

Migrant Remittances and Information Flows: Evidence from a Field Experiment

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Abstract

Do information flows matter for remittance behavior? We design and implement a randomized control trial to quantitatively assess the role of communication between migrants and their international network on the extent and value of remittance flows. In the experiment, a random sample of 1,500 migrants residing in Ireland was offered the possibility of contacting their networks outside the host country for free over a varying number of months. We find a sizeable positive impact of our intervention on the value of migrant remittances sent. Larger remittance responses are associated with individuals who are employed and earn higher incomes. This evidence is consistent with the idea that the observed increase in remittances is not a consequence of relaxed budget constraints due to subsidized communication costs but rather a likely result of improved information, perhaps due to better migrant control over remittance use, enhanced trust in remittance channels due to experience sharing, or increased remittance recipients' social pressure on migrants.

JEL classification: F22, J61, O15

Key words: migrant remittances, information flows, international migration, migrant networks, randomized control trial

Migrant remittances have grown substantially over the past decades, while showing remarkable resilience in the face of recent economic crises around the world. The financial flows generated by international migrants are surpassing the national public budget resources of several developing countries, as well as the Foreign Direct Investment and Official Development Aid flows these countries receive. It is therefore of great interest to learn more about the determinants and consequences of such important international financial flows.¹

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1 See Yang (2011) for a literature review on this topic.

One area of study crucial to understanding the determinants of migrant remittances concerns the relationship between migrants and their transnational networks and how it affects migrant decisions to remit. Often, migrants are part of a transnational household that was separated by considerable geographic distance at the time of migration. Distance between migrants and their networks is likely to affect this relationship in a variety of ways. For instance, this separation creates asymmetric information in the sense that neither the migrant nor the network can accurately observe each other's actions. In particular, at most times, the network outside the immigration country cannot accurately know the migrant's occupation, earnings, or standard of living, while migrants cannot perfectly observe their networks' true needs and uses of any financial transfers received.

In this context, it becomes most relevant to examine the role of information flows between migrants and their network outside the country of immigration in determining migrant remittance behavior. The impact of these information flows on migrant transfers is eminently an empirical question. Indeed, one can conjecture about several possible mechanisms that could affect remittances in different directions. First, communication flows should contribute to an increase in the information available within transnational households, thereby mitigating asymmetric information problems, which could increase or decrease migrant remittances depending on the direction of earlier informational deficiencies. Second, additional contact between migrants and their networks may stimulate the demand for remittances on the recipients' side, which would cause upward pressure on remittances. Third, the increased communication flows may lower the remittance costs and enhance trust in remittance channels due to experience sharing, which would likely increase remittance flows. A fourth mechanism could be that improved communication between migrants and their networks could actually substitute for remittances in the sense that contacts by migrants may be interpreted as a form of attention and caring, a role that could alternatively be performed by remittances. In this instance, improved informational flows would have a negative impact on transfers sent by migrants.

In this paper, we examine the role of information flows between migrants and their networks abroad in determining remittance behavior. To do so we design a randomized control trial under which we vary the magnitude of information flows between migrants and their transnational networks by distributing international calling credit to a randomly selected treatment group. This field experiment is conducted on a random sample of 1,500 immigrants residing in the greater Dublin area in Ireland.

The high incidence of phone use to contact the transnational network in our sample provides us with a clear indication of the potential impact of the calling credit, which could be used either on a mobile phone or on a landline phone. In particular, we provide evidence of a sizeable, statistically significant impact of the treatment on the extent of the communication flows in terms of the number of individuals contacted abroad, number of calls made, and conversation topics the migrant discussed with his/her transnational network in the month prior to the interview.

Our results show that the increased information flows that we generate experimentally have a significant and substantial role in raising the value of remittances sent to existing recipients. However, we find only modest support for the hypothesis that increased contact with non-remittance recipients positively affects the decision to remit to those individuals.

Migrants are mobile by definition and, due to the length of the intervention, this project experienced high levels of attrition. Our analysis is particularly careful in examining the impact of potential selective attrition in the estimation of the treatment effects of our intervention. Even though we find no evidence that the attrition in our sample was selective, we use the [Lee \(2009\)](#) bounds estimator accounting for potential selective attrition and obtain that our uncorrected estimates are all within the confidence intervals estimated in this way.

The role of information flows on remittance behavior has been previously examined in the existing migration literature. [McKenzie, Gibson, and Stillman \(2013\)](#) describe survey evidence according to which migrants underreport their earnings when contacting their family members in the country of origin in order to moderate their remittance requests and limit new immigrant arrivals. This finding is

consistent with ours, but we further show using experimental evidence that increasing information exchanges between migrants and their transnational networks increases the amount of remittance flows.

There are several recent papers on remittance-related strategic behavior by both migrants and their networks when their relationship is characterized by asymmetric information. [Ashraf, Aycinena, Martinez, and Yang \(2015\)](#) find, in a randomized field experiment, that savings in migrant-origin households in El Salvador rise when migrants (in the United States) are given new financial products that improve migrant control of savings in remittance-recipient households. Consistent with this finding, [Batista, Silverman, and Yang \(2015\)](#) use a lab-in-the-field experiment to show that urban individuals in Mozambique prefer to remit in kind (as opposed to in cash) in ways that express their preference to control recipient use of their transfers. [Ambler \(2015\)](#) conducts a lab-in-the-field experiment confirming that remittance recipients use resources differently when migrants can monitor this use. [Chen \(2013\)](#) also finds evidence of non-cooperative behavior related to the use of household resources in migrant households. [Ambler, Aycinena, and Yang \(2015\)](#), however, find that migrants offered a channel through which they could channel funds toward the education of a student of their choice choose not to use this service unless the use of this service is subsidized. Finally, [Seshan and Zubrickas \(2016\)](#) describe evidence of existing asymmetric information within transnational households and of its impact on remittance flows in the context of an adapted model of costly state verification. All of this work is consistent with our finding that improving information flows, and hence diminishing asymmetric information problems, can increase remittance flows.

An additional strand of related literature emphasizes the importance of transaction costs and trust in the remittance channel as determinants of remittance flows. [Aycinena, Martinez, and Yang \(2010\)](#) conducted a Randomized Control Trial (RCT) among Salvadorian migrants in the Washington DC area, showing that lower remittance costs increased both the magnitude and frequency of remittance flows, while [Batista and Vicente \(2013, 2016\)](#) also present experimental evidence for migrants in Mozambique, indicating that not only lower remittance costs but also the availability of a more trustworthy mobile banking remittance channel increase the magnitude and frequency of remittance flows. These results are also consistent with our findings in the sense that increased communication flows may lower remittance costs and enhance trust in remittance channels due to the sharing of experience between migrants and their network.

Finally, the positive role of information flows on remittance behavior can also be related to better integration of migrants in their networks at the origin country. [Chort, Gubert, and Senne \(2012\)](#) and [Batista and Umblijs \(2016\)](#) emphasize how remittances are used as a reciprocation or insurance mechanism from which migrants hope to benefit upon return to their home country. This idea is consistent with our findings in the sense that improved contact between migrants and their networks at origin is likely to deepen migrants' integration in these networks, a mechanism that is complementary to remittances in this framework.²

In the remainder of the paper, Section I describes our experimental design and the identification strategy. Section II presents the data collection procedure, summary statistics, and a discussion of balance at baseline. Section III discusses the econometric model and the empirical results. Section IV concludes.

I. Experimental Design and Identification Strategy

In order to quantitatively assess the role of communication flows in determining the extent and value of remittance flows between migrants and their contacts abroad, we implement a randomized field experiment, which consists of distributing international calling credit to a randomly selected treatment group.

2 A related branch of literature examines the role of networks and information on migration behavior. Notable recent examples of this line of work are [McKenzie and Rapoport \(2007\)](#), [Beine et al. \(2011\)](#), [Aker, Clemens, and Ksoll \(2012\)](#), [Umblijs \(2012\)](#), [Farre and Fasani \(2013\)](#), [Bryan, Chowdhury, and Mobarak \(2014\)](#), [Elsner, Narciso, and Thijssen \(2014\)](#), and [Beam, McKenzie, and Yang \(2016\)](#).

Respondents in the treatment groups received a letter at the end of the baseline survey with the information on how to redeem the calling credit.³ The international calling credit could be used to contact any number outside of Ireland, either landline or mobile, with the objective of increasing the communication flows between immigrants in Ireland and their family and friends outside of Ireland. The total amount of calling credit was 90 minutes irrespective of the destination country to be called. The cost of the international calling credit was about € 0.12 per minute to the researchers, and it was not disclosed to the participants.⁴ However, the actual value of the calling credit to the respondent could vary, depending on the destination country. For example, a phone call from Ireland to South Africa could cost between €1.12 and €1.26 per minute with the main Irish landline operator (Eircom) while the cost of a call to Poland was about €0.39 per minute.⁵

Participants in the experiment were randomly assigned to one of three groups. Respondents in Treatment Group 1 received 90 minutes of free international calling credit every month for five months. Migrants in Treatment Group 2 received 90 minutes of free international calling credit for three months (every other month). Finally, one third of the participants were assigned to the Control Group.⁶ Differences in the remittance behavior between the treated and control groups will allow identification of the intention-to-treat (ITT) effects of our intervention. Differences between the two treatment groups would arise as a result of the treatment frequency.

Upon completion of the baseline survey, participants were contacted by Computer-Assisted Telephone Interviewing (CATI) every month for a period of five months. The aim of the short (about 15 minutes in duration) monthly surveys was to gather information about remittance behavior, contacts with family and friends outside of Ireland, and the main topics of conversation. The calling credit accounts were topped up by the calling card provider on a monthly basis. The top-up was provided independently of the actual usage in the previous month. The respondents were informed about the top up at the end of the monthly survey.

About six to nine months after the fifth monthly survey, the final round of the survey was conducted and all participants were contacted again by CATI to elicit information about remittance behavior.⁷ Figure 1 outlines the timeline adopted for the various surveys and the intervention.⁸ All participants were informed of the timeline of the initial and follow-up surveys before the baseline interview could be initiated. Respondents in the treatment groups were made aware of the calling credit at the end of the baseline interview, and they were also informed at that stage about the timing of future top-ups.

II. Data Collection and Summary Statistics

The data used in our analysis consist of a representative household sample of 1,500 immigrants,⁹ aged 18 years or older, residing in the greater Dublin area, who arrived in Ireland between the year 2000 and

3 The letter provided the account details, that is, the number to call to activate the calling credit, the account number, and the PIN number. Participants were given the option to change the PIN number and to save the account information.

4 The international calling credit was provided by Swiftcall/Ninetel.

5 <http://www.eircom.ie/bveircom/pdf/Part2.1.pdf>.

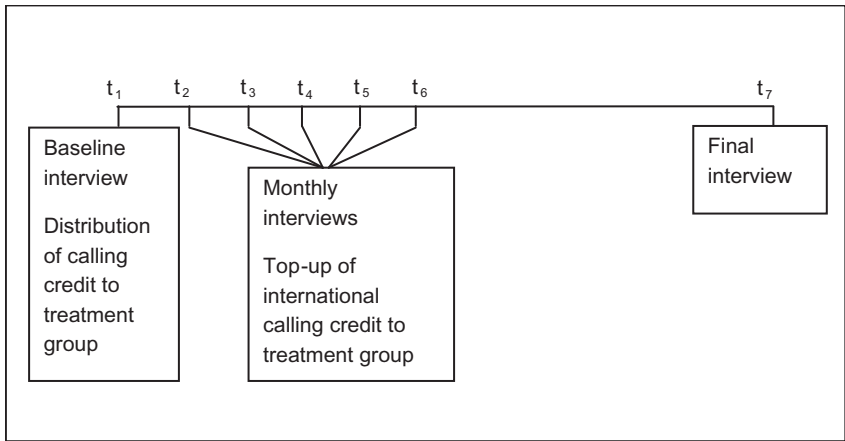
6 Due to funding constraints, it was not possible to distribute the equivalent amount of the calling credit to the control group.

7 To guarantee that the person being interviewed was the initial respondent, the CATI agent would ask some basic questions to confirm the identity of the migrant.

8 McKenzie (2012) discusses the advantages of conducting multiple follow-ups, which increase statistical power in the case of outcomes with low autocorrelation.

9 Immigrants in our sample are defined as not being Irish or British citizens. British citizens were excluded due to the close historical ties between Ireland and Great Britain, which still entitle British citizens to vote at parliamentary elections, for instance.

Figure 1. Timeline



six months prior to the interview date. The baseline sample was collected between February 2010 and December 2011.

Survey activities were conducted by Amarach Research, a reputable survey company with experience conducting research surveys in Ireland, under the close supervision of the authors and their research team.

Eligibility requirements for survey respondents were set to maximize the probability that migrants still kept contacts outside of Ireland (hence the 2000 initial arrival threshold) but were already minimally established in Ireland (for at least six months) so that contacts with their networks abroad could provide useful information. Due to missing relevant information about eligibility for nine respondents, the final sample size is 1,491.

Random sampling was performed in the following way. First, 100 Enumeration Areas (EAs) were randomly selected out of the 323 Electoral Districts in the greater Dublin area. This selection was performed according to probability-proportional-to-size sampling, in which size is defined as the total number of non-Irish and non-British individuals residing in Ireland, according to the 2006 Census of Ireland. Second, 15 households were selected within each EA using a random route approach.¹⁰ Finally, in the presence of more than one eligible respondent in the household, the individual respondent was randomly selected based on a next-birthday rule. In the absence of the designated respondent, an appointment was set up for a later date.

The random route approach consisted of the following procedure: each enumerator was given a map of the assigned EA and a pre-selected random starting address within the allocated EA. After a successful interview, enumerators were instructed to exit the house, turn left, count five houses down and approach this new address.¹¹ In the case of an absent household, interviewers were requested to call back to the address for a maximum of five times at different times of the day and different days of the week. Each call-back was recorded on the interviewer's report. When an address was exhausted after five call-backs, deemed ineligible, or in the case of a refusal, the interviewer followed predefined instructions in order to get the next address, namely the address next door to the left when exiting the house.

All enumerators were initially trained by the research team and were subsequently supervised by the survey company and, randomly, by members of the research team. Each enumerator had to complete an

10 The 15 households are drawn from the non-Irish/non-British population.

11 A set of standard rules were given in the case of crossroads, apartment buildings, and culs de sac.

enumeration report, listing each address approached, the number of call-backs, and the outcome of each visit. The enumeration reports were closely inspected and verified by the research team. If the randomization instructions were not followed, interviews had to be replaced.

Descriptive Statistics

Most immigrants included in our sample are of Nigerian nationality (19%), followed by Polish (11%), Indian (6%) and South African (5%). In total, the sample covers 101 nationalities.¹²

Table 1 presents the summary statistics for a set of basic demographic characteristics of migrants for both treatment and control groups at baseline. The average participant age is 32 and a slight majority of respondents is female. About 42% of the respondents are married and the average length of stay in Ireland is five years. A large majority of respondents have parents living in the country of origin. Survey participants report a high degree of education with about 70% having a post-secondary degree or higher and 28% having a secondary school degree. About 75% of the respondents in our sample are employed, compared to 51.4% of the overall population in Ireland in 2011 (ILO). The net monthly income earned by surveyed individuals is around €1,200 per month, with an average of 23 working hours per week. About half of the respondents planned to return to their home country in five years or less at the moment of arrival. However, when asked about their current intentions to move away from Ireland, less than 40% of the respondents intended to leave the host country in the following five years.

Table 1. Demographics of Respondents at Baseline vs. Census 2011

Variable	Own Survey			Sample Size	Census 2011
	Treatment Mean	Control Mean	Difference T-C (S.E.)		
Age	32.80	32.20	0.59 (0.44)	1491	32.6
Female	0.55	0.52	0.03 (0.03)	1491	0.50
Married	0.42	0.42	0.00 (0.03)	1491	0.49
Years in IRL	5.38	5.29	0.09 (0.16)	1489	–
College or Secondary Education	0.69	0.72	–0.02 (0.02)	1483	0.70
Secondary Education	0.28	0.27	0.01 (0.02)	1483	0.31
Employed	0.75	0.76	–0.02 (0.02)	1491	0.58
Number of children	0.96	0.88	0.08 (0.07)	1491	–
Parents living in country of origin	0.84	0.83	0.01 (0.02)	1491	–
Net Monthly Income (in Euro)	1,165	1,193	–28 (63.94)	1356	–
Number of working hours per week	22.95	24.32	–1.38 (0.96)	1375	–
Intended to return in 5 years or less at arrival	0.51	0.52	–0.01 (0.03)	1389	–
Currently intends to return in 5 years or less	0.39	0.36	0.03 (0.03)	1370	–
Average monthly communication costs (in Euro)	20.04	18.26	1.78 (1.17)	1458	–
Remitted in previous year (binary variable)	0.36	0.32	0.04 (0.03)	1458	–
Value of monthly remittances sent in previous year (in Euro)	47.79	47.62	0.17 (7.68)	1458	–

Notes: ***p < 0.01, **p < 0.05, *p < 0.1.

12 Table S.1 in the online appendix (available at <https://academic.oup.com/wber>) presents the distribution of the top nationalities in our sample. The distribution of the top nationalities is balanced between treatment and control group. The comparison between our survey and the census (2011) distribution of the main immigrant nationalities in the greater Dublin area shows that our survey over-represents the proportion of African immigrants in our sample, while under-representing immigrants of Eastern European nationality. The proportion of immigrants from Asia and Latin America is similar in our survey and in the census (2011).

A great fraction of the individuals in our sample moved to Ireland for work reasons (40%), although acquiring education and the presence of an existing migration network are also cited as motives to migrate to Ireland (15% and 16% respectively). Language seems to matter as 9% of respondents chose Ireland because it is an English-speaking country. About 6% of respondents picked Ireland for religious motives, and a similar percentage moved to Ireland due to its immigration policies and visa requirements.¹³

The baseline survey also provides extensive information regarding the transnational network of migrants, namely the size of this network, the cost of keeping in contact with it, whether remittances are sent and, if so, the amount remitted. On average respondents are in contact with two people living outside of Ireland and the average monthly cost of contacting a network contact abroad is around €20.¹⁴ As shown at the bottom of table 1, about one third of the participants in our sample send remittances with a monthly amount of remittances sent averaging around €47 (and over €125 when restricting to positive amounts only).

We do not find any evidence of statistically significant differences between control and treatment groups for any of the described variables at baseline. The last column of table 1 presents the relevant demographics from the Irish Census (2011) and compares them to the ones in our sample. Overall, our sample captures the majority of features the migrant population according to the Census (2011).

Follow-up Surveys and Attrition

Migrants are mobile by definition, and, given the length of the project,¹⁵ selective attrition could be a cause of concern. Respondents in the treatment group received an international calling credit at the end of the baseline survey and upon completion of short phone surveys. We therefore anticipated a higher dropout rate in the control group relative to the treatment group.¹⁶ A higher dropout rate in the control group is indeed confirmed by the attrition analysis presented in table 2. Initially, about 35% of the respondents in the treatment group dropped out compared to 44% of the control group. These attrition rates worsened after each round of the survey, ending up at 84% and 89% for the treatment and control groups, respectively. The difference in the dropout rates between the treatment group and the control group is statistically significant for each round of the survey.

Table 2. Attrition

	Control Mean	Treatment Mean	Difference (S.E.)
Dropout – 2 rounds	44%	35%	0.08 (0.03)**
Dropout – 3 rounds	56%	51%	0.05 (0.03)*
Dropout – 4 rounds	67%	62%	0.05 (0.03)**
Dropout – 5 rounds	74%	68%	0.06 (0.02)**
Dropout – 6 rounds	78%	72%	0.06 (0.02)**
Dropout – 7 rounds	89%	84%	0.06 (0.02)**

Notes: ***p < 0.01, **p < 0.05, *p < 0.1.

13 See table S.2 in the online appendix for further details.

14 Participants mainly contact their parents (35%), siblings (31%), and friends (23%). See table S.3 in the online appendix for more information about the relationship between participants and their transnational network.

15 More than one year went by between the first baseline and last follow-up interviews.

16 In order to counter dropout rates, we provided incentives to all participants in the project by giving away five lottery prizes with a €100 value and a final lottery prize of €500. The prizes were highly advertised by the enumerators.

To exclude the possibility of selective attrition, we evaluate the difference between treatment and control dropouts relative to the set of baseline observable variables presented in the descriptive statistics. We focus on the participants who dropped out after the first round of the survey at each of the following survey rounds. The results of this analysis are presented in table S4 of the online appendix. We find no systematic evidence of selective attrition as differences between characteristics in the control group and in the treatment group are nearly always not statistically significant. These results are reassuring in terms of the validity of the analysis. We nevertheless address the impact of potential selective attrition in the estimation of treatment effects by following Lee (2009) to estimate bounds on our coefficients of interest. These estimation results are presented in section V.

III. Estimation Strategy

In order to estimate the effect on remittance behavior of increased information flows between migrants and their network outside of the host country, we focus on two main dependent variables: the probability of remitting (extensive margin) and the value of monthly remittances (intensive margin). The design of the RCT and multiple-round survey we conducted allows us to estimate the effect of the treatment in two ways. First, we adopt a single difference approach by analyzing the post-intervention data (rounds two to seven of the survey) and we estimate the following specification:

$$Y_{it} = \beta_0 + \beta_3 T_i + X'_i \delta + \vartheta_t + \epsilon_{it} \quad (1)$$

where Y_{it} is either an indicator variable taking the value 1 if the migrant remits and 0 otherwise, or the amount of monthly remittances sent by respondent i at time t , where t is the time of the intervention period (round two to round seven of the survey). X_i is a vector of individual baseline characteristics: age, employment status, marital status, gender, number of individuals regularly contacted abroad, average monthly cost of calling network abroad, post-secondary education, whether the parents of the respondent are alive and live outside of Ireland, number of years in Ireland, continent of origin, and enumeration area fixed effects. Finally, ϑ_t represents survey round fixed effects.

Given the availability of pre-intervention data on outcome variables from the baseline survey, we also use a difference-in-differences approach and estimate the following specification:

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 post_t + \beta_3 T_i * post_t + X'_i \delta + \vartheta_t + \epsilon_{it} \quad (2)$$

where $post_t$ is an indicator variable that takes the value 1 for post-intervention period (rounds two to seven) and 0 for the pre-intervention period (round one). Y_{it} , X_i and ϑ_t are defined as before. As a further robustness check, we estimate a difference-in-differences specification with individual fixed effects (ϑ_i):

$$Y_{it} = \beta_2 post_t + \beta_3 T_i * post_t + \delta_i + \vartheta_t + \epsilon_{it} \quad (3)$$

where the impact of increased communication flows is captured by the β_3 coefficient.

In both specifications, we are interested in identifying the intention-to-treat effect, that is, the impact of the treatment T_i on remittance behavior variable Y_{it} , which is given by the coefficient β_3 . Regular least squares estimates are used to estimate β_3 . Standard errors are clustered at the individual and time level, following Cameron et al. (2011).

IV. Main Empirical Results

We begin the empirical analysis by showing that the experimental intervention effectively increased communication flows between migrants and their network abroad. According to the baseline survey, mobile

phones, landline phones, and international calling cards make the primary mode of contacting people abroad for 75% of our participants.¹⁷

The high incidence of international phone use in our data provided us with a first indication of the potential usage of the calling credit, which is similar to an international calling card and could be used either on a mobile or landline phone. This suggestive evidence is strengthened by the estimation results reported in [table 3](#), according to which there was a sizeable, statistically significant impact of the treatment on the extent of the communication flows. The monthly CATI interviews reported information about the number of individuals contacted abroad, number of calls made, and conversation topics the migrant discussed with his/her transnational network in the month prior to the interview.¹⁸ On average, respondents in the treatment group contact more people, make a greater number of calls, and talk about a larger number of topics regarding both Ireland and the country of residence of the contact person. Overall, it seems that the international calling credit was effective in increasing the information flows between migrants and their network abroad. These findings hold also when we include the set of demographic controls (column 2), time-fixed effects (column 3), enumeration area-fixed effects (column 4), and continent-fixed effects (column 5).

Effect On Remittances

Having established the effectiveness of our intervention in terms of its take-up, we now turn to examining the impact of the intervention in terms of our outcome of interest: migrant remittances. The lower panel of [table 3](#) reports the results of the single difference estimation of specification (1) for the extensive margin, that is, the probability of remitting using a linear probability model. The dependent variable in this specification is an indicator variable that takes the value 1 if the respondent sends monthly remittances and 0 otherwise. We find that the treatment has a positive and statistically significant impact on the probability of remitting. Treated migrants are 5.3 percentage points more likely to remit than respondents in the control group, an effect that is robust to the inclusion of demographic and communication controls as well as survey round-fixed effects. The estimated coefficient is still statistically significant when we introduce enumeration area-fixed effects (column 4) and continent of origin-fixed effects (column 5).

The strongest results in our analysis arise when we analyze the impact of the increased communication flows on the value of monthly remittances.¹⁹ The last row of [table 3](#) presents the effect that providing additional free calling credit to individuals in the treatment group has on the value of monthly remittances. This impact is positive and highly statistically significant. Treated migrants increase the amount of monthly remittances sent to their transnational network by about €40. Adding demographic and communication controls in column (2) slightly increases the magnitude of the treatment impact without changing its statistical significance. In columns (3)–(5) we progressively add time-fixed effects (column 3), enumeration area-fixed effects (column 4), and continent of origin-fixed effects (column 5). Treated migrants are still found to remit more than respondents in the control group. The average treatment effect in the specification with all controls and fixed effects included is about €45, as shown in column (5).

Overall, we conclude that the increased communication flows triggered by the treatment (upper panel of [table 3](#)) produce a strong, significant increase in the amount of remittances sent (intensive margin) and also a smaller increase in the probability of remitting (extensive margin).

17 Please see table S.5 for further details.

18 These conversation topics include the level of wages, opportunities to find a job, cost of living, regulation for foreign migrants, unemployment benefits and other social benefits, health care system, education system, and taxes both in Ireland and in the country of residence of the contact person.

19 Our analysis is based on the unconditional value of gross remittances sent, including zeros.

Table 3. Intention-to-Treat Effects of Intervention on Communication and Remittance Outcomes

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of individuals contacted in previous month	Coefficient	0.341*** [0.075]	0.319*** [0.074]	0.227*** [0.069]	0.214*** [0.067]		
	Standard Error						
Number of calls made in previous month	Sample Size	2,764	2,702	2,702	2,702		
	Coefficient	2.498*** [0.860]	2.287*** [0.858]	2.382*** [0.857]	1.773** [0.853]	1.671** [0.823]	
Number of host country related topics talked about in previous month	Standard Error						
	Sample Size	2,605	2,544	2,544	2,544	2,544	
Number of origin country related topics talked about in previous month	Coefficient	1.015*** [0.223]	0.991*** [0.221]	1.003*** [0.223]	0.956*** [0.208]	0.918*** [0.197]	
	Standard Error						
Remittances sent (indicator variable)	Sample Size	2,622	2,560	2,560	2,560	2,560	
	Coefficient	0.971*** [0.191]	0.967*** [0.178]	0.978*** [0.180]	0.901*** [0.166]	0.870*** [0.156]	
Remittances sent (monthly value in EUR)	Standard Error						
	Sample Size	2,622	2,560	2,560	2,560	2,560	
	Coefficient	0.053** [0.021]	0.055*** [0.020]	0.052** [0.020]	0.048** [0.023]	0.043** [0.020]	0.029 [0.023]
	Standard Error						
	Sample Size	2,702	2,639	2,639	2,639	2,639	4,160
	Coefficient	38.082*** [9.501]	40.759*** [9.915]	42.048*** [9.672]	45.389*** [11.365]	44.562*** [11.306]	38.541*** [12.094]
	Standard Error						
	Sample Size	2,702	2,639	2,639	2,639	2,639	4,160
	Specification	Single Diff	Single Diff	Single Diff	Single Diff	Single Diff	Diff-in-Diff
	Controls	No	Yes	Yes	Yes	Yes	No
	Time FE	No	No	Yes	Yes	Yes	Yes
	EA FE	No	No	No	Yes	Yes	No
	Continent FE	No	No	No	No	Yes	No
	Individual FE	No	No	No	No	No	Yes

Notes: ***p < 0.01, **p < 0.05, *p < 0.1.

Controls include employment status, age, post-secondary degree or college dummy, whether parents are alive and live abroad, gender, number of contacts abroad, average monthly cost of calling, and length of stay in Ireland. Standard errors are two-way clustered at the level of the individual and time.

Difference-in-Differences Estimation

The analysis presented so far made use of the post-intervention data, that is, survey rounds two to seven. Using the baseline survey allows us to also adopt a difference-in-differences estimation strategy. Column (6) of [table 3](#) reports the estimation results for the specification detailed in [equation \(2\)](#). The estimated ITT effect (the coefficient on the interaction between the treatment and the post-intervention indicator) takes a positive and statistically significant value. Treated migrants send €43 more remittances than the control group. Column (7) presents the specification outlined in [equation \(3\)](#), that is, a difference-in-differences specification with individual fixed effects in addition to the survey round-fixed effects already included in column (6). The estimated coefficient of interest keeps a similar positive magnitude with statistical significance at the 5% significance level. Columns (6) and (7) replicate the analysis for the extensive margin as well. We do not find any statistically significant impact of the treatment on the probability of remitting.

We summarize by stating that the treatment had a strong effect on the intensive margin while its impact on the extensive margin appears less robust. In section V, we analyze some of the possible mechanisms at play.

Two Treatments

As described in section I, the two treatment groups in the experimental intervention differ only in the frequency of the calling credit top-up. Migrants in treatment group 1 received a monthly calling credit top-up for a total of five months. Respondents in treatment group 2 received a calling credit top-up every other month for a total of three times. Table S.6 in the online appendix reports the results of the estimation of [equation \(1\)](#) differentiating between the two treatments. Both treatments have a statistically significant impact on the amount of remittances with an estimated average treatment effect of €42 for treatment 1 and €35 for treatment 2, according to the difference-in-differences specification using individual fixed effects. The two treatments increase the probability of sending remittances, although the effect is only statistically significant for the most frequent treatment, which increases the probability of remitting by five percentage points according to the difference-in-differences specification with individual fixed effects.

The test of equality of the coefficients of the two treatments cannot reject the null hypothesis that the two coefficients are of the same magnitude in any of the specifications for either the intensive or the extensive margins. This is only close to happening with a 0.15 p-value in the case of the extensive margin where the more frequent treatment seems to trigger substantially stronger treatment effects. Interpreting this result precisely would require further experimentation: the evidence at hand does not allow us to distinguish whether this result is due to the fact that a single episode of improved communication is capable of breaking asymmetries in information in a way that increases remittances or whether some other frequency of change in communication patterns is necessary to achieve that result.

Since there is no statistically significant difference between the two treatments we proceed by evaluating the joint impact of the two treatments in the remainder of the analysis.

V. Robustness Checks

Given the extent of attrition in our sample and the fact that we cannot *a priori* predict whether attrition could generate an upward or downward bias in our treatment effect estimates, we estimate lower and upper bounds to our estimates following the methodology put forward by [Lee \(2009\)](#).²⁰

20 The [Lee \(2009\)](#) bounds estimator relies on two main assumptions: random assignment of the treatment, which we already verified in our balance tests, and monotonicity. Monotonicity implies that the assignment of the treatment might affect attrition in one way only. This appears to be the case in our study, as attrition is higher in the control than in the treatment group for each of the survey rounds as shown in table S.4.

According to our estimates (displayed in table S.7 in the online appendix), both the lower and upper Lee bounds are of the same sign and close magnitude to our main point estimate of the impact of our intervention on the value of remittances—the comparable point estimate is 38 (see column (1) in table 3) whereas our lower bound estimate is 37, and the upper bound estimate is 50. In addition, all our bound estimates are statistically significant at the 1% level.

Similar results apply to our Lee (2009) bound estimates of the impact of our intervention on the probability of remitting. The comparable point estimate is 0.05 (see column (1) in table 3) whereas our lower bound estimate is 0.05, and the upper bound estimate is 0.06. Again, both bound estimates are statistically significant at the 1% level.

These bound estimates are supportive in that, despite the high levels of attrition experienced over the course of this project, potential differential attrition does not seem to have been a cause of bias in our estimates.

Is It Just A Fungibility Effect?

One possible concern is that treated migrants are simply using the savings from the decreased costs of calling their international networks to increase the remittances they send. In order to tackle this potential alternative explanation of our findings, one should first of all find a good proxy for the value of the savings provided by the calling credit given to treated individuals.

The most expensive official Eircom rates (which would place the value of the calling credit between €35.1 and €113.40, depending on the country called as discussed in the first paragraph of section 2 of the paper) provide an upper bound to the value of the savings provided by the calling credit that was offered to the treated migrants. This is, however, an unlikely upper bound to be generally achieved since the baseline survey responses show that only 10% of the respondents use landline phones to communicate with their network abroad, and these are not necessarily all using the most expensive Eircom international calling rates. An alternative could be to consider the €10.80 that were paid monthly by the research team for the calling credit of each treated migrant in our sample. This amount provides a reasonable average of the value of the calling credit since some individual migrants are likely to be knowledgeable of country-specific saving forms of communication whereas others may be less savvy or interested in this type of saving. This would seem like a good average approximation to the value of the calling credit and should definitely be closer to a lower bound than the official landline Eircom rates. A conservative alternative assumption is to consider the individual baseline average monthly calling cost of the migrants in our sample as a good proxy for the value of the calling credit that was offered to treated individuals in our sample. This is a conservative assumption in the sense that it assumes that all the migrant's monthly communication costs were paid by the research team, that is, we are assuming that the migrants were at the baseline not talking more than 90 minutes per month to their networks abroad. Under this assumption, the value of the calling credit varies between €0 and €350 and averages €19.4. It is an intermediate assumption between the two scenarios discussed above.

Under this assumption that the savings provided by the calling credit can be well approximated by the migrant's average monthly communication costs, we performed a simple accounting exercise to evaluate the impact of the savings provided by the intervention on the value of remittances—assuming perfect substitutability between saved communication costs and remittances, a somewhat strong conservative assumption. To perform this accounting exercise, we subtracted the average baseline communication cost from the remittance value sent by each treated individual after the intervention. This adjusted remittance value is now on average €19 higher in the treatment group relative to the remittance value sent by control individuals. Using this adjusted remittance value as the dependent variable (in single difference and difference-in-differences regressions with individual controls) yields significant intention-to-treat coefficients as displayed in table S.8. These estimates yield point estimates a little in excess of €20 with the

confidence intervals ranging between €3.20 and €48.94 in additional remittance flows after accounting for potential fungibility of the calling credit provided to migrants in the treatment group.

Overall, there seems to be a significant positive effect of the intervention on the value of remittance flows even when accounting for a relatively large substitution effect. However, as could be expected, this reduces the magnitude and economic significance of the estimated treatment effect.

An additional test that allows us to refute the fungibility effect concern over our results is provided in [table 4](#). This table reports the results from a difference-in-differences specification (with and without individual fixed effects), which now also includes an interaction term between the treatment indicator and the monthly average calling cost.²¹ As shown in columns (1) and (2), treated migrants are found to remit between €66 and €61 more than migrants in the control group after the intervention, and the estimated coefficient is statistically significant at the 1% level. In addition to the positive impact of the treatment on the value of monthly remittances, the triple interaction term between the treatment, the average communication costs, and the after intervention indicator is negative and statistically significant also at the 1% level. This means that the greater the communication costs between migrants and their network abroad, the lower the impact of the treatment on the value of monthly remittances.

Columns (3) and (4) of [table 4](#) show that a similar pattern emerges in the analysis of the impact of the intervention on the extensive margin of remittances. Treated migrants are about eight percentage points more likely to remit once we control for the interaction between the treatment and the average cost of

Table 4. Interaction with Calling Costs

Variables	(1)	(2)	(3)	(4)
	Value of monthly remittances		Monthly remittances - dummy	
Treatment	−5.415 [7.452]		0.011 [0.017]	
Treatment* Post	66.454*** [11.473]	61.473*** [15.464]	0.088*** [0.019]	0.086*** [0.030]
Treatment* Avg. cost of Calling*Post	−1.221*** [0.136]	−1.112*** [0.305]	−0.004*** [0.000]	−0.003*** [0.001]
Treatment*	0.115 [0.384]		0.001 [0.001]	
Avg. cost of Calling				
Avg. cost of calling	0.558* [0.286]		0.003*** [0.001]	
Individual Controls	Yes	No	Yes	No
Round FE	Yes	Yes	Yes	Yes
EA FE	Yes	No	Yes	No
Continent FE	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Specification	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff
Sample Size	4089	4108	4089	4108
Number of individuals		1458		1458
R-squared	0.012	0.003	0.024	0.005

Notes: ***p < 0.01, **p < 0.05, *p < 0.1.

Controls include employment status, age, post-secondary degree or college dummy, whether parents are alive and live abroad, gender, number of contacts abroad, length of stay in Ireland. Standard errors are two-way clustered at the level of the individual and time.

21 The monthly average of the cost of calling is included in the list of communication controls used in all regression specifications.

Table 5. Interaction with Employment (Indicator) and Income (in Thousands of Euro) Variables

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Value of monthly remittances		Monthly remittances - Indicator		Value of monthly remittances		Monthly remittances - Indicator	
Treatment*Employed/ Income	54.413*** [10.342]	57.785*** [13.601]	-0.034* [0.021]	-0.037 [0.031]	0.035*** [0.012]	0.036*** [0.011]	-0.000 [0.000]	-0.000 [0.000]
*Post								
Treatment*Post	2.357 [7.970]	-4.147 [8.304]	0.042* [0.023]	0.056* [0.031]	-1.807 [10.204]	-8.499 [9.077]	0.015 [0.024]	0.024 [0.029]
Treatment*Employed/ Income	-13.736 [16.255]		0.004 [0.024]		0.001 [0.007]		0.000 [0.000]	
Treatment	7.872 [12.760]		0.023 [0.018]		-3.847 [8.866]		0.021 [0.015]	
Employed/Income	14.772 [13.371]		0.075*** [0.022]		0.003 [0.007]		0.000 [0.000]	
Specification	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff	Diff-in-Diff
Controls	Yes	No	Yes	No	Yes	No	Yes	No
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
EA FE	Yes	No	Yes	No	Yes	No	Yes	No
Continent FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes	No	Yes	No	Yes
Sample Size	4089	4160	4089	4160	3771	3829	3771	3829
Number of individuals		1473		1473		1343		1343
R-squared	0.063	0.006	0.125	0.044	0.067	0.007	0.139	0.053

Notes: ***p < 0.01, **p < 0.05, *p < 0.1.
Controls include employment status, age, post-secondary degree or college dummy, whether parents are alive and live abroad, gender, number of contacts abroad, and length of stay in Ireland. Standard errors are two-way clustered at the level of the individual and time.

calling, as can be seen in column (3). The estimated coefficient is positive and statistically significant at the 1% level. This result also holds when we consider the difference-in-differences controlling for individual fixed effects as shown in column (4). The coefficient on the triple interaction between treatment, post intervention, and calling costs is again negative and statistically significant at the 1% level.

Were our results driven by fungibility of the calling credit provided, then we would have expected to see the opposite relationship between the cost of calling and remittance behavior for treated participants, that is, we would have observed a greater positive impact of the treatment on remittance behavior for those with higher cost of calling. In fact, the results reported in [table 4](#) present the opposite effect: the impact of the treatment is larger for the participants with *lower* communication costs. It may be argued that higher communication costs at baseline may capture a higher disposable income. We explore the relationship between income and remittance behavior in the next section and provide further evidence to refute a fungibility or substitution effect between the decreased costs of communication elicited by the experiment and remittance behavior.²²

Interpretation of the Findings

The increased communication flows might improve migrant's control over remittance use and enhance trust in remittance channels due to experience-sharing. If this is the case, we can expect treated migrants who are regularly employed and who have higher income to send more remittances, the assumption being that these individuals are more likely to have the financial liquidity to send more remittances should they wish to do so. We test this hypothesis by focusing on the interaction between the employment status dummy and the treatment indicator ([table 5](#), columns 1–4) and, as a further robustness check, the interaction between income and the treatment indicator ([table 5](#), columns 5–8).

The estimation results confirm the hypothesis: treated migrants who are employed tend to remit more while no clear effect is found on the probability of remitting. A similar result emerges when we consider the interaction with the income variable (columns 5 and 6). The greater the earned income the greater the increase in the amount of money remitted by treated migrants. No effect is found on the probability of remitting (columns 7 and 8).

[Table 5](#) provides further support to the idea that the observed increase in remittances is not due to relaxed budget constraints thanks to subsidized communication costs but rather a result of improved information. In this sense, these findings offer further evidence to contradict the substitution effect discussed previously.

VI. Conclusions

Our results show that improving communication flows between migrants and their networks abroad may promote more migrant remittances. In particular, we identify a significant positive increase in the value of remittances sent (which nearly doubles relative to baseline) as a result of experimentally subsidizing communication between migrants and their networks outside of the immigration country. We find, however, only a relatively small (about 25% relative to baseline) increase in the probability of migrants in our sample sending remittances to a larger number of individuals in their network.

22 Communication costs may also be correlated with transfer costs, that is, the cost of sending remittances. While migrants could use the savings from the calling credit to transfer money to their friends and family members, they might also have to pay higher remittance fees. To this end, we use data on remittance costs at baseline and include this information in our specification. The impact of the treatment is robust to the inclusion of remittance costs in the regression (table S.9 in the online appendix).

In our analysis, we devote particular attention to the high levels of attrition experienced in the project participation, which could potentially affect our estimation results. We find that the main findings are robust even when adopting the Lee bounds estimator that takes into account selective attrition.

Even though our research design did not explicitly test for the mechanisms underlying this finding, our analysis shows that we can confidently exclude that the remittance effect we identify is a simple substitution or fungibility effect whereby those with higher subsidized communication costs increase their remittance flows by more. To exclude this substitution effect, we start from noting the necessary condition that the effects of the intervention on various indicators of communication between migrants and their international networks are economically meaningful and statistically significant at the 1% level. In addition, we perform an accounting exercise where we test for treatment effects on an adjusted measure of remittances that excludes the value of the subsidized communication costs. Our main results still hold even though the magnitude of the effects on the value of remittances is decreased, a mechanical result of this exercise. We also find that the impact of the treatment is significantly larger for the participants with lower communication costs, which is the opposite of what we could expect to happen if our results were driven by a positive income effect of the intervention. Similar results and reasoning hold when controlling for the cost of sending remittances.

Finally, we obtain that larger remittance responses are associated with individuals who are employed and earn higher incomes. This evidence is consistent with the idea that the observed increase in remittances is not a consequence of relaxed budget constraints due to subsidized communication costs, but rather a likely result of improved information perhaps due to better migrant control over remittance use, enhanced trust in remittance channels due to experience-sharing, or increased remittance recipients' social pressure on migrants. While additional research is necessary to distinguish the different mechanisms potentially at play, we believe this paper achieves an important first step in showing in a rigorous experimental way that information flows do play a role in determining migrant behavior.

The findings of our work highlight the importance of investment in technology that increases the reach and efficiency of communication flows. In addition to other beneficial effects already documented in the literature, such an investment may be valuable to developing countries with substantial emigration stocks as there may be increased remittances flowing back to these migration countries of origin.

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