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

Kate Ambler
University of Michigan
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Abstract: Although most theoretical models of the household have assumed perfect information, empirical studies suggest that information asymmetries can have large impacts on resource allocation. In this study, I demonstrate their importance in transnational households, where physical distance between family members can make information barriers especially acute. I implement an experiment among 1,300 Salvadoran migrants in Washington, DC and their family members in El Salvador that examines how (1) changing the ability of participants to observe each other and (2) revealing migrant preferences can affect the sending and spending of remittances. Migrants make an incentivized decision over how much of a cash windfall to keep and how much to send home, and recipients over how to allocate the spending of a remittance. Migrants remit significantly more when their choice is observed by recipients, and this effect is concentrated among pairs where recipient ability to punish migrants is plausibly high. The results support a model of remittance sending where migrants react strategically to being monitored, but only when recipients can enforce remittance agreements. Recipients make spending choices closer to the migrants' preferences when they are revealed, suggesting that recipients' choices may be inadvertently affected by imperfect information on migrant preferences. Together, these results indicate that information imperfections in families are varied and can affect resource allocation in both strategic and inadvertent ways.

*Department of Economics. Email: ambler@umich.edu. Diego Aycinena and Dean Yang contributed as co-authors on a companion experiment. Jessica Snyder provided excellent research assistance. I thank Sarah Burgard, David Lam, Rebecca Thornton, Dean Yang, Morgen Miller, Caroline Theoharides and seminar participants at the University of Michigan for comments and guidance. I am grateful for the support of the Salvadoran consulates. I also thank Alan de Brauw and Angelino Viceisza. This project was funded by a grant from the Tokyo Foundation. The companion project was funded by the Inter-American Development Bank. This research was supported in part by an NICHD training grant to the Population Studies Center at the University of Michigan (T32 HD007339).

I. 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 intra-household resource allocation have largely assumed perfect information (Chiappori, 1988, 1992; Manser and Brown, 1980; McElroy and Horney, 1981; Lundberg and Pollack, 1993).[1,2] Despite this, a growing body of empirical literature has shown that informational 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, 2010; Schaner, 2012).[3] This paper brings the study of how information asymmetries affect intra-household resource allocation to a new setting: transnational households, or international migrants and their family members in the home country, in this case El Salvador.

The context of transnational households is significant because migrants and their family members are making financial decisions about remittances, which are a large and important financial flow. Global aggregate remittances to the developing world were $332 billion in 2010, more than any other kind of resource flow with the exception of FDI (World Bank, 2012). In El Salvador specifically remittances received were 16 percent of GDP in 2010 (World Bank, 2012). In 2009, 22 percent of households in El Salvador received remittances and average monthly remittances were $168 for families that received them, a figure that is almost 50 percent of average monthly household expenditures for remittance recipients (DIGESTYC, 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 Zentano, 2007; Yang, 2008; Adams and Cuecuecha, 2010).

These decisions about remittances are made 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

[1] Exceptions include Bloch and Rao (2002) and Chen (2013).
[2] Empirical studies including Lundberg, Pollack and Wales (1996), Duflo (2000), Duflo (2003), Qian (2008) and Ambler (2012) have supported some of the predictions of these models but do not account for information asymmetries.
[3] In a separate group of empirical studies Udry (1996), Dercon and Krishnan (2000), Goldstein, de Janvry and Sadoulet (2005), and Dubois and Ligon (2011) show that intra-household resource allocation may be inefficient in some contexts, results that may be indicative of the presence of information asymmetries.

be the most pronounced. A number of studies have documented the existence of these asymmetries in households with migrants. For example, De Laat (2008) shows that migrants in Kenya spend resources on costly monitoring of their rural wives. Chen (2006, 2013) finds that in China wives with migrant husbands exhibit non-cooperative behavior more often for activities that are more difficult to monitor, and Seshan and Yang (2012) find suggestive evidence that Indian migrants underestimate how much their wives at home are saving. However, this is the first study to causally examine how information asymmetries directly affect behavior, specifically decisions about the sending and spending of remittances. Given the importance of remittances for development, a more complete understanding of how these decisions are made is crucial for policy makers hoping to maximize their productive potential.

This paper addresses two types of information asymmetries that may affect decisions about the sending and spending of remittances. The first are those that can lead to *strategic* behavior, specifically the limited abilities of remittance recipients to observe migrant income and of migrants to observe recipient spending. The second are those that can have *inadvertent* impacts, 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 full understanding of these preferences. I first develop a theoretical framework that derives predictions for 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 contracts (whether implicit or explicit) that dictate how much of their income migrants should send to recipients and what that money should be spent on when received by the recipients. The contracts are enforced through the threat of punishment for noncompliance. I show that under imperfect and incomplete information about migrant income and recipient spending, strategic deviation by both migrants and recipients can be a characteristic feature of these contracts. However, the strategic effects will be less important in pairs where punishment costs are low and remittances are therefore mostly motivated by altruism. Additionally, communication barriers, specifically in regards 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 behavior. They were designed to mimic real life decisions about remittances made by migrants and their family members and by randomly assigning treatment I am able to causally identify the impacts of the informational conditions being tested. An experiment was first conducted among Salvadoran migrants recruited in the Washington, DC area. They were asked of how much of $600 they wished to keep and how much they wished to send to a family member in El Salvador. The decision was incentivized; 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.

Then, these same family members participated in a companion experiment. They made an incentivized decision about how to spend a $300 remittance. 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 barriers to communication, half of the recipients were informed of the migrant's preferences for how they should spend the money, and the other half were not.

I find that migrants remit $24 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 and that this effect is concentrated (and is 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. In other words, recipients who are told their choice will be revealed do not make choices that are more similar to the migrants' preferences than recipients whose choices are not revealed. 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 has 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) finds 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. In another experiment among Salvadoran migrants, Torero and Viceisza (2011) find little evidence that migrants send more when they are able to control how remittances are spent, but attribute this to the fact that the control offered by their experiment (vouchers for groceries) was too limiting.

The main limitation of these papers is that while they acknowledge that migrants might have difficulty controlling recipient spending of remittances, they do not consider that information problems might run in both directions. Observational studies documenting information asymmetries in migrant households have also focused on migrant monitoring of recipient behavior (Chen, 2006, 2012; 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 empirically shown.

This paper also fits into a growing, broader literature on how information asymmetries affect intra-household resource allocation. 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 (2012) 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 others' finances.[4]

This study builds on this literature in several ways. First, while these papers have largely focused on just one choice (whether or not to share income), the present experiment considers how information asymmetries can affect the different decisions made by families about

[4] 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, 2010).

economic resources. Specifically, I also examine the next step of the resource allocation process, how income is spent once it is shared, and acknowledge that family members may also make strategic decisions in the face of information asymmetries in this second stage. Second, I study the effects of different types of information asymmetries, strategic and inadvertent, allowing me to evaluate their relative effects in the same population. Finally, this study documents the importance of information asymmetries outside of the husband-wife pair, the setting that has been the context of almost all the previous work in this area. People in developing countries often transfer resources within extended families (whether within or across households) and 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 given that migrants only react to being monitored when proxies for the recipients' ability to punish them are high, they also indicate that they may not matter in all families where resources are shared.

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

II. Theoretical framework

In this section I develop a simple model to frame my experimental results that shows how information asymmetries can lead to strategic behavior that affects both migrant sending and recipient spending of remittances. I achieve this by modeling both decisions as contingent contracts with an altruistic component between the migrant and the recipient. The structure of the model is similar to Chen's (2013) description of migrant monitoring of wives' behavior. Specifically, Chen shows that when a migrant has imperfect information about his wife's actions and, further, incomplete information about her preferences, the contingent contract offered to the wife by the migrant may not be incentive compatible in all circumstances. I adapt this framework to describe the decisions considered in this paper.

A. Migrant remittance decision

I characterize migrants' decisions to remit as being determined both by their altruism for their families at home and contingent contracts with those same families, where the families

compel the migrants to send remittances through the threat of potential punishment.[5] An extensive literature exists on the varying motivations of migrants to send remittances. Commonly cited motives include altruism, exchange, loan repayment, repayments of other investments made by the family such as education, desire to return, insurance, and others (Lucas and Stark, 1985; de la Briére et al., 2002; Yang and Choi, 2007 among many others. Also 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 discussion then is not to explicitly describe all reasons why migrants might send remittances, but to model the decision in a way that allows both for motivations that may be affected by strategic behavior and those that will not be. Although this model is not specific about the exact motivations of the remittances sent by the 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 potential punishment that enforces the contingent contract will be represented as a utility cost to the migrant and can take several forms. 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 where there are 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 refusing to send home as much money as their families want may damage their relationships with their families, relationships that migrants with tenuous positions in foreign countries may view as important.[6] Many of these potential punishments are related to the social closeness of migrants and recipients and, indeed, in a qualitative study of Ghanian migrants to the Netherlands, Mazzucato (2009) emphasizes the importance of the social proximity of migrants and recipients for the effective enforcement of remittance agreements.

[5] 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.

[6] 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 one or more of 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.

Description of the model

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.[7] 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 $u'' < 0$. γ is the migrant's altruism parameter and is between zero and one.[8] In every period migrants earn either low income (I^L) or high income (I^H) where $I^H > I^L$.[9] The recipient strategy is to offer migrants a contingent 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 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.

First consider the case where migrant income is fully observable to both migrants and recipients. Migrants' 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:

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

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

The optimal values of r^{c^i} and r^{d^i} are solved for using backward induction. First, given I and γ,

[7] For simplicity the framework does not include recipient altruism toward the migrant.

[8] Migrant altruism has been modeled in similar ways in Lucas and Stark (1985), Stark (1995), and Rapoport and Docquier (2006).

[9] 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. This is a reasonable assumption. Although I do not have detailed information on the income of the recipients in the study, 78 percent report that they believe they are in the bottom sixth of the income distribution in El Salvador.

migrants choose r^{d^i} to maximize their payoffs when deviating such that

$$u^{M\prime}\left(I^i - r^{d^i}\right) = \gamma u^{R\prime}\left(Y + r^{d^i}\right)$$

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.[10]

In order 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:

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

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 more realistic 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. 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

[10] I assume that conditions hold for r^{d^i} to be non negative. For example, assuming that both $u^M()$ and $u^R()$ are equal to $ln()$, $r^{d^i} \geq 0$ if $\gamma I^i - Y \geq 0$.

sending the contracted amount for the lower income level (r^{c^L}).[11] With probability p the recipients will not discover their true income levels 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.[12] The payoffs for migrants earning I^H are now:

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

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

When deviating the migrant will send r^{c^L} because that is the only possible method of deceiving the recipient and avoiding punishment.[13]

As in the case of observable income 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 $p = p^l$*:*

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

Periods when $p = p^h$:

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

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.

[11] Note that nothing has changed for migrants earning I^Las the imperfect information does not afford them any more attractive deviation possibilities.

[12] For example, imagine a situation 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.

[13] Migrants could also deviate by sending $r^{a^H}$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.

However, depending on the values of p^l, p^h and k recipients have another option: to offer a contract that is binding on the high probability of detection period's incentive compatibility constraint but which 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, then they will receive the amount that satisfies the constraint for p^lin 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

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

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, then 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: Variation in the probability that recipients can observe migrant income can cause migrants to strategically deviate from their agreements with recipients when that probability is low. When earning high income and under the conditions described above, migrants will take advantage of recipients' imperfect and incomplete knowledge of their income to send less money home.

Prediction 2: In pairs where C^M is low, migrants' motivations for sending remittances are dominated by altruism and these altruistic remittances are not affected by changes in the probability that income will be observed.

B. Recipient spending decision

The framework for the recipient's decision over how to spend remittances received 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.[14] Although recipient altruism is modeled here as the recipient 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 only know what they are told by recipients (and believe that to be true). Recipient deviations that 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 in this case 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.

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).[15] α is the recipient's altruism parameter. Recipient payoffs can be expressed as follows:

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

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

[14] 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.

[15] 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 then the recipient has followed the migrant's preferences on $100 of the $200 remittance.

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: Variation in the probability that migrants will observe recipient remittance spending can cause recipients to strategically deviate from their agreements with migrants when that probability is low, spending less according to migrant preferences and more according to their own preferences.

Prediction 2: In pairs where C^R is low, recipients' only motivations for following migrant preferences are altruistic and will not be affected by changes in the probability that the migrant will observe their behavior.

The recipient choice is further complicated by the fact that barriers to communication may result in confusion on the recipient's part over what the migrant's preferences actually are and consequently in inadvertent (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 over preferences may become difficult, the migrant may feel uncomfortable expressing what he wants, and family members may sometimes have to make decisions without time to directly consult with the migrant. Family members may also incorrectly assume that they know what the migrant would prefer. If these communication costs do play a role, decreasing them by making migrant preferences clearer could increase b, leading to the following prediction:

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

The main point of this discussion is that strategic deviation can be a feature of the optimal contracts between migrants and their family members. The extent to which deviation is important will depend on the distribution of the probability of detection and the size of the punishments that can be inflicted. At the same time, communication costs can lead to inadvertent deviation when recipients make remittance spending decisions.

III. Project Design

Testing for the effects of information asymmetries in the choices made by migrants and their family members is difficult for several reasons. First, the observability of both migrant

income and recipient spending and the extent of communication costs may be correlated with unobserved characteristics of the migrant-recipient pair, making it difficult to causally identify their impacts. Second, measuring any of these (observability of income and spending and communication costs) is difficult in a standard survey context. Finally, capturing reliable information about the behavior of both migrants and their family members is logistically complicated. To solve these problems I implement a randomized experiment to test the predictions of the framework. This experiment is conducted within the context of the survey work for a separate field experiment on remittances and education among Salvadoran migrants in Washington, DC and their families in El Salvador.[16] 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 will be 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[17] and were interviewed while they were waiting for consular services.[18] 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 have a high school or college-aged relative in El Salvador.[19] Those who qualified and agreed to participate were administered a baseline survey followed by the randomized offer of a product designed to facilitate the sending of remittances for education to El Salvador.[20] The experiment described in this paper was conducted at the end of the survey but before the randomized marketing treatment.

[16] "Subsidies for Human Capital Accumulation: A Field Experiment Among Migrants from El Salvador." Joint with Diego Aycinena and Dean Yang.
[17] 96% of migrants interviewed live in Washington, DC, Maryland or Virginia. The others live in states served by these consulate locations.
[18] The most common reason to go to the consulate is passport renewal, but other services include renewal of temporary protected status (TPS), registry of births and deaths, and notarization of documents.
[19] 24% of migrants approached participated. Of those that did not participate, 77% did not have an eligible student, 14% refused, 7% were not from El Salvador, and 2% had other reasons.
[20] 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.

Over the course of the survey migrants identified a high school or college aged student in El Salvador whom they were interested in supporting.[21] Interviews were subsequently conducted with a member of the household of that student. If the student was 18 years of age or older the student was to be interviewed (45% of cases), and for those students under 18 a guardian was identified to be interviewed (40% of cases). If the indicated person was not available, an alternative adult in the household was interviewed instead (15%). 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 survey was eight. The El Salvador surveys concluded in mid March 2012, roughly two weeks after the conclusion of field work in the United States. 82 percent of El Salvador families completed the survey. The experiment in the El Salvador survey was also conducted at the end of the survey. Figure 1 describes the phases of the project in the order that they occurred for each pair of participants.

A. 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 send to their family member in El Salvador. Migrants could split the $600 as they wished, but were restricted to using $100 intervals for simplicity.[22] The prize was awarded through a lottery.[23] 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.[24] In the Ashraf, et al. (2011) study of a similar population of Salvadoran migrants, median monthly income was $2,080. Consequently, $600 represents a significant, but not unrealistic, increase in monthly income. The question text can be found in Appendix A. 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. In all cases the family member referred to in the question was the person to be surveyed in El Salvador: the student if the student was 18 or over, or the student's guardian if the student was under 18. A description of the treatments is presented in Figure 2.

[21] Although the migrants were not required to select a family member as the student, in practice 97% did.
[22] In pilot surveys where migrants were not limited to $100 intervals, almost all chose to split the money in $100 intervals.
[23] Two prizes were awarded. If asked, surveyors told migrants the number of prizes and the date of the drawing.
[24] Laury (2005) conducts a laboratory experiment in which respondents are shown to make the same choices when payoffs are random as when payoffs are guaranteed.

By offering migrants the chance to win $600 in extra income, this experiment essentially places migrants in the high income state discussed in the model and randomly varies the probability that that extra income will be observed.[25] This allows for an explicit test of whether, as predicted by the model, migrants are more likely to deviate from their agreements with family members and send less money home when the probability that deviation will be detected is low. Because changes in the probability of detection essentially vary the ability of the recipient to monitor the migrant's actions, I will refer to this treatment as the migrant monitoring treatment. Viewing the experiment in the context of the model leads to the following hypothesis:

Hypothesis 1:

Migrants in the treatment group where the migrant choice is revealed to recipients (i.e. where the probability of detection is one) should send more than migrants whose choice is not revealed. However, this effect should vary by migrant-recipient pair. In pairs where recipients cannot threaten strong punishments, migrants are not affected by the probability of detection and therefore there will be no impact of the monitoring treatment when they make the decision about how much money to keep and how much to send to the recipient. Their entire remittance will be motivated by altruism. Migrant responses from the baseline survey can be used to proxy for the recipient ability to punish migrants.

B. 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 in any way they wished among four spending categories: restaurant meals, education, daily expenses, and health expenses. The median monthly remittance in the Ashraf, et al. (2011) study was $325. If among one of the winners, the recipient would receive the allocation that they requested.[26] They were limited to four categories for simplicity of implementation in the context of a phone survey. The question text can be found in Appendix B. Two separate treatments were administered to

[25] The design of the experiment assigns $p^l = 0$ and $p^h = 1$. Use of the extreme values does not alter the predictions of the model, although information asymmetries are more likely to be important the greater the difference between p^l and p^h. Additionally, the baseline recipient ability to monitor the migrant may result in the recipient observing the migrant's windfall if the prize if won even if they are in the "not revealed" treatment group. This means that, in practice, p^l may actually be greater than zero.

[26] Four prizes were awarded. If asked, surveyors told recipients the number of prizes and the date of the drawing.

recipients, the recipient monitoring experiment and the recipient communication experiment.

Recipient monitoring treatment:

In a parallel treatment 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 is an explicit test of the model's prediction that recipients are more likely to strategically deviate when the probability of detection is low.

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, and this treatment is therefore a test of whether or not communication costs can lead to inadvertent deviation from migrant preferences by the recipient.

The two recipient treatments were cross randomized, also allowing for the analysis of their interaction. They are depicted in Figure 3. Viewing the recipient experiment in the context of the model results in the following hypotheses for recipient behavior:

Hypothesis 2:

Recipients in the treatment group where their choices are revealed to the migrant (and therefore where the probability of detection is one) should make choices that are closer to the migrants' preferences. This effect should not be evident in pairs where the migrant cannot threaten a strong punishment. In these cases the extent to which the recipient complies with the migrant preferences will depend wholly on altruism.

Hypothesis 3:

Revealing migrant preferences to the recipient should decrease the difference between the recipients' choices and the migrants' preferences when communication problems exist. This effect will not necessarily depend on the potential punishment because communication issues may affect compliance with migrant preferences compelled by the migrant as well as altruistic compliance by the recipient.

C. Experiment logistics

For all respondents in the "choice revealed" treatment groups of the monitoring experiments, an effort was made to inform the recipient or migrant of the family member's choice. After both the migrant and recipient survey had been completed text messages were sent to the appropriate participants informing them of the choice of their family members. Participants without cellular phones received a phone call from a project staff member with the information. More important however, is what the respondents believed would happen, and whether 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.

The randomization in this study was performed at the individual level. Surveys were pre-assigned treatment status (both migrant and recipient) before being sent into the field. Because remittance behavior can vary by season it was important to ensure that treatments were balanced over time.[27] I achieved this by stratifying the randomization for all treatments within groups of 16 surveys. The recipient treatments were also stratified by the migrant treatment. Because the experiment was conducted in conjunction with the baseline survey it was not possible to stratify on individual baseline characteristics.

IV. Data and estimation strategy

A. Data

The migrant baseline survey collected extensive information on migrant and recipient demographics and family relationships both in the United States and in El Salvador. It contained detailed information on remittances sent by the migrant both to the recipient household and other households and a set of questions to assess the quality of the migrant's relationship with the recipient household and the migrant's involvement in household affairs. The recipient survey, administered by phone, was shorter and contained demographic information and some limited questions on remittances received from the migrant.[28] Table 1 shows summary statistics from both the migrant and the recipient surveys. For the migrant survey statistics are shown for the

[27] In particular, the time period of the study included December, the most popular month for remittance sending (due to Christmas).
[28] It also contained an extensive module on the education of children in the household.

full sample and the sample with completed recipient surveys. No meaningful differences are apparent between the two samples; therefore I limit the analysis sample to the 1,298 migrant-recipient pairs with completed El Salvador surveys.[29] This allows me to examine the behavior of migrants and recipients in the exact same sample. Results from the migrant experiment do not change significantly when limiting the sample in this way. Additionally, I show that attrition from the full sample of migrant surveys to the sample of completed recipient surveys is not related to treatment (Tables 2 and 3 below).

In the final analysis sample, the treatment breakdown is as follows. For the migrant monitoring treatment there are 648 migrants in the "migrant choice not revealed to recipient" group and 650 in the "migrant choice revealed to recipient" group. In the recipient monitoring experiment there are 638 people in the "recipient choice not revealed to migrant" group and 660 in the "recipient choice revealed to migrant" group. For the recipient communication experiment there are 641 people in the "migrant preference not revealed to recipient" group and 657 in the "migrant preference revealed to recipient" group. These breakdowns into treatment groups can be seen in Figure 2 (migrant experiment) and Figure 3 (recipient experiment).

Despite the fact that the participation in the study is conditioned on having a high school or college aged relative in El Salvador, Table 1 shows that the sample is fairly diverse. The migrants are half male and half female with an average age of 38. Importantly, 85 percent have 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.[30] The mean number of years in the United States is 11, so the sample is composed largely of migrants who are established in the United States. 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.[31] 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

[29] Additionally 10 observations are lost because respondents did not answer the questions that made up the experiment.

[30] Remittance data on the recipient survey was collected by asking the migrant for the average value of remittances sent and the frequency of those remittances. The migrant was additionally asked to report the annual amounts of remittances sent for special occasions or emergencies.

[31] Respondents were asked to classify the combined income of them and their co-resident spouses into one of four categories: $400 weekly or less, $401 - $600 weekly, $601 to $800 weekly, $801 or more weekly.

percent of interviews were done with a different adult in the household if the indicated student or guardian could not be reached. The recipient sample is heavily female (68 percent) because identified student guardians tend to be female.

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 Appendix Table 1, I compare characteristics of the migrants from the baseline survey (gender, age, time in the US, 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.

Table 1 also provides motivation for conducting the experiment. First, I examine whether migrants and recipients have different preferences, because monitoring and communication costs should be much less important when migrants and recipients agree about how remittances should be spent. During the baseline surveys, both were asked to list the three most important budget priorities for the recipient household from a set list of seven categories: food and other basic expenses, health, education, savings, entertainment, home improvement and transport. Despite significant bunching of responses in the first three categories (food, health and education), only 48 percent of pairs report the same three priorities, suggesting that migrant and recipient preferences for the spending of remittances do differ to some extent. I also check whether communication costs are likely to be important by testing the migrant's knowledge of the recipient household. Only 24 percent of migrants could correctly report the student's GPA as reported by the recipient and 43 percent the mode of transport a student uses to get to school.[32] Although this is not the same thing as recipients not understanding how migrants want them to spend remittances, it is evidence that knowledge does not necessarily flow freely between countries.

[32] The questions about student GPA and transport to school were only asked when the student is 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 was said to have correctly reported the GPA if the recipient's response was within the range the migrant indicated.

The random assignment of the treatments in this experiment allows for the causal identification of their impacts. Randomization should provide treatment groups that are the same on average so that any difference between the groups can be attributed to the treatment and not some pre-existing difference between groups. Tables 2 and 3 test whether the treatment groups are balanced on observed characteristics from the baseline survey for the treatment groups for the migrant experiment and the recipient experiments respectively. In Table 2 the means for both treatment groups in the migrant monitoring treatment are presented in the first two columns and the p-value from the test of whether or not those means are equal is in the third column. Overall the treatment groups are well balanced: only two of 34 differences are significantly different from zero at the 10 percent level. Table 3 shows the means by treatment group for the two recipient experiments and p-values for differences in those means. Again the groups are well balanced, only three of the 34 p-values for the recipient monitoring treatment and one of the 34 p-values 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.

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

B. Estimation strategy

Migrant experiment

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

$$Remit_i = \delta + \alpha ChoiceRevealed_i + X_i'\gamma + \varepsilon_i \quad (1)$$

where $Remit_i$ is the dependent variable indicating how much of the $600 the migrant chose to send to the recipient, or, alternatively, an indicator for whether or not the migrant chose to send all $600. $ChoiceRevealed_i$ is the treatment indicator for the monitoring treatment, and it is equal to one when the migrant's choice is revealed to the recipient. The coefficient α is the average difference between how much migrants choose to send when their decisions are not revealed and when they are. If α is positive, migrants send more money to the recipients when $ChoiceRevealed_i$ equals one. X_i is a vector of control variables that includes migrant age,

gender, education, household size, years in the United States, remittances to recipient household, and other migrant background characteristics. It also includes fixed effects for randomization stratification group. Because treatment is randomly determined, the inclusion of control variables is not necessary for casual inference, but I will show specifications with and without the controls for completeness. ε_i is the robust error term.

Recipient experiment

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

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

$$Difference_i = \delta + \beta_1 ChoiceRevealed_i + \beta_2 PreferenceRevealed_i + X_i'\theta + \mu_i \quad (2)$$

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 one when the recipient's choice is revealed to the migrant. $PreferenceRevealed_i$is the treatment indicator for the communication treatment and is equal to one when the migrant's preferences are revealed to the recipient before the recipient decides how to allocate the remittance funds. The coefficient β_1 is the difference in the difference between migrant preferences and recipient choices when the recipient choice is not revealed as compared to when it is revealed. Similarly, β_2 is the difference in the difference between migrant preferences and recipient choices when the migrant's preferences are revealed

to the recipient as compared to 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 makes choices more similar to the migrants' preferences, then the difference variable will be smaller in the "choice revealed" and "preference revealed" treatment groups and β_1 and β_2 should be negative. X_i is as in the migrant experiment with additional controls for recipient gender, age, education, household size and the number of days between the migrant and recipient survey. Fixed effects for randomization stratification variables (survey group and migrant treatment) are also included. $\mu_i$is the robust error term.

I also examine the interactions of the two treatments. It is possible, for example, that any impacts of the monitoring treatment could be amplified by revealing migrant preferences. Consequently, an alternative specification that examines the separate impact of each of the four unique combinations of the two recipient experiments will also be estimated.

V. Results

A. Migrant experiment

I first analyze the results of the migrant experiment in which migrants make an incentivized decision over how much of a $600 windfall to send to the recipient and how much to keep. Figure 4 shows the distribution of the amount sent by migrants, separately by treatment group. Choices for migrants for whom choices were not revealed are in panel A and choices for migrants for whom the choice was revealed to the recipient are in panel B. Because the experimental protocol limited migrants to splitting the money in 100 dollar increments, the distributions are discrete. The first observation to be made from these figures is that the migrants send large amounts: over half of the migrants in both treatment groups choose to send the entire $600 to the recipient. The other smaller spike in both distributions is at $300 where migrants decide to split the money equally between themselves and the recipient. Despite the fact that the two distributions follow the same basic shape, differences are evident. Specifically, the spike at sending everything is smaller in Panel A where choices are not revealed (53 percent versus 58 percent) and the percent of migrants choosing to send $400 and less is higher (44 percent versus 38 percent).

The fact that almost all migrants in the "choice not revealed" treatment group choose to send something is consistent with the model presented in Section II, where migrants who deviate when the probability of detection is low still send positive amounts in remittances. Additionally,

given that the experimentally induced "low" probability that recipients will observe migrant income is zero, the fact that most migrants in this group choose to send the entire $600 is suggestive that the altruistic component of remittances is high.[34] However, the differences between the two distributions are evidence that information asymmetries also seem to play a role. Migrants whose choices are not revealed are choosing to send less home.

The difference between the two treatment groups is made more explicit in Table 4 which gives the mean of the amount sent by the migrant by treatment group. Because of the large number of migrants who send everything, I also consider an indicator variable that is equal to one if the migrant chose to send all of the money to the recipient. Migrants whose choice is not kept secret send $24 more to the recipient on average and are 5 percentage points more likely to send everything to the recipient. The table also shows the p-values from a test for difference in means. Both differences are statistically significant: the means of amount sent at the 2 percent level and the means of the indicator for sent all at the 7 percent level.

These results are replicated in Table 5 using a regression framework that estimates regression equation 1 from Section IV of this paper. Columns 1, 2 and 3 show results for amount sent by the migrant and columns 4, 5 and 6 for whether or not the migrant sent everything. Columns 1 and 4 are a simple regression of the dependent variable on treatment status, columns 2 and 5 include stratification cell fixed effects, and columns 3 and 6 further add the demographic control variables.[35] The results are robust to the exclusion of control variables, although the impact of treatment on the migrant sending everything is no longer significant when controls are included because the magnitude of the coefficient drops slightly. Migrants send $20 to $24 more when their choice will be revealed, which represents a 5 percent increase over the "not revealed" group mean. Additionally, migrants are 4 to 5 percentage points less likely to send everything when their decision will be revealed to the recipient, but the coefficient is no longer significant when control variables are added.

Table 5 also reports the coefficients on the demographic control variables included in

[34] But the amount sent by the "not revealed" treatment group is not necessarily completely due to altruism. Recall that, according to the framework, migrants must send an amount that makes their deviation credible. Additionally, migrants may be reacting to a certain level of baseline recipient monitoring that exists outside of the experimental construct.

[35] Control variables are migrant gender, age, years of education, household size, years in the US, whether the migrant lives with his/her spouse, whether the migrant has a child 22 or under in El Salvador, whether the recipient is the migrant's close relative, whether the migrant is in the lowest income bracket, migrant's annual remittances to the recipient household and whether the migrant communicates with the recipient household weekly.

columns 3 and 6. 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, available from the author on request). Migrants who have been in the United States for longer send more, although the effect is small. Migrants who live with their spouses send $29 less than those who do not. This is likely because they have greater financial obligations in the United States and are more likely to have their immediate family with them in the United States. 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 the relationship suggests that migrant behavior in the experiment is related to real world migrant behavior.

The results in Table 5 show that information asymmetries can affect migrants making 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 to $24 increase in amount sent when the migrant's choice is revealed) is not large, but it is similar to the size of the documented correlations with the demographic variables in column 3 of Table 5. The size of the effect is also in line with experimental studies in families (Hoel, 2012) 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. Additionally, Ligon and Schechter (2012) 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.

Information from the baseline survey allows for further investigation of the mechanics of this result. Specifically, the model presented in Section II predicts that information asymmetries are only important in pairs where the recipient is inducing the migrant to send remittances above what he would have sent altruistically through the threat of punishment. If recipients cannot threaten punishment then no differences between treatment groups should be observed.

Several variables from the baseline survey can plausibly be thought to proxy for punishment costs described in Section II. I look at how the treatment effect varies for their different values. 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 and the rationale for choosing them are described below.

- *Migrant years in the United States:* 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:* Migrants who have left a non-adult child in El Salvador may have left that child in the care of the recipient. The possibility of child care that does not meet the migrant's preferences could be a powerful tool to compel migrants to send more money home.[36] 34 percent of migrants have a son or daughter aged 22 or under in El Salvador.
- *Migrant and the recipient are closely related:* This is defined as spouses or parent and child. Being closely related can mean both that migrants have entrusted recipients with the care of things that are important to them and that positive relationships with the recipients are valuable to the migrants. 31 percent of migrants and recipients are closely related.
- *Migrant communicates with recipient household at least once a week:* Frequent communication is a sign that migrants value their relationships with recipients. 71 percent of migrants report communicating at least weekly with the recipient household.
- *Remittances sent by migrant to recipient household:* Because remittance relationships where recipients induce migrants to send money result in higher remittance 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 it could be argued that 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 model, altruistic

[36] The 22 and under cutoff is used because it was available on the survey which measured the number of young relatives up to college age the migrant had in El Salvador. The structure of the question does not allow me to identify whether the child is in the recipients home or not.

remittances should not be affected by variations in monitoring. However, one way in which the results could be affected is if for very altruistic migrants, $600 is not a large enough sum for information asymmetries to matter. This would however induce bias *against* finding an effect of revealing the migrant's choice for migrants with high levels of altruism. Additionally, the mean amount sent in the treatment group where decisions are not revealed is in every case lower in the high punishment costs sub-groups than in the low punishment cost sub-groups. Because payments in this "not revealed" treatment group should be largely motivated by altruism, this is evidence that the variables chosen are representing more than just higher levels of altruism.

Table 6 presents regression results by subsamples of these variables. For the continuous variables (years in the US 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. Panel 1 presents regressions without control variables and Panel 2 presents regressions with demographic control variables. 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 1, 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 56 more dollars sent when the choice is revealed. These numbers are about 7 to 13 percent of the average amount sent in the "choice not revealed" group. In the subsamples where punishment costs should be low (columns 2, 3, 5, 7, and 9) the coefficients are all 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 Panel 1 and three of the five in Panel 2 are statistically significantly different from each other. An alternate specification that utilizes the first principal component of the five proxy variables as a summary measure yields similar results (results not shown, available from the author upon request). 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, instead of being motivated by a remittance contract with the recipient, simply care about being perceived as altruistic and utilize 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; however, several factors suggest that it is unlikely.
For example, the strong patterns of heterogeneity by sub-group are directly connected to the theoretical framework presented here, but it is not obvious how they would relate to a story about signaling. To be consistent with the sub-group results, the variables that describe recipient punishment ability would also have to represent groups to which migrants cared about appearing altruistic. However, across all sub-groups, the migrants' allocations in the "not revealed" treatment group are high, suggesting that migrants are altruistic to all recipients. If the signaling story were true it would then have to be the case that true altruism and the desire to signal altruistic behavior were not at all correlated. Additionally, as noted in Table 6, the effect of revealing the migrant's choice is concentrated in pairs where actual remittances to the recipient are above the sample median. Given that these are migrants who would have repeatedly signaled their generosity already, it is not clear why migrants who send fewer remittances overall would not take advantage of this low cost opportunity to do so. [37]

B. Recipient 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 7. The first two columns show the mean amounts allocated by recipients broken down by the recipient monitoring treatment and columns three and four show mean recipient allocations by the recipient communication treatment. The fifth column shows the means of the preferences reported by the migrant.[39] Across both recipients and migrants education is the most popular choice.[40] Daily expenses are the next most popular category, followed closely by health and finally restaurant meals. As discussed previously, unless clear differences between migrant and recipient preferences are evident in the population on average, an analysis of the impact of treatment on amounts allocated to different categories will not be interesting. Although migrants allocate less to education than recipients and more to daily expenses, health expenses and

[37] Although the context is different, in an experiment studying the social networks of Harvard students, Leider, et al. (2009) are able to definitely rule out the signaling explanation for non-anonymous giving in favor of one based on reciprocity and future interactions.
[39] Recall that migrant preferences for the recipient's decision were solicited from all migrants.
[40] 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 than the other.

restaurant meals, regressions of treatment on the raw amounts recipients choose to allocate to the different categories do not reveal any interesting patterns (shown in Appendix Table 2).

A more powerful analysis utilizes the data collected from both the migrant and the recipient to analyze how the treatments affect the pair-level difference between their choices. Table 8 displays the mean differences by recipient treatment. The results are displayed separately for the monitoring treatment and the communication treatment; the means from the monitoring treatment and p-values testing the equality of those means are in the first three columns and the corresponding information for the communication treatment is in the last three columns. Means of the differences for the four spending categories as well as the mean total difference are shown. For both treatments the prediction is that the difference will be smaller in the "revealed" treatment group. When the probability spending choices will be observed is high 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, the means across the two treatment groups are essentially equal. However, differences are evident for the communication treatment. In all categories the difference between recipient choices and migrant preferences are smaller when the migrants' preferences are revealed than when they are not. Although of the spending categories only the difference for education is significant, importantly so is the total difference, implying that migrants and recipients are getting closer together overall. The $14 reduction in the total difference is driven by the difference in education spending with the corresponding reductions in differences in other categories being split between daily and health expenses and, to a lesser extent, spending on restaurant meals.[41]

Table 9 shows these results in regression format and adds control variables. Panel 1 shows the results from estimating the regression equation that estimates the effect of each treatment separately and panel 2 presents the results of estimating an alternate specification that considers the separate effects of the four distinct treatment combinations. The "recipient choice **not revealed** to migrant, migrant preference **not revealed** to recipient" group is the omitted category. The dependent variables in columns 1 through 4 are the migrant-recipient differences

[41] Mechanically the sum of the differences between the "not revealed" and "revealed" groups over the four categories must be equal to twice the difference in the total difference.

in restaurant spending, education spending, spending on daily expenses, and health spending respectively. The dependent variable in columns 5, 6 and 7 is the total migrant-recipient difference. Column 6 adds stratification cell fixed effects and column 7 additionally adds demographic control variables. The control variables are the same as those presented in Table 5 with the addition of recipient gender, age, years of education and household size and a control for number of days between the migrant and the recipient survey.

The results in panel 1 replicate the results from Table 8 almost exactly. Controlling for the other treatment does not change either estimate. In addition, the results are robust to the addition of all control variables (results for individual spending categories not shown but available upon request). The results in panel 2 show that the same conclusion is drawn when considering the separate impacts of the four groups. Focusing on the total difference results in columns 5 and 6, the coefficients on the "recipient choice **not revealed** to migrant, migrant preference **revealed** to recipient" and "recipient choice **revealed** to migrant, migrant preference **revealed** to recipient" groups are both negative and significant, meaning that the total difference in these groups is smaller than in the omitted category. These are the two groups where migrant preferences are revealed to the recipient and the estimated coefficients are quite similar in magnitude to the coefficient on the communication treatment in panel 1. The coefficient on the remaining group ("recipient choice **revealed** to migrant, migrant preference **not revealed** to recipient") is small and statistically indistinguishable from zero. Essentially the specification that considers the separate effects of the interacted treatment groups shows the same pattern as the specification that considers the treatments separately. Revealing the migrant's preferences lessens the total difference between recipient choices and migrant preferences by $14 or approximately ten percent of the "recipient choice **not revealed** to migrant, migrant preference **not revealed** to recipient" mean. The monitoring treatment has no effect.

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 model presented in Section II 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 model, 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, given that, as reported in Table 1, only 24 percent of migrants could correctly report student GPA and 43 percent correctly report how students travel to school, it does not seem plausible that existing monitoring is good enough across the board as 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. Certainly this may be true for some families, but if it were true for most, there should not be an impact of the recipient communication treatment. Additionally, only 48 percent of migrant-recipient pairs report the same three budget priorities (Table 1), further evidence that there is heterogeneity in preferences within families.

For completeness, it is instructive to examine how the impacts of the recipient treatments may vary by sub-group. For symmetry I present the results of the recipient experiments broken down by the same sub-groups used for analysis in the migrant experiment. Although the ways in which the migrant may punish the recipient are less obvious, the variables that indicate valuable family relationships should be important, as well as average remittances, given that the threat of withholding remittances may be one of the migrant's most valuable tools.

Table 10 shows the results of the recipient experiments by subsample. This table focuses only on the total migrant-recipient difference. Panel 1 presents regressions without control variables and Panel 2 presents regressions with control variables. In contrast to the migrant experiment the subsample analysis reveals no consistent patterns. Besides two positive coefficients which may be due to chance, there continues to be no significant impact of the monitoring treatment, a result that is not surprising given how close to zero the coefficients in the full sample analysis were. The results of the communication treatment are also fairly consistent across subsamples. Given that there are no impacts of the monitoring treatment, this suggests that

any changes due to increased information about migrant preferences will happen because of the altruism of the recipients, and altruism does not necessarily vary with the migrant's ability to punish. The stronger impacts of the communication treatment in cases where the migrant has a child 22 or under in El Salvador and where the migrant communicates with the recipient household at least weekly could simply suggest that recipients in those groups have a greater desire to follow the migrant's preferences than other recipients.

C. Discussion

Economic studies of information asymmetries in households with migrants have until now focused on migrant monitoring of recipient behavior (Chen, 2012; 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 – the migrant sending of remittances as well as the recipient 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, it is only migrants, and not recipients, who 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[42] should take the role of the recipient in determining remittance amounts into account.

Additionally, the fact that the monitoring treatment had no effect on the recipient 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 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

[42] See Yang (2011) for a discussion.

of recipient remittance spending does not seem to matter, suggest that if migrants do indeed desire control (and the results are mixed on whether or not they do) it 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. The results of the communication experiment imply that for migrants who wish to change the spending behavior of their family members, improved communication about spending preferences may be an inexpensive way to achieve a higher level of compliance with their preferences. Also, increasing the observability of migrant income could lead to increased remittances to the recipient household. The welfare implications of such an increase are, however, unclear. Although the many studies that cite positive impacts of remittances on recipients indicate that increased remittances would benefit recipients,[43] the low income status of the migrants in this study suggests the possibility that the extra funds could be better used by the migrants in the United States.

VI. 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 recipient ability to monitor migrant income can lead to migrants strategically deviating from this agreement when the probability that their deviation will be detected is low.

[43] See Cox-Edwards and Ureta (2003), Adams and Page (2005), Yang and Martinez (2005), Woodruff and Zentano (2007), Yang (2008) and Adams and Cuecuecha (2010) among others for examples of the positive impacts of remittances.

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 only present 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 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 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 desire for migrant control over remittances previously emphasized 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.

The fact that the experiment shows that information asymmetries can affect the amount of remittances sent home also suggests a careful empirical analysis of how information asymmetries may further affect the migrant-recipient relationship. A particularly interesting question is whether migrants' ability to hide income may be related to their effort in generating income. In other words, do migrants work harder when they know they may be able to keep more income for themselves? Similarly, although recipients do not exhibit strategic behavior when spending remittances, the question of whether or not they exert less effort working

knowing that their remittance payment will depend on their income is still open.[44]

The results also provide support for the view of intra-household resource allocation as an informal contract that can be effective only when the ability to enforce that contract exists, represented in this model by the ability of family members to punish each for deviating from agreements. This is an important result for policy makers because it suggests that while information asymmetries are important, they may not be relevant for all families where resources are shared. This may be particularly true in extended family networks where altruism can lead to resource sharing, but the ability to punish for deviation from agreements may vary widely.

More generally, this study examines the effects of information asymmetries on various types of decisions in the resource allocation process (both sharing and spending). Importantly, it also considers different classes of information asymmetries: those with strategic effects and those with inadvertent effects. The results show that in this context, for migrant households, strategic effects are only a factor in the sharing stage, while inadvertent effects are important in the spending stage. However, it should not be assumed that the same will be true for household resource allocation more generally. In cohabiting families there may be more variation in bargaining power between the people making the sharing and spending decisions, as well as greater opportunities for both monitoring and enforcement.

[44] Some research exists on this second point but the results are mixed (see Joulfain and Wihelm, 1994, Cox, Eser and Jimenez, 1998, and Azam and Gubert, 2004).

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Figure 1: Project timeline

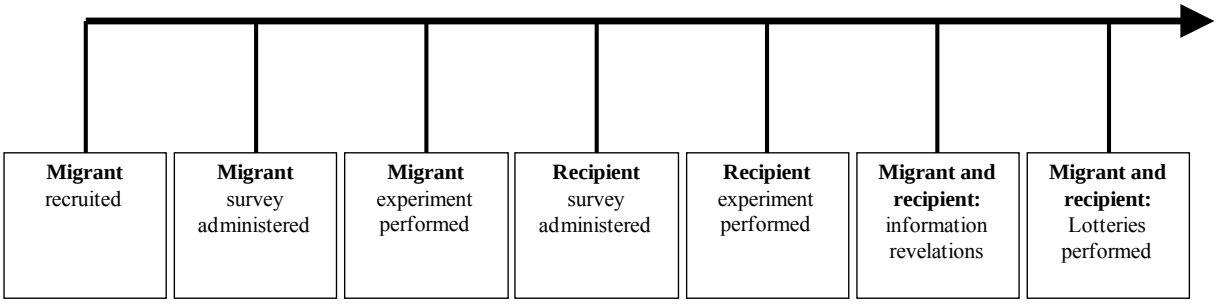


Figure 2: Migrant experiment: treatments

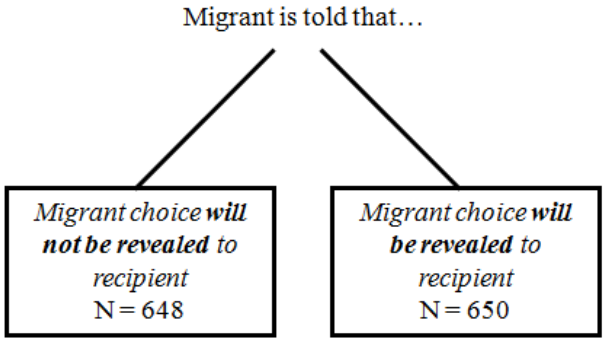
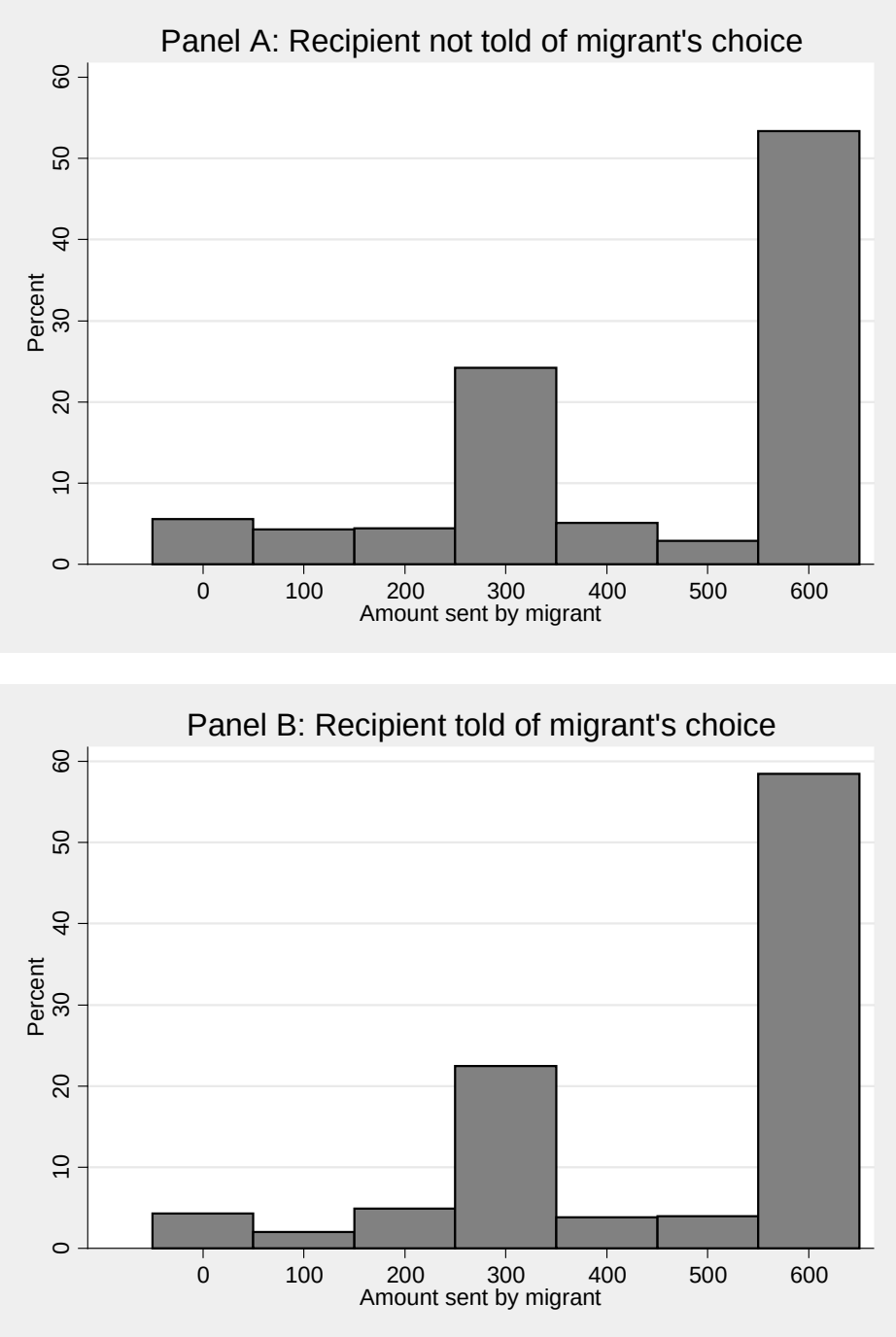


Figure 3: Recipient experiment: treatments

Monitoring treatment	*Communication treatment*: Migrant preference **not revealed** to recipient	Migrant preference **revealed** to recipient	
Recipient choice **not revealed** to migrant	N = 314	N = 324	N = 638
Recipient choice **revealed** to migrant	N = 327	N = 333	N = 660
	N = 641	N = 657	

Figure 4: Distribution of amount sent by migrant by treatment group



Notes: Sample is observations with non-missing values for experiment questions and completed recipient survey. Panel A: N = 648. Panel B: N = 650.

Table 1: Baseline summary statistics

	All Observations			*Observations with completed recipient survey*		
	Mean	SD	N	Mean	SD	N
Baseline variables from migrant survey						
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 US	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 children in El Salvador	1.01	1.43	1,577	1.07	1.47	1,294
Migrant's children in US	1.26	1.32	1,575	1.25	1.29	1,293
Migrant's hh size in US	4.32	1.98	1,581	4.36	1.96	1,298
Migrant 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	0.2891	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
Baseline variables from recipient survey						
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
Baseline comparison variables						
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

Notes: All observations sample is respondents with non-missing data for questions in the migrant experiment. Completed recipient survey sample additionally conditions on completion of the recipient survey and non-missing migrant and recipient information for questions in the recipient experiment. 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 co-resident 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 one if the migrant and recipient responses match. Both respondents were asked to choose the three most important budget priorities for the recipient hh from a list of seven categories. Student refers to the student identified by the migrant during the baseline survey. GPA and mode of transport were only asked when student was reported to be in school.

Table 2: Balance tests: Migrant experiment

	Treatment group means:		P-value for difference of means: Choice not revealed and choice revealed
	Migrant choice **not revealed** to recipient	Migrant choice **revealed** to recipient	
Attrition			
Recipient survey completed	0.82	0.83	0.819
Baseline variables from US Survey			
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 children in El Salvador	1.03	1.10	0.365
Migrant's children in US	1.28	1.22	0.410
Migrant's hh size in US	4.34	4.38	0.720
Migrant 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
Baseline variables from recipient survey			
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
Baseline comparison variables			
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

Notes: Sample is observations with non-missing 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 1. P-values come from a regression of each variable on treatment, with robust standard errors.

Table 3: Balance tests: Recipient experiment

	Monitoring treatment			Communication treatment		
	Treatment group means:			*Treatment group means:*		
	Recipient choice **not revealed** to migrant	Recipient choice **revealed** to migrant	P-value for difference of means: Choice not revealed and choice revealed	Migrant preference **not revealed** to recipient	Migrant preference **revealed** to recipient	P-value for difference of means: Pref. not revealed and pref. revealed
Attrition						
Recipient survey completed	0.81	0.83	0.315	0.82	0.83	0.730
Baseline variables from US Survey						
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 children in El Salvador	1.01	1.12	0.206	1.04	1.09	0.557
Migrant's 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
Baseline variables from recipient survey						
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
Baseline comparison variables						
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

Notes: Sample is observations with non-missing 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 1. P-values come from a regression of each variable on treatment, with robust standard errors.

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

	Treatment group means:		P-value for difference of means: Choice not revealed and choice revealed
	Migrant choice **not revealed** to recipient	Migrant choice **revealed** to recipient	
Amount sent by migrant ($)	441.36	465.38	0.020
Migrant sent everything	0.53	0.58	0.066
Observations	648	650	

Notes: Sample is observations with non-missing 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. Migrant sent everything is an indicator for whether or not the migrants chose to send everything to the recipient. P-values for difference in means were calculated by regressing the dependent variables on treatment, with robust standard errors.

Table 5: Impact of monitoring treatment on migrant remittance decision

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: *Amount sent by migrant*			Dependent variable: *Migrant sent everything*		
Migrant choice **revealed** to recipient	24.03**	20.40**	19.50*	0.0507*	0.0424	0.0352
	[10.35]	[10.27]	[10.26]	[0.0275]	[0.0273]	[0.0272]
Migrant is female			-26.46**			-0.0793***
			[11.09]			[0.0297]
Migrant age			-0.487			0.000173
			[0.741]			[0.00184]
Migrant's years of education			-0.119			0.00251
			[1.225]			[0.00322]
Migrant's years in the US			1.968*			0.00625**
			[1.071]			[0.00277]
Migrant lives with spouse			-28.75**			-0.0586*
			[11.83]			[0.0312]
Migrant's hh size in US			1.293			0.00404
			[2.800]			[0.00766]
Migrant has child 22 or under in ES			0.984			0.0379
			[12.41]			[0.0331]
Recipient is migrant's close relative			-0.675			-0.00535
			[12.74]			[0.0340]
Migrant in lowest income bracket			-21.73*			-0.0400
			[12.65]			[0.0338]
Migrant's annual total remittances to recipient hh			0.00319*			7.05e-06
			[0.00192]			[4.79e-06]
Migrant communicates with recipient hh at least weekly			-1.122			-0.0249
			[12.68]			[0.0339]
Observations	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.004	0.133	0.159	0.003	0.123	0.149
Mean in treatment = Migrant choice **not revealed** to recipient	441.4			0.53		
Stratification group fixed effects	NO	YES	YES	NO	YES	YES

Notes: Robust standard errors in brackets. Sample is observations with non-missing 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. Migrant sent everything is an indicator for whether or not the migrants chose to send everything to the recipient. 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. Stratification group fixed effects are dummy variables for the groups of survey numbers within which
*** p<0.01, ** p<0.05, * p<0.1

Table 6: 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									
	Years in the United States		*Migrant has child 22 or under in El Salvador*		*Recipient is close relative of migrant*		*Migrant communicates with recipient hh weekly*		*Migrant's annual remittances to recipient hh*	
	Below sample median	Above sample median	No	Yes	No	Yes	No	Yes	Below sample median	Above sample median
Panel 1: Regressions without control variables										
Migrant choice **revealed** to recipient	39.61***	8.824	7.370	56.06***	13.22	47.36**	5.209	31.94***	1.612	45.02***
	[14.49]	[14.79]	[12.58]	[18.30]	[12.45]	[18.75]	[19.77]	[12.12]	[15.27]	[14.08]
P-value for equality of treatment effect	0.137		0.028		0.111		0.201		0.037	
Observations	656	639	853	445	896	402	379	919	611	670
R-squared	0.011	0.001	0.000	0.021	0.001	0.016	0.000	0.007	0.000	0.015
Panel 2: Regressions with control variables										
Migrant choice **revealed** to recipient	36.39**	8.401	4.177	51.92***	7.834	47.60**	-1.822	31.67***	0.598	42.61***
	[14.41]	[14.99]	[12.47]	[18.40]	[12.45]	[19.04]	[20.55]	[12.15]	[15.74]	[14.21]
P-value for equality of treatment effect	0.179		0.031		0.079		0.158		0.048	
Observations	656	639	853	445	889	402	376	919	611	670
R-squared	0.062	0.021	0.045	0.070	0.038	0.061	0.063	0.038	0.027	0.046
Mean in treatment = Migrant choice **not revealed** to recipient	426.1	457.8	449.3	424.3	444.4	433.9	449.4	438.4	447.0	437.5

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for all experiment questions, completed recipient survey and non-missing values for variables used for division into sub-samples. Amount sent by migrant is the amount that migrants chose to send when splitting $600 between themselves and recipients. 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 US is 10 and the median remittances sent to the recipient household are $1,800. Control variables are migrant gender, age, years of education, years in the United States, and household size in the US. Controls also include 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, and whether the migrant and recipient communicate at least weekly. Control variable is omitted when it is used to create the sub-samples.

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

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

	Means of recipient choices by treatment group:				
	Monitoring treatment		*Communication treatment*		
	Recipient choice **not revealed** to migrant	Recipient choice **revealed** to migrant	Migrant preferences **not revealed** to recipient	Migrant preferences **revealed** to recipient	Means of migrant preferences:
Amount allocated to:					
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
Observations	638	660	641	657	1298

Notes: Sample is observations with non-missing 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.

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

	Monitoring treatment			*Communication treatment*		
	Recipient choice **not revealed** to migrant	Recipient choice **revealed** to migrant	P-value for difference of means: Choice not revealed and choice revealed	Migrant preferences **not revealed** to recipient	Migrant preferences **revealed** to recipient	P-value for difference of means: Pref, not revealed and pref. revealed
Difference in:						
Restaurant meals	15.89	14.80	0.604	16.66	14.05	0.215
Education	107.29	110.92	0.463	116.28	102.17	0.004
Daily expenses	78.02	81.38	0.421	83.01	76.52	0.120
Health expenses	75.02	73.47	0.709	76.55	71.97	0.271
Total difference	138.11	140.28	0.649	146.25	132.36	0.004
Observations	638	660		641	657	

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: Migrant-recipient difference in…				*Dependent variable:*		
	Restaurant spending	Education spending	Daily expenses spending	Health spending	Total migrant-recipient difference		
Panel 1:							
Recipient choice **revealed** to migrant	-1.097	3.582	3.336	-1.567	2.126	3.019	3.330
	[2.100]	[4.923]	[4.172]	[4.159]	[4.753]	[4.775]	[4.827]
Migrant preference **revealed** to recipient	-2.612	-14.09***	-6.476	-4.588	-13.88***	-13.61***	-13.69***
	[2.103]	[4.926]	[4.168]	[4.159]	[4.751]	[4.753]	[4.818]
Observations	1,298	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.001	0.007	0.002	0.001	0.007	0.105	0.122
Mean in recipient choice **not revealed**	15.9	107.3	78.0	75.0	138.1		
Mean in migrant preference **not revealed**	16.7	116.3	83.0	76.6	146.2		
Panel 2:							
Recipient choice **not revealed** to migrant, migrant preference **revealed to recipient**	-4.132	-17.56**	-0.621	-5.745	-14.03**	-14.51**	-13.86**
	[3.080]	[6.819]	[5.949]	[5.892]	[6.673]	[6.752]	[6.918]
Recipient choice **revealed** to migrant, migrant preference **not revealed to recipient**	-2.611	0.129	9.164	-2.719	1.981	2.119	3.158
	[3.253]	[6.942]	[5.738]	[5.887]	[6.489]	[6.619]	[6.714]
Recipient choice **revealed** to migrant, migrant preference **revealed to recipient**	-3.753	-10.61	-2.971	-6.189	-11.76*	-10.62	-10.36
	[3.128]	[6.940]	[5.969]	[5.735]	[6.752]	[6.844]	[6.922]
Observations	1,298	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.002	0.007	0.004	0.001	0.007	0.105	0.122
Mean in recipient choice **not revealed**, migrant preference **not revealed**	18.0	116.2	78.3	77.9	145.2		
Stratification group fixed effects	NO	NO	NO	NO	NO	YES	YES
Control variables	NO	NO	NO	NO	NO	NO	YES

Notes: Robust standard errors in brackets. Sample is observations with non-missing 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 two. Omitted category in panel 2 regressions is "Recipient choice not revealed, migrant preference not revealed." Stratification group fixed effects are 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 in between migrant and recipient survey.

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

Table 10: Impact of monitoring and communication treatments on recipient allocation decision: By proxies for punishment ability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent variable: Total migrant-recipient difference									
	Years in the United States		*Migrant has child 22 or under in El Salvador*		*Recipient is close relative of migrant*		*Migrant communicates with recipient hh weekly*		*Migrant's annual remittances to recipient hh*	
	Below sample median	Above sample median	No	Yes	No	Yes	No	Yes	Below sample median	Above sample median
Panel 1: Regressions without control variables										
Recipient choice **revealed** to migrant	-7.420	11.63*	3.895	-1.768	-1.534	9.902	21.53**	-6.626	1.450	1.675
	[6.647]	[6.805]	[5.890]	[8.059]	[5.813]	[8.352]	[8.680]	[5.674]	[6.957]	[6.619]
Migrant preference **revealed** to recipient	-16.83**	-11.71*	-9.160	-23.17***	-11.63**	-16.89**	-7.156	-17.39***	-12.92*	-15.56**
	[6.641]	[6.818]	[5.881]	[8.080]	[5.802]	[8.462]	[8.705]	[5.665]	[6.973]	[6.607]
P-values for equality of treatment effects:										
Monitoring treatment	0.045		0.570		0.261		0.007		0.981	
Communication treatment	0.591		0.161		0.608		0.324		0.784	
Observations	656	639	853	445	889	402	376	919	611	670
R-squared	0.011	0.009	0.003	0.018	0.005	0.014	0.017	0.011	0.006	0.008
Panel 2: Regressions with control variables										
Recipient choice **revealed** to migrant	-7.840	11.85*	4.066	-5.225	-0.504	6.333	24.22**	-6.749	4.289	1.765
	[6.847]	[6.960]	[5.918]	[8.329]	[5.840]	[8.622]	[9.432]	[5.786]	[7.069]	[6.795]
Migrant preference **revealed** to recipient	-15.45**	-12.02*	-8.642	-25.56***	-11.78**	-16.14*	-5.121	-17.25***	-12.81*	-14.04**
	[6.783]	[7.029]	[5.936]	[8.340]	[5.826]	[8.824]	[8.806]	[5.816]	[6.964]	[6.857]
P-values for equality of treatment effects:										
Monitoring treatment	0.044		0.361		0.509		0.005		0.797	
Communication treatment	0.725		0.097		0.678		0.246		0.900	
Observations	656	639	853	445	889	402	376	919	611	670
R-squared	0.044	0.036	0.043	0.061	0.040	0.050	0.103	0.030	0.060	0.028
Mean in recipient choice **not revealed**	141.3	134.5	137.6	139.1	139.0	136.5	134.8	139.4	138.6	138.1
Mean in migrant preference **not revealed**	146.5	145.8	144.1	150.3	144.3	149.9	149.0	145.2	145.8	147.0

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for experiment questions, completed recipient survey and non-missing values for variables used for division into sub-samples. 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 total difference is the sum across the four difference variables for each observation, divided by two. The median years in the US is 10 and the median remittances sent to the recipient household is $1,800. 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 in between migrant and recipient survey. Control variable is omitted when it is used to create the sub-samples.

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

Appendix Table 1: Comparison of migrants in study with DC-area Salvadorans and Hispanics in the American Community Survey

		American Community Survey: 2008-2010 3-year sample	
	Baseline survey	Salvadoran-born, not US citizen	Hispanic, not US citizen
Migrant is female	0.51	0.46	0.46
Age of migrant	36.92	36.05	36.39
	[9.30]	[10.39]	[10.85]
Migrant's years in the US	11.13	12.93	11.74
	8.09	[7.89]	[8.09]
Migrant's hh size in the US	4.36	4.95	4.64
	[1.96]	[2.12]	[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

Notes: Baseline survey sample is observations with non-missing values for all experiment questions and completed recipient survey. Sample size varies slightly with variable: 1,264 for age; 1,295 for years in US; 1,290 for education variables. ACS sample is the IPUMS three year 2008-2010 ACS sample restricted to individuals 18-65 in the Washington, DC metro area (as defined by the ACS, includes MD and VA suburbs). Standard deviation in brackets for continuous variables.

Appendix Table 2: Impact of monitoring and communication treatments on recipient allocation decision: Raw amounts

	(1)	(2)	(3)	(4)
	Dependent variable:Amount allocated by recipient to…			
	Restaurant spending	Education spending	Daily expenses spending	Health spending
Panel 1:				
Recipient choice **revealed** to migrant	-0.650	-9.328*	9.528**	0.450
	[1.060]	[5.326]	[3.881]	[3.602]
Migrant preference **revealed** to recipient	0.789	-0.365	-3.825	3.401
	[1.058]	[5.329]	[3.887]	[3.602]
Observations	1,298	1,298	1,298	1,298
R-squared	0.001	0.002	0.005	0.001
Mean in recipient choice **not revealed**	6.1	175.5	66.0	52.3
Mean in migrant preference **not revealed**	5.4	171.0	77.9	50.8
Panel 2:				
Recipient choice **not revealed** to migrant, migrant preference **revealed to recipient**	0.407	-4.912	4.799	-0.295
	[1.474]	[7.449]	[5.393]	[5.122]
Recipient choice **revealed** to migrant, migrant preference **not revealed to recipient**	-1.030	-13.85*	18.11***	-3.229
	[1.355]	[7.582]	[5.648]	[5.119]
Recipient choice **revealed** to migrant, migrant preference **revealed to recipient**	0.128	-9.825	5.953	3.743
	[1.633]	[7.432]	[5.236]	[5.240]
Observations	1,298	1,298	1,298	1,298
R-squared	0.001	0.003	0.009	0.001
Mean in recipient choice **not revealed**, migrant preference **not revealed**	5.9	178.0	63.6	52.5
Control variables	NO	NO	NO	NO
Stratification group fixed effects	NO	NO	NO	NO

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for all experiment questions and completed recipient survey. Dependent variables are the raw amounts allocated by recipient to different spending categories. Omitted category in panel 2 regressions is "Recipient choice not revealed, migrant preference not revealed."

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

Appendix A:
Text used in migrant experiment

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

Question 1:

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

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

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

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

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

Question 2:

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

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

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

Appendix B:
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 only be spent on a limited number of things. In order to participate you must tell me now how you would like to allocate the remittance among the following categories, and if you win, you will receive exactly what you have told me that you want. The categories are: meals at local restaurants, education related expenses, daily expenses like groceries, and health related expenses**.** You can spend it all on one thing or break it up among different things.

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

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

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

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

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

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

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

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

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

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