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## **Don't Tell on Me**

Experimental Evidence of Asymmetric Information in  
Transnational Households

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## **INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE**

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## ABSTRACT

Although most theoretical models of household decisionmaking assume perfect information, empirical studies suggest that information asymmetries can have large impacts on resource allocation. In this study, I demonstrate the importance of these asymmetries in transnational households, where physical distance between family members can make information barriers especially acute. I implement an experiment among migrants in Washington, DC and their families in El Salvador that examines how information asymmetries can have strategic and inadvertent impacts on remittance decisions. Migrants make an incentivized decision over how much of a cash windfall to remit and recipients decide how to spend a remittance. Migrants strategically send home less when their choice is not revealed to recipients but only when recipients can punish migrants for deviation from remittance agreements. Recipients make spending choices closer to migrants' preferences when those preferences are revealed, suggesting that recipients' choices are inadvertently affected by imperfect information.

**Keywords:** remittances, intrahousehold allocation, information asymmetries, transnational households

JEL Codes: D13, D82, F24, O15

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# 1. INTRODUCTION AND MOTIVATION

Although the implications of asymmetric information have been well documented in the study of important economic institutions such as labor, credit, and insurance markets, theoretical models of intrahousehold resource allocation have largely assumed perfect information (Chiappori 1988, 1992; Manser and Brown 1980; McElroy and Horney 1981; Lundberg and Pollack 1993).<sup>1</sup> Despite this, a growing body of empirical literature has shown that information asymmetries do exist in households and, further, that household members take strategic advantage of opportunities to use these asymmetries to alter the allocation of resources in the household (Ashraf 2009; Ashraf, Field, and Lee 2013; Schaner 2013). This paper brings the study of how information asymmetries affect intrahousehold resource allocation to a different setting: transnational households, defined as households composed of international migrants and their family members in the home country, in this case El Salvador. Using experimental methods, I examine the effects of a set of information imperfections on remittance decisions made by both migrants and their family members.

The context of transnational households is significant because migrants and their family members are making financial decisions in a situation where information asymmetries are especially acute. Because of the physical distance separating family members, families with a migrant living away from the household are precisely those where information asymmetries may be the most pronounced. A number of studies have documented the existence of these asymmetries in households with migrants. For example, De Laat (2014) shows that domestic migrants in Kenya spend resources on costly monitoring of their wives. Chen (2006, 2013) finds that in China, wives with migrant husbands exhibit noncooperative behavior more often for activities that are more difficult to monitor, and Seshan and Yang (2013) find suggestive evidence that Indian migrants underestimate how much their wives at home are saving. McKenzie, Gibson, and Stillman (2013) find that potential Tongan migrants underestimate earnings in New Zealand, a fact the authors partly attribute to underreporting of earnings by current migrants. However the empirical analysis in these papers is largely observational. This is the first study to causally examine how information asymmetries directly affect behavior, specifically decisions about the sending and spending of remittances.

The importance of understanding how information asymmetries affect decisions in transnational households is heightened by the fact that migrants and their family members are financially linked through the sending of remittances, a large financial flow. Global aggregate international remittances to the developing world were US\$332 billion in 2010, more than any other kind of resource flow with the exception of foreign direct investment (Ratha and Silwal 2012). In El Salvador specifically, remittances received were 16 percent of gross domestic product in 2010 (Ratha and Silwal 2012). In the same year, 21 percent of households in El Salvador received remittances from abroad and average monthly remittances were \$166 for families that received them—a figure that is almost 50 percent of average monthly household expenditures for remittance recipients (Dirección General de Estadística y Censos 2010). Additionally, the receipt of remittances has been shown to have large, positive impacts on a variety of measures of well-being, underscoring their importance as a tool for development (Cox-Edwards and Ureta 2003; Adams and Page 2005; Yang and Martinez 2005; Woodruff and Zenteno 2007; Yang 2008; Adams and Cuenca 2010). Given the importance of remittances for development, a more complete understanding of how these decisions are made is crucial for policymakers who are hoping to maximize their economic impact.

This paper addresses two types of information asymmetries that may affect decisions about the sending and spending of remittances. The first are asymmetries that can lead to *strategic* behavior, meaning that migrants and recipients recognize that the asymmetry exists and use it for their benefit. The specific asymmetries considered here are the limited abilities of remittance recipients to observe migrant income and of migrants to observe recipient spending. The second type are those that can have

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<sup>1</sup> Exceptions include Bloch and Rao (2002) and Chen (2013).

*inadvertent* impacts, defined as asymmetries that unintentionally affect decisions. These asymmetries are represented here as communication barriers that result in recipients having an incomplete understanding of migrant preferences for how the remittances they send should be spent. Communication barriers should be interpreted broadly as any obstacle—social, financial, or logistical—to understanding these preferences. I first describe a simple framework that shows how these two types of information asymmetries can affect remittance decisions, and then I test these predictions using experiments conducted with a matched sample of migrants from El Salvador and their family members at home.

The framework views the decisions made by migrants and remittance recipients as being driven both by altruism and by contracts that dictate how much of their income migrants should send to recipients and what the money should be spent on when received by the recipients. The contracts are enforced through the threat of punishment for noncompliance, and migrants and recipients may strategically deviate from these agreements when it is not likely their behavior will be observed. However, in pairs where the potential for punishment is low, decisions will be motivated mostly by altruism, and strategic effects will therefore be less important. Additionally, communication barriers, specifically in regard to migrants' preferences over recipients' spending habits, may lead to inadvertent deviation from migrant preferences by recipients.

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

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

I find that migrants remit \$20 more on average out of the possible \$600 (an increase of 5 percent over the control group mean of \$440 sent) when their decisions are revealed. This effect is concentrated (and larger) in subsamples where the recipient's ability to punish the migrant for deviation is plausibly high. There is no corresponding evidence of strategic behavior in the recipient experiment. However, reducing communication costs by revealing migrant preferences to recipients does have an impact, resulting in a 10 percent reduction in the difference between migrant preferences and recipient choices.

This paper is related to a set of field experiments that have examined the effects of offering migrants varying degrees of control over remittances. The idea behind these experiments is that offering control to migrants will mitigate a moral hazard problem in how recipients spend remittances. Ashraf et al. (2011) show that savings levels in bank accounts in El Salvador increase when migrants are given greater control over these accounts. Chin et al. (2011) find that the impacts of an experiment that offered migrants assistance in opening bank accounts in the United States are concentrated among migrants who report having no control over how their remittances are spent. However, in a lab experiment, Torero and Viceisza (2011) find little evidence that Salvadoran 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 was too limiting.

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

This paper also fits into a growing, broader literature on how information asymmetries affect resource allocation in families. Ashraf (2009) shows that in the Philippines, men whose wives are the household financial managers hide income from their wives when that decision is private. When their decision is public, men choose to divert income to committed consumption that cannot be undone. Only when spouses communicate about their choices before they make them do men choose to share the income with their wives. Schaner (2013) finds that spouses are more likely to choose to save in individual (as opposed to joint) savings accounts when they are not well informed about each other’s finances.<sup>2</sup> Also related, Jakiela and Ozier (2012) find that women in Kenya sacrifice investment returns to keep income secret from family members outside their household and avoid the pressure to share that income.

This study builds on the literature in several ways. First, while these studies focus on strategic behavior; I study responses to different types of information asymmetries, strategic and inadvertent, allowing me to evaluate the relative effects in the same population. Second, these papers largely focus on a single choice in the resource allocation process (whether or not to share income); the present experiment considers how information asymmetries can affect two different decisions made by families about economic resources. Finally, this study documents that information asymmetries can be important outside of the husband–wife pair, which has been the context of most previous experimental work in this area.<sup>3</sup> People in developing countries often transfer resources within extended families, and therefore decisions about resource allocation consequently are likely to involve people beyond just the husband and wife. The results show that information asymmetries can have important impacts in extended families, but because migrants react to being monitored only when the recipients’ ability to punish them is high, the results also indicate that asymmetries may not matter in all families where resources are shared.

The paper proceeds as follows: Section 2 describes a framework for understanding the empirical results. Section 3 explains the experiment. Section 4 describes the data and the empirical strategy. Section 5 presents and discusses the results, and Section 6 concludes.

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<sup>2</sup> In another experiment in Zambia, women are more likely to take advantage of vouchers for contraception and use concealable forms of contraception when these vouchers are given to them outside of the presence of their husbands, showing that strategic reactions to information asymmetries extend beyond simply the allocation of funds in the household (Ashraf, Field and Lee 2012).

<sup>3</sup> The dynamics of transfer arrangements in extended family networks has been studied (Foster and Rosenzweig 2001), but little is known about how information imperfections affect behavior in these arrangements. The notable exception is Jakiela and Ozier (2012), but the main focus of that paper is investment decisions, as opposed to sharing and spending decisions.

## 2. THEORETICAL FRAMEWORK

In this section I describe the intuition behind a simple model that frames my experimental results. A detailed exposition of the model can be found in Appendix B. The intent of the model is to show that the probability that recipients observe migrant income and the probability that migrants observe recipient spending can impact migrant sending and recipient spending decisions. The structure of the model is based on Chen's (2013) description of how male migrants in China monitor their wives' behavior. Specifically, Chen shows that when a migrant has imperfect information about his wife's actions and incomplete information about her preferences, the contract offered to the wife may not always be incentive compatible. I adapt a simplified version of this model to describe the outcomes considered in this paper.

In this framework, the migrant's decision about how much to send home in remittances is determined by a combination of altruism and a contract with the recipient that is enforced through the threat of punishment. One example of such a cost is substandard care for or attention to people (children or elderly relatives) or possessions (land, livestock, or new investments) left by the migrant in the care of his family. Another is social sanctions against the migrant: many migrants come from areas with high rates of migration and strong social norms and expectations regarding the amount of money that migrants send home. Particularly for migrants who wish to return home one day, a damaged reputation may be seen as quite costly. Finally, migrants who refuse to send home as much money as their families expect may damage their relationships with their families, relationships that migrants with tenuous positions in foreign countries may view as important.<sup>4</sup> Many of these potential punishments are related to the social closeness of migrants and recipients, and indeed, in a qualitative study of Ghanaian migrants in the Netherlands, Mazzucato (2009) emphasizes the importance of the social proximity of migrants and recipients for the effective enforcement of remittance agreements.<sup>5</sup>

The amount that the migrant is compelled to send through the contract depends on the size of the punishment: The greater the ability of the recipient to punish, the more the recipient can push the migrant past the amount the migrant would choose to send voluntarily. These contracts are defined based on the migrants' earnings, which vary over time. Recipients will expect different amounts depending on how much a migrant earns in any given period. Because of the geographical distance between migrants and recipients, recipients observe migrant income only with a certain probability, and that probability varies from period to period.<sup>6</sup> I model this probability as being either high or low. Migrants always know the value of this probability, but recipients know only its distribution. If migrant income is not observed by the recipient, the recipient must rely only on information from the migrant. Therefore, migrants can deviate from the contract by underreporting their income to the recipients and will be discovered and punished only if that income is actually observed.

Recipients must set up these contracts to be incentive compatible: Migrants' utility from compliance must be greater than or equal to the expected utility from deviating. This expected utility will be higher when the probability that income will be observed is low; therefore, the cooperation-inducing remittance is lower when that probability is low than when it is high. Because recipients do not know the

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<sup>4</sup> A similar description of enforcement mechanisms can be found in Rapoport and Docquier (2006). Brown (1997), Hoddinott (1994), Lucas and Stark (1985), and Poirine (1997) all describe remittance contracts enforced through the discussed mechanisms. Additionally, in studies of dictator games within social networks, Leider et al. (2009) and Ligon and Schecter (2012) document the importance of the expectation of reciprocity in motivating giving.

<sup>5</sup> Additionally, in focus groups done prior to the start of the project, migrants repeatedly cited high levels of pressure from family members as a key reason why they sent remittances home. Relatedly, in their work on Tongan migration to New Zealand, McKenzie, Gibson, and Stillman (2013) find suggestive evidence that migrants underreport earnings to avoid pressures from family members to remit.

<sup>6</sup> This observation of income will likely occur through the migrant's social and family network. Given that the networks within which migrants in the United States live and work are often closely related to their home country networks (Munshi 2003), instances when the probability that income will be observed is quite high are likely.

probability of observing migrant income, they must set one contract that satisfies incentive compatibility for all migrants and will lose potential surplus when that probability turns out to be high.

However, one other option is available to the recipients. They can set a contract that binds when the probability of observing migrant income is high but that is not incentive compatible when that probability is low. Because recipients have to lower the remittance amount so much to induce compliance when the probability is low, they may be better off receiving the higher amount only some of the time instead of the lower amount all the time. The model leads to two predictions for migrant behavior: First, migrants will act strategically and send less money home when the probability that recipients will observe their income is low. Second, this will happen only when recipients have power to punish migrants for not sending home enough money. If recipients cannot punish migrants, then migrants will send only the amount they choose to send altruistically. This altruistic amount will not depend on whether the migrant's income can be observed.

The decision made by recipients about how to spend the remittances that they receive is modeled in a parallel manner. Recipients choose the extent to which they follow the migrants' preferences for how remittances should be spent. Their choice is motivated by altruism (in this case simply the extent to which recipients want to follow migrant preferences) and a contract with migrants enforced by the migrant's ability to punish the recipient. The probability that migrants will observe actual recipient spending can be either high or low. This probability is known to the recipient, while the migrant knows only its distribution. As with the migrant remittance decision, this leads to a situation where migrants may offer contracts that are incentive compatible only when the probability that recipient spending will be observed is high. The recipient decision may additionally be complicated by barriers to communication that result in confusion over what the migrant's preferences actually are. I will refer to these barriers as *communication costs*, but the concept is broader than just the cost of a telephone call. With distance, specificity about preferences may become difficult, migrants may feel uncomfortable expressing what they want, and recipients may sometimes have to make decisions without time to directly consult with migrants. Family members may also incorrectly assume that they know what the migrant would prefer. If the recipient does not know the migrant's preferences, the preferences will not be followed regardless of the recipient's intentions.

This setup leads to three predictions for the recipient decision: First, recipients will strategically spend less money according to migrant preferences when the probability their spending will be observed is low. Second, when migrants cannot punish recipients, the probability that spending will be observed will not affect spending decisions because the recipient choice will be strictly voluntary. Finally, if recipients do not have full information, they may be inadvertently deviating from migrant preferences. Improved information can increase the extent to which recipients follow those preferences.

The main point of this discussion is that strategic behavior can be a part of the relationship between migrants and their family members, but the extent to which it is important will depend on their ability to pressure and punish each other. At the same time, communication costs can lead to inadvertent deviation when recipients make remittance spending decisions.

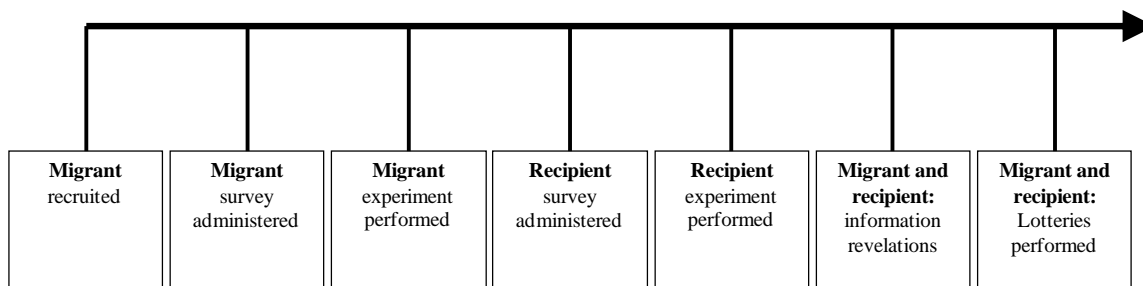
### 3. PROJECT DESIGN

Given that information asymmetries in transnational families are difficult to measure and may be correlated with a number of unobserved characteristics, I implemented a randomized experiment to test the predictions of the framework discussed in the previous section. This experiment is conducted within the context of survey work for a separate field experiment on remittances and education among Salvadoran migrants in Washington, DC, and their families in El Salvador (Ambler, Aycinena, and Yang 2013). Specifically, I exploit an unusual feature of this data collection exercise; it involves surveys with matched pairs of migrants and family members, allowing me to investigate the preferences and choices of both. In the experiment, I randomly vary (1) whether migrant income and recipient spending are observed and (2) the size of communication costs, allowing me to identify the causal impacts of both of these factors on migrant and recipient remittance behavior. Demographic survey data are used to explore how impacts vary by punishment ability.

Migrants were recruited in the Washington, DC, metro area at the two area locations of the Salvadoran consulate and were interviewed while they were waiting for consular services.<sup>7</sup> The migrant survey was conducted between late September 2011 and late February 2012. Surveyors in the consulate approached migrants and invited them to participate. Because the focus of the companion experiment was remittances and education, participants were required to name a high school or college-aged relative or acquaintance in El Salvador.<sup>8</sup> Those who qualified and agreed to participate were administered a baseline survey.<sup>9</sup> The experiment described in this paper was conducted at the end of the survey.

During the survey migrants identified a high school or college-aged student in El Salvador. Although the migrants were not required to select a family member as the student, in practice 97 percent did. Interviews were subsequently conducted with the student or a household member. If the student was 18 years of age or older, the student was to be interviewed; and if under 18, a guardian was identified to be interviewed. If the indicated person was not available, an alternative adult in the household was interviewed instead. The El Salvador survey was conducted by phone in the days following the migrant survey in the United States; the median number of days between the US and El Salvador surveys was eight. The El Salvador surveys concluded in mid-March 2012, two weeks after fieldwork ended in the United States. The experiment in the El Salvador survey was also at the end of the survey. Figure 3.1 describes the phases of the project in the order that they occurred for each pair of participants.

**Figure 3.1 Project timeline**



Source: Author.

<sup>7</sup> The most common reason to go to the consulate is passport renewal, but other services include renewal of temporary protected status, registry of births and deaths, and notarization of documents.

<sup>8</sup> Among the migrants approached, 24 percent participated. Of those that did not participate, 77 percent did not know an eligible student in El Salvador, 14 percent refused, 7 percent were not from El Salvador, and 2 percent had other reasons.

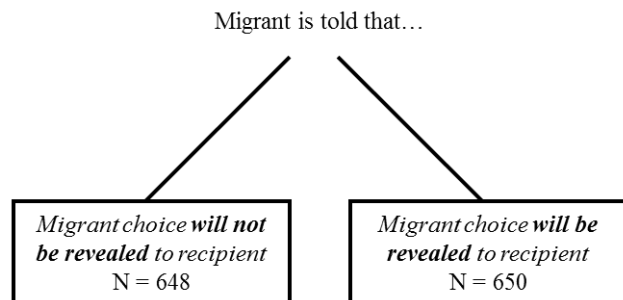
<sup>9</sup> This was followed by the randomized offer of a product designed to facilitate the sending of remittances for education to El Salvador. This was a randomized intervention, and migrants received offers of different versions of the product depending on their assigned treatment group. Migrants in a control group received only information and no product offer.

## Migrant Experiment

The migrant experiment consisted of an incentivized remittance-sending decision. Migrants were told that they were being given the chance to win \$600 and would have to decide how much of the prize to keep for themselves and how much to remit to their family member in El Salvador. Migrants could split the \$600 as they wished but were restricted to using \$100 intervals for simplicity.<sup>10</sup> The prize was awarded through a lottery.<sup>11</sup> Although budgetary restrictions did not allow for all participants to win the prize, the use of the lottery incentivized participants to treat this as a real decision. In the Ashraf et al. (2011) study of a similar population of Salvadoran migrants, migrant median monthly income was \$2,080. Consequently, \$600 represents a significant increase in monthly income. The question text can be found in Appendix C. Migrants were randomly allocated into two groups: those who were told that their choice would be revealed to their family member, and those who were told that their choice would not be revealed. The family member referred to in the question was the person to be surveyed in El Salvador. A description of the treatments is presented in Figure 3.2. Because the treatment varies the ability of the recipient to monitor the migrant's actions, I refer to this treatment as the migrant-monitoring treatment.

This experiment exogenously varies the probability that recipients will observe migrant income and measures the extent to which migrants strategically take advantage of asymmetric information. The framework described in Section 2 predicts that migrants will send less money home when the probability their income will be observed is low. In the context of the experiment, this means that migrants whose choice is not revealed to the recipient should send less than those whose choice is revealed. However, following the predications of the framework, migrants should act strategically only when recipients can punish them for noncompliance. Migrant responses from the baseline survey will be used to proxy for recipients' ability to punish migrants. Differences between the two groups—choice revealed and choice not revealed—should be evident only when those proxies indicate that the ability to punish exists.

**Figure 3.2 Migrant experiment: Treatments**



Source: Author.

## Recipient Experiment

The recipient experiment consisted of an incentivized remittance spending decision. The respondents in the El Salvador phone survey were told that because their family member in the United States participated in the study, they now had the chance to win a remittance worth \$300. They had to decide what to spend the remittance on and were asked to split the \$300 among four spending categories: restaurant meals, education, daily expenses, and health expenses. Recipient choices were limited to four categories for simplicity in the context of a phone survey. If among the winners, recipients would receive exactly the

<sup>10</sup> In pilot surveys where migrants were not given restrictions, almost all chose to split the money in \$100 intervals.

<sup>11</sup> Two prizes were awarded. If asked, surveyors told migrants the number of prizes and the date of the drawing. The first prize was awarded midway through survey work, and the second when survey work had concluded. Migrants were eligible for only one drawing.

allocations that they requested.<sup>12</sup> Prizes were awarded in kind. The median monthly remittance in the Ashraf et al. (2011) study was \$325, so a \$300 remittance is a standard amount for many recipients. The question text can be found in Appendix D. Two separate treatments were administered to recipients: the recipient-monitoring treatment and the recipient communication treatment. The two recipient treatments were cross-randomized, also allowing for the analysis of their interaction. They are depicted in Figure 3.3.

**Figure 3.3 Recipient experiment: Treatments**

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

Source: Author.

### ***Recipient Monitoring Treatment***

The recipient-monitoring treatment is parallel to the migrant-monitoring treatment. Recipients were randomly allocated into two groups: those who were told that their choice would be revealed to the migrant, and those who were told that their choice would not be revealed to the migrant. This treatment randomly varied the probability that recipient spending would be observed and measures the extent to which recipients strategically take advantage of this asymmetric information. The framework predicts that recipients are more likely to strategically deviate by spending less according to the migrant’s preferences when the probability that spending will be observed is low. Therefore, the results of the experiment should show that recipients make choices closer to migrant preferences when those choices are revealed to the migrant. However, this effect will be present only in pairs where the migrant can punish the recipient for noncompliance.

### ***Recipient Communication Treatment***

During the US survey, migrants were told about the lottery for recipients and asked what their preferences were for how the recipients would spend the money. Again, recipients were randomly allocated into two groups: those for whom the migrant’s preferences were revealed and those for whom the migrant’s preferences were not revealed. Making these preferences clear is a proxy for improving communication,

<sup>12</sup> Four prizes were awarded. If asked, surveyors told recipients the number of prizes and the date of the drawing. Two prizes were awarded midway through survey work and the other two when survey work had concluded. Recipients were eligible for only one drawing.

so the experiment exogenously improves communication about migrant preferences for expenditures. This treatment is therefore a test of whether or not communication costs can lead to inadvertent deviation from migrant preferences by the recipient. Revealing migrant preferences to the recipient should decrease the difference between the recipients' choices and the migrants' preferences when communication problems exist.

## **Experiment Logistics**

For the experiment to work as intended, respondents must have believed that the threat of revealing their choices to their family members was credible. Because the interviewer collected contact information for the recipient families from the migrants and allowed the migrants to use a project phone during the interview to call their family members and tell them about the study, migrants were aware that their family members could indeed be contacted. Similarly, because recipients being interviewed knew that they had been contacted through the migrant, they also knew that their migrant family members could be contacted. Although it has no impact on the results of the experiment, for all respondents in the choice-revealed treatment groups of the monitoring treatments, an effort was made to inform their family member of the choice made by the participant.

The randomization in this study was performed at the participant level. Surveys were preassigned treatment status, and migrant and recipient treatments were randomized separately. Because remittance behavior can vary by season, it was important to ensure that treatments were balanced over time. I achieved this by stratifying the randomization for all treatments within groups of 16 surveys and by the treatment offered in the companion experiment. The recipient treatments were additionally stratified by the migrant treatment.

## **Threats to Interpretation**

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

A second potential issue is that in both the migrant and the recipient experiments, prizes were awarded by lottery, meaning that the expected value of the prize for each participant is much lower than the value of the actual prize. Participants may be more generous or less likely to make controversial decisions because they know they are unlikely to win the lottery prize. Little research has been done into how subjects react to lottery prizes, but some evidence does exist. Laury (2005) conducts a laboratory experiment in which respondents make the same choices when payoffs are random as when payoffs are guaranteed. Additionally, because the questions in this paper relate to the differences between the treatment groups, if the lottery does impact participants' decisions, the estimated effect can be considered to be a lower bound.

The final issue is that of the fungibility of choices made during the experiment. Both migrants and recipients could potentially undo their choices during the experiment through their actions afterward. For example, migrants could choose to not send a remittance that they would have sent otherwise. Although some of this behavior may be occurring, it is not necessarily important for the interpretation of the treatment. If the results show differences between the two treatments then that is evidence that people are reacting to variations in information.<sup>13</sup>

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<sup>13</sup> Even without evidence of differences between treatments, if participants wish to do something that their partner would disagree with, it makes sense for them to take advantage of the experiment to do so, when the probability of keeping that action secret is high.

## 4. DATA AND ESTIMATION STRATEGY

### Data

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

Table 4.1 shows summary statistics from both surveys: 1,581 migrant surveys and 1,298 recipient surveys were performed, a completion rate of 82 percent for the recipient surveys. For the migrant survey, summary statistics are shown for both the full sample and the sample with completed recipient surveys. No meaningful differences are evident between the two samples; therefore, I limit the analysis sample to the 1,298 migrant–recipient pairs with completed El Salvador surveys. Results from the migrant experiment do not change significantly between the two samples. Additionally, I show that attrition from the full sample of migrant surveys to the sample of completed recipient surveys is not related to treatment status (Tables 4.2 and 4.3). The breakdown of participants in the final analysis sample into the different treatment groups can be seen in Figure 3.2 (migrant experiment) and Figure 3.3 (recipient experiment).

**Table 4.1 Baseline summary statistics**

	All observations			Observations with completed recipient survey		
	Mean	SD	N	Mean	SD	N
<b>Baseline variables from migrant survey</b>						
Migrant is female	0.50	0.50	1,581	0.51	0.50	1,298
Migrant age	36.83	9.41	1,538	36.92	9.29	1,264
Migrant can read and write	0.96	0.20	1,554	0.96	0.20	1,275
Migrant’s years of education	9.08	4.67	1,560	9.01	4.67	1,282
Migrant’s years in the United States	11.31	6.38	1,577	11.13	6.27	1,295
Migrant is married	0.62	0.48	1,575	0.63	0.48	1,294
Migrant lives with spouse	0.49	0.50	1,579	0.50	0.50	1,296
Migrant’s total number of children	2.28	1.69	1,579	2.34	1.69	1,296
Migrant’s number of children in El Salvador	1.01	1.43	1,577	1.07	1.47	1,294
Migrant’s number of children in United States	1.26	1.32	1,575	1.25	1.29	1,293
Migrant’s hh size in United States	4.32	1.98	1,581	4.36	1.96	1,298
Migrant has child 22 or under in El Salvador	0.32	0.47	1,581	0.34	0.47	1,298
Recipient is migrant’s close relative	0.29	0.45	1,574	0.31	0.46	1,291
Migrant has worked in last 12 months	0.89	0.31	1,581	0.89	0.31	1,298
Migrant in lowest income bracket	0.52	0.50	1,429	0.53	0.50	1,181
Migrant sent remittances to recipient hh	0.85	0.36	1,580	0.87	0.34	1,297
Migrant’s annual regular remittances to recipient hh (\$)	2,298	2,907	1,565	2,440	2,998	1,283
Migrant’s annual irregular remittances to recipient hh (\$)	337	706	1,575	344	707	1,293
Migrant’s annual total remittances to recipient hh (\$)	2,629	3,199	1,563	2,777	3,284	1,281
Migrant’s annual total remittances to other hhs (\$)	1,097	1,905	1,567	1,123	1,944	1,284
Migrant communicates with recipient hh at least weekly	0.69	0.46	1,578	0.71	0.45	1,295

**Table 4.1 Continued**

	All observations			Observations with completed recipient survey		
	Mean	SD	N	Mean	SD	N
<b>Baseline variables from recipient survey</b>						
Recipient is target student				0.45	0.50	1,298
Recipient is student's guardian				0.40	0.49	1,298
Recipient is female				0.68	0.47	1,298
Recipient age				34.20	15.84	1,295
Recipient is married				0.36	0.48	1,298
Recipient's years of education				9.37	5.27	1,292
Recipient lives in urban area				0.43	0.50	1,298
Recipient's hh size				4.99	2.04	1,296
Annual remittances received from migrant (\$)				1,522	1,916	1,203
<b>Baseline comparison variables</b>						
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

Source: Author's calculations.

Notes: SD = standard deviation; N = number; hh = household; GPA = grade point average. The "All Observations" sample is respondents with nonmissing data for questions in the migrant experiment. The "Completed Recipient Survey" sample additionally conditions on completion of the recipient survey and nonmissing migrant and recipient information for questions in the recipient experiment. Number of observations varies slightly with missing values. *Recipient* is defined as close relative if migrant reports recipient to be spouse, parent, or child. Migrants in the lowest income bracket chose \$400 or less as the weekly income of themselves plus their coresident spouses. The other categories were \$401–\$600, \$601–\$800, and \$801 and above. Annual regular remittances were collected by asking for the frequency of remittances sent and the average amount sent each time. Annual irregular remittances are remittances sent for special occasions or emergencies. The recipient variables in all cases refer to the person completing the recipient survey. The baseline comparison variables were asked on both surveys and are equal to 1 if the migrant and recipient responses match. Both respondents were asked to choose the three most important budget priorities for the recipient hh from a list of seven categories. *Student* refers to the student identified by the migrant during the baseline survey. GPA and mode of transport were only asked when student was reported to be in school.

The migrants are half female with an average age of 38. 85 percent sent remittances to the recipient household in the last 12 months, indicating that most pairs in the sample have an established remittance relationship. Average annual remittances to the recipient household (reported by the migrant) are \$2,629. Average annual remittances to other households in El Salvador are \$1,059. The \$1,600 difference between average remittances to the recipient household and those to other households suggests that most recipient households are the migrant's primary remittance recipient. The mean number of years in the United States is 11, so the migrants are largely established in the United States. In my sample, 32 percent of migrants report having a son or daughter aged 22 or under in El Salvador, and 69 percent report communicating with the recipient household at least weekly. The sample is also low income; half of the migrants report earning \$400 a week or less. Because of the structure of the project, the interviewed recipients are either the student identified by the migrant (45 percent) or the student's guardian if the student is under 18 (40 percent). The remaining 15 percent of interviews were done with a different adult in the household if the student or guardian could not be reached. The recipient sample is heavily female (68 percent) because identified student guardians tend to be female.

Because migrants were recruited in the Salvadoran consulate and screened into the study on the basis of having a young adult relative in El Salvador, a concern may be that the respondents are not representative of the larger migrant community and that the results are therefore not indicative of what might be found in a more representative sample. In Table A.1 in the Appendix, I compare characteristics of the migrants from the baseline survey (gender, age, time in the United States, household size, and education) to migrants in the 2008–2010 American Community Survey (ACS). I restrict the ACS sample

to non-US citizens aged 18 to 65 who live in the Washington, DC, metro area who are either Salvadoran-born or Hispanic. The study participants are quite similar to the ACS samples, in particular to the Salvadoran-born sample, suggesting that study participants are not overly different from the greater migrant population.

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 to some preexisting difference between groups. Tables 4.2 and 4.3 test whether the treatment groups are balanced on observed characteristics from the baseline survey for the treatment groups for the migrant experiment and the recipient experiments, respectively. In Table 4.2 the means for both treatment groups in the migrant-monitoring treatment are presented in the first two columns, and the p-value of the hypothesis test of whether or not those means are equal is in the third column. Overall, the treatment groups are well balanced: Only 2 of 34 differences are significantly different from zero at the 10 percent level. Table 4.3 shows the means by treatment group for the two recipient treatments and p-values for differences in those means. Only 3 of the 34 p-values for the recipient-monitoring treatment and one for the recipient communication treatment are less than 0.10. Some differences between treatment groups may occur by chance, and these few small differences are not cause for concern. However, to allay any concerns of an unbalanced sample affecting results, I include regression specifications with control variables.

**Table 4.2 Balance tests: Migrant experiment**

	<i>Treatment group means:</i>		<b>P-value for difference of means: Choice not revealed and choice revealed</b>
	<b>Migrant choice not revealed to recipient</b>	<b>Migrant choice revealed to recipient</b>	
<b>Attrition</b>			
<b>Recipient survey completed</b>	<b>0.82</b>	<b>0.83</b>	<b>0.819</b>
<b>Baseline variables from US survey</b>			
Migrant is female	0.53	0.49	0.165
Migrant age	36.90	36.94	0.941
Migrant can read and write	0.95	0.97	0.150
Migrant's years of education	9.01	9.00	0.966
Migrant's years in the US	10.90	11.37	0.178
Migrant is married	0.61	0.65	0.151
Migrant lives with spouse	0.50	0.50	0.956
Migrant's total number of children	2.34	2.34	0.956
Migrant's number of children in El Salvador	1.03	1.10	0.365
Migrant's number of children in US	1.28	1.22	0.410
Migrant's hh size in US	4.34	4.38	0.720
Migrant has child 22 or under in El Salvador	0.32	0.37	0.059
Recipient is migrant's close relative	0.29	0.33	0.178
Migrant has worked in last 12 months	0.90	0.89	0.943
Migrant in lowest income bracket	0.53	0.53	0.886
Migrant sent remittances to recipient hh	0.87	0.86	0.586
Migrant's annual regular remittances to recipient hh (\$)	2,494	2,386	0.520
Migrant's annual irregular remittances to recipient hh (\$)	354	334	0.627
Migrant's annual total remittances to recipient hh (\$)	2,828	2,726	0.579
Migrant's annual total remittances to other hhs (\$)	1,059	1,185	0.245
Migrant communicates with recipient hh at least weekly	0.73	0.69	0.057

**Table 4.2 Continued**

	<i>Treatment group means:</i>		<i>P-value for difference of means: Choice not revealed and choice revealed</i>
	<i>Migrant choice not revealed to recipient</i>	<i>Migrant choice revealed to recipient</i>	
<b>Attrition</b>			
<b>Recipient survey completed</b>	<b>0.82</b>	<b>0.83</b>	<b>0.819</b>
<b>Baseline variables from recipient survey</b>			
Recipient is target student	0.45	0.45	0.907
Recipient is student's guardian	0.42	0.38	0.160
Recipient is female	0.69	0.67	0.331
Recipient age	35.09	33.31	0.043
Recipient is married	0.36	0.36	0.941
Recipient's years of education	9.21	9.54	0.285
Recipient lives in urban area	0.43	0.44	0.649
Recipient's hh size	4.90	5.08	0.111
Annual remittances received from migrant (\$)	1,491	1,553	0.580
<b>Baseline comparison variables</b>			
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

Source: Author's calculations.

Notes: GPA = grade point average; hh = household. Sample is observations with nonmissing values for the experiment questions and completed recipient survey. Attrition is measured from sample of all migrants who completed the survey and the migrant experiment to sample with completed recipient survey and recipient experiment. Sample size for each comparison of means varies slightly by missing values for each variable. The percentage of missing values for each variable is also tested for balance across treatment groups with no significant differences. Other notes on variable construction are as in Table 3.1. P-values come from a regression of each variable on treatment, with standard errors adjusted for heteroskedasticity.

**Table 4.3 Balance tests: Recipient experiment**

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

**Table 4.3 Continued**

	Monitoring treatment			Communication treatment		
	<i>Treatment group means:</i>		<i>P-value for difference of means: Choice not revealed and choice revealed</i>	<i>Treatment group means:</i>		<i>P-value for difference of means: Preference not revealed and preference revealed</i>
	<i>Recipient choice not revealed to migrant</i>	<i>Recipient choice revealed to migrant</i>		<i>Migrant preference not revealed to recipient</i>	<i>Migrant preference revealed to recipient</i>	
<b><i>Baseline comparison variables</i></b>						
Migrant and recipient report same hh budget priorities	0.46	0.50	0.189	0.47	0.49	0.401
Migrant and recipient report same student GPA	0.25	0.24	0.844	0.24	0.24	0.952
Migrant and recipient report same student mode of transport	0.41	0.45	0.228	0.43	0.42	0.671

Source: Author's calculations.

Notes: GPA = grade point average; hh = household. Sample is observations with nonmissing values for the experiment questions and completed recipient survey. Attrition is measured from sample of all migrants who completed the survey and the migrant experiment to sample with completed recipient survey and recipient experiment. Sample size for each comparison of means varies slightly by missing values for each variable. The percentage of missing values for each variable is also tested for balance across treatment groups with no significant differences. Other notes on variable construction are as in Table 3.1. P-values come from a regression of each variable on treatment, with standard errors adjusted for heteroskedasticity.

The first row of Tables 4.2 and 4.3 also test whether attrition from the full sample of migrants to the estimation sample of migrant–recipient pairs with completed recipient surveys is related to treatment. Attrition is not significantly related to treatment for migrants or recipients.

### Estimation Strategy: Migrant Experiment

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

$$Remit_i = \delta + \alpha ChoiceRevealed_i + \gamma_i + \varepsilon_i \quad (1)$$

where  $Remit_i$  is the dependent variable indicating the amount that the migrant chose to send to the recipient.  $ChoiceRevealed_i$  is the treatment indicator for the monitoring treatment, and it is equal to 1 when the migrant’s choice is revealed to the recipient. The coefficient  $\alpha$  is the average difference between how much migrants choose to send when their decisions are not revealed and when they are revealed. If  $\alpha$  is positive, migrants send more money to the recipients when  $ChoiceRevealed_i$  equals 1.  $\gamma_i$  are stratification cell fixed effects. There are 111 survey group stratification cells in all regressions. The inclusion of baseline control variables is not necessary for causal inference, but I will show specifications with controls to show that they do not affect the results. The control variables include migrant age, gender, education, household size, years in the United States, remittances to recipient household, and other migrant background characteristics.  $\varepsilon_i$  is the error term, which I adjust for heteroskedasticity.

### Estimation Strategy: Recipient experiment

Unless migrant and recipient preferences are different on average, regressions examining the impact of the treatment on the amounts allocated to the four different categories by the recipients will be uninformative. However, because the US survey collected the migrant’s preferences for 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 implement this by calculating 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 2 to scale the total to 300. I refer to this as the total difference, and it is the primary dependent variable of interest. It is a measure of the number of dollars out of the \$300 on which the migrant and recipient match. For example, a total difference of 100 would mean the recipient’s choices matched the migrant’s preferences on \$200 of the \$300, but that they allocated the remaining \$100 to different categories.

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

$$Difference_i = \varphi + \beta_1 ChoiceRevealed_i + \beta_2 PreferenceRevealed_i + \theta_i + \mu_i \quad (2)$$

where  $Difference_i$  is the difference between migrant preferences and recipient choices in each of the four spending categories or the total difference.  $ChoiceRevealed_i$  is the treatment indicator for the recipient-monitoring treatment and is equal to 1 when the recipient’s choice is revealed to the migrant.  $PreferenceRevealed_i$  is the treatment indicator for the communication treatment and is equal to 1 when the migrant’s preferences are revealed to the recipient before the recipient decides how to allocate the remittance funds. The coefficient  $\beta_1$  is the average difference in the difference between migrant preferences and recipient choices when the recipient choice is not revealed compared with when it is revealed. Similarly,  $\beta_2$  is the average difference in the difference between migrant preferences and recipient choices when the migrant’s preferences are revealed to the recipient compared with when they are not revealed. If, as predicted, revealing the recipients’ choices to the migrants and communicating the migrants’ preferences to the recipients causes the recipients to make choices more similar to the migrants’ preferences, then the difference variable will be smaller in the choice-revealed and preference-revealed

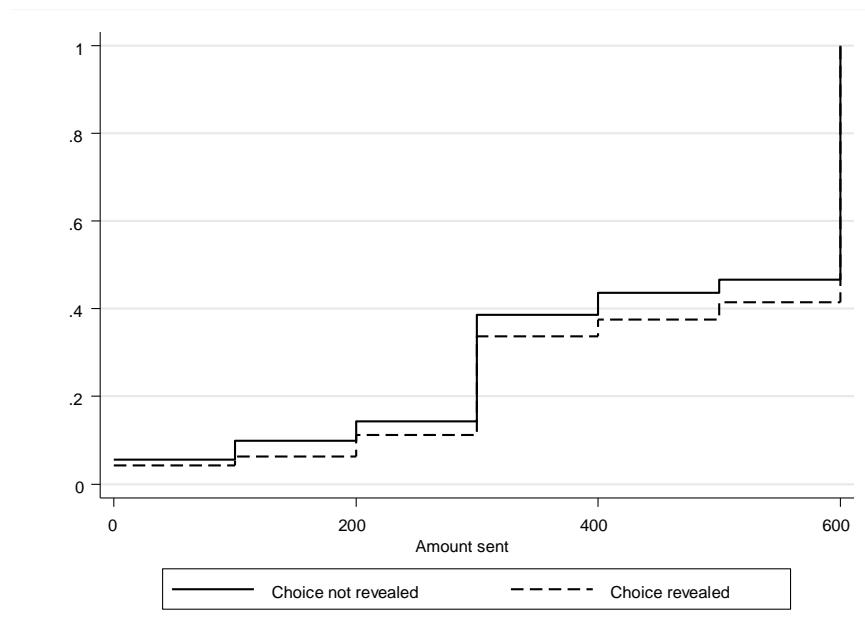
treatment groups, and  $\beta_1$  and  $\beta_2$  should be negative.  $\theta_i$  are stratification cell fixed effects (survey group and migrant treatment). Again, I also present specifications with control variables. I use the same variables as in the migrant experiment as well as recipient gender, age, education, household size, and the number of days between the migrant and recipient surveys.  $\mu_i$  is the error term, which I adjust for heteroskedasticity.

## 5. RESULTS

### Migrant Experiment

I first analyze the results of the migrant experiment in which migrants make an incentivized decision over how much of a potential \$600 windfall to send to the recipient and how much to keep. Figure 5.1 shows the cumulative distribution of the amount sent by migrants, separately by treatment group. Because the experimental protocol limited migrants to splitting the money in \$100 increments, the distributions are discrete. The first observation to be made from this figure is that the migrants send large amounts: More than half of the migrants in both treatment groups chose to send the entire \$600 to the recipient. The other, smaller concentration 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. The percentage of migrants sending everything is smaller when choices are not revealed (53 percent versus 58 percent) and the percent of migrants choosing to send \$400 or less is higher (44 percent versus 38 percent). It is also easy to see that the distribution of the choices in the choice-revealed treatment group is always below the distribution of choices in the choice-not-revealed group; that is, the choice-revealed distribution stochastically dominates the choice-not-revealed distribution.<sup>14</sup>

**Figure 5.1 Cumulative distribution of amount sent by migrant by treatment group**



Source: Author's calculations.

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

The fact that almost all migrants in the choice-not-revealed treatment group chose to send something is consistent with the model presented in Section 2, where migrants who deviate when the probability their income will be observed is low still send positive amounts in remittances. The fact that most migrants in this group choose to send the entire \$600 is suggestive that the altruistic component of remittances is high. However, the differences between the two distributions are evidence that information asymmetries also play a role. Migrants whose choices are not revealed are choosing to send less home.

<sup>14</sup>The p-value on the two-sample Wilcoxon rank-sum for equality of distributions is 0.034.

These results are formalized in Table 5.1, which presents the results of estimating regression equation 1. Column 1 is a simple regression of the dependent variable on treatment status, and column 2 adds the demographic control variables. All regressions include stratification group fixed effects. The results are robust to the inclusion of control variables. Migrants send \$20 more when their choice will be revealed, which represents an approximate 5 percent increase over the choice-not-revealed group mean.

**Table 5.1 Impact of monitoring treatment on migrant remittance decision**

Variable	Dependent variable: Amount sent by migrant	
	(1)	(2)
Migrant choice <b>revealed</b> to recipient	20.40** [10.27]	19.50* [10.26]
Migrant is female		-26.46** [11.09]
Migrant age		-0.487 [0.741]
Migrant's years of education		-0.119 [1.225]
Migrant's years in the US		1.968* [1.071]
Migrant lives with spouse		-28.75** [11.83]
Migrant's hh size in US		1.293 [2.800]
Migrant has child 22 or under in El Salvador		0.984 [12.41]
Recipient is migrant's close relative		-0.675 [12.74]
Migrant in lowest income bracket		-21.73* [12.65]
Migrant's annual total remittances to recipient hh		0.00319* [0.00192]
Migrant communicates with recipient hh at least weekly		-1.122 [12.68]
Observations	1,298	1,298
R-squared	0.133	0.159
Mean in treatment = Migrant choice <b>not revealed</b> to recipient	441.4	

Source: Author's calculations.

Notes: hh = household. Robust standard errors in brackets. Sample is observations with nonmissing values for all experiment questions and completed recipient survey. Amount sent by migrant is the amount that migrants chose to send when splitting \$600 between themselves and recipients. All regressions include stratification group fixed effects: dummy variables for the groups of survey numbers within which randomization was stratified. *Recipient* is defined as close relative if migrant reports recipient to be his spouse, parent, or child. Migrants in the lowest income bracket chose \$400 or less as the weekly income of themselves plus their co-resident spouses. The other categories were \$401–\$600, \$601–\$800 and \$801 and above. Annual total remittances are the combination of regular and irregular remittances. Annual regular remittances were collected by asking for the frequency of remittances sent and the average amount sent each time. Annual irregular remittances are remittances sent for special occasions or emergencies. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table 5.1 also reports the coefficients on the demographic control variables included in column 2. Five characteristics predict the migrant's choice. Female migrants send on average \$26 less than male migrants. Although women keep more on average than men, the effect of the treatment does not vary by gender (results not shown). Migrants who have been in the United States longer send more, although the effect is small. Migrants who live with a spouse send \$29 less than those who do not. This is possibly because they are more likely to have their immediate family with them. Migrants in the lowest income bracket are estimated to send \$22 less on average than those in the other income brackets. Finally, total annual remittances sent are positively correlated with amount sent in the experiment. The coefficient is small, but it suggests that migrant behavior in the experiment is related to real-world migrant behavior.

The results in Table 5.1 show that information asymmetries can affect migrants' remittance decisions and that at least some migrants take strategic advantage of a situation where the probability that their income will be observed is very low. The size of the effect (a \$20 increase in amount sent) is not large, but it is similar to the size of the correlations with the demographic variables in Table 5.1. The effect size is also comparable to other experimental studies in families (Hoel 2013) and social networks (Leider et al. 2009; Ligon and Schechter 2012) that study the effects of making choices in dictator games known to the recipient. For example, Ligon and Schechter find that 91 percent of sharing in their experiment is related to altruistic motives. However, they also find that strategic behavior in their games predicts real-world strategic behavior, while altruistic behavior in the games does not predict any real-world activity. This suggests that strategic behavior may in fact be even more important outside of the experimental context than within it. The effect size can also be compared with other studies with experimental designs that are not as similar. Jakiela and Ozier (2012) estimate a 4 percent kin tax on income in an experiment where participants sacrifice returns on income in order to keep it secret. Goldberg (2011) estimates a 7 percent sharing tax on income in an experiment where she compares the spending of the winners of public lotteries with that of the winners of private lotteries.

The framework presented in Section 2 predicts that if recipients cannot threaten to punish migrants, no differences between treatment groups should be observed. Several variables from the baseline survey can plausibly be thought to proxy for punishment costs, and I examine how the treatment effect varies by these variables. I do not have a perfect measure of these potential punishment costs (and certainly one would be hard to obtain); but by showing a consistent pattern with all five of these variables the argument that ability to punish is important is convincing. The five variables, the predicted relationship with punishment ability, and the rationale for choosing them are described below.

- *Migrant years in the United States* (negative correlation): A migrant's reputation at home is important for migrants who wish to return, and the probability of return may decline with length of time in the United States. With time it is also more likely that the migrant has paid off any debts related to his initial migration costs. The median number of years in the United States is 10.
- *Migrant has a child 22 or under in El Salvador* (positive correlation): Migrants may have left children in the care of the recipient, and the possibility of child care that does not meet the migrant's preferences could be a powerful tool; 34 percent of migrants have a son or daughter aged 22 or under in El Salvador.<sup>15</sup>
- *Migrant and recipient are closely related* (positive): This is defined as spouses or parent and child. Migrants may have entrusted recipients with the care of things that are important to them, and positive relationships with the recipients may be valuable to the migrants; 31 percent of migrants and recipients are closely related.
- *Migrant communicates with recipient household weekly* (positive correlation): Frequent communication is a sign that migrants value their relationships with recipients; 71 percent of migrants report communicating weekly with the recipient household.

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<sup>15</sup> The 22-and-under cutoff is used because it was available on the survey that measured the number of young relatives up to college age the migrant had in El Salvador.

- *Remittances sent by migrant to recipient household* (positive correlation): Because remittance relationships where recipients induce migrants to send money result in higher payments, higher remittances may indicate high punishment costs. The median annual remittance total to the recipient household reported by the migrant is \$1,800.

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

Table 5.2 presents regression results by subsamples of these variables. For the continuous variables (years in the United States and remittances) the sample is split at the sample median, and for the binary variables (child in El Salvador, close relationship, and weekly communication) the sample is split according to the two values of the variable. The results are striking in that for each of these variables, the treatment effects are almost entirely concentrated in the subsample where punishment costs should be higher (columns 2, 4, 6, 8, and 10). These treatment effects are larger and more precisely estimated than in the full sample: Depending on the subsample, coefficients range from \$32 to \$59 more sent when the choice is revealed. These numbers are about 7–14 percent of the average amount sent in the choice-not-revealed group. In the subsamples where punishment costs should be low (columns 1, 3, 5, 7, and 9), the coefficients are mostly small and do not approach statistical significance. The table also reports the p-values on the test for equality of the treatment effects in the two subsamples for each of the five proxy variables. Two of the five coefficient pairs in both panels are statistically significantly different from each other. Alternate specifications that use the first principal component of the five proxy variables as a summary measure or interact the proxy variables with the treatment indicator in the full sample yield similar results. These results are consistent with the model’s prediction that when punishment costs are low, variation in the observability of migrant income will not affect migrant remittance decisions.

An alternative explanation of the results in the migrant experiment is that migrants simply care about being perceived as altruistic and use the choice-revealed treatment to signal that altruism to their family. This concept of signaling altruism was developed by Bénabou and Tirole (2006). I cannot definitively rule this out, but the results suggest that it is unlikely. The strong patterns of heterogeneity by subgroup are directly connected to the framework presented here, but it is not obvious how they would relate to a story about signaling. To be consistent with the subgroup results, the variables that describe recipient punishment ability would also have to represent groups to which migrants care about appearing altruistic. However, across all subgroups, the migrants’ allocations in the not-revealed treatment group are high, suggesting that most migrants are altruistic. If the signaling story was true, it would then have to be the case that true altruism and the desire to signal altruistic behavior were not correlated.<sup>16</sup>

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<sup>16</sup> Although the context is different, in an experiment studying the social networks of Harvard students, Leider et al. (2009) are able to definitively rule out the signaling explanation for non-anonymous giving in favor of one based on reciprocity and future interactions.

**Table 5.2 Impact of monitoring treatment on amount sent by migrant: By proxies for punishment ability**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent variable: amount sent by migrant									
	<i>Years in the United States</i>		<i>Migrant has child 22 or under in El Salvador</i>		<i>Recipient is close relative of migrant</i>		<i>Migrant communicates with recipient household weekly</i>		<i>Migrant's annual remittances to recipient household</i>	
	Above sample median	Below sample median	No	Yes	No	Yes	No	Yes	Below sample median	Above sample median
Migrant choice revealed to recipient	19.83 [15.33]	32.75** [15.32]	10.45 [12.79]	59.12*** [20.73]	12.21 [12.78]	36.29* [20.11]	9.414 [22.03]	24.97** [12.47]	-1.532 [15.91]	36.98** [14.84]
P-value for equality of treatment effect	0.551		0.040		0.318		0.573		0.077	
Observations	639	656	853	445	889	402	376	919	611	670
R-squared	0.221	0.201	0.154	0.299	0.159	0.314	0.306	0.152	0.236	0.190
Mean in treatment = Migrant choice not revealed to recipient	457.8	426.1	449.3	424.3	444.4	433.9	449.4	438.4	447.0	437.5

Source: Author's calculations.

Notes: Robust standard errors in brackets. Sample is observations with nonmissing values for all experiment questions, completed recipient survey, and nonmissing values for variables used for division into subsamples. Amount sent by migrant is the amount that migrants chose to send when splitting \$600 between themselves and recipients. All regressions include stratification group fixed effects: dummy variables for the groups of survey numbers within which randomization was stratified. *Recipient* is defined as close relative if migrant reports recipient to be his spouse, parent, or child. Annual total remittances are the combination of regular and irregular remittances. Annual regular remittances were collected by asking for the frequency of remittances sent and the average amount sent each time. Annual irregular remittances are remittances sent for special occasions or emergencies. The median years in the United States is 10 and the median remittances sent to the recipient household are \$1,800. \*\*\* p < 0.01, \*\* p < 0.05, \* p < 0.1

## Recipient Experiment

Analysis of the migrant experiment found that migrants react strategically to variations in the ability of recipients to monitor their income. Previous literature examining information asymmetries in remittance behavior has suggested that migrant monitoring of recipients should also be important (de Laat 2008; Ashraf et al. 2011; Chen 2013). To look for these effects in the context of this experiment, I now turn to analysis of the recipient experiment in which recipients allocated a potential \$300 remittance prize among four different spending categories.

Mean amounts allocated to different spending categories by recipients and migrants are presented in Table 5.3. The first two columns show the mean amounts allocated by recipients broken down by the recipient-monitoring treatment, and columns 3 and 4 show mean recipient allocations by the recipient communication treatment. The fifth column shows the means of the preferences reported by the migrant. Across both recipients and migrants, education is the most popular choice.<sup>17</sup> Daily expenses are the next

<sup>17</sup> The preference for education may be partly due to the fact that participants answered this question at the conclusion of a survey that was rather heavily focused on questions about education, meaning that they may have been primed to consider education. This is not necessarily a problem as there is no reason to believe that either migrants or recipients were more primed

most popular category, followed closely by health and finally restaurant meals. Migrants allocate less to education than recipients and more to daily expenses, health expenses, and restaurant meals, but no strong evidence indicates that migrants on average prefer different expenditure categories than recipients.

**Table 5.3 Mean amounts allocated to spending groups by recipients and migrants: Recipient experiment**

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

Source: Author's calculations.

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

I utilize the data collected from both the migrant and the recipient to analyze how the treatments affect the pair-level differences between their choices. Table 5.4 shows the results from estimating regression equation 2. The dependent variables in columns 1 through 4 are the migrant–recipient differences in restaurant spending, education spending, spending on daily expenses, and health spending, respectively. The dependent variable in columns 5 and 6 is the total migrant–recipient difference. Column 6 adds demographic control variables. The control variables are the same as those presented in Table 5.1 with the addition of recipient gender, age, years of education, household size, and a control for number of days between the migrant and the recipient surveys.

than the other.

**Table 5.4 Impact of monitoring and communication treatments on recipient allocation decision**

Treatment	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: migrant–recipient difference in...				Dependent variable:	
	Restaurant spending	Education spending	Daily expenses spending	Health spending	Total migrant–recipient difference	
Recipient choice <b>revealed</b> to migrant	–0.999 [2.141]	4.287 [4.922]	4.001 [4.206]	–1.251 [4.234]	3.019 [4.775]	3.330 [4.827]
Migrant preference <b>revealed</b> to recipient	–2.741 [2.119]	–14.18*** [4.936]	–6.154 [4.228]	–4.137 [4.196]	–13.61*** [4.753]	–13.69*** [4.818]
Observations	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.101	0.104	0.092	0.091	0.105	0.122
Mean in recipient choice <b>not revealed</b>	15.9	107.3	78.0	75.0	138.1	
Mean in migrant preference <b>not revealed</b>	16.7	116.3	83.0	76.6	146.2	
Control variables	NO	NO	NO	NO	NO	YES

Source: Author’s calculations.

Notes: Robust standard errors in brackets. Sample is observations with nonmissing values for all experiment questions and completed recipient survey. Dependent variables are the absolute difference between the recipient’s choice and the migrant’s preferences in each category. The total difference is the sum across the four difference variables for each observation, divided by 2. All regressions include stratification group fixed effects: dummy variables for the groups of survey numbers within which randomization was stratified and treatment status in the migrant experiment. Control variables are migrant and recipient gender, age, years of education, and household size. Controls also include migrant years in the United States, whether migrant lives with spouse, whether migrant has a child 22 or under in El Salvador, whether the migrant and recipient are close relatives, if the migrant is in the lowest income bracket, annual total remittances to recipient household, whether the migrant and recipient communicate at least weekly, and the number of days between migrant and recipient surveys. \*\*\* p < 0.01, \*\* p < 0.05, \* p < 0.1.

For both the monitoring and the communication treatments, the prediction is that the difference will be smaller in the revealed treatment group. When the probability is high that spending choices will be observed or when recipients are well informed about migrant preferences, recipients should more greatly adhere to those preferences. This prediction is not borne out for the monitoring treatment. For all spending categories and the total difference across specifications, the coefficients for the monitoring treatment are small in magnitude and statistically insignificant. However, differences are evident for the communication treatment. For all spending categories, the coefficient on the variable indicating that the migrant preference was revealed to the recipient is negative. This means that the differences between recipient choices and migrant preferences are smaller when the migrants’ preferences are revealed than when they are not. Of all the spending categories, only the difference for education (column 3) is statistically significant, but importantly, so is the total difference (columns 5 and 6), implying that migrant and recipient choices are getting closer together overall. Across specifications the results indicate that there is a \$14 reduction in the total difference when the migrant’s preferences are revealed. This represents about a 10 percent reduction relative to the mean in the preference-not-revealed group. The effect is driven by the difference in education spending, with the corresponding reductions in the differences in other categories being split between daily and health expenses and, to a lesser extent, spending on restaurant meals.

I also examine the interaction of the two treatments (results not shown) and find that the impact of the communication treatment does not vary with the monitoring treatment. There is also no consistent pattern of variation when the results of the monitoring treatment are examined by punishment ability (results not shown). This is not surprising given the very small estimated coefficients in the in the full sample.

The impact of the communication treatment suggests that migrant preferences do matter to recipients and that some deviation from those preferences may be inadvertent. However, the lack of impact of the monitoring treatment further implies that recipients do not react strategically to changes in the probability of detection. The framework presented in Section 2 proposes an explanation for why recipients may not take advantage of the opportunity to hide their spending choices from migrants. Migrants simply may have limited ability to punish the recipients for not following their preferences. While the results in the migrant experiment are that recipient ability to punish varies across recipients, these results from the recipient experiment suggest that, in this context, migrant ability to punish is low across the population. In practice, this would result in a situation where the migrants have very little power to compel recipients to spend the remittances as they wish.

Although a limited ability to punish is the explanation for the lack of effect of the monitoring treatment that is suggested by the framework, it is important to consider other possible explanations. The first alternative explanation is that migrant monitoring of recipients is essentially perfect and that recipients know that their choices will be discovered if they win. However, some information collected on the baseline survey suggests that this is not the case. Table 4.1 provides summary statistics of some questions that aimed to measure migrant knowledge of the recipient household. Only 24 percent of migrants could correctly report student grade point average (GPA), and 43 percent correctly report how students travel to school; therefore it does not seem plausible that existing monitoring is good enough across the board to render the experimental variation irrelevant. A second explanation is that migrants and recipients have the same preferences for spending, and therefore they make the same choices regardless of punishment ability. This may be true for some families, but if it were true for most, there should be no impact of the communication treatment. Additionally, only 48 percent of migrant–recipient pairs report the same three budget priorities (Table 4.1), further evidence of heterogeneity in preferences.

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

## Discussion

Economic studies of information asymmetries in households with migrants have until now focused on migrant monitoring of recipient behavior (Chen 2013; de Laat 2008) and the impacts of offering migrants greater control over how remittances are spent (Ashraf et al. 2011; Chin et al. 2011; Torero and Viceisza 2011). This is the first study that explicitly looks at the effect of information asymmetries on *both* sides of the remittance relationship—migrants' sending of remittances as well as recipients' spending of those remittances. Despite the previous emphasis on migrant monitoring, the results of the two monitoring

treatments presented in this paper are that, in this context, only migrants, and not recipients, strategically react to variations in the probability that their actions will be monitored.

This is an important finding, not only because it shows that information asymmetries have an important impact on the remittance sending decision, but also because this implies that recipients have important power in the migrant–recipient relationship. Although this influence has been considered in the extensive literature on the motivations to send remittances, it has not previously been rigorously documented empirically. Policymakers who seek to design tools to facilitate the sending of remittances and enhance their impacts<sup>18</sup> should consider the role of the recipient in determining remittance amounts. Policies that assume that migrants have complete autonomy over the manner in which remittances are sent may fall short of their full potential.

The analysis in this paper indicates that because migrants are responding to the opportunity to hide income, some of them are already sending home more than they would choose to voluntarily. This implies that programs that seek to increase remittances further will face difficulties within this group. Policymakers should also consider the welfare implications of such a policy. The pressure that migrants face to send remittances is related to a growing literature on sharing pressures in family networks in developing countries. Recent work by di Falco and Bulte (2011) and Jakiela and Ozier (2012) suggests that the expectation that resources will be shared with extended family may inhibit individual economic progress. In this context, particularly given the low income status of the migrants, the results indicate the possibility that any potential extra remittance funds may be more efficiently used by the migrants in the United States than by their family members at home.

Additionally, the fact that the monitoring treatment had no effect on the recipients' spending decision adds a new angle to the recent work on the impact of control on remittance behavior (Ashraf et al. 2011; Chin et al. 2011; Torero and Viceisza 2011). To varying degrees, these studies offer migrants direct control over money sent to family members at home. Viewed through the framework presented in this paper, control over remittances improves both the monitoring and enforcement of remittance-spending contracts, but the existing studies are not able to distinguish between the two channels. The results of this study—that migrant monitoring of recipient remittance spending does not seem to matter—suggest that if migrants do desire control (and the literature is mixed on whether or not they do), this desire may not be due to an inability to monitor the recipients but rather to an inability to effectively punish recipients and therefore compel recipients to spend remittances in a certain way. In the absence of punishment ability, the ability to control would act as the enforcement mechanism in the migrant–recipient contract.

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

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<sup>18</sup> See Yang (2011) for a discussion.

## 6. CONCLUSION

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

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

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

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

## APPENDIX A: SUPPLEMENTARY TABLES

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

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

Source: Author's calculations.

Notes: Baseline survey sample is observations with nonmissing values for all experiment questions and completed recipient survey. Sample size varies slightly with variable: 1,264 for age; 1,295 for years in United States; 1,290 for education variables. American Community Survey (ACS) sample is the Integrated Public Use Microdata Series (IPUMS) three year 2008–2010 ACS sample restricted to individuals aged 18–65 in the Washington, DC, metro area (as defined by the ACS, includes Maryland and Virginia suburbs). Standard deviation in brackets for continuous variables.

**Table A.2 Impact of monitoring and communication treatments on recipient allocation decision: By information quality measure**

	(1)	(2)	(3)	(4)
<b>Dependent variable: Total migrant–recipient difference</b>				
	<i>Migrant correctly reports student grade point average (GPA)</i>		<i>Migrant correctly reports student mode of transport to school</i>	
<b>Indicator</b>	No	Yes	No	Yes
Recipient choice <b>revealed</b> to migrant	2.248 [6.215]	3.501 [14.18]	−1.354 [7.147]	11.14 [8.461]
Migrant preference <b>revealed</b> to recipient	−16.77*** [6.147]	4.624 [13.94]	−23.99*** [7.351]	−4.349 [8.413]
<i>P-values for equality of treatment effects:</i>				
Monitoring treatment		0.928		0.257
Communication treatment		0.118		0.078
Observations	787	254	633	474
R-squared	0.157	0.397	0.228	0.243
Mean in recipient choice <b>not revealed</b>	136.9	128.7	139.7	132.9
Mean in migrant preference <b>not revealed</b>	148.5	132.2	148.5	140.7

Source: Author's calculations.

Notes: Robust standard errors in brackets. Sample is observations with nonmissing values for experiment questions, completed recipient survey, and nonmissing values for variables used for division into subsamples. Responses to GPA and transport questions were recorded only if student was reported to be in school. Migrants were asked to report the student's GPA within a 2 point (out of 10) range, while recipients reported an exact number. The migrant is said of have correctly reported the GPA if the recipient's response was within the range the migrant indicated. All regressions include stratification group fixed effects: dummy variables for the groups of survey numbers within which randomization was stratified and treatment status in the migrant experiment. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## APPENDIX B: THEORETICAL FRAMEWORK

In this appendix I more formally develop the theoretical framework described in Section 2 of this paper. As explained in the main text, the structure of the model is based on Chen's (2013) description of how male migrants in China monitor their wife's behavior. I adapt a simplified version of this framework to describe the outcomes considered in this paper.

### Migrant Remittance Decision

I characterize migrants' decisions to remit as being determined by both their altruism for their families at home and contracts with those same families, where the families compel the migrants to send remittances through the threat of potential punishment.<sup>19</sup> An extensive literature exists on the motivations of migrants to send remittances. Commonly cited motives include altruism, payments for services provided by the family, loan repayment, repayments of other investments made by the family such as education, desire to return home, and insurance (see Rapoport and Docquier 2006 for a review). These motives may operate simultaneously, and while there is empirical evidence to support the existence of them all, the literature has been less successful in defining their relative importance. The purpose of this framework is to model the remittance decision in a way that allows for both motivations that may be affected by strategic behavior and those that will not be. Although this model is not specific about the exact motivations for the remittances sent by migrants, the idea of a remittance contract enforced through the threat of a punishment cost encompasses most possible motivations previously examined in the literature. The clear exception is altruistically motivated remittances, which will enter separately in this framework.

The model is constructed as a game with two types of players: migrants who send remittances, and members of their families who receive those remittances. Migrants and recipients both get utility from consumption, which is defined for migrants as migrant income ( $I$ ) minus remittances sent to the recipient ( $r$ ), and for recipients as recipient income ( $Y$ ) plus remittances received from the migrant ( $r$ ). Because they are altruistic, migrants additionally derive utility from the consumption of recipients.<sup>20</sup> Migrant utility is then defined as

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

and recipient utility as

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

For both  $u^M$  and  $u^R$ ,  $u' > 0$  and  $u'' < 0$ .  $\gamma$  is the migrant's altruism parameter and is between 0 and 1.<sup>21</sup> In every period migrants earn either low income ( $I^L$ ) or high income ( $I^H$ ) where  $I^H > I^L$ .<sup>22</sup> The recipient strategy is to offer migrants a contract that specifies the remittance amounts that should be sent for each income level. Migrants then decide whether to comply with this contract or deviate from it. Migrants who deviate (and whose deviation is discovered by the recipient) will suffer a utility cost ( $C^M$ ) imposed by the recipient. This cost is assumed to be exogenous to the model but will vary across migrant and recipient pairs. Migrants and recipients know each other's preferences and the value of  $C^M$ .

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<sup>19</sup> The incorporation of these two motives together in one framework is drawn from Lucas and Stark's (1985) suggestion of a model of remittance sending that includes both altruism and migrant self-interest.

<sup>20</sup> For simplicity the framework does not include recipient altruism toward the migrant.

<sup>21</sup> Migrant altruism has been modeled in similar ways in Lucas and Stark (1985), Stark (1995), and Rapoport and Docquier (2006).

<sup>22</sup> Although variation in recipient income can affect migrant remittance decisions (as in Lucas and Stark 1985) for the purposes of this paper, I assume  $Y$  to be fixed and low relative to migrant income.

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

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

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

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

$$u^{M'}(I^i - r^{d^i}) = \gamma u^{R'}(Y + r^{d^i}).$$

This first-order condition implies that migrants set the marginal cost of remittances equal to their marginal benefit. Any further increase in remittances will therefore incur a higher cost than benefit for the migrant and lead to a net loss in utility.<sup>23</sup>

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

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

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

## Asymmetric Information

Now consider the case where recipients have imperfect information about migrant income. At the time of the remittance, the only information about migrant income that recipients have is what they are told by migrants. However, after the remittance is sent, with probability  $p$  recipients will receive accurate information about migrant income, informing them of whether the migrants earned  $I^H$  or  $I^L$ .<sup>24</sup> Recipients who do not receive this information continue to believe what the migrants have told them about their income. This gives migrants who have earned  $I^H$  the opportunity to deviate without being discovered by claiming they earned  $I^L$  and sending the contracted amount for the lower income level ( $r^{c^L}$ ).<sup>25</sup> With

<sup>23</sup> I assume that conditions hold for  $r^{d^i}$  to be nonnegative. For example, assuming that both  $u^M()$  and  $u^R()$  are equal to  $\ln()$ ,  $r^{d^i} \geq 0$  if  $\gamma I^i - Y \geq 0$ .

<sup>24</sup> For example, imagine a situation where a migrant earns  $I^H$  because he finds some extra temporary work. The recipient may hear about this work from another relative or family friend living in the same community as the migrant in the United States.

<sup>25</sup> Note that nothing has changed for migrants earning  $I^L$ , as the imperfect information does not afford them any more attractive deviation possibilities.

probability  $1 - p$  the recipients will not discover the true income level, and the migrants will not have to pay  $C^M$ . For migrants who deviate in this way,  $p$  is the probability that that deviation will be detected.

Furthermore,  $p$  is not constant and can vary across time for each migrant. In every period the migrants know what  $p$  is; however, recipients have incomplete information about  $p$ , knowing only the distribution of its possible values. Assume that  $p$  can be either low ( $p^l$ ) or high ( $p^h$ ) and that the recipient believes that  $p = p^h$  with probability  $k$ .<sup>26</sup> The payoffs for migrants earning  $I^H$  are now

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

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

When deviating the migrant will send  $r^{c^L}$  because that is the only possible method of deceiving the recipient and avoiding punishment.<sup>27</sup>

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

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

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

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

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

Because  $p^l < p^h$ ,  $r^{c^H}$  must be lower in periods when  $p = p^l$  than in periods when  $p = p^h$  in order to satisfy the migrant's incentive compatibility constraint. Given that recipients do not know the value of  $p$ , they must satisfy the constraint for  $p^l$  in order to ensure participation in all periods. The constraint for low probability of detection periods will bind, but the constraint for high probability of detection periods will not.

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

<sup>26</sup> Continuing with the example where a migrant earns  $I^H$  because he finds some extra temporary work,  $p$  may be high if another migrant from the migrant's home village has the same job and can relay this information to family members.

<sup>27</sup> Migrants could also deviate by sending  $r^{c^H}$  and paying  $C^M$  for sure. It is possible that the utility of this strategy is greater than the expected utility of sending  $r^{c^L}$ . This would lead to an incentive-compatible contract unaffected by information asymmetries and therefore will not be considered here.

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

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

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

- *Prediction 1:* When the probability that recipients will observe migrant income is low, migrants earning high income may strategically take advantage of recipients' imperfect and incomplete knowledge of their income to send less money home.
- *Prediction 2:* In pairs where the recipient ability to punish migrants is low, migrants' motivations for sending remittances are dominated by altruism, and these altruistic remittances are not affected by the probability that migrant income will be observed.

## Recipient's Spending Decision

I now consider the recipient's decision about how to spend remittances in a separate framework that can be developed in a parallel way. The decision that recipients make is modeled as the extent to which they follow the migrant's preferences for that spending decision. Recipients get utility from spending the remittance money on the things that they prefer, but they are also altruistic in that they get utility from spending remittances according to the migrants' wishes.<sup>28</sup> Although recipient altruism is modeled here as the recipient's getting utility from the migrant's utility, the concept could also include recipients who follow migrant preferences simply because they want to. For example, they may value migrant advice on household budgeting and investment.

Migrants offer recipients a contingent contract specifying the extent to which remittances should be spent according to migrant preferences. Recipients then decide whether to comply with or deviate from that contract. With probability  $q$  migrants will learn how the recipients spent the remittance; otherwise, they will know only what they are told by recipients (and believe that to be true).<sup>29</sup> Recipients who deviate and are discovered by the migrant will pay a utility cost  $C^R$ , which is the punishment that the migrant can impose on the recipient. Potential punishments include withholding of future remittances, social sanctions (to the extent that the migrant can impose them from a distance), and familial discord. The size of the punishment ( $C^R$ ) need not be equal to the punishment the recipient can use against the migrant ( $C^M$ ), meaning that one may well have greater influence than the other.

In the equations below,  $d$  is what recipients would consume if they followed only their own preferences, and  $b^c$  and  $b^d$  are the extent to which the recipients follow migrant preferences when they comply with the contract ( $b^c$ ) and when they deviate from it ( $b^d$ ).<sup>30</sup>  $\alpha$  is the recipient's altruism parameter. Recipient payoffs can be expressed as follows:

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

<sup>28</sup> For simplicity of exposition I ignore a third category of consumption: expenditures on which the migrant and recipient agree. Incorporation of this category does not change the qualitative predictions of the model.

<sup>29</sup> Migrants could find out about recipient spending behavior by, for example, communicating with other family members in El Salvador that may have knowledge of what the recipient has done.

<sup>30</sup> For example, imagine that a migrant sends a \$200 remittance for which the migrant wants \$100 to be spent on food and \$100 to be spent on education. The recipient wants to spend \$200 on home improvements. If the recipient actually spends \$100 on food and \$100 on home improvements, the recipient has followed the migrant's preferences on \$100 of the \$200 remittance.

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

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

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

- *Prediction 1:* When the probability that migrants will observe recipient spending is low, recipients may strategically take advantage of migrants' imperfect and incomplete knowledge of their spending to spend less according to migrant preferences and more according to their own preferences.
- *Prediction 2:* In pairs where migrant ability to punish the recipient is low, recipients' motivations for spending remittances according to the migrants' preferences are dominated by altruism, and this altruistic spending is not affected by the probability that recipient spending will be observed.

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

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

In sum, strategic deviation can be a feature of the optimal contracts between migrants and their family members. The extent to which deviation occurs will depend on the distribution of the probability of detection and the size of the punishments that can be inflicted. In addition, barriers to communication can lead to inadvertent deviation when recipients make remittance spending decisions.

## APPENDIX C: TEXT USED IN MIGRANT EXPERIMENT

**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)*

- |                                      |     |                                      |
|--------------------------------------|-----|--------------------------------------|
| <input type="checkbox"/> KEEP: \$600 | and | <input type="checkbox"/> SEND: \$0   |
| <input type="checkbox"/> KEEP: \$500 | and | <input type="checkbox"/> SEND: \$100 |
| <input type="checkbox"/> KEEP: \$400 | and | <input type="checkbox"/> SEND: \$200 |
| <input type="checkbox"/> KEEP: \$300 | and | <input type="checkbox"/> SEND: \$300 |
| <input type="checkbox"/> KEEP: \$200 | and | <input type="checkbox"/> SEND: \$400 |
| <input type="checkbox"/> KEEP: \$100 | and | <input type="checkbox"/> SEND: \$500 |
| <input type="checkbox"/> KEEP: \$0   | and | <input type="checkbox"/> 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)	
<b>Total (verify adds up to \$300):</b>	

## APPENDIX D: TEXT USED IN RECIPIENT EXPERIMENT

**Question 1:** Because name of migrant participated in our study, you now have the chance to receive a remittance worth \$300. Some participants like you will be chosen to receive this remittance. However, this remittance can be spent on only 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)	
<b>Total (verify adds up to \$300):</b>	

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