" informed about recipient hh	0.26	0.44	1,532	0.27	0.45	1,258
Migrant is "well" informed about recipient hh	0.53	0.50	1,532	0.54	0.50	1,258
Migrant participates in decisions about remittance spending	0.38	0.48	1,137	0.39	0.49	970
Migrant desires more influence in recipient hh	0.35	0.48	1,569	0.36	0.48	1,290
<i>Baseline variables from recipient survey</i>						
Recipient is target student				0.45	0.50	1,298
Recipient is student's guardian				0.40	0.49	1,298
Recipient is female				0.68	0.47	1,298
Age of recipient				34.20	15.84	1,295
Recipient is married				0.36	0.48	1,298
Recipient years of education				9.38	5.27	1,293
Recipient lives in urban area				0.43	0.50	1,298
Recipient's hh size				4.99	2.04	1,296
Annual remittances received from migrant (\$)				1,522	1,916	1,203
<i>Baseline comparison variables</i>						
Migrant and recipient report same hh budget priorities				0.48	0.50	1,231
Migrant and recipient report same student GPA				0.24	0.43	1,041
Migrant and recipient report same student mode of transport				0.43	0.50	1,107
Migrant and recipient report same student health status				0.43	0.50	1,295

Notes: All observations sample is all respondents who answered the long form migrant survey with non-missing data for migrant experimental questions. Completed recipient survey sample additionally conditions on completion of the recipient survey and non-missing migrant and recipient information for recipient experimental questions. Migrants in the lowest income bracket chose \$400 or less as the weekly income of themselves plus their co-resident spouses. The other categories were \$401 -600, \$601 - 800 and \$801 and above. Annual irregular remittances are remittances sent for special occasions or emergencies. Information about if the migrant participates in remittance spending decisions was asked only of those who report sending regular remittances to recipient hh. The recipient variables in all cases refer to the person completing the recipient survey. The baseline comparison variables were asked on both surveys and are equal to one if the migrant and recipient responses match. Both respondents were asked to choose the three most important budget priorities for the recipient hh from a list of seven categories. In the case of GPA, mode of transport, and health status the student refers to the student identified by the migrant during the baseline survey. GPA and mode of transport were only asked when student was reported to be in school.

Table 2: Means of baseline variables by treatment group: Migrant experiment

	<i>Treatment group mean: Recipient is...</i>		P-value for difference of means: Not told and Told
	Not told migrant choice	Told migrant choice	
<i>Attrition</i>			
El Salvador survey completed	0.82	0.83	0.819
<i>Baseline variables from US Survey</i>			
Migrant is female	0.53	0.49	0.165
Age of migrant	36.90	36.94	0.941
Migrant is married	0.61	0.65	0.151
Migrant can read and write	0.95	0.97	0.150
Migrant's years of education	9.01	9.00	0.966
Migrant's years in the US	10.90	11.37	0.178
Migrant's total number of children	2.34	2.34	0.956
Migrant's children in El Salvador	1.03	1.10	0.365
Migrant's children in US	1.28	1.22	0.410
Migrant's hh size in US	4.34	4.38	0.720
Migrant lives with spouse	0.50	0.50	0.956
Migrant has child under 23 in El Salvador	0.32	0.37	0.059
Migrant has worked in last 12 months	0.90	0.89	0.943
Migrant in lowest income bracket	0.53	0.53	0.886
Migrant sent remittances to recipient hh	0.87	0.86	0.586
Migrant's annual regular remittances to recipient hh (\$)	2,494	2,386	0.520
Migrant's annual irregular remittances to recipient hh (\$)	354	334	0.627
Migrant's annual remittances to recipient hh (\$)	2,828	2,726	0.579
Migrant's annual total remittances to other hhs (\$)	1,059	1,185	0.245
Migrant has preference for how remittances are spent	0.30	0.27	0.148
Migrant communicates with surveyed hh at least weekly	0.73	0.69	0.057
Migrant is "very well" informed about recipient hh	0.25	0.29	0.164
Migrant is "well" informed about recipient hh	0.56	0.53	0.282
Migrant participates in decisions about remittance spending	0.40	0.38	0.570
Migrant desires more influence in recipient hh	0.36	0.36	0.928
<i>Baseline variables from El Salvador survey</i>			
Recipient is target student	0.45	0.45	0.907
Recipient is student's guardian	0.42	0.38	0.160
Recipient is female	0.69	0.67	0.331
Age of recipient	35.09	33.31	0.043
Recipient is married	0.36	0.36	0.941
Recipient years of education	9.22	9.54	0.285
Recipient lives in urban area	0.43	0.44	0.649
Recipient's hh size	4.90	5.08	0.111
Annual remittances received from migrant (\$)	1,491	1,553	0.580
<i>Baseline comparison variables</i>			
Migrant and recipient report same hh budget priorities	0.48	0.48	0.926
Migrant and recipient report same student GPA	0.25	0.24	0.709
Migrant and recipient report same student mode of transport	0.44	0.42	0.573
Migrant and recipient report same student health status	0.42	0.44	0.625

Notes: Sample is observations with long form migrant survey, non-missing values for experimental questions, and completed recipient survey. Sample size for each comparison of means varies slightly by missing values for each variable. The percentage of missing values for each variable is also tested for balance across treatment groups with no significant differences. Other notes on variable construction are as in Table 1. P values come from a regression of each variable on treatment, with robust standard errors.

Table 3: Means of baseline variables by treatment group: Recipient experiment

	Treatment group means				P-values for difference of means			
	T1:	T2:	T3:	T4:	T1 = T2 = T3 = T4	T1 = T4	T2 = T4	T3 = T4
	Migrant not told recipient choice, recipient not told migrant preference	Migrant not told recipient choice, recipient told migrant preference	Migrant told recipient choice, recipient not told migrant preference	Migrant told recipient choice, recipient told migrant preference				
<i>Attrition</i>								
El Salvador survey completed	0.81	0.82	0.84	0.83	0.726	0.344	0.705	0.924
<i>Baseline variables from US Survey</i>								
Migrant is female	0.47	0.56	0.51	0.50	0.123	0.594	0.075	0.697
Age of migrant	36.44	36.68	37.33	37.21	0.576	0.307	0.475	0.871
Migrant is married	0.63	0.66	0.63	0.59	0.310	0.333	0.058	0.335
Migrant can read and write	0.95	0.95	0.97	0.95	0.379	0.851	0.917	0.152
Migrant's years of education	8.96	9.07	8.99	9.01	0.990	0.892	0.859	0.961
Migrant's years in the US	11.42	10.96	10.86	11.30	0.619	0.820	0.488	0.356
Migrant's total number of children	2.24	2.35	2.49	2.28	0.259	0.775	0.588	0.120
Migrant's children in El Salvador	0.86	1.16	1.22	1.02	0.005	0.121	0.208	0.090
Migrant's children in US	1.37	1.17	1.26	1.22	0.238	0.157	0.572	0.731
Migrant's hh size in US	4.50	4.36	4.35	4.22	0.349	0.070	0.345	0.377
Migrant lives with spouse	0.49	0.52	0.51	0.47	0.604	0.656	0.198	0.373
Migrant has child under 23 in El Salvador	0.32	0.34	0.37	0.34	0.592	0.573	0.930	0.411
Migrant has worked in last 12 months	0.89	0.88	0.89	0.91	0.652	0.521	0.207	0.466
Migrant in lowest income bracket	0.50	0.52	0.55	0.54	0.598	0.354	0.695	0.700
Migrant sent remittances to recipient hh	0.86	0.87	0.89	0.86	0.695	0.764	0.847	0.392
Migrant's annual regular remittances to recipient hh (\$)	2,243	2,622	2,385	2,503	0.393	0.240	0.638	0.619
Migrant's annual irregular remittances to recipient hh (\$)	374	389	333	283	0.139	0.094	0.045	0.264
Migrant's annual remittances to recipient hh (\$)	2,584	3,015	2,709	2,794	0.417	0.385	0.423	0.743
Migrant's annual total remittances to other hhs (\$)	1,037	1,233	1,097	1,119	0.695	0.562	0.470	0.877
Migrant has preference for how remittances are spent	0.28	0.30	0.30	0.25	0.387	0.367	0.143	0.131
Migrant communicates with surveyed hh at least weekly	0.70	0.75	0.71	0.68	0.193	0.622	0.039	0.475
Migrant is "very well" informed about recipient hh	0.26	0.30	0.23	0.29	0.177	0.441	0.771	0.084
Migrant is "well" informed about recipient hh	0.54	0.54	0.56	0.54	0.904	0.960	0.980	0.545
Migrant participates in decisions about remittance spending	0.37	0.44	0.38	0.39	0.440	0.675	0.273	0.822
Migrant desires more influence in recipient hh	0.34	0.35	0.38	0.37	0.637	0.384	0.583	0.747
<i>Baseline variables from El Salvador survey</i>								
Recipient is target student	0.45	0.43	0.47	0.46	0.755	0.914	0.480	0.709
Recipient is student's guardian	0.39	0.44	0.38	0.39	0.443	0.907	0.213	0.768
Recipient is female	0.70	0.66	0.67	0.69	0.799	0.788	0.510	0.682
Age of recipient	34.56	34.33	34.04	33.90	0.952	0.592	0.719	0.911
Recipient is married	0.40	0.41	0.30	0.34	0.006	0.143	0.060	0.208
Recipient years of education	9.36	9.08	9.25	9.80	0.332	0.295	0.077	0.182
Recipient lives in urban area	0.40	0.41	0.44	0.48	0.185	0.042	0.085	0.266
Recipient's hh size	5.10	4.98	5.02	4.89	0.621	0.194	0.583	0.427
Annual remittances received from migrant (\$)	1,542	1,526	1,426	1,592	0.725	0.760	0.668	0.268
<i>Baseline comparison variables</i>								
Migrant and recipient report same hh budget priorities	0.46	0.46	0.48	0.52	0.396	0.132	0.147	0.265
Migrant and recipient report same student GPA	0.26	0.23	0.23	0.26	0.774	0.916	0.559	0.438
Migrant and recipient report same student mode of transport	0.39	0.43	0.47	0.42	0.302	0.557	0.880	0.198
Migrant and recipient report same student health status	0.45	0.41	0.41	0.45	0.521	0.846	0.282	0.221

Notes: Sample is observations with long form migrant survey, non-missing values for experimental questions, and completed recipient survey. Sample size for each comparison of means varies slightly by missing values for each variable. The percentage of missing values for each variable is also tested for balance across treatment groups with no significant differences. Other notes on variable construction are as in Table 1. P values come from a regression of each variable on treatment, with robust standard errors.

Table 4: Means of amount kept variables by treatment group: Migrant experiment

	<i>Treatment group mean:</i> <i>Recipient is...</i>		P-value for difference of means: Not told and Told
	Not told migrant choice	Told migrant choice	
Amount kept by migrant (\$)	158.64	134.62	0.020
Migrant keeps any money	0.47	0.42	0.066
<i>Number of observations</i>	648	650	

Notes: Amount kept by migrant is the amount that migrants chose to keep when splitting \$600 between themselves and recipients. Migrant keeps any money is an indicator for whether or not the migrants chose to keep anything for themselves. P-values for difference in means were calculated by regressing the dependent variables on treatment, with robust standard errors.

Table 5: Impact of moral hazard treatment on migrant remittance decision

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Amount kept by migrant</i>		<i>Dependent variable: Migrant kept anything</i>	
Treatment: Recipient told migrant choice	-24.03** [10.35]	-20.10* [10.31]	-0.0507* [0.0275]	-0.0385 [0.0274]
Migrant is female		34.97*** [10.55]		0.101*** [0.0281]
Migrant age		0.791 [0.711]		0.00103 [0.00182]
Migrant years of education		0.286 [1.220]		-0.000241 [0.00323]
Migrant yeas in the US		-2.722*** [1.024]		-0.00820*** [0.00264]
Migrant hh size		-1.035 [2.644]		-0.00335 [0.00707]
Migrant lives with spouse		21.57** [10.93]		0.0507* [0.0288]
Migrant has child 22 or under in ES		-2.653 [12.12]		-0.0328 [0.0317]
Annual remittances by migrant to recipient hh		-0.00281 [0.00173]		-6.41e-06 [4.28e-06]
Recipient is female		1.315 [11.58]		0.0118 [0.0302]
Recipient age		0.461 [0.497]		0.000153 [0.00122]
Recipient years of education		-1.392 [1.526]		-0.00544 [0.00380]
Recipient hh size		-3.679 [2.537]		-0.00903 [0.00680]
Observations	1,298	1,298	1,298	1,298
R-squared	0.004	0.045	0.003	0.048
Mean in treatment = Recipient not told migrant choice	158.6		0.47	
Survey month fixed effects	NO	YES	NO	YES

Notes: Robust standard errors in brackets. Sample is observations with long form migrant survey, non-missing values for experimental questions, and completed recipient survey.

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

Table 6: Impact of moral hazard treatment on migrant remittance decision: By closeness of migrant-recipient ties

	(1)	(2)	(3)	(4)
	Below median of closeness index		At or above median of closeness index	
<i>Panel 1: Dependent variable is amount kept by migrant</i>				
Recipient told migrant choice	6.204 [14.54]	8.587 [14.46]	-54.03*** [14.64]	-48.67*** [14.75]
Observations	643	643	649	649
R-squared	0.000	0.059	0.021	0.072
Mean in Treatment = NOT TOLD	137.3		178.2	
<i>Panel 2: Dependent variable is migrant kept anything</i>				
Recipient told migrant choice	0.0269 [0.0390]	0.0339 [0.0387]	-0.127*** [0.0388]	-0.109*** [0.0396]
Observations	643	643	649	649
R-squared	0.001	0.076	0.016	0.068
Mean in treatment = Recipient not told migrant choice	0.41		0.52	
Control variables	NO	YES	NO	YES
Survey month fixed effects	NO	YES	NO	YES

Notes: Robust standard errors in brackets. Sample is observations with long form migrant survey, non-missing values for experimental questions, completed recipient survey and non-missing values for variables used to construct closeness index. Control variables are as in Table 5. Closeness index is constructed from the first principal component of whether or not migrant has been in the US fewer years than the sample median, whether or not the migrant has a child under 23 in El Salvador, and whether or not the migrant communicates at least weekly with the recipient hh.

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

Table 7: Mean amounts allocated to spending groups by recipients and migrants: Recipient experiment

	Means of recipient choices by treatment group:				Means of migrant preferences:
	<i>T1:</i>	<i>T2:</i>	<i>T3:</i>	<i>T4:</i>	
	Migrant not told recipient choice, recipient not told migrant preference	Migrant not told recipient choice, recipient not told migrant preference	Migrant told recipient choice, recipient not told migrant preference	Migrant told recipient choice, recipient told migrant preference	
<i>Amount allocated to:</i>					
Restaurant meals	5.91	6.31	4.88	6.04	11.74
Education	178.04	173.13	164.18	168.21	141.41
Daily expenses	63.61	68.41	81.72	69.56	76.56
Health expenses	52.45	52.15	49.22	56.19	70.28
<i>Number of observations</i>	314	324	327	333	1298

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

Table 8: Differences between recipient and migrant choices by treatment group: Recipient experiment

	<i>Moral hazard treatment</i>			<i>Communication treatment</i>		
	Migrant not told recipient choice	Migrant told recipient choice	P-value for difference of means: Not told and Told	Recipient not told migrant preference	Recipient told migrant preference	P-value for difference of means: Not told and Told
<i>Difference in:</i>						
Restaurant meals	15.89	14.80	0.604	16.66	14.05	0.215
Education	107.29	110.92	0.463	116.28	102.17	0.004
Daily expenses	78.02	81.38	0.421	83.01	76.52	0.120
Health expenses	75.02	73.47	0.709	76.55	71.97	0.271
Total difference	138.11	140.28	0.649	146.25	132.36	0.004
<i>Number of observations</i>	638	660		641	657	

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

Table 9: Impact of moral hazard and communication treatments on recipient allocation decision

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Migrant-recipient difference in...				Dependent variable:	
	<i>Restaurant spending</i>	<i>Education spending</i>	<i>Daily expenses spending</i>	<i>Health spending</i>	<i>Total migrant-recipient difference</i>	
<i>Panel 1:</i>						
Migrant told recipient choice	-1.097 [2.100]	3.582 [4.923]	3.336 [4.172]	-1.567 [4.159]	2.126 [4.753]	2.101 [4.776]
Recipient told migrant preference	-2.612 [2.103]	-14.09*** [4.926]	-6.476 [4.168]	-4.588 [4.159]	-13.88*** [4.751]	-13.96*** [4.773]
Observations	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.001	0.007	0.002	0.001	0.007	0.028
Mean in T1	18.0	116.2	78.3	77.9	145.2	
<i>Panel 2:</i>						
<i>T2:</i> Migrant not told recipient choice	-4.132 [3.080]	-17.56** [6.819]	-0.621 [5.949]	-5.745 [5.892]	-14.03** [6.673]	-13.55** [6.787]
Recipient told migrant preference						
<i>T3:</i> Migrant told recipient choice	-2.611 [3.253]	0.129 [6.942]	9.164 [5.738]	-2.719 [5.887]	1.981 [6.489]	2.508 [6.583]
Recipient not told migrant preference						
<i>T4:</i> Migrant told recipient choice	-3.753 [3.128]	-10.61 [6.940]	-2.971 [5.969]	-6.189 [5.735]	-11.76* [6.752]	-11.85* [6.813]
Recipient told migrant preference						
Observations	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.002	0.007	0.004	0.001	0.007	0.028
Mean in T1	18.0	116.2	78.3	77.9	145.2	
Control variables	NO	NO	NO	NO	NO	YES
Survey month fixed effects	NO	NO	NO	NO	NO	YES

Notes: Robust standard errors in brackets. Sample is observations with long form migrant survey, non-missing values for experimental questions, and completed recipient survey. Dependent variables are the absolute difference between the recipient's choice and the migrant's preferences in each category. The total difference is the sum across the four difference variables for each observation, divided by two. Omitted category in panel 2 regressions is T1: Migrant not told recipient choice, recipient not told migrant preference. Control variables are as in Table 5 plus the number of days in between migrant and recipient survey.

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

Table 10: Impact of moral hazard and communication treatments on recipient allocation decision: By closeness of migrant-recipient ties

	(1)	(2)	(3)	(4)
	Dependent variable: <i>Total migrant-recipient difference</i>			
	Below median of closeness index		At or above median of closeness index	
<i>Panel 1:</i>				
Migrant told recipient choice	17.23**	17.55**	-14.03**	-14.57**
	[6.753]	[6.955]	[6.700]	[6.793]
Recipient told migrant preference	-10.96	-11.32	-18.89***	-19.77***
	[6.765]	[6.870]	[6.699]	[6.784]
Observations	643	643	649	649
R-squared	0.013	0.041	0.017	0.049
Mean in T1	139.2		151.6	
<i>Panel 2:</i>				
<i>T2:</i> Migrant not told recipient choice	-12.22	-13.10	-17.10*	-17.55*
Recipient told migrant preference	[9.709]	[9.910]	[9.369]	[9.730]
<i>T3:</i> Migrant told recipient choice	16.06*	15.89*	-12.15	-12.29
Recipient not told migrant preference	[8.807]	[9.013]	[9.565]	[9.825]
<i>T4:</i> Migrant told recipient choice	6.215	6.161	-32.91***	-34.34***
Recipient told migrant preference	[9.340]	[9.676]	[9.759]	[9.930]
<i>P value T2 = T3</i>	0.004	0.004	0.591	0.571
<i>P value T2 = T4</i>	0.073	0.067	0.093	0.080
<i>P value T3 = T4</i>	0.297	0.307	0.031	0.023
Observations	643	643	649	649
R-squared	0.013	0.041	0.018	0.050
Mean in T1	139.2		151.6	
Control variables	NO	YES	NO	YES
Survey month fixed effects	NO	YES	NO	YES

Notes: Robust standard errors in brackets. Sample is observations with long form migrant survey, non-missing values for experimental questions, completed recipient survey, and non-missing values for the variables used to construct the closeness index. Dependent variables are the absolute difference between the recipient's choice and the migrant's preferences in each category. The total difference is the sum across the four difference variables for each observation, divided by two. Omitted category in panel 2 regressions is T1: Migrant not told recipient choice, recipient not told migrant preference. Control variables are as in Table 5 plus the number of days in between migrant and recipient survey. Closeness index is constructed from the first principal component of whether or not migrant has been in the US fewer years than the sample median, whether or not the migrant has a child under 23 in El Salvador, and whether or not the migrant communicates at least weekly with the recipient hh.

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

Appendix 1: Text used in experimental questions

Migrant survey:

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

Question 1:

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

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

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

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

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

Question 2:

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

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

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

Recipient survey:

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

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

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

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

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

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

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

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

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

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

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