Information and the Acquisition of Social Network Connections*

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Abstract

We study a randomly-assigned program providing information on U.S. settlement for new Filipino immigrants. Improved information leads immigrants to acquire fewer new social network connections (16-28% fewer new friends and acquaintances and 65% lower probability of receiving support from immigrant organizations) and has no effect on employment or subjective wellbeing. Consistent with a simple model, the treatment reduces social network links more when costs of acquiring network links are lower. Information and social network links appear to be substitutes in this context. Offsetting reductions in acquisition of social network connections can reduce the effectiveness of information interventions.

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1 Introduction

Failures of the perfect information assumption – that agents are endowed with full information relevant for the decisions they make – are a popular focus of research in economics. Imperfect information takes center-stage in economic studies of health (Dupas, 2011; Einav and Finkelstein, 2018), labor market search (Calvó-Armengol, 2004), and financial literacy (Lusardi and Mitchell, 2014), among other areas. Programs to mitigate information imperfections are ubiquitous, and are themselves a frequent subject for applied research in economics.

Individuals and households also make their own efforts to fill information gaps. A commonly-studied information source is social networks, which can channel information from more- to less-informed network members and thus provide *information capital* (Jackson, 2019). Social networks facilitate flows of information about new agricultural technologies (Foster and Rosenzweig, 2010; Carter, Laajaj and Yang, forthcoming), health goods (Dupas, 2014), microfinance products (Banerjee et al., 2013), employment opportunities (including migration) (Munshi, 2003; Beaman, 2012; Beaman and Magruder, 2012; Dustmann et al., 2016; Blumenstock, Chi and Tan, 2019), and business opportunities (Cai and Szeidl, 2018).

To date, there has been scarce research on how interventions to improve information might interact with the acquisition of social network connections. Immigrants who have just arrived in their country of destination are a fruitful context in which to study this interaction. Immigrants typically have imperfect information about their new societies. At the same time, immigrants usually have small social networks at arrival. They are hence likely to invest in social networks to reduce information imperfections. Substantial past research documents the important role of social networks for immigrants.[1] Immigrants frequently live and work with compatriots in ethnic enclaves, motivated in part by eased sharing of information that comes with geographic proximity (Portes

[1] Key citations include Massey (1988); Borjas (1992); Carrington, Detragiache and Vishwanath (1996); Munshi (2003); Calvó-Armengol and Jackson (2004); Orrenius and Zavodny (2005); Amuedo-Dorantes and Mundra (2007); Dolfin and Genicot (2010); Docquier, Peri and Ruyssen (2014); Mahajan and Yang (2020).

and Jensen, 1989; Beaman, 2012).

We implemented a randomized controlled trial on the impact of reducing imperfect information problems among immigrants. In collaboration with the Philippine government, we designed an information intervention for new immigrants to the U.S.: an enhanced "pre-departure orientation seminar" (PDOS) and an accompanying paper handbook. We randomly assigned these to Filipinos about to depart for the U.S. as new legal permanent residents ("green card" holders). A control group received the standard PDOS, which was substantially less informative in terms of both quantity and quality of information provided. We surveyed treatment and control group participants after arrival on their settlement in the U.S.,[2] social networks, employment, and overall life satisfaction. Our empirical analyses are guided by a pre-analysis plan and account appropriately for multiple hypotheses.

We find that the treatment led to reductions in the number of social network links in the U.S. As pre-specified, we measure social network size with an index combining information on the number of new friends and acquaintances, and support received from Filipino organizations. This effect is substantial in magnitude, amounting to 0.14 to 0.17 standard deviations of the network size index, and is stable across the short- and longer-run. The treatment has negative effects on each component of the index, reducing the number of friends and acquaintances by 16-28 percent, and reducing support received from organizations by two-thirds. The treatment reduces the number of network links across the board including the number of Filipino and non-Filipino friends and acquaintances and the number of close friends. This pattern suggests that the treatment does not change the type of social network links acquired.The treatment has no large or statistically significant impacts on settlement, employment, or self-reported wellbeing.

Our finding of a negative effect on social network links was unanticipated. Because the new PDOS explicitly encourages migrants to make new friends and join Filipino associations in the U.S., in our pre-analysis plan we hypothesized a positive treatment effect on social network connections. After

[2] We measure "settlement" as the fraction of the following items the immigrant has acquired: bank account, Social Security number, health insurance, and driver's license.

seeing the actual negative treatment effect, we wrote down a simple theoretical model that can rationalize such an effect. We consider individuals with imperfect information deciding on the optimal number of first-degree network links ("friends").[3] Friends are costly to acquire, but reduce information imperfections. We consider the impact of exogenously reducing information imperfections. Information and friends could be substitutes, meaning additional information provided by the treatment reduces the marginal benefit of friends, and correspondingly reduces friend acquisition. Alternately, information and friends could be complements: the treatment could increase the marginal benefit of friends, increasing friend acquisition. In the context of the model, our empirical results are consistent with information and friends being substitutes: improved information leads to offsetting reductions in acquisition of network links.

In exploratory analyses, we examine the heterogeneity of the treatment effect with respect to a proxy for the cost of finding friends, the size of the local Filipino community. We test a theoretical prediction: the lower the cost of acquiring friends, the more likely information and friends are to be substitutes rather than complements. The heterogeneity in the treatment effect on the social network size index indeed follows this pattern, as does heterogeneity in the treatment effect on subjective wellbeing. While the treatment does not affect labor market outcomes such as wages or employment, it does change the way immigrants search for jobs. Immigrants who received employment-related information in the new PDOS are less likely to have found their job through social networks, which also suggests that information and networks are substitutes.

Our work contributes to the economics literature on social networks (Sacerdote, 2014; Chuang and Schechter, 2015). Ours is the first study to examine the causal impact of an exogenous reduction in information imperfections on social network links. Few studies examine factors influencing strategic network formation. Comola and Mendola (2015) and Barr, Dekker and Fafchamps

[3] The number of first-degree links is a measure of the expansiveness of the network. The literature on social networks has argued that network expansiveness is important for efficient information transmission (cf. Granovetter, 1973).

(2015) examine correlates of new network connections. Very few studies measure the causal impact of *any* kind of exogenous treatment on social networks. We are aware of only five other randomized controlled trials where social network connections are an outcome of interest, and in none of these does the randomized treatment relate to information. Three studies examine the impact of a microfinance treatment. Comola and Prina (forthcoming), Banerjee et al. (2018) and Cecchi, Duchoslav and Bulte (2016) find that savings, credit, and insurance interventions (respectively) reduce social network connections. Heß, Jaimovich and Schündeln (forthcoming) find that a community-driven development program in Gambia reduces social network connections. Caria, Franklin and Witte (2018) show that a job-search assistance intervention in Ethiopia reduces social interactions between treated and untreated individuals.

We also contribute to the literature on immigrant integration. A well-documented finding is that the economic assimilation of immigrants takes time and is usually imperfect. Especially in the first years after arrival, immigrants typically earn considerably less than natives (Borjas, 1985; Lubotsky, 2007). Identifying policies that facilitate the arrival and settling-in process of immigrants is therefore important and only few studies have rigorously evaluated policies that aim to improve the early integration path of immigrants (Rinne, 2013; National Academies of Sciences, Engineering, and Medicine, 2015).

This paper also contributes to studies showing how the intended impacts of social policies can be undone by behavioral responses of intended beneficiaries. Peltzman (1975) argues that the benefits of automotive safety regulations are offset by increases in risky driving, leading safety regulation to have no net impact on highway deaths. Filmer, Hammer and Pritchett (2000) highlight concerns that health gains from increases in public health provision could be attenuated if households respond by reducing private demand for health goods and services. We raise related concerns about offsetting behavioral responses to information interventions: beneficiaries of programs providing information may reduce their efforts to expand and acquire information from social networks, so that overall gains in wellbeing are attenuated.

In addition, we provide a new Stata command that adjusts p-values for multiple hypothesis testing. It modifies the List, Shaikh and Xu (2019) method to be regression-based and allow for inclusion of control variables.

From a policy standpoint, the intervention we study – provision of information to migrants about their destinations – is widespread.[4] Many governments and NGOs in developing countries implement trainings of migrants (IOM, 2011), but prior to our study there has been no causally well-identified assessment of their impacts (Rinne, 2013; McKenzie and Yang, 2015). More generally, our results suggest that the effectiveness of information interventions might be attenuated due to offsetting reductions in social network links.

2 Theory

We are interested in the interplay between information imperfections and efforts to increase first-degree social network links. In particular, we are interested in the impact of interventions alleviating information imperfections. We wrote down the following simple model after learning that our treatment had a negative impact on new social network connections, which is the opposite of what we had anticipated. The model allows for either a positive or negative treatment effect on efforts to acquire new social network connections.[5]

Individuals have imperfect information about a variety of things in life that matter to them, such as jobs, financial services, and government services. People also have social network connections ("friends", which includes acquaintances), which provide information, helping reduce information imperfections. Network theory suggests that efficient information gathering typically requires expansive networks with many short network paths (cf. Granovetter, 1973). Thus, we use the number of first-degree friends as a proxy for network expansiveness. Because friends are valuable, people make efforts to acquire them,

[4] Past research has also examined migrant integration programs carried out in destination countries (Joona and Nekby, 2012; Sarvimäki and Hämäläinen, 2016; Shrestha and Yang, 2019).

[5] This is related to models where individuals endogenously form social contacts (Calvó-Armengol, 2004; Jackson and Wolinsky, 1996; Jackson and Rogers, 2007; Herskovic and Ramos, 2020) and where socializing takes effort (Cabrales, Calvó-Armengol and Zenou, 2011; Canen, Jackson and Trebbi, 2019; Currarini, Jackson and Pin, 2009).

but making friends is costly. Costs of friend acquisition may include effort and monetary costs.

We focus on the benefits friends bring by reducing information imperfections. We abstract away from other benefits of friends, which the network literature typically refers to as *cooperation capital*, such as various forms of assistance (transfers, informal insurance, and psychological support).[6]

Utility depends on baseline or starting-point information imperfections, θ, and endogenous friend investment $f \geq 0$. Individuals choose f to maximize the benefits from friends $B(\theta, f)$ minus the cost of friend acquisition $C(f)$:

$$U = B(\theta, f) - C(f)$$

People acquire friends only up to the point at which the marginal cost does not exceed the marginal benefit of friends.

Simple assumptions and functional forms generate useful possibilities. Information imperfections θ range from 0 to 1 ($\theta \in [0, 1]$). Individuals have both exogenous friends (those that are given at baseline without cost), e, and endogenous friends, f, which they acquire at a cost. Let $e \geq 1$.[7] Let an individual's amount of information I be a function of information imperfections θ, exogenous friends e, and endogenous friends f as follows:

$$I = 1 - \frac{\theta}{e + f}$$

One's amount of information can range from 0 (no information) to 1 (full information). If baseline information imperfections θ are 0, then one starts with full information. A higher number of friends $e + f$ reduces the importance of one's baseline information imperfections and raises one's amount of information I.

For simplicity, let the cost of endogenous friends be linear with a per-friend cost c, so the total cost of friend acquisition is cf.

[6] These other non-information benefits of friends could be thought of as entering the cost term in the maximization problem we write down below, reducing the *net* cost of friends.

[7] For new immigrants, the exogenous friend could be the individual who officially sponsors their immigration visa.

We analyze three cases: constant, decreasing, and increasing returns to information. Analysis of the different cases makes clear that reductions in information imperfections have indeterminate impacts on friend acquisition.

When returns to information I (in utility) are either constant or decreasing, a reduction in information imperfections θ (e.g., our information treatment for new immigrants) always reduces friend acquisition. We flesh out this case in Appendix A.

By contrast, when there are increasing returns to information (for example, if better information allows one to search more efficiently for additional information), the impact of information imperfections is ambiguous. We capture increasing returns to information simply by letting the benefit function include a quadratic term in information:

$$B(\theta, f) = 1 - \frac{\theta}{e+f} + \alpha(1 - \frac{\theta}{e+f})^2$$

The parameter α measures the strength of increasing returns to information (if $\alpha = 0$, we have constant returns to information).

We analyze this case graphically in Figure 1. The parameter values used in the figure are $e = 1$ and $\alpha = 5$. The marginal benefit functions for the control and treatment groups are MB_C (green curve) and MB_T (orange curve), with $\theta = 0.9$ and $\theta = 0.6$ respectively. The marginal benefit functions can have upward-sloping (increasing returns) and downward-sloping (decreasing returns) sections.

Consider, first, optimal decisions when marginal costs are "high" ($c = 2.4$), represented by the upper horizontal black line, MC_H. The optimum is found at the intersection of the marginal cost function and the downward-sloping part of the relevant marginal benefit function.

When marginal costs are "high", for the control group (blue curve, MB_C) there is no amount of friend investments for which the marginal benefit of friends exceeds marginal costs. This is a corner solution with zero friend acquisition.

From this starting point, a reduction in θ (from 0.9 to 0.6) can lead the marginal benefit function to shift so that there is an interior solution with positive friend acquisition ($f^* > 0$), where MB_T and MC_H intersect. In this case, an information treatment that lowers θ leads to more friend acquisition.

There is also the possibility, for lower values of the cost of friends c, that reductions in θ reduce friend acquisition. This would be the case if the marginal cost of friends was lower, such as at MC_L(assuming $c = 1.2$), so that the marginal cost function (the horizontal dashed line) would intersect both the control group and treatment group marginal benefit functions on their downward-sloping portions. If this were the case, a reduction in θ would lead to a reduction in friend acquisition, from f' to f".

Overall, therefore, it is possible for an intervention that reduces information imperfections to either raise or lower efforts to build one's social network. (The prediction that the treatment effect on friends becomes more negative as marginal costs fall also holds for the case where returns to information are constant or decreasing.)

In Appendix A, we examine the impact of a treatment that reduces information imperfections for a continuous range of marginal cost levels, from high to low. Starting from the highest marginal costs, reductions in marginal costs make the treatment effect on friend acquisition even more positive, because this is the region where information and friends are complements. As we lower marginal costs further, we enter the region where friends and information are substitutes. The treatment effect on friends becomes negative, and becomes even more negative as marginal costs fall further.

The treatment effect on utility follows a similar pattern. Starting from the highest marginal costs, reductions in marginal costs make the treatment effect on utility even more positive, as long as one remains in the region where information and friends are complements. As we lower marginal costs further, friends and information become substitutes, the treatment effect on utility declines, and eventually can be even lower than when marginal costs were very high.

In our empirical analyses, we ask whether a treatment that reduces information imperfections reduces or increases friend acquisition. We also examine the heterogeneity in the treatment effect with respect to a proxy for the marginal cost of friend acquisition.

3 Context, Treatments, and Hypotheses

The Philippines is a major emigration country. In 2013, 4.8 million Filipino-born individuals were permanent migrants, 4.2 million temporary migrants, and 1.2 million undocumented migrants in other countries. By comparison, the Philippine population was 98.5 million in that year (CFO, 2013). The U.S. is by far their largest destination, accounting for 64.4% of Filipino permanent migrants in 2015 (CFO, 2015). From the U.S. standpoint, the Philippines is the fourth-largest immigrant origin, after Mexico, China and India (López, Ruiz and Patten, 2017).

The Philippine government implements a number of policies related to international migration of its citizens. Our collaborator on this study, the Commission on Filipinos Overseas (CFO), enacts policies related to permanent migrants. Pre-departure orientation seminars (PDOS) are one of the government's most prominent migration policies. Filipinos intending to leave the country with a permanent migration visa must register with CFO and attend a PDOS before departure. Attendees already have their immigration visa and are about to leave the Philippines. Individuals lacking proof of PDOS attendance may be denied departure at airports. Seminar content is tailored to the destination. We recruited our study participants among individuals attending the PDOS for permanent migrants to the U.S., which were attended annually by roughly 40,000 individuals from 2005-2015 (CFO, 2015).

The migration policies of the Philippines are regarded as a model for other migrant-sending countries that have PDOS in place or are considering introducing them (Testaverde et al., 2017). As a major destination country, Canada also provides a PDOS for migrants moving to Canada known as *Canadian Orientation Abroad*.

Treatments

Figure 2 shows the treatment conditions. We randomly assign study participants to either a control group attending the original PDOS ("old PDOS") or to a treatment group attending the "new PDOS". The old PDOS focused on travel and immigration procedures, only briefly covering issues such as cultural differences, settlement, and employment, and not covering financial literacy or engagement with Filipino associations. An instructor conveyed the information in a presentation lasting 1.5 hours on average. Participants took away with them a short 30-page paper booklet with related but not very practical information.

The new PDOS was developed collaboratively by the CFO and our research team from scratch and goes significantly beyond the content of the old PDOS in terms of both topics and depth of coverage. It comes with a much more comprehensive and practical paper handbook. New PDOS development drew upon interviews with past and prospective migrants, the International Organization for Migration's Canadian Orientation Abroad program, and input from TIGRA, a U.S. Filipino immigrant NGO. The new PDOS covered an extended set of topics related to longer-term socio-economic integration: (i) preparing for departure and entering the U.S., (ii) getting settled in the U.S., (iii) building a support network, (iv) finding a job, (v) managing one's finances, and (vi) maintaining and strengthening ties with the Philippines. Participants attended a longer presentation (2.5 hours on average) and took away a comprehensive 116-page paper handbook, which covers the above topics in detail and provides easy-to-follow checklists as well as links to online resources.

Compared to the old PDOS, the new PDOS shifts the focus from topic (i) to topics (ii)-(vi). Figure 3 documents this shift in focus. It shows the number of slides and handbook pages of the old and the new PDOS by topic. In addition, the delivery of the new PDOS centers around the handbook. During the PDOS, the instructor provides an overview of the topics covered by the handbook and shows migrants where to find which information. The primary objective of the new PDOS is hence to improve migrants' ability to find information, rather than their knowledge of different topics. This makes the handbook an important part of the new PDOS as it gives migrants the possibility to look up information when they actually need it. While the old PDOS provides written information in the form of a booklet, the handbook of the new PDOS offers much richer and practical information. Figures B.3 and B.4 in Appendix B illustrate this difference in terms of both quantity and quality for information provided on opening a bank account.

Our primary analyses compare control group individuals to treatment group individuals exposed to the new PDOS. We implemented the new PDOS in two different versions. One version contained all components listed above (henceforth "new PDOS with employment module"), another version omitted the employment section from both the presentation and handbook ("new PDOS without employment module"). The distinction allows us to measure the specific impact of topic area (iv) on employment, as most migrants in the preparatory interviews identified finding a job in the U.S. as the single most important challenge after arrival.

Among migrants who attended the new PDOS, we also randomly assigned an intervention ("association email") aimed at facilitating social network connections in the U.S. CFO sent emails (at one and two months after arrival in the U.S.) to randomly selected new PDOS study participants encouraging them join Filipino associations, providing contact details of associations in the migrant's U.S. state. The email could have reduced the cost of network formation and should therefore expand the social network. Appendix B shows an example of the association email for migrants moving to Northern California.

All material used in the different treatment conditions including the presentation slides and handbooks can be downloaded at https://sites.google.com/view/tomanbarsbai/pdos.

Random Assignment

To identify causal effects, we randomly assigned migrants to the different treatment conditions (Figure 2). We randomized PDOS versions across 112 calendar dates. From April 21 to October 3, 2014, the PDOS session of each calendar date was randomly assigned to either the new or old PDOS. Out of five weekly working days, two were randomly assigned to the old PDOS, and three to the new PDOS. New PDOS sessions were then randomly assigned to having the employment module (or not) with equal probability. The association email was separately randomly assigned at the individual level to study participants in the new PDOS who had a valid email address and were migrating to a state with a CFO-approved association.

On April 1, 2014, we randomized the PDOS dates and informed CFO leadership of the treatment schedule. Our staff confirmed by direct, in-person observation on each date that instructors implemented the treatments correctly. We randomized the association email on a rolling basis, twice a month as additional batches of study participants were enrolled. CFO sent new batches of emails twice a month to study participants on lists we provided with 2-3 days' advance notice. For further details on treatment implementation, see Appendix B.

Sampling and Survey Data Collection

Enrollment of study participants took place at CFO's Manila PDOS location. Immediately prior to the start of a PDOS, study staff approached prospective migrants, inviting them to participate in the study. Screening criteria were: 1) being 20-50 years of age on the enrollment date, 2) not ever having lived in the U.S. for longer than three months, 3) planning to depart for the U.S. within three months, and 4) not migrating to the U.S. as a spouse of a non-Filipino (marriage migrants), as such migrants attend a cross-cultural marriage counseling session rather than a PDOS. No more than one member per family was enrolled in the study. Screened-in individuals were invited to participate in the study, including permission to contact them and their Philippines-based families for future surveys. In total, enumerators approached 2,639 migrants, out of which they successfully interviewed 1,273 migrants who met the screening criteria (or about eleven migrants per PDOS date). 1,042 migrants did not meet the screening criteria and 324 migrants refused to be interviewed before screening. The refusal rate is hence relatively low (324/2639 = 12%).

Individual study participants themselves chose the date they would show up for a PDOS (no appointments were necessary), but could not know in advance the type of PDOS they would be exposed to. Prior to the start of the PDOS on that date, enrolled migrants were administered a baseline survey on the spot by our survey staff. Migrants are on average 33 years old. 55% are female. 47% have college or more education. 18% have a job waiting for them in the U.S. Half migrated alone, and the remainder migrated with family members. California (41%) and Hawaii (17%) were the two most important destination states. The vast majority of study participants (93.5%) obtained their U.S. green cards via family sponsorship, i.e. they have family already in the U.S.[8]

Balance checks reveal no major differences between observable characteristics of study participants across treatment conditions. For balance tests and summary statistics, see Appendix E, Tables E.1-E.3. Out of ten baseline variables, only one (indicator for female) is statistically significantly related to treatment status. This is approximately what would be expected to occur by chance. These baseline characteristics are also included as controls in all regressions (as pre-specified).

Analyses of treatment effects use data from follow-up phone interviews of migrants and direct interviews with their Philippine households at about seven, 15, and 30 months after arrival in the U.S. For further details on survey implementation, see Appendix B.

Pre-Analysis Plan

This study is registered with the AEA RCT Registry.[9] We submitted our first pre-analysis plan (PAP) on September 17, 2014 before completion of the baseline phase and availability of any post-treatment data. We submitted subsequent PAPs to guide analysis of the mid-term survey data (submitted July 19, 2015) and final survey data (submitted July 28, 2016). These latter two PAPs add additional hypotheses related to employment and network characteristics.

[8] Of the 6.5% of study participants not reporting family sponsorship, about two-thirds report obtaining their green cards through an employer, and the remainder do not clearly specify the nature of their sponsor.

[9] https://www.socialscienceregistry.org/trials/1389/

For simplicity, all analysis in this paper will be based on the first PAP of September 2014, the only PAP that was submitted before the collection of any outcome data. Analyses based on subsequent PAPs are provided in Appendix E. All conclusions are robust to estimating longer-run impacts using methods from longer-run PAPs.

In a few ways, we deviate from the pre-analysis plan. Most importantly, we correct test statistics to address multiple hypothesis concerns, following List, Shaikh and Xu (2019). We had not pre-specified that we would do this for tests on the main outcome domains. Our inferences are therefore (correctly) more conservative. Also more conservatively than the PAP, we report standard errors clustered by PDOS date, rather than unclustered robust standard errors.

In addition, we did not anticipate large outliers in the number of new friends and acquaintances outcome variable in later survey waves. In the longer-term surveys, this variable has a mean of 67, a median of 40, a minimum of 0, 90^{th} percentile of 120, and a maximum of 2,500. In retrospect, such numbers may reflect the fact that some study participants are reporting "weak" social network links as well as stronger connections (Granovetter, 1973). In the PAP, we said we would examine the simple count of new friends and acquaintances. Instead, to reduce the influence of these unexpected outliers, we take the inverse hyperbolic sine (IHS) transformation (Bellemare and Wichman, 2019). Results are robust to alternate approaches, as discussed below.

Outcomes and Hypotheses

We examine outcomes and hypotheses as specified in our pre-analysis plan. We are interested in outcomes in several domains. In each domain, we construct an aggregate index or a standardized treatment effect (STE). When we construct a STE, we follow Kling, Liebman and Katz (2007).[10] Details on the

[10] We normalize each outcome by subtracting the mean of the control group and dividing by the control group standard deviation. Let Y_k be the k^{th} of K outcomes of a given outcome domain, μ_k be the control group mean and σ_k the control group standard deviation. The normalized outcome is $Y_k^* = (Y_k - \mu_k)/\sigma_k$. The summary index is $Y^* = \sum_K Y_k^*/K$. We reverse the sign for adverse outcomes, so that higher values indicate more beneficial outcomes. Treatment effect estimates based on the STE quantify the difference between

construction of indices are in Appendix C.

Our pre-specified hypotheses are as follows.

Hypothesis 1: Treatment reduces **problems experienced during travel to the U.S.** (Fraction of the following travel problems experienced: missed a flight, overweight luggage, problems with immigration authorities.)

Hypothesis 2: Treatment leads to faster completion of administrative matters related to **settlement in the U.S.** (Fraction of the following obtained: Social Security number, health insurance, driver's license, bank account.)

Hypothesis 3A: Treatment improves **employment outcomes in the U.S**. (STE of the following: indicator for having paid employment, IHS of monthly income, expected probability of having a job in 9 months, expected probability of having a job that corresponds to one's educational level.)

Hypothesis 3B: The new PDOS with employment module treatment has larger positive effects on employment outcomes than the new PDOS without employment module treatment. (Outcome same as Hypothesis 3A.)

Hypothesis 4A: Treatment leads to increases in new **social network connections in the U.S.** (STE of the following: number of new friends and acquaintances, indicator for having received support from a Filipino club or organization in the U.S.)

Hypothesis 4B: The new PDOS with association email treatment has more positive effects on **social network in the U.S.** than the new PDOS without association email treatment. (Outcome same as Hypothesis 4A.)

means in the treatment and control groups in standard deviation units.

Hypothesis 5: Treatment improves individual **wellbeing**. (STE of the following: mental health index [sum of scores on five questions], migrant-specific wellbeing [sum of scores on two questions].)

To reiterate, Hypothesis 4A – that the treatment increases new social network connections – reflects our initial expectation before we had seen our empirical results. We originally expected the treatment to increase new social network connections because the new PDOS explicitly encourages migrants to reach out and build a support network in the U.S.

4 Empirical Analyses

We use the following regression specification to estimate treatment effects on outcome Y_i:

$$Y_i = \alpha + \beta T_i + X_i' \theta + \varepsilon_i \tag{1}$$

T_i is an indicator for attending any new PDOS. X_i is a vector of pre-specified baseline controls, which improve precision and help address chance imbalances (including age, age squared, gender, level of education, log days since arrival in the U.S., an indicator for migrating alone, indicators for migrating to Hawaii and California, indicator for daily internet use, self-assessed English skills, indicator for having a U.S. job prior to departure, and an indicator that the outcome was reported in a proxy interview). For each outcome domain, we also pre-specified that we would include controls relevant to the specific domain.[11] Standard errors are clustered at the level of 112 daily PDOS sessions.

β is the causal effect of treatment. This treatment effect is the average effect of the different sub-treatments, and will be the basis for testing Hypotheses 1, 2, 3A, 4A, and 5.

By direct observation, we confirmed perfect adherence to treatment assignment (attendance at the assigned PDOS, and receipt of the corresponding handbook). β therefore captures the average treatment effect (ATE). In our

[11] For example, the regression for the network size index includes baseline controls for knowing a Filipino association in the U.S., wanting to join a Filipino association in the U.S., and wanting to join other clubs/associations in the U.S. See the PAP for complete details.

case, the ATE is equivalent to the average treatment effect on the treated (ATT) for migrants satisfying our screening criteria.

We use the following regression specification to estimate the differential effect of the new PDOS with employment module:

$$Y_i = \alpha + \gamma T_i + \delta T_{Emp_i} + X_i^{'}\theta + \varepsilon_i \tag{2}$$

This regression equation modifies equation (1) by adding δT_{Emp_i}, an indicator for being assigned to the new PDOS with employment module. The coefficient γ is the treatment effect of the new PDOS *without* the employment module, and the coefficient δ is the incremental impact of adding the employment module to the new PDOS. The total effect of the new PDOS with the employment module (compared to the control group) is $\gamma + \delta$. The coefficient δ will be the basis for testing Hypothesis 3B.

In addition, we estimate the following regression specification to determine the differential effect of the new PDOS with the association email:

$$Y_i = \alpha + \phi T_i + \lambda T_{Assoc_i} + X_i^{'}\theta + \varepsilon_i \tag{3}$$

Compared to equation (1), this equation adds T_{Assoc_i}, an indicator for assignment to the new PDOS with association email treatment. The coefficient ϕ is the treatment effect of the new PDOS *without* the association email, and the coefficient λ is the incremental impact of adding the association email to the new PDOS. The total effect of the new PDOS with the association email, compared to the control group, is $\phi + \lambda$. The test of Hypothesis 4B refers to the coefficient λ.

Multiple Hypothesis Corrections

We examine multiple hypotheses. To conduct correct statistical inference, we follow Finkelstein et al. (2010) and Almeida et al. (2014). As discussed above, we construct indices for different outcome domains. We provide details on the construction of indices in Appendix C. Then, across regressions for the different outcome domains, we build on the method of List, Shaikh and Xu (2019) to correct for multiple hypotheses, and report the resulting p-value adjusted for the familywise error rate on the treatment coefficient for each domain. We modified the List, Shaikh and Xu (2019) method to be regression-based and allow for inclusion of control variables. We provide details on the modifications of the procedure, simulations, and access to our Stata command `mhtreg` in Appendix D.

Attrition

Attrition over time was a key challenge as the entire migrant sample moved from the Philippines to the U.S. and changed their contact details between the baseline and follow-up interviews. To minimize attrition, we asked study participants to provide contact information for the household in the Philippines they would remain most closely connected to after their departure, which we then also surveyed. We also fully informed migrants of expectations of multiple follow-up surveys at time of consent and provided financial incentives for completed surveys. We regularly updated and intensively used contact data of multiple types (phone, email, Skype, and social media) and solicited household assistance in contacting migrants if necessary. We used Philippine-household proxy reports on migrant outcomes if migrants could not be surveyed. Proxy reports account for about 40 percent of the outcomes collected in the short-term survey and 50 percent in the long-term survey.[12]

Our re-interview rates reach 87 percent in the short-term survey and 61 percent in the long-term survey. These success rates are comparable to those of other studies that survey and track migrants from their origin to their destination countries. Ambler (2015) successfully tracked 73 percent of migrants from El Salvador to Washington DC, Ashraf et al. (2015) 57 percent of migrants from El Salvador to Washington DC, Shrestha and Yang (2019) 60 percent of Filipino maids to Singapore, and Gibson et al. (2019) 64 percent of migrants from Tonga to New Zealand.

[12] Our results hold when we restrict the analysis to directly reported data from migrants, which might be more reliable (Baseler, 2020). See Appendix Tables E.8 and E.18.

We examine a range of potential attrition problems. A crucial question is whether attrition from the follow-up survey sample is related to treatment status. If so, concerns arise about selection bias in treatment effect estimates. We do not find that attrition is related to treatment status in different survey rounds (Appendix Tables E.5 and E.15). Because attrition is specific to given outcome measures, we also examine this outcome by outcome (Appendix Tables E.6 and E.16).[13] Again, this analysis raises no concerns. Likewise, treatment status cannot explain whether an interview is conducted directly with the migrant or indirectly with a family member in the Philippines via a proxy survey (Appendix Tables E.7 and E.17). Across the large number of tests where we check whether treatment predicts attrition, in only very few cases are coefficients statistically significant at conventional levels, no more than would be expected to occur by chance.

Throughout, baseline characteristics have little power to predict re-interview status (attrition or proxy survey status). The R-squared of the corresponding regressions is low (<0.03) suggesting that baseline characteristics do not systematically correlate with re-interview status. There is no indication that our sample loses specific types of migrants over time.

Validating the Measures of Social Network Connections

As pre-specified, we measure social network size with an index combining information on the number of new friends and acquaintances and contact with Filipino organizations. To validate the network size index as a meaningful economic variable, we examine the correlation between the network size index and our key other outcomes, the settlement, employment, and wellbeing indices.

[13] Attrition varies across different outcomes, depending on a number of factors: (i) whether an interview was conducted as a direct interview with the migrant or a proxy interview with a family member (as some outcomes could not be collected in proxy interviews), (ii) whether a family member was knowledgeable on a given outcome (as the share of "don't know"-responses was considerable higher in proxy interviews), (iii) the common number of observations for the individual indicators used to build aggregate indices, (iv) whether we analyze the new PDOS with association email (as the email could only be randomized among the subset of those with a valid email address migrating to a state with a CFO-approved association).

Using data from the long-term survey, we regress the other outcome indices on the network size index. Coefficients on the network size index presented in Appendix Table E.4 reveal that there is a positive and statistically significant relationship between the network size index, on the one hand, and the settlement and employment indices, on the other. A one standard deviation increase in the network size index is associated with a 0.06 standard deviation increase in the settlement index and a 0.14 standard deviation increase in the employment index. The association between the network size index and the subjective wellbeing index is also positive, but not at conventional levels of statistical significance. Coefficients are similar in the full sample, and in regressions run separately in the control and treatment groups. While the correlations between the network size index and these other indices do not necessarily represent causal effects, they do increase confidence that the variation in our network index is economically meaningful and not simply noise.

5 Results

Table 1 presents regression results for our primary hypothesis tests, using data from the short-term survey. Panel A presents coefficients from Equation (1) on the indicator for receiving the new PDOS (either version) for the five outcome indices, testing Hypotheses 1, 2, 3A, 4A, and 5. The treatment leads to substantial reductions in the number of travel related problems (column 1), with multiple-hypothesis-corrected p-value 0.30. This result points to the importance of the enhanced handbook as the new PDOS featured considerably less travel-related content than the old PDOS. The treatment also leads to a lower network size index (column 4). This is the sole outcome that is statistically significant after multiple-hypothesis correction (p-value 0.03). The coefficients on the treatment indicator in regressions for the other outcomes are small in magnitude, and none are statistically significantly different from zero.

Panel B presents coefficients from estimating Equation (2) on the employment index for receiving the new PDOS (either version) and the new PDOS with employment module. The latter coefficient, testing Hypothesis 3B, is negative but not statistically significant at conventional levels.

Panel C presents coefficients from estimating Equation (3) on the network size index for receiving the new PDOS (either version) and the new PDOS with association email. The latter coefficient, testing Hypothesis 4B, is not precisely estimated. But the economically meaningful positive coefficient is consistent with the email reducing the cost of acquiring social network connections. In this regression, the coefficient on the indicator for new PDOS (either version) is interpreted as the effect of receiving the new PDOS without the association email. This coefficient is negative, large in magnitude, and statistically significant after multiple-hypothesis correction (p-value 0.05).

Using data from the long-term survey, Table 2 presents similar regression results. (The travel-related problems regression is excluded; it was pre-specified only as a short-term outcome.) As pre-specified in the long-term PAP, we replace a missing long-term value with the mid-term or short-term value, in that order. Because observations missing from the short-term survey may be found in a later survey, the samples in Table 2 have higher sample sizes (lower attrition) than Table 1.

Table 2's results are very similar to Table 1's. In Panel A, of the four outcome areas, the treatment has a statistically significant impact on only the network size index; the multiple-hypothesis-corrected p-value is 0.07. In Panels B and C, neither the coefficient on the new PDOS with employment module nor that on the new PDOS with association email are statistically significantly different from zero. In Panel C of Table 2, as in the corresponding panel of Table 1, the coefficient on the indicator for new PDOS (either version) is negative, large in magnitude, and statistically significant after multiple-hypothesis correction (p-value 0.03).

The stability of the findings in Table 2's expanded sample and longer time frame provides an indication of the robustness of the empirical findings. In Appendix Tables E.8 and E.18, we also show that our results hold when we exclude proxy reports from household members and restrict the analysis to directly reported data from migrants.

These results suggest that better-informed immigrants invest less in developing social networks in their new societies, thus attenuating the overall gains from the new information. From the perspective of our simple theoretical model, information and social network links are substitutes. The suggestive evidence in favor of fewer travel-related problems and no treatment effects on settlement, employment, and wellbeing is consistent with this interpretation. The new PDOS could affect migrants' travel experience before they had formed networks in the U.S. In contrast to post-arrival outcomes, endogenous reductions in social network connections could hence not attenuate the effects on travel-related problems.Appendix Table E.10 shows short-term treatment effects on the component variables of the network size index. The treatment has negative effects on both components. Treatment causes the number of friends to fall by 28%,[14] the rate of receiving support from associations to fall by 3.2 percentage points (control mean 4.9%). It also lowers the rate of contacting an association by 5 percentage points (control mean 12.3%).

Short-term results are robust to different ways of dealing with outliers in the friends variable (including doing nothing). This is true for the long-term results as well, except when we do not deal with outliers at all (using the raw count of friends for which later survey waves include extreme values); in this case, the treatment effect on the number of friends is close to zero with standard errors nine times larger than in the short-run (Appendix Table E.19). We also show robustness to defining the network measure as specified in the long-term PAP (Appendix Table E.23).

Density plots of the number of friends provide an alternate view of the treatment effects. Figure 4 presents probability density functions of the number of friends for the control group (old PDOS) and the treatment group (new PDOS, any version). The PDF for the treatment group lies to the left of the control group's PDF. The PDF of the treatment group has substantially greater probability mass under 30 friends, and less mass above 30 friends.

The treatment might induce migrants to invest in fewer, but different types of social network connections. In the long-run PAP, we distinguish between Filipino and non-Filipino friends and acquaintances as well as close friends (we

[14] We use the method of (Bellemare and Wichman, 2019) to convert IHS coefficients into percentage changes.

did not collect these outcomes in the short-term survey). Appendix Table E.24 shows that the new PDOS particularly reduces the number of Filipino friends and acquaintances and close friends. The effect is negative for non-Filipino friends, but not statistically significant. In addition, we do not find that the new PDOS affects other network characteristics (Appendix Table E.25). The corresponding index is defined as a STE that summarizes whether the two closest new contacts in the U.S. have a college degree or higher and whether they are of non-Filipino ethnicity, whether the migrant has visited people of U.S. origin in their home, whether the migrant has received visitors of U.S. origin, and how often the migrant has received everyday favors from non-Filipino individuals. The new PDOS has no effect on the index or any of its components. Overall, our results suggest a reduction in the number of network links across the board with few changes in the type of links.

However, in exploratory and not pre-specified analyses, we find evidence that the new PDOS affects whether migrants use social networks to find a job. Overall, as the first three columns of Table 3 show, none of our treatments has a significant effect on migrants' propensity to have a job. Yet, migrants who attended the new PDOS with employment module are 7.8 percentage points (control mean 70.2%) less likely to have found their current job through social networks (column 5). This finding potentially reflects that the employment module significantly improves migrants' job-search knowledge (see column 2 of Appendix Table E.14), which reduces their reliance on social networks. By contrast, migrants who received the association email, which explicitly encourages them to expand their social network to find a job, are 9.6 percentage points more likely to have found a job through social networks (column 6). The opposing effects of the sub-treatments explain why the overall treatment effect of the new PDOS on having found a job through social networks is close to zero and not statistically significant (column 4).

In additional exploratory and not pre-specified analyses, we test a theoretical possibility highlighted above in Section 2. When friend-acquisition costs are lower, information and friends are more likely to be substitutes. In this case, the treatment leads to greater reduction in friends and has a less positive impact on wellbeing because utility gains from better treatment-provided information are offset by reductions in friend-provided information. When friend-acquisition costs are higher, information and friends are more likely to be complements. In this case, the treatment may lead to an increase in friend acquisition, as well as an increase in well-being because utility gains from friend-provided information are added to gains from treatment-provided information.

We estimate Equation (1) when including an interaction term between treatment and a proxy for lower friend-acquisition costs: the number of Filipino-born individuals in one's county of destination (in inverse hyperbolic sine transformation and demeaned).[15] The main effect of number of Filipinos is also included in the regression. The results, in Panel D, Table 2, are consistent with the prediction. The treatment causes friend acquisition, and wellbeing, to fall more in counties with more Filipinos.

There is no corresponding heterogeneity in regressions for the settlement and employment indices. This may reflect that there are factors important for overall wellbeing that are not related to, or well-measured by, our rather coarse settlement or employment indices. For example, immigrants with better information may have lower stress levels, perhaps because they feel more confident in their ability to respond to unexpected future shocks or changes in circumstances.

These patterns also reveal themselves in the nonparametric estimation of Figure 5. In the figure we plot on the vertical axis a nonparametric regression estimate of the treatment effect of the new PDOS (any version) for study participants in destination counties with different-sized Filipino populations (horizontal axis). The nonparametric estimate uses a Gaussian kernel. We show 90% confidence intervals of the nonparametric regression estimate, based

[15] One might worry that the number of Filipinos in the destination is endogenous to treatment. We therefore use the U.S. destination county stated by the study participant in their baseline interview, ignoring any subsequent moves. The original U.S. destination county is often determined by the location of the immigrant's visa sponsor, so is more plausibly exogenous. We also find no empirical evidence that the number of Filipinos in one's destination county is endogenous to treatment. When estimating equation 1 with the inverse hyperbolic sine of number of Filipinos in the destination county as the dependent variable, the coefficient on treatment is small in magnitude and is not statistically significantly different from zero.

on 200 bootstrap replications. To give a sense of ranges of the horizontal axis accounting for more of our study population, we also present the density in our study sample of the inverse hyperbolic sine of the number of Filipinos in their destination county (the light gray solid line). The figure suggests that in counties with the fewest Filipinos (those below the 15th percentile, or a value on the horizontal axis of 6), the impact of the treatment on the social network size index is zero, and the impact on wellbeing is positive.

6 Conclusion

We study an intervention that provides immigrants with information about their new societies, with the aim of facilitating settlement and improving their socioeconomic outcomes. We find that when new immigrants are better-informed, they acquire fewer new social network connections. At the same time, we find no evidence of positive impacts of the information intervention on immigrant settlement, employment, or overall well-being. In the context of a simple model, these findings suggest that information and social network connections are substitutes. Exogenously-provided information (such as from an information intervention) may be beneficial in itself, but its impact on overall well-being may be attenuated if beneficiaries respond to the information provided by reducing their acquisition of information from social networks.

The intervention we study is widespread and important in and of itself. Many national governments and NGOs seek to provide information to migrants and other populations more broadly. Thus, the results may also be relevant for understanding the impacts of other interventions that involve provision of information, such as financial education or health information programs. The empirical record of the effectiveness of such programs is mixed (Kaiser and Menkhoff 2017, Fernandes, Lynch Jr and Netemeyer 2014). In future research, it will be important to examine whether information interventions in other contexts also lead to offsetting reductions in social networks, thus attenuating the overall gains from these interventions.

We do find evidence that the impact of the information intervention we study is heterogeneous in our study population. The intervention has less negative effects on social network connections, and positive effects on well-being, for those in localities with relatively few prior immigrant co-nationals. This could be due to the fact that acquisition of social network connections is costlier in such localities. From the standpoint of the theoretical model, the higher the cost of acquiring social network connections, the less likely it is that information and social network connections are substitutes, and the more positive can be the impact of the information intervention on well-being. This finding has a policy implication: information interventions may have the highest positive impacts on the well-being of beneficiaries – and therefore should be considered more seriously – in situations where beneficiaries have high costs of acquiring new (or maintaining pre-existing) social network connections (e.g., immigrants arriving in locations with relatively few prior immigrant compatriots).

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Figures and Tables

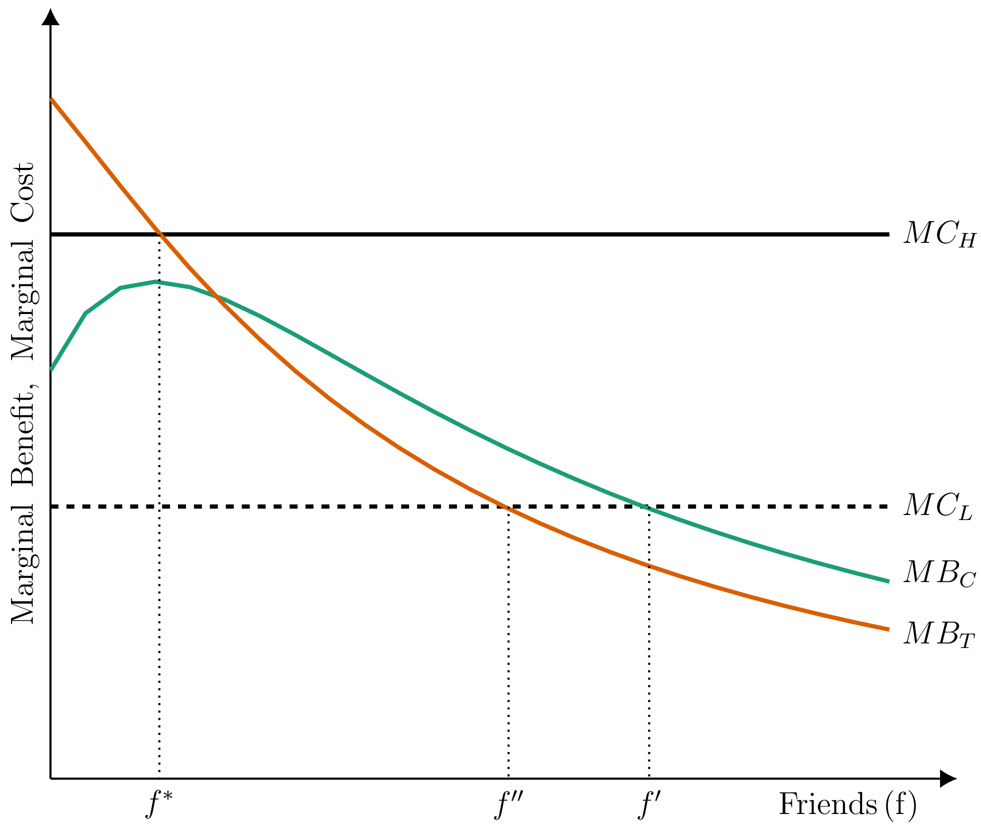

Figure 1: Increasing returns to information

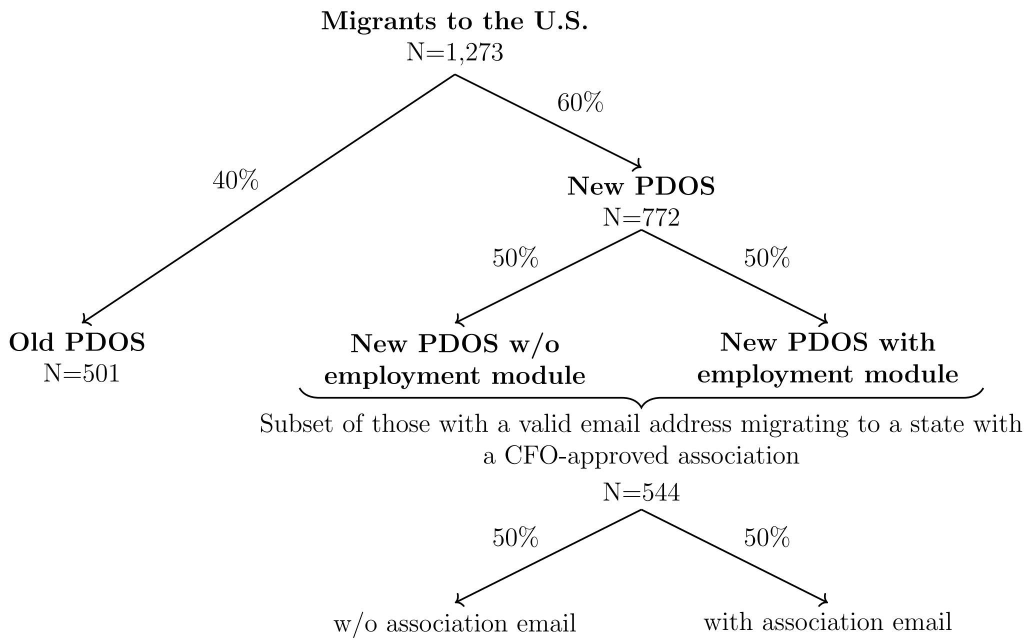

Figure 2: Treatment conditions

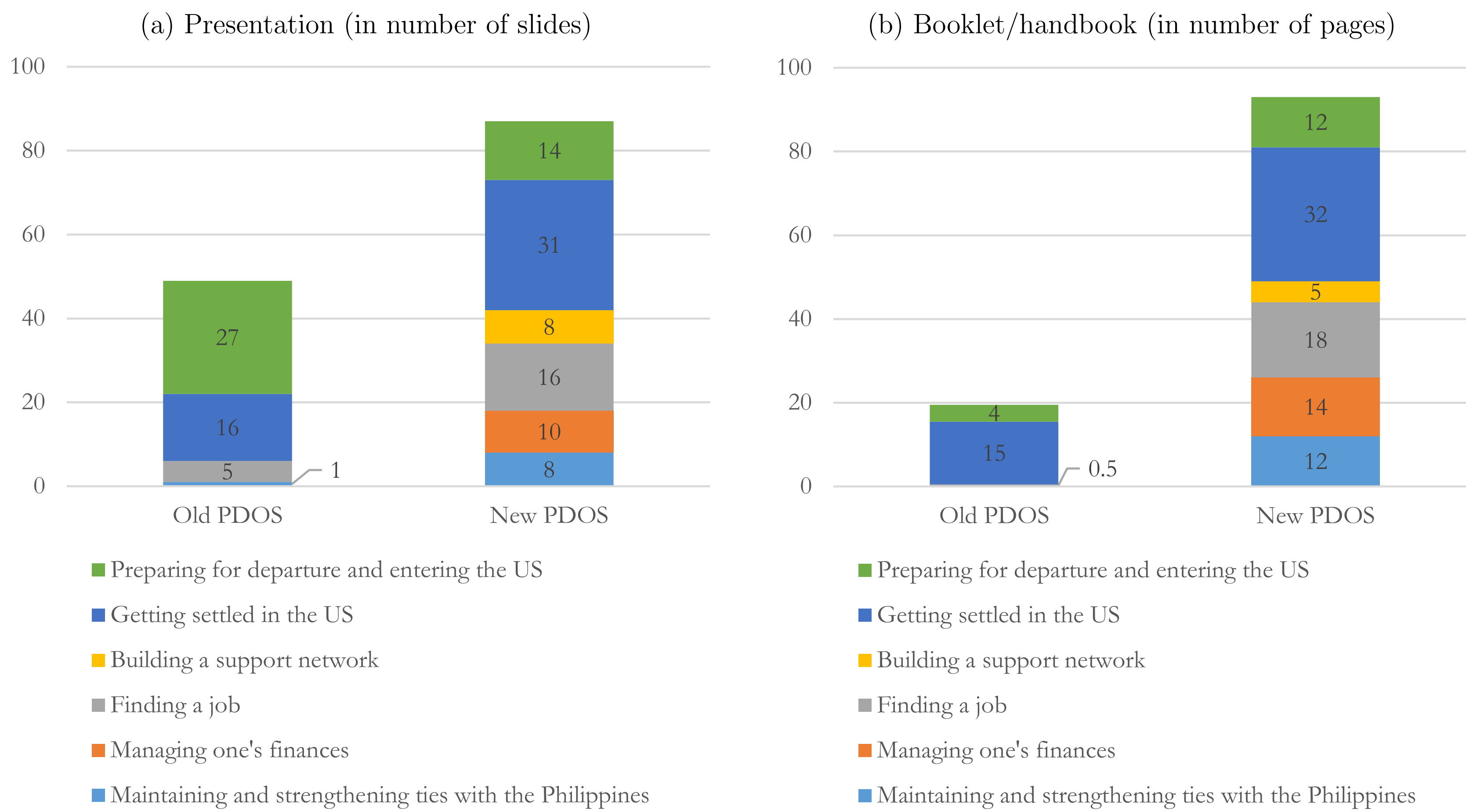

Figure 3: Differences in coverage of topics between the old and new PDOS (in terms of quantity)

Note: The number of pages in the handbook is not directly comparable as the format of the old PDOS handbook is much smaller (it is a booklet). The old PDOS does not cover “Building a support network” and “Managing one’s finances”. The presentation of the old PDOS has one slide on “Maintaining and strengthening ties with the Philippines” and the booklet half a page on “Finding a job”. Compared to the old PDOS, the new PDOS shifts the focus from “Preparing for departure and entering the U.S.” to the other topics, which are more relevant for longer-term socio-economic integration. **Another major difference is that the new PDOS offers much richer and practical information. Figures B.3 and B.4 in Appendix B illustrate this difference in terms of both quantity and quality for information provided on opening a bank account.** All presentation slides and handbooks can be downloaded at https://sites.google.com/view/tomanbarsbai/pdos.

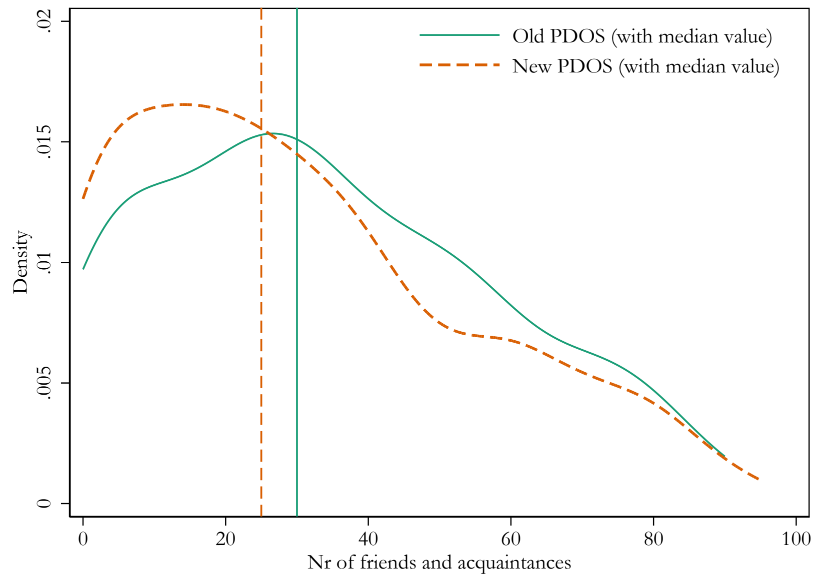

Figure 4: Density plot of number of friends after 30 months in the U.S. by treatment status
Note: Number of friends is from long-term survey. Missing data replaced with value from mid-term survey or short-term survey (in that order).

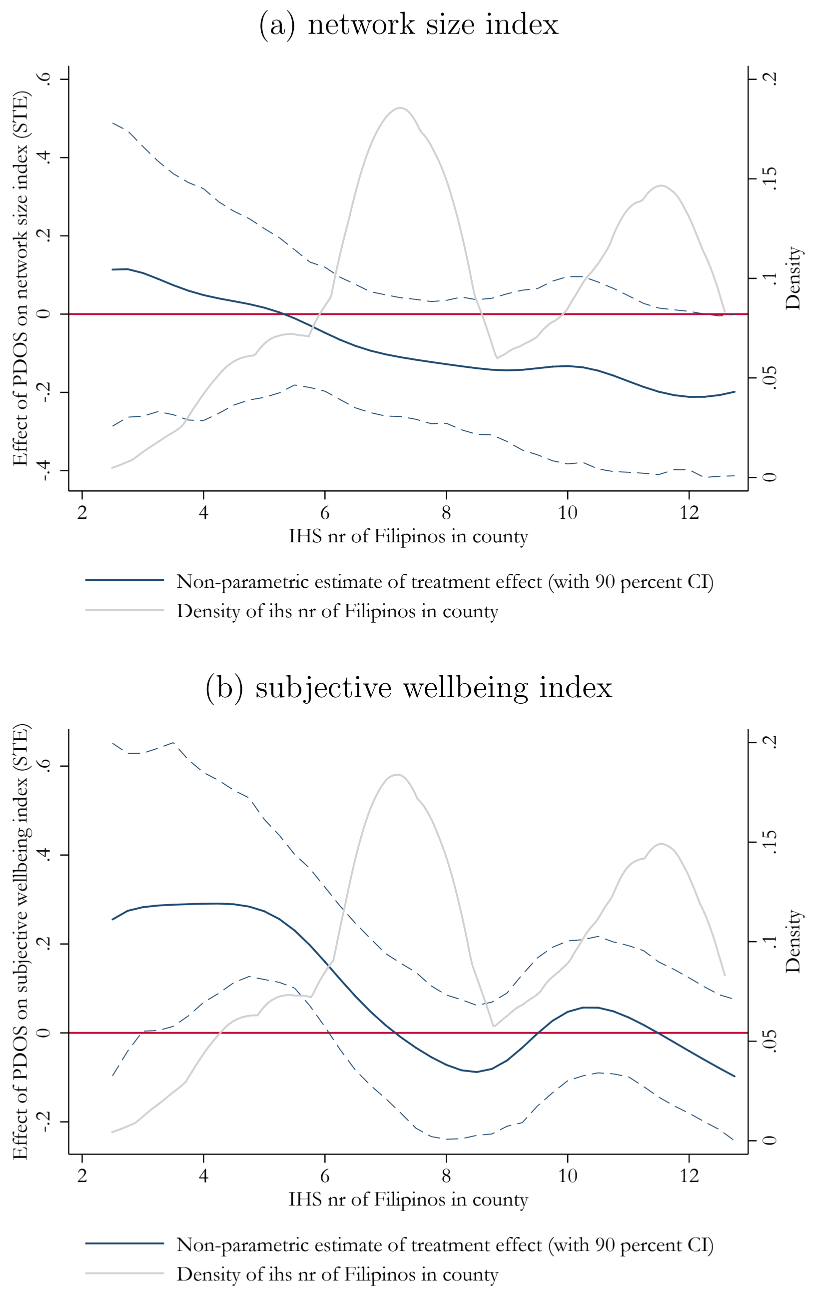

Figure 5: Nonparametric treatment effects of PDOS on network size index and subjective wellbeing index by size of Filipino community
Note: Gaussian kernel. Bootstrapped standard errors with 200 replications.

Table 1: Short-term effects (after about seven months in the U.S.)

	(1) Travel-related problems (0-1)	(2) Settlement index (0-1)	(3) Employment index (STE)	(4) Network index (STE)	(5) Subjective wellbeing index (STE)
PANEL A					
New PDOS (either version)	-0.012	0.028	-0.012	-0.169	-0.020
	(0.006)	(0.017)	(0.070)	(0.056)	(0.076)
MHT-adjusted p-value	0.300	0.435	0.864	0.029	0.987
Mean outcome control group	0.020	0.590	-0.000	0.000	0.000
R2	0.021	0.223	0.130	0.166	0.072
Observations	1077	728	362	614	578
PANEL B					
New PDOS (either version)			0.016		
			(0.090)		
New PDOS with emp. module			-0.053		
			(0.095)		
MHT-adjusted p-value treatment			0.967		
MHT-adjusted p-value interacted treatment			0.939		
R2			0.130		
Observations			362		
PANEL C					
New PDOS (either version)				-0.223	
				(0.078)	
New PDOS with ass. email				0.092	
				(0.077)	
MHT-adjusted p-value treatment				0.052	
MHT-adjusted p-value interacted treatment				0.698	
R2				0.165	
Observations				436	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table 2: Long-term effects (after about 30 months in the U.S.)

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Subjective wellbeing index (STE)
PANEL A				
New PDOS (either version)	-0.009	-0.065	-0.136	0.035
	(0.016)	(0.087)	(0.053)	(0.049)
MHT-adjusted p-value treatment	0.918	0.916	0.072	0.920
Mean outcome control group	0.797	-0.027	-0.067	-0.009
R2	0.234	0.134	0.108	0.032
Observations	989	601	751	917
PANEL B				
New PDOS (either version)		-0.050		
		(0.098)		
New PDOS with emp. module		-0.028		
		(0.088)		
MHT-adjusted p-value treatment		0.830		
MHT-adjusted p-value interacted treatment		0.751		
R2		0.135		
Observations		601		
PANEL C				
New PDOS (either version)			-0.238	
			(0.080)	
New PDOS with ass. email			0.095	
			(0.079)	
MHT-adjusted p-value treatment			0.032	
MHT-adjusted p-value interacted treatment			0.726	
R2			0.139	
Observations			533	
PANEL D				
New PDOS (either version)	-0.007	-0.042	-0.127	0.041
	(0.015)	(0.092)	(0.053)	(0.051)
IHS nr of Filipinos in county (demeaned)	-0.001	-0.015	0.043	0.026
	(0.005)	(0.026)	(0.018)	(0.017)
New PDOS x IHS nr of Filipinos in county	-0.001	0.010	-0.042	-0.044
	(0.006)	(0.032)	(0.020)	(0.021)
R2	0.243	0.141	0.133	0.040
Observations	938	570	710	871

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table 3: Long-term effects (after about 30 months in the U.S.): Has a job and found job through social network

	(1) Has a job	(2) Has a job	(3) Has a job	(4) Found job through network	(5) Found job through network	(6) Found job through network
New PDOS (either version)	-0.007 (0.022)	-0.013 (0.023)	-0.015 (0.028)	-0.013 (0.031)	0.028 (0.039)	-0.026 (0.047)
New PDOS with emp. module		0.011 (0.023)			-0.078 (0.040)	
New PDOS with ass. email			0.050 (0.030)			0.096 (0.050)
Mean outcome control group	0.860	0.860	0.850	0.702	0.702	0.655
R2	0.130	0.130	0.150	0.095	0.099	0.086
Observations	1162	1162	810	892	892	616

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Standard errors clustered at the PDOS session level in parentheses.

APPENDIX (FOR ONLINE PUBLICATION)

Information and the Acquisition of Social Network Connections

Authors

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Appendices

A A Simple Model of Social Network Investment

We present here a simple model of social network investment in the context of imperfect information. We are interested in the interplay between information imperfections and individual efforts to expand one's social network. In particular, we are interested in the potential impact of interventions to alleviate information imperfections. If an intervention reduces information imperfections, does this raise or reduce individual efforts to expand one's social network? We will see that it is theoretically possible for reductions in information imperfections to either raise or lower optimal choice of social network size.

Individuals have imperfect information about a variety of things in life that matter to them, such as jobs (how to find them and what jobs are available), financial services, government services, and the like. People also have social networks ("friends"), which provide *information*, helping to reduce information imperfections. This can come about simply in the process of friends conversing and sharing information with one another about topics relevant to their lives. Network theory suggests that efficient information gathering typically requires *expansive* networks with many short network paths (cf. Granovetter, 1973). Thus, we use the number of first-degree friends as a proxy for network expansiveness.

Because friends are valuable, people make efforts to acquire them, but making friends is to some degree costly. The costs of friend acquisition may include effort costs of socializing, as well as monetary costs incurred to facilitate networking, such as travel costs to meetings and social events, costs of membership in clubs or organizations, and the like.

We focus on the benefits friends bring by reducing information imperfections. We abstract away from other benefits of friends that the network literature typically refers to as *cooperation capital*, such as various forms of *assistance* (transfers, informal insurance, and psychological support).[1]

[1] These other non-information benefits of friends could be thought of as entering the cost term in the maximization problem we write down below, reducing the *net* cost of friends.

We model individual utility as depending on baseline or starting-point information imperfections (prior to any reduction in information imperfections resulting from friend investments), θ, and endogenous friend investment $f \geq 0$. For simplicity, we abstract from other determinants of utility that are independent of friends. Individuals choose f to maximize the benefits from friends $B(\theta, f)$ net of the cost of friend investment $C(f)$:

$$U = B(\theta, f) - C(f)$$

People therefore acquire friends only up to the point at which the marginal cost does not exceed the marginal benefit of friends. With reasonable assumptions on functional forms one can obtain an interior solution for the optimal number of friends. A corner solution is also possible of course, if the cost of friend investments is so high relative to benefits that the optimal number of friends is zero. Once functional forms are posited, we can make statements about the responsiveness of friend investments to changes in baseline information imperfections θ.

Some simple assumptions and functional forms generate useful possibilities. Let information imperfections θ range from 0 to 1 ($\theta \in [0, 1]$), and allow individuals to have both exogenous friends (those that are given at baseline without cost), e, and endogenous friends, f, which they acquire at a cost. Let $e \geq 1$.[2] Let one's amount of information I be a function of information imperfections θ, exogenous friends e, and endogenous friends f as follows:

$$I = 1 - \frac{\theta}{e + f}$$

In this setup, one's amount of information can range from 0 (no information) to 1 (full information). If baseline information imperfections θ are 0, then one starts with full information. A higher number of friends $e + f$ reduces the importance of one's baseline information imperfections and raises one's amount of information I.

[2] In our empirical context, assuming that individuals start with at least one friend is reasonable. For new immigrants to the U.S., the exogenous friend could be the individual who officially sponsors the immigrant for their immigration visa.

For simplicity, let the cost of endogenous friends be linear with a per-friend cost c, so the total cost of friend acquisition is cf.[3] As mentioned, exogenous friends e, as part of one's endowment, are costless.

We now analyze three distinct cases: constant, decreasing, and increasing returns to infomation. Analysis of the different cases makes clear that reductions in information imperfections (increases in information) have indeterminate impacts on friend investments.

Case 1: Constant Returns to Information

Let the benefit $B(\theta, f)$ be constant or linear in the amount of information I. The individual's maximization problem is as follows:

$$\max_f 1 - \frac{\theta}{e+f} - cf$$

The first order condition is:

$$\frac{\theta}{(e+f)^2} = c$$

The individual chooses endogenous friends f so that the marginal benefit of friends equals their marginal cost. Solving for f gives the optimal number of friends f^*:

$$f^* = \sqrt{\frac{\theta}{c}} - e$$

(Checking the second order condition confirms this is a maximum.)

Now we can ask: what effect do baseline information imperfections have on the optimal number of friends? We can take the partial derivative of f^* with respect to θ:

$$\frac{\partial f^*}{\partial \theta} = \frac{1}{2c\sqrt{\frac{\theta}{c}}} > 0$$

This partial derivative is always positive. Therefore a reduction in information imperfections θ (e.g., our information treatment for new immigrants) should reduce friend investments.

[3] The main predictions of the model are robust to the assumption of increasing per-friend net cost, which might result from decreasing per-friend assistance benefits in larger networks.

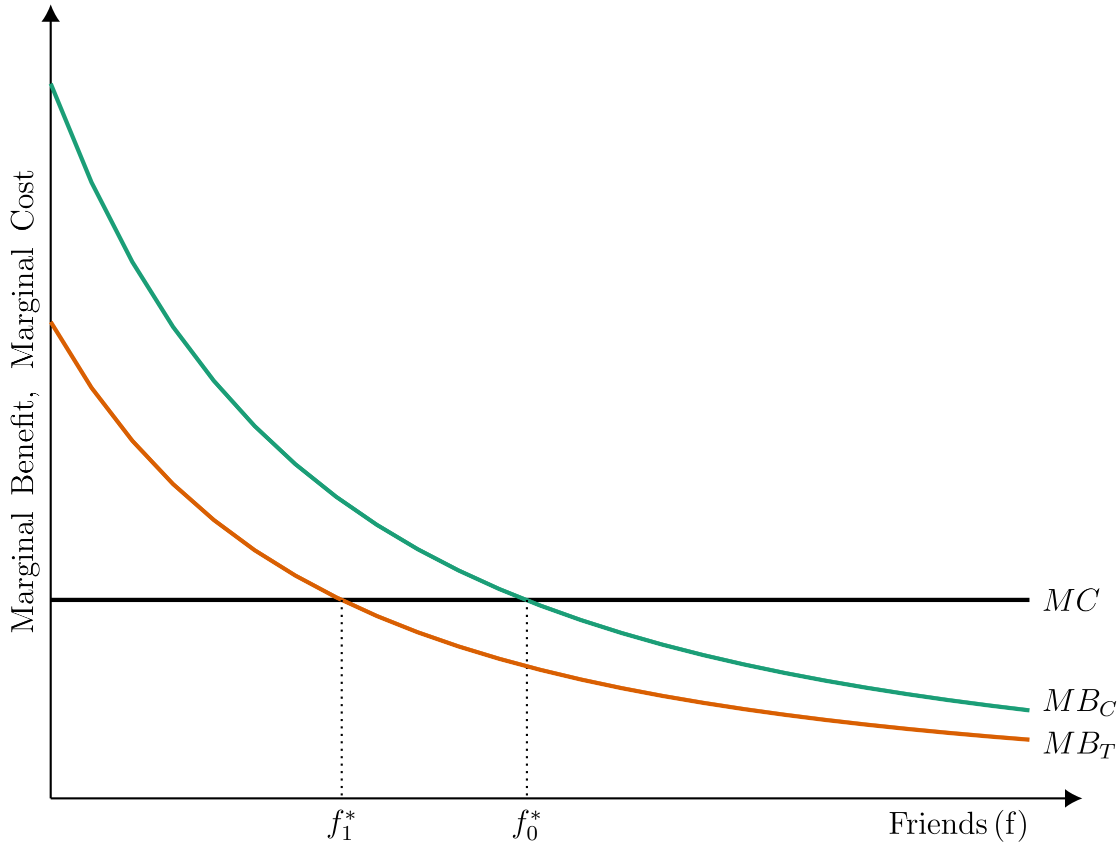

Figure A.1: Constant returns to information

Figure A.1 graphically shows the impact of reducing information imperfections when returns to information are constant. Parameter values used in the figure are: $e = 1$, $c = 0.25$. The red line is the marginal cost function, which is horizontal because the cost of friends is constant. The green curve is the marginal benefit function for the control group (without the information treatment), with $\theta = 0.9$. The orange curve is the marginal benefit function for the information treatment group, which due to the treatment has lower information imperfections ($\theta = 0.6$). The reduction in information imperfections due to treatment lowers the marginal benefit of friends (the orange curve is always lower than the green curve).

The optimal number of friends is given by the intersection of the marginal benefit and marginal cost functions. In the control group, the optimal number of friends is f_0^*. In the treatment group, the optimal number of friends is f_1^*, which is lower than f_0^*. The reduction in information imperfections due to treatment lowers the marginal benefit of friends, lowering the optimal number of friends.

Case 2: Decreasing Returns to Information

The case of decreasing returns to information is very similar to the constant-returns case. We modify the benefit function so that benefits are a function of the square root of information, so the migrant's optimization problem is:

$$\max_f (1 - \frac{\theta}{e+f})^{\frac{1}{2}} - cf$$

The first order condition is:

$$\frac{\theta}{2(1 - \frac{\theta}{e+f})f^2} = c$$

Aside from the change in the benefit function and thus the marginal benefit functions, assumptions are otherwise the same as for the constant-returns case in Figure A.1. As in Figure A.1, the reduction in information imperfections due to treatment lowers the marginal benefit of friends (the orange curve is always lower than the green curve).

Case 3: Increasing Returns to Information

Assuming increasing returns to information leads to ambiguous predictions regarding the impact of information imperfections on friend investments. We modify the benefit function to add a quadratic term in information, allowing for increasing returns to information. So the migrant's optimization problem is:

$$\max_f 1 - \frac{\theta}{e+f} + \alpha(1 - \frac{\theta}{e+f})^2 - cf$$

The parameter α measures the strength of increasing returns to information. The first order condition is now:

$$\frac{\theta}{(e+f)^2} + \frac{2\alpha\theta}{(e+f)^2}(1 - \frac{\theta}{(e+f)}) = c$$

These marginal benefit and cost curves now allow an information treatment (that lowers θ) to either raise or lower optimal friend investments.

We analyze this case graphically in Figure 1. The parameter values used in the figure are $e = 1$, $c = 2.4$, and $\alpha = 5$. As in Figure A.1, the horizontal red line is the marginal cost function. The green and orange curves are the marginal benefit functions for the control group and treatment groups with, $\theta = 0.9$ and $\theta = 0.6$ respectively. The marginal benefit functions can have upward-sloping (increasing returns to friends) and downward-sloping (decreasing returns to friends) sections. The optimum is found at the intersection of the marginal cost function and the downward-sloping part of the relevant marginal benefit function. (The optimum would not be at the intersection with the upward-sloping part of the marginal benefit function, because at that intersection the marginal benefit of friends is increasing, so the individual could continue to increase utility by raising friend investments.)

The figure depicts the case where the cost of friend investments is high enough that for the control group (blue marginal benefit curve), there is a corner solution where $f^* = 0$ (utility is maximized with no friend investments.) For the control group, there is no amount of friend investments for which the marginal benefit of friends is positive, so the individual makes no friend investments.

From this starting point, a reduction in θ (in this figure, from 0.9 in the control group to 0.6 in the treatment group) can lead the marginal benefit function to shift so that there is an interior solution with positive friend investments ($f^* > 0$).

There is also of course the possibility, for lower values of the cost of friends c, that reductions in θ lead to reductions in the optimal number of friends. This would be the case if the marginal cost of friends was lower, so that the marginal cost function (the horizontal red line) would intersect both the control group and treatment group marginal benefit functions on their downward-sloping portions. If this were the case, a reduction in θ would have effects similar to those depicted in Figure A.1: a reduction in optimal friend investments.

Overall, therefore, depending on parameter values and functional forms, it is possible for an information intervention, such as the one we implemented among new U.S. immigrants from the Philippines, to either raise or lower investments made in building one's social network. This possibility arises when there are increasing returns to information (Case 2 above), but not when returns to information are constant (Case 1 above).

In Figure A.2, we examine the impact of the information treatment on friend acquisition and on utility for a range of marginal cost levels, from the highest (on the left of the x-axis) to the lowest (to the right of the x-axis).

In the upper panel of Figure A.2, we show optimal friends in the treatment group (orange line) and control group (green line). In the control group, there is no friend acquisition for the highest cost levels. Friend acquisition only becomes positive as costs fall below a certain threshold, and increase as marginal costs continue to fall. In the treatment group, on the other hand, there is always (for these cost values) positive friend acquisition, and optimal friends rise continuously as costs fall.

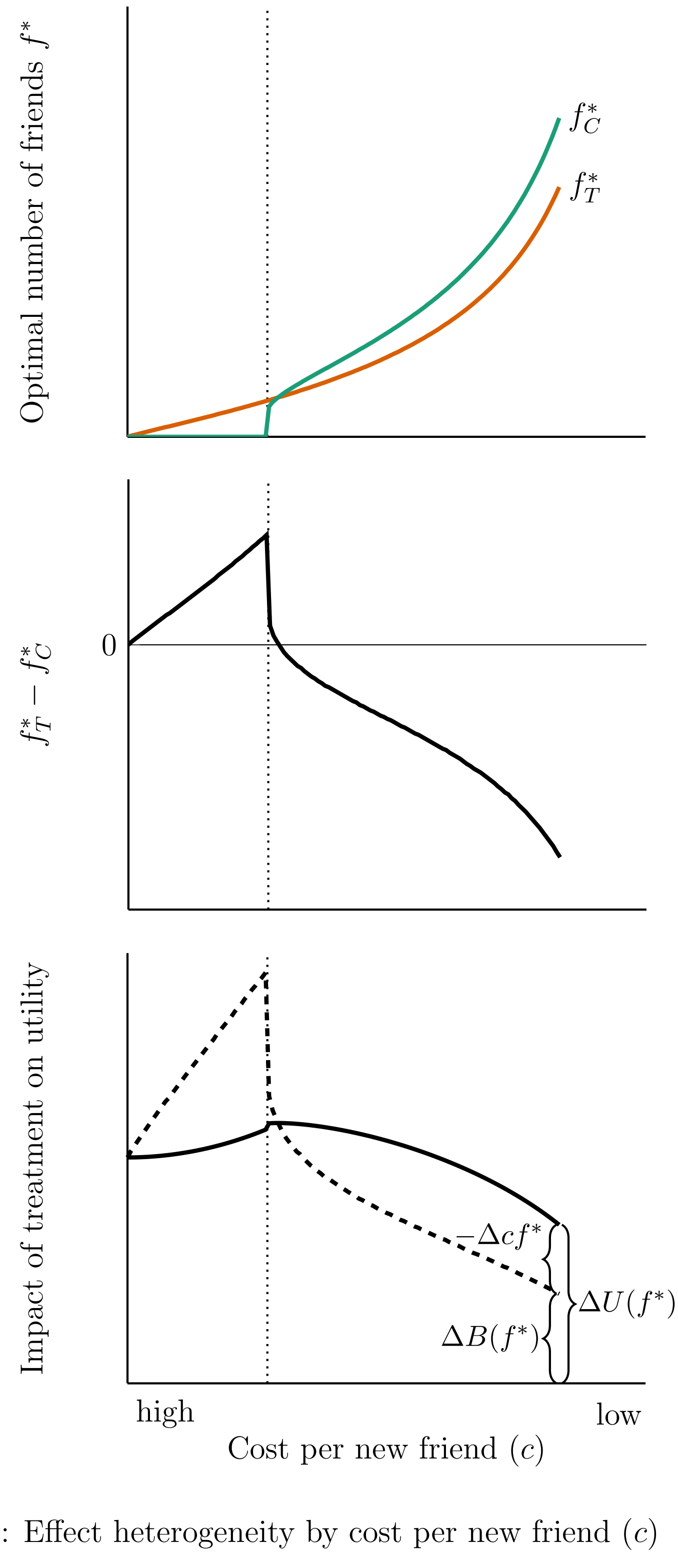

Figure A.2: Effect heterogeneity by cost per new friend (c)

In the central panel of Figure A.2, we examine the treatment effect on friend acquisition (optimal friends in the treatment group minus optimal friends in the control group). Initially, reductions in marginal costs make the treatment effect on friend acquisition even more positive, because we are in the region where information and friends are complements. As we lower marginal costs further into the zone where there is now positive friend acquisition in the control group, friends and information are now substitutes. The treatment effect on friends becomes negative, and increasingly so as marginal costs fall more.

In the lower panel of Figure A.2, we examine the treatment effect on utility. The treatment effect on utility (solid line) follows a pattern similar to the central panel. Starting from the highest marginal costs, reductions in marginal costs make the treatment effect on utility even more positive, as long as one remains in the region where information and friends are complements. As we lower marginal costs further, friends and information become substitutes, and the treatment effect on utility declines, and eventually can be even lower than when marginal costs were very high.

All told then, the impact of the treatment on friend acquisition and on utility is not monotonic. There is a range (when marginal costs start from a "high" level) where information and friends are complements, during which the treatment effect on friends and on utility rises as marginal costs fall. Then as marginal costs continue to fall, we transition to a region where information and friends are substitutes. In this region, the treatment effect on friends becomes negative, and the treatment effect on both friends and utility declines as marginal costs continue to fall.

B Further Details on Treatments and Survey Implementation

Content of the New PDOS

The new PDOS and the corresponding handbook consist of the following components.

Travel – This short module helps migrants to prepare for the journey to the U.S.. It covers travel-related issues such as travel documents, airport and immigration procedures, luggage, and restricted items. The new module is considerably shorter than the previous module, but the new expanded handbook provides comprehensive information on these matters.

Settlement – This is the broadest of all modules and covers issues related to migration in general and migration to the U.S. in particular. The module addresses topics such as cultural differences and culture shock, rights and obligations of U.S. permanent residents, important things to take care of after arrival (such as obtaining a social security number, health insurance, a driver's license, etc.) as well as information about health care, education, and housing.

Associations in the U.S. – Filipino associations, but also non-Filipino associations such as neighborhood associations, may be an important provider of post-arrival support for migrants. The module informs migrants about the potential benefits of associations for expanding their social network. Such contacts may ultimately help migrants to integrate into the U.S. and find a decent job.

Employment – This module aims to help migrants to find a decent job in the U.S., which our preparatory interviews identified as the single most important challenge for Filipino migrants. It informs about the U.S. labor market and addresses important issues such as the recognition of certificates and diplomas, job search strategies, how to prepare a CV and cover letter, and behave in a job interview. There are two versions of the new PDOS, one with and one without employment module.

Financial literacy – This module is based on the fact that migrants often experience a substantial increase in income when starting a job abroad. The module teaches basic rules of thumb on opening a bank account, financial planning, savings, sending remittances, and making a joint financial plan with the family in the Philippines on the amount and use of remittances.

Diaspora engagement – This module aims to strengthen the links between Filipino migrants and the Philippines. It covers Filipino culture and values, overseas voting rights, the right to re-acquire Filipino citizenship and government programs such as BalinkBayan and Linkapil, which help migrants to stay in touch with their home country and give them the possibility to contribute to development causes in the Philippines.

The new PDOS provides each migrant with a comprehensive 116-page paper handbook, which covers the above topics in detail and provides easy-to-follow checklists as well as links to online resources. While the old PDOS provides written information in the form of a booklet, the handbook of the new PDOS offers much richer and practical information. Figures B.3 and B.4 below illustrate this difference in terms of both quantity and quality for information provided on opening a bank account.

All material used in the different treatment conditions including the presentation slides and handbooks can be downloaded at https://sites.google.com/view/tomanbarsbai/pdos.

Opening a Bank Account

One of your first steps in the US should be opening a US bank account. Having an account allows you to manage your day-to-day financial transactions, which may involve buying daily necessities, or renting or purchasing a new home. Moreover, with an account, you are a step closer to fully realizing your financial goals – may it be short, mid-, or long term.

Below are the general steps to be taken in opening an account:

1 **Do your research**. Since banks come in various types and sizes and offer a diverse range of services and fees, it is best to do research on which bank is suitable to your goals, needs, and lifestyle. You may want to ask around, read online, or speak directly to a bank representative to have your questions answered.

2 **Know more about your prospective bank**. Consult the website of your preferred bank or call a bank representative for the complete requirements. Factor in considerations such as banking hours, distance, fees, interest rates, etc. Ask about ATM related bank services like location, customer protection, fees, withdrawing limit, etc. For you it might be of particular importance what the fees are for international transactions to or from the Philippines. Some banks have special relationships with foreign banks that reduce fees for international transactions—this will be listed on their websites.

3 **Determine what type of account you want**. Familiarize yourself with the various types of accounts and know what suits your needs best. The two common kinds of bank accounts are: Savings Account and Checking Account. Compare your options.

4 **Gather the requirements**. Make sure you have the complete requirements with you before heading to the bank. Each financial institution has its own requirements, but the standard requirement includes:

- Social Security Number
- Identification Card with Photo (Passport, Driver's License, etc.)
- Individual Taxpayer Identification Number (for some banks)

Note: Banks sometimes require an additional deposit. Many banks also wave the fees if you keep a minimum balance in the account. Consult your bank for the exact amount.

5 **Head to the Bank**. Present additional documents and fill out the provided forms. Schedule an appointment, if necessary. Check with the bank for online applications.

Note: For joint bank accounts, consult with the bank if both signatories need to be present when opening the account.

Remember:

- Prepare your questions in advance and do not hesitate to ask them.
- Consult with a bank representative who can walk you through the various products, offers, and services available.
- Decide on what is convenient for you – telephone, mobile, or online banking.
- Do not sign up for a bank account without comparing several options.
- Do not sign the terms and agreements unless you have read and understood the document.

Finding a Place to Live (Housing)

The decision whether to rent or buy a home is greatly influenced by your personal consideration, for instance your preferences, suitability, and financial situation.

The US Department of Housing and Urban Development (HUD) website (**http://portal.hud.gov/hudportal/HUD**) provides an exhaustive list of tools that can guide you in your decision. You will see vast number of services, checklists, and information by state.

The first decision to make is whether buying a home or renting is the best option for you. Below is a guide to help you evaluate your options. [19]

Buying	Renting
If you plan to stay in one location	If you plan to move around and don't want to be tied in one location
If you have funds for a down payment and closing costs	If you don't have the funds for a down payment and closing costs
If you can afford the maintenance costs of owning (repairs, lawn care, etc.)	If you can't afford the potential maintenance costs of owning (repairs, lawn care, etc.)
If you want to build equity over the long-term	If you are saving for the future
If you want the potential tax advantages	

19. Decide what's right—rent or buy? (n.d.). Retrieved from FannieMae KnowYourOptions: http://knowyouroptions.com/rent/overview/decide-whats-right-rent-or-buy

Figure B.3: Information on how to open a bank account provided in the handbook of the new PDOS to present your social security number and other documents to confirm your identity.

Bank Account

Open a bank account to safe keep your money. It will also help facilitate your financial transactions. Before opening bank accounts, compare the services, fees, working hours and location of banks so you can choose the one that best meets your needs.

Taxes

As permanent residents, you will be taxed by the U.S. Government for your income inside and outside of the U.S. You must file your income tax statements at the Internal Revenue Service regardless of whether you are earning an income or not.

For more information, please visit the website http://www.irs.gov/localcontacts/index.html, or call 1-800-829-1040.

U.S. Military Selective Service

All male permanent resident aliens aged 18 to 25 years must register with the Selective Service System (SSS). Registration must be accomplished within 30 days before and after the 18th birthday. If the age upon arrival in the U.S. is between 18 and 25 years, registration must be done within 30 days upon arrival. There are no exceptions to the said age bracket. Even mentally or physically disabled persons must register.

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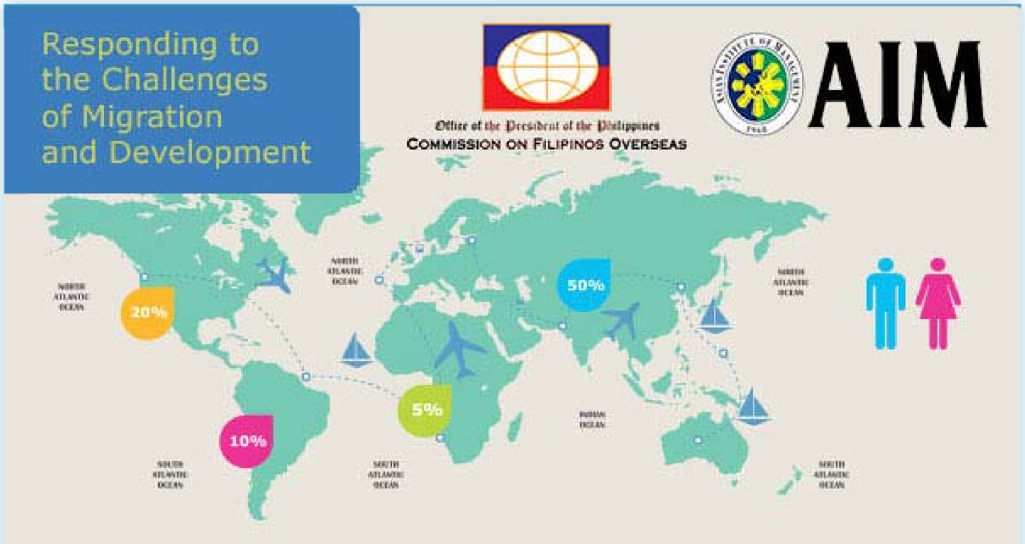
Figure B.4: Information on how to open a bank account provided in the booklet of the old PDOS

Association Email

Below is the template for the association email. Each email provides contact details of Filipino associations in the migrant's U.S. state. The email below is for migrants moving to Northern California.

An invitation to get in touch with Filipino Association



Dear <<Salutation>> <<First Name>> <<Last Name>> ,

Greetings from the Commission on Filipinos Overseas (CFO)!

Kamusta na po kayo? We hope you are doing well. By now, you are most likely in the midst of preparing for your new life in the US. We recognize that post-arrival support for newly-settled migrants like you is very important to help you in your adjustment period – from learning about job opportunities, expanding social networks, accessing government services including social security benefits, to enrolling children in school.

The good news is that several Filipino associations in the US have long been providing such support by linking newly arrived Filipinos to other Filipinos in the area. These contacts open great opportunities in getting guidance on how to make the best of your new life in the US, find a job, locate the best schools in the area and available scholarships, or simply, discover new activities to try, places to explore, and make new friends!

We therefore strongly encourage you and your family to get in touch with Filipino associations to find out about their programs and advocacies that could potentially suit you.

To start your search, we invite you to browse and contact the following organizations in Northern California:

Transnational Institute for Grassroots Research and Action (TIGRA)
900 Alice Street #400, Oakland, CA 94607
Contact person: Francis Calpotura
Email: tigra@transnationalaction.org
Website – Facebook
Phone: (510) 338-4915

Filipina Women's Network
P.O Box 192143, San Francisco, CA 94119
Contact person: Marily Mondejar
Email: marilym@ffwn.org or filipina@ffwn.org
Website – Facebook
Phone: (415) 935-4396

Filipino American Development Development Foundation / Bayanihan Community Center
1010 Mission St Ste. B, San Francisco, CA 94103 Bernadette Sy
Contact person: MC Canlas
Email: b_sy@att.net or mccanlast@aol.com
Website
Phone: (415)348-8042 / (415) 974-0349

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may still want to get in touch with them through email or phone. They have a large network and may recommend you to another association close to your place of residence. These associations are dedicated in helping migrants such as yourself and may help you a great deal in transitioning to your new home.

If you get to connect with a Filipino association in your area, please do tell us how it went and how else we can assist you. Feel free to reach us through Filsupport@cfo.gov.ph.

Hangad namin na maiayos sa madaling panahon ang inyong bagong buhay sa America. Sa pamamagitan ng mga grupong ito, maaari kang makatanggap ng suporta at tulong na iyong kinakailangan. Bukod dito, maaari ka ding makatulong sa ibang migranteng Pilipino na tulad mo.

Maraming salamat po!

Very truly yours,

Secretary Imelda M. Nicolas
Chairperson
Commission on Filipinos Overseas

Northern California
2195 Cobblehill Pl, San Mateo, CA 94402
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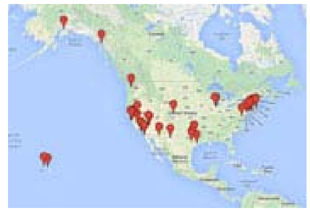
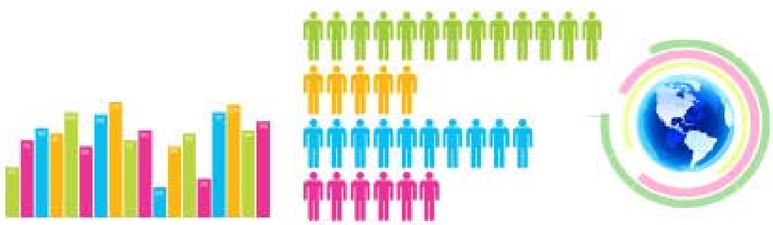
This map provides information on many more Filipino organizations in the US.





Treatment Implementation

Our protocols were designed to minimize spillover of information from treatment to control study participants. Scheduling the new and old PDOS on different dates minimizes the possibility of interaction between the two groups. The CFO leadership did not share the full schedule or email list with instructors or other implementation staff. Instructors were informed one week in advance of the PDOS version to be given on a particular day. Prospective PDOS participants were never informed that different PDOS versions were given on different dates, and would have had great difficulty discovering the schedule in advance.

To avoid control group contamination through instructors, different groups of instructors conducted the new and old PDOS. Instructors of the old PDOS were not informed about the content of the new PDOS and had no access to the new training materials, including the handbook. To assign instructors to the new or old PDOS and balance their characteristics, we ranked them by instruction quality and used paired random assignment. Distribution of the new, enhanced handbook was also tightly controlled. No new handbooks were available on "old PDOS" dates, and only the matching version (with and without employment module) for the corresponding new PDOS was available on each date. In addition, handbooks were not available for download on the internet during the randomized implementation period.

CFO instructors gave the old and new PDOS presentations at a central location in Manila. The delivery of both the new and the old PDOS was highly standardized. Written instructions specified the content to be delivered for each presentation slide, and we gave instructors substantial advance training prior to study initiation.

Survey Data Collection

Due to the complexity of data collection involving face-to-face interviews across the Philippines and phone interviews with migrants in the U.S., we hired the Philippine branch of TNS, a large international survey firm, to conduct the fieldwork of the project. TNS could provide field staff in all parts of the Philippines and the infrastructure needed for phone interviews.

Preparation for fieldwork followed standard practice including pre-tests of the survey instrument and extensive training of enumerators. In all survey rounds, training, data collection, and monitoring were the same across treatment and control groups. In addition, field staff was blind to both the treatment status of each respondent and the content of the interventions. All interviews were computer-assisted and administered on tablets. Computer assistance facilitated tracking individuals over time and improved data quality through automated routing and error checks. To further improve data quality, a supervisor monitored all phone interviews. Field supervisors audited ten percent of the interviews conducted with household members in the Philippines. In addition, backchecks, with a focus on non-changing information, were conducted on 20 percent of the interviews.

There was a modest compensation for participation in the survey. For completed baseline interviews, migrant respondents received PHP 200 gift certificates and household respondents bags worth PHP 110. For completed follow-up interviews, migrant respondents received phone credit worth PHP 100 to be sent to a person of their choice in the Philippines. Household respondents received phone credit worth PHP 200 and an additional PHP 100 for completed proxy interviews. To maximize response rate, we increased compensation for migrant interviews in the endline survey. In this final round, migrants received a gift certificate worth USD 10, which they could choose to keep or donate to the Red Cross. To further increase response rates, we also experimented with higher tokens. In the very last weeks of the endline survey, we offered PHP 1,000 for completed migrant and household interviews. This strategy led to the completion of about three dozen additional interviews.

C Construction of Indices

We use indices for different outcomes domains to reduce the number of outcomes to examine. Here we provide more details on how we construct the different indices (as pre-specified in the first PAP). We also reprint the exact survey questions and answer options in italics.

Travel-related problems – Average of having (i) missed a flight, (ii) had luggage problems, (iii) had customs problems, (iv) had problems with authorities in the Philippines, (v) had problems with authorities in the U.S.. Ranges from 0 to 1.

Please think back to your travel from the Philippines to the U.S.. Did you experience the following problems: (i) Missed flight from the Philippines or connecting flight, (ii) problems with airline because of too much luggage or prohibited items in luggage, (iii) problems with custom authorities because of prohibited items in luggage, (iv) problems with authorities in the Philippines because of wrong/missing documents, (v) problems with authorities in the U.S. because of wrong/missing documents? Yes / No

Settlement index – Average of having (i) a social security number, (ii) health insurance, (iii) a driver's license, (iv) a bank account. Ranges from 0 to 1.

Do you have a Social Security number in the United States? Yes / No, but I have already applied / No, I have not applied yet

Do you have health insurance in the United States? Yes / No, but I have already applied / No, I have not applied yet

Do you have a U.S. driver's licence? Yes / No, but I am planning to get one / No, I am not planning to get one

Do you have a bank account in the United States? Yes, I have my own bank account / Yes, I have a joint account with my spouse/partner / No, but I am planning to get one / No, I am not planning to get one

Employment index – Standardized treatment effect[4] (STE) of (i) having a job, (ii) inverse hyberbolic sine of monthly earnings, (iii) perceived chance of having a job in the near future, (iv) perceived chance of having a job that

[4] We normalize each outcome by subtracting the mean of the control group and dividing by the standard deviation of the control group. Let Y_k be the k^{th} of K outcomes of a given outcome domain, μ_k be the control group mean and σ_k the control group standard deviation of Y_k. The normalized outcome is $Y_k^* = (Y_k - \mu_k)/\sigma_k$. The summary index is $Y^* = \sum_K Y_k^*/K$. We reverse the sign for adverse outcomes, so that higher values indicate more beneficial outcomes. Treatment effect estimates based on the STE quantify the difference between means in the treatment and control groups in standard deviation units.

matches the qualification in the future. We exclude (iii) and (iv) when estimating long-term effects as these outcomes were not collected in later interviews. We deviate from the PAP and do not include the number of invitations to a job interview since arrival in the U.S.. Due to a routing error in the script, this indicator was unfortunately not systematically collected.

Do you currently work or have a job or business? Yes / No

How much are your monthly earnings from that job? Please state the amount before tax.

What would you say is the probability that you will have a job half a year from now? Please give me a percentage number, 0 means you think it is impossible, 100 means you are sure that you will have a job.

And what would you say is the probability that you will have a job that corresponds to your qualification half a year from now? Please give me a percentage number, 0 means you think it is impossible, 100 means you are sure that you will have a job that corresponds to your qualification.

Network size index – STE of (i) having received support from an association in the U.S. and (ii) inverse hyperbolic sine of the number of friends and acquaintances made in the U.S. since arrival. We replace (i) with having had contact with an association in the U.S. when estimating long-term effects as this outcome was not collected in later interviews.

Have you received any support (information, help to find housing or work, etc.) from a Filipino community or diaspora association in the U.S.? Yes / No

How many new people in the U.S. have you got to know on a personal basis since your arrival in the U.S.?

Subjective wellbeing index – STE of (i) mental wellbeing index and (ii) migrant wellbeing index. The mental wellbeing index is the sum of five five-point items. It measures how often during the past month the respondent (i) was happy, (ii) felt calm and peaceful, (iii) was not very nervous, (iv) did not feel downhearted and blue, (v) did not feel so down in the dumps that nothing could cheer her/him up. The migrant wellbeing index is the sum of two five-point items. It measures how often during the past month the respondent did not feel (i) homesick and (ii) overwhelmed by the challenges faced in the U.S..

During the past month, how much of the time (i) were you a happy person, (ii) did you feel calm and peaceful, (iii) were you a very nervous person, (iv) did you feel down-hearted and blue, (v) did you feel so down in the dumps that nothing could cheer you up, (vi) did you feel homesick, (vii) did you feel overwhelmed by the challenges you face in the U.S.? None of the time / A little of the time / Some of the time / Most of the time / All of the time

D Multiple Hypothesis Testing

We estimate treatment effects using variants of the following regression specification:

$$Y_{i,k} = \beta_0 + \beta_1 D_{i,1} + \ldots + \beta_L D_{i,L} + \mathbf{X_i'}\theta + u_{i,k}, \tag{D.4}$$

where $Y_{i,k}$ denotes the kth outcome of interest for the ith unit, $D_{i,1} \ldots D_{i,L}$ the independent variables of interest (treatments), $\beta_1 \ldots \beta_L$ the parameters of interest and X_i a set of further independent variables (baseline covariates). We might further estimate these parameters in subgroups formed by the values of variables Z_i. Note that the set of variables in X_i and Z_i might be overlapping. Testing multiple hypotheses simultaneously arises due to investigating the effects on multiple outcomes of interest, the effects of multiple independent variables of interest (in the same regression specification or in different ones), the effects in multiple subgroups, or any combination thereof. In other words, we make simultaneous inference on the elements of a parameter vector $\beta = (\beta_1, ..., \beta_S)$ with individual null hypothesis of the form $H_S : \beta_s = 0$. In these situations, we want to control for the familywise error rate (FWER) – the probability of one or more false rejections.

List, Shaikh and Xu (2019) provide a bootstrap-based stepwise procedure for simultaneously testing null hypotheses from settings with multiple outcomes, treatments, and subgroups. The procedure is based on the results in Romano and Wolf (2010). It asymptotically controls the FWER and is asymptotically balanced in that the marginal probabilities of rejecting true null hypotheses are approximately equal in large samples. Information about the dependence structure between hypotheses yields greater statistical power to reject truly false null hypotheses compared to procedures such as the Bonferroni (1935) and Holm (1979) corrections that assume independence between hypotheses. However, the procedure and the Stata package introduced in List, Shaikh and Xu (2019) are designed for experimental data in which simple random sampling is used to assign a discrete treatment status to units. It is not designed for hypothesis testing of parameters from regressions with multiple independent variables.

We modify the procedure of List, Shaikh and Xu (2019) to make it suitable for regression analysis.[5] Below, we describe the procedure and indicate where we deviate from the setup of List, Shaikh and Xu (2019). Our key modification is how we define the "unbalanced" studentized test statistic for H_s. For samples of size n, the test statistic is

$$T_{s,n}^{stud} = \frac{|\hat{\beta}_{n,s}|}{se(\hat{\beta}_{n,s})}$$

and it's re-centered version is[6]

$$\tilde{T}_{s,n}^{stud}(P) = \frac{|\hat{\beta}_{n,s} - \beta_s|}{se(\hat{\beta}_{n,s})}.$$

The regression framework does not require D_i, X_i, and Z_i to be discrete as required by Assumption 2.3 in List, Shaikh and Xu (2019). We consider the observed data (Y_i, D_i, X_i, Z_i), $i = 1, ..., n$ i.i.d. but we discussion an extension that allows for deviations from the i.i.d. assumption below. Denote by $\hat{P}_n$ the empirical distribution of the observed data. The multiple testing procedure consists of the following steps (see Algorithm 3.1 in List, Shaikh and Xu, 2019):

[5]We implement this procedure in Stata. It can be applied to other regression based settings. The module can be installed by typing `net install mhtreg, from(https://sites.google.com/site/andreassteinmayr/mhtreg)` in the Stata prompt. The Stata procedure is based on modifications of the code provided by Joseph Seidel (https://github.com/seidelj/mht-source). We thank Azeem Shaikh for helpful suggestions for the modifications.

[6]The corresponding test statistics in List, Shaikh and Xu (2019) are in Equations (6) and (7) and Remark 3.4.

Step 0. Set $S_1 = S$.

$\vdots$

Step j. If $S_j = \emptyset$ or

$$\underset{s \in S_j}{max} J_n(T^{stud}_{s,n}, s, \hat{P}_n) \leq L^{-1}_n(1-\alpha, S_j, \hat{P}_n),$$

then stop. Otherwise reject any H_s with $J_n(T^{stud}_{s,n}, s, \hat{P}_n) > L^{-1}_n(1-\alpha, S_j, \hat{P}_n)$, set

$$S_{j+1} = \{s \in S_j : J_n(T^{stud}_{s,n}, s, \hat{P}_n) \leq L^{-1}_n(1-\alpha, S_j, \hat{P}_n)\},$$

and continue to the next step.

$\vdots$

The adjusted p-value for H_S, $\hat{p}^{adj}_{s,n}$ can be computed as the smallest value of α for which H_S is rejected in Algorithm 3.1. Furthermore, the procedure allows calculating an unadjusted bootstrap p-value for H_S, $\hat{p}_{s,n} = 1 - J_n(T_{s,n}, s, \hat{P}_n)$. We use bootstrap resamples to approximate $\overset{s,n}{J}_n(x, s, \hat{P}_n)$ and $L_n(x, S', \hat{P}_n)$. For $b = 1, ..., B$ draw a sample of size n from $\hat{P}_n$ and denote by $\tilde{T}^{*,b,stud}_{s,n}(\hat{P}_n)$ the quantity $\tilde{T}^{stud}_{s,n}(P_n)$ using the bth resample and $\hat{P}_n$ as an estimate of P. In our modified version this is

$$\tilde{T}^{*,b,stud}_{s,n}(\hat{P}_n) = \frac{|\hat{\beta}^{*,b}_{n,s} - \hat{\beta}_{n,s}|}{se(\hat{\beta}^{*,b}_{n,s})}$$

We approximate $\overset{s,n}{J}_n(x, s, \hat{P}_n)$ as

$$\hat{J}_n(x, s, \hat{P}_n) = \frac{1}{B} \sum_{1 \leq b \leq B} I\{\tilde{T}^{*,b,stud}_{s,n}(\hat{P}_n) \leq x\}$$

and $L_n(x, S', \hat{P}_n)$ as

$$\hat{L}_n(x, S', \hat{P}_n) = \frac{1}{B} \sum_{1 \leq b \leq B} I\{\underset{s \in S'}{max} \hat{J}_n(\tilde{T}^{*,b,stud}_{s,n}(\hat{P}_n), s, \hat{P}_n) \leq x\}.$$

Simulations

To evaluate the algorithm in terms of correct rejection rates and statistical power, we run a set of simulations based on different data-generating processes (DGP).[7] Let μ be a ten-dimensional vector of zeros $(0, 0, ..., 0)'$. Let I be a 10×10 identity matrix. Let Σ be a 10×10 covariance matrix where all off-diagonal elements are equal to 0.9. Let $D = 1[\mathcal{N}(0, 1) > 0]$ be a binary indicator equal to one with probability 0.5 for all scenarios except scenario five. The data-generating processes for each simulations are:

1. Normal i.i.d errors (ten outcomes)

   $\epsilon \sim \mathcal{N}(\mu, I)$; $Y = \epsilon$

2. Uniform i.i.d errors (ten outcomes)

   $\epsilon \sim \mathcal{N}(0, 1)$; $Y = \epsilon$

3. Normal i.i.d errors (one outcome, ten subgroups)

   $\epsilon \sim \mathcal{U}(0, 1)$; $Y = \epsilon$

4. Lognormal i.i.d. errors with balanced treatment (ten outcomes)

   $\epsilon \sim e^{\mathcal{N}(\mu,I)}$; $Y = \epsilon$

5. Lognormal i.i.d. errors with unbalanced treatment (ten outcomes)

   $D = 1[\mathcal{N}(0, 1) > 1]$; $\epsilon \sim e^{\mathcal{N}(\mu,I)}$; $Y = \epsilon$

6. Correlated errors (ten outcomes)

   $\epsilon \sim \mathcal{N}(\mu, \Sigma)$; $Y = 0.2D + \epsilon$

We run 2,000 simulations based on these data-generating processes. In each simulation, we estimate ten regressions of the form:

$$Y_k = \beta_{0,k} + \beta_{1,k}D_k + u_k, k = 1..10.$$

[7] We base the structure of these simulations on similar simulations for a multiple-hypothesis procedure based on Westfall and Young (1993) in the Appendix C of Jones, Molitor and Reif (2019).

The ten null hypothesis that correspond to these ten regressions are: $\beta_{1,k} = 0, k = 1..10$. These null hypotheses are true in scenarios one to five and false in scenario six. We use samples of size 100 for each scenario, for scenario two that implies 10 subgroups with 100 observations each. For all scenarios, we estimate an unadjusted p-value, a p-value adjusted with the procedure above, and adjustments based on the Bonferroni and Holm procedures. We provide a comparison between the regression based version mhtreg and the original procedure mhtexp for the unadjusted p-values and the adjustments based on Theorem 3.1 in List, Shaikh and Xu (2019).

Table D.1 present the results of this simulation. The first two rows of column (1) show the unadjusted familywise (FW) rejection rates using mhtreg (0.378) and mhtexp (0.382).[8] As a comparison, the FW rejection rate using Theorem 3.1 is 0.047 with mhtreg and 0.049 using mhtexp. Bonferroni and Holm adjustments result in a FW rejection rate of exactly 0.038.

Results are very similar in column (2), that uses a DGP with uniform errors. All methods are overly conservative in the case of lognormal errors with 50% treatment share (column 3). Using mhtreg, the unadjusted FW rejection rate is 0.263 and the adjusted is 0.009. Results using mhtexp are almost identical. Bonferroni and Holm result in FW rejection rates of 0.009. In contrast, column (4) shows results for lognormal errors but with a share of treated of only 16%. In such a scenario standard inference methods tend reject too often. Indeed, we see unadjusted FW rejection rates to be 0.55 using mhtreg and 0.588 using mhtexp. The adjusted rate is 0.095 using mhtreg and 0.205 using mhtexp, which suggests that the type of test statistic matters in this scenario. Column (5) shows results for multiple subgroups. All results are very close to the theoretical predictions with little differences between methods.

[8] Remember that the probability of at least one false rejection at $\alpha = 0.05$ is $1 - (1 - 0.05)^{10} = 0.401$ for ten independent hypotheses.

Table D.1: Familywise rejection rate at $\alpha = 0.05$, $n = 100$

	(1)	(2)	(3)	(4)	(5)	(6)
Adjustment method	Normal errors	Uniform errors	Lognormal errors (50% treat.)	Lognormal errors (16% treat.)	Multiple subgroups	Correlated errors
Unadjusted mhtreg	0.378	0.424	0.263	0.550	0.380	0.306
Unadjusted mhtexp	0.382	0.427	0.269	0.586	0.382	0.304
Thm. 3.1 mhtreg	0.047	0.062	0.009	0.095	0.057	0.178
Thm. 3.1 mhtexp	0.049	0.060	0.010	0.205	0.058	0.180
Bonferroni	0.038	0.051	0.009	0.083	0.049	0.090
Holm	0.038	0.051	0.009	0.083	0.049	0.096
Num. observations	100	100	100	100	100	100
Num. hypotheses	10	10	10	10	10	10
Hypotheses are true	Y	Y	Y	Y	Y	N

Note: Table reports the fraction of 2,000 simulations where at least one null hypothesis in a family of 10 hypotheses was rejected. All hypotheses are true for the simulations reported in columns (1) to (5), i.e., lower rejection rates are better. All hypotheses are false for the simulation reported in column (6), i.e., higher rejection rates are better. Bootstaps are performed with 2,000 replications.

Finally, column (6) shows results for the DGP with correlated errors when the null hypotheses are not true. Thus, in this scenario higher FW rejection rates are better. In the unadjusted case, the FW rejection rate is 0.306. Adjustment using Theorem 3.1 results in a FW rejection rate of 0.178, which is substantially higher than Bonferroni (0.09) and Holm (0.096). Again, results are similar for mhtreg and mhtexp.

Clustering

List, Shaikh and Xu (2019) do not take into account situations in which model errors are correlated within clusters. To capture the dependence structure, we follow Romano and Wolf (2010) who suggest using a block bootstrap in such situations. In addition, we allow the test statistics to be computed with cluster-robust standard errors. We also allow using a combination of the two strategies. The option `cluster(cluster_id)` of the `mhtreg` command identifies the cluster variable. The option `cltype(t)` specifies the type of clustering. Value `t=0` specifies no clustering at all, `t=1` specifies the use of a clustered bootstrap, `t=2` specifies the use of cluster-robust standard errors for the model, and `t=3` specifies the use of both.

We run a simulation to evaluate the performance of the different types of clustering. Again, let μ be a ten-dimensional zero vector $(0, 0, ..., 0)'$, and let I be a 10×10 identity matrix. The data-generating process for this simulation scenario is

1. Errors correlated within clusters (ten outcomes)

   $c = 1...100$ clusters

   $i = 1...10$ observations within clusters

   $\eta_c \sim \mathcal{N}(\mu, I)$

   $\epsilon_{ci} \sim \mathcal{N}(\mu, I)$

   $Y_{ci} = \eta_c + \epsilon_{ci}$

We again simulate 2,000 datasets. In each simulation, we estimated the following ten regressions:

$$Y_{k,ci} = \beta_{0,k} + \beta_{1,k} D_c + u_{k,ci}, k = 1..10.$$

where the dummy variable $D_c = 1[\mathcal{N}(\mu, I) > 0]$ varies only at the level of clusters.

Column (1) of Table D.2 shows the results without accounting for clustering. In the unadjusted case, at least one out of ten hypotheses is rejected almost every time (0.993). The adjustment methods also result in rejection proportions of more than 90%. Column (2) shows results when a clustered bootstrap is used but model standard errors are not adjusted. FW rejection rates are close to the theoretical predictions, 0.416 in the unadjusted case, 0.065 with Theorem 3.1 adjustment, and 0.058 using Bonferroni or Holm. Column (3) uses a non-clustered bootstrap but cluster-robust model standard errors. Again, results are close to the theoretical predictions with slightly smaller FW rejections rates. Finally, column (4) uses a clustered bootstrap and cluster-robust model standard errors, which again delivers results close to the theoretical predictions.

Table D.2: Familywise rejection rate at $\alpha = 0.05$, with clustered DGP

	(1)	(2)	(3)	(4)
Unadjusted mhtreg	0.993	0.416	0.394	0.393
Thm. 3.1 mhtreg	0.933	0.065	0.054	0.054
Bonferroni	0.925	0.058	0.051	0.046
Holm	0.926	0.058	0.051	0.046
Num. observations	1,000	1,000	1,000	1,000
Num. hypotheses	10	10	10	10
Model std. errors	Homoskedastic	Homoskedastic	Clustered	Clustered
Cluster bootstrap	N	Y	N	Y

Notes: Table reports the fraction of 2,000 simulations where at least one null hypothesis in a family of ten hypotheses was rejected. All hypotheses are true. Bootstaps are performed with 2,000 replications.

While it does not seem to make a difference, we use the double-clustering as presented in column (4) for results where clustering appears to be appropriate.

E Additional Figures and Tables

This section provides additional figures and tables that support our analysis. It also contains all analyses that we pre-specify in the different PAPs. We briefly summarize the results here.

Figures

Figure E.1 shows how migrants evaluate the old and the new PDOS. Immediately after each session, CFO asks migrants to complete a feedback form. All PDOS attendees, not only those who were part of our sample, received these feedback forms. Feedback is anonymous, so we cannot link it with survey responses. We analyze all feedback forms that CFO collected during the randomized implementation period. The new PDOS receives higher ratings on almost every aspect, in particular on the usefulness of various topics and the quality of the slides and the written material.

Summary Statistics and Balance Tests

Tables E.1, E.2 and E.3 provide summary statistics and balance tests of baseline characteristics and outcome variables by treatment status. They show that there are no major differences in baseline characteristics of study participants between different treatment conditions. Consistent with the main results, they also show that study participants in the treatment group have fewer travel-related problems and a lower value of the network size index.

Short-term Effects

Tables E.5-E.14 present additional results using data from the short-term survey. Tables E.5, E.6 and E.7 examine a range of potential attrition problems. They show that treatment status does not predict a migrant's re-interview status in various ways.

Tables E.8 shows that our main results hold when we exclude proxy reports and restrict the analysis to directly reported data.

Tables E.9 and E.10 show short-term effects of the new PDOS on the component variables of the travel and network size index. The incidence of travel-related problems is lower for every single indicator in the treatment group, significantly so for having missed a flight and problems with authorities in the Philippines. The new PDOS significantly reduces the number of friends and also makes study participants less likely to have received support from an association.

Tables E.11, E.12 and E.13 test for effect heterogeneity by education (below college degree vs college degree or higher), gender, and baseline knowledge about the U.S. (share of correct answers on different aspects of the U.S., split at the median). To do so, we interact the treatment status with the respective variable of interest. We find limited evidence for effect heterogeneity along these dimensions. The new PDOS improves settlement and subjective wellbeing for study participants with a college degree. All other interaction coefficients do not point towards statistically significant differences.

Table E.14 examines a few mechanisms through which the new PDOS might affect our main outcomes. We first look at employment-related mechanisms. The employment module has a negative effect on the job-search behavior of study participants. This result is surprising because the employment module provides migrants with information on how to get their qualifications recognized and explicitly encourages migrants to do so. At the same time, the employment module improves the job-search knowledge of study participants. We also find that the new PDOS affects how migrants establish networks in the U.S. (the index summarizes whether a migrant has had contact with a Filipino or non-Filipino association in the U.S. since arrival and whether the migrant has enrolled in an English language class). There is no evidence that migrants attending the new PDOS are more likely to have discussed the amount of remittances with their family and agreed on an amount. The new PDOS explicitly encourages migrants to do so in order to manage financial expectations on both sides.

Long-term Effects

Tables E.15-E.29 present additional results using data from the long-term survey. When the long-term datum is not available, we replace it with the mid-term or short-term value, in that order. Our presentation follows the same structure as the presentation of short-term effects. We start by examining potential attrition problems. As before, we do not find that treatment status predicts a migrant's re-interview status (Tables E.15, E.16 and E.17).

Tables E.18 shows that our main results hold when we exclude proxy reports and restrict the analysis to directly reported data.

Table E.19 shows long-term effects of the new PDOS on the component variables of the network size index. We still find that the new PDOS significantly reduces the number of friends. The effect on the rate of contacting an association remains negative but ceases to be statistically significant.

Tables E.20, E.21 and E.22 test for effect heterogeneity along education, gender, and baseline knowledge about the U.S.. Again, we find little effect heterogeneity. The only exception is that the new PDOS improves subjective wellbeing for study participants with a college degree.

Our main analysis is based on the first PAP of September 2014. We also registered subsequent PAPs to guide analysis of the mid-term survey data (submitted July 19, 2015) and final survey data (submitted July 28, 2016). These latter two PAPs add additional hypotheses related to employment and the characteristics of networks. For completeness, we show the main results from these two PAPs in this appendix. Our conclusions are robust to to estimating longer-run impacts using methods from longer-run PAPs. Most importantly, we also find that the new PDOS significantly reduces network size (column 3 of Table E.23). However, the effect ceases to be significant after adjustment for multiple hypothesis testing (adjusted p-value 0.21).

In the long-run PAP, we distinguish between Filipino and non-Filipino friends and acquaintances as well as close friends. Table E.24 shows long-term effects of the new PDOS on these components of the network size index. The treatment particularly reduces the number of Filipino friends and acquaintances and close friends. The effect is negative for non-Filipino friends, but not statistically significant. We do not find that the new PDOS affects the type of networks that migrants build in the U.S. (column 4 of Table E.23). The corresponding index is defined as a STE that summarizes whether the two closest new contacts in the U.S. have a college degree or higher and whether they are of non-Filipino ethnicity, whether the migrant has visited people of U.S. origin in their home, whether the migrant has received visitors of U.S. origin, and how often the migrant has received everyday favors from non-Filipino individuals. Similarly, the new PDOS has no effect on any other outcome domain.

Table E.26 tests for spillover effects on family members in the Philippines. We look at a range of outcomes: (i) an index that summarizes the respondents' perceived situation of the migrant in the U.S. in terms of meeting new people, social life, language skills, employment, degree recognition, adjusting to culture in the U.S., adjusting to weather in the U.S., dealing with U.S.

authorities, housing, and finances, (ii) family members' intention to travel to the U.S., (iii) family members' intention to emigrate to the U.S., (iv) respondents' perception that it would be good for young household members to live in the U.S., (v) respondents' perceived ease of living and finding a job in the U.S. her/himself, (vi) an index that summarizes respondents' perceived effect of migrant's emigration on the household in terms of financial security, standard of living, housing, health, education, family life, social life, and social status, (vii) the inverse hyperbolic sine amount of remittances received by the household. We find no evidence for spillover effects.

Table E.27 looks at secondary outcomes and mechanisms. It shows that the new PDOS, with or without employment module, does not affect the use of welfare programs in the U.S. or employment quality. There is also no evidence that the treatment helps migrants to initiate and complete the process of having their qualifications recognized.

Finally, we present results using data from the mid-term survey, following the short-term PAP (Table E.28) and the medium-term PAP (E.29). When the medium-term datum is not available, we replace it with the mid-term value. As before, we find that the new PDOS significantly reduces network size. However, the effect ceases to be significant after adjustment for multiple hypothesis testing.

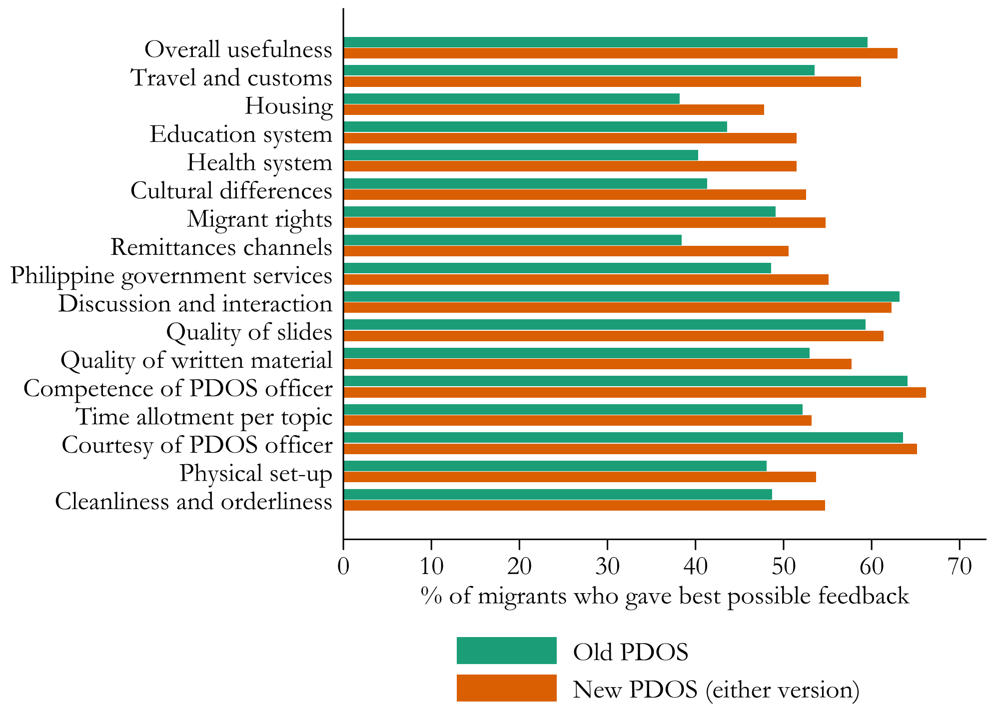


Figure E.1: Share of migrants giving best possible feedback right after PDOS
Note: Based on administrative feedback forms that migrants complete immediately after each PDOS. All PDOS attendees, not only those who are part of our sample, receive these feedback forms. Migrants rate various aspects of the PDOS on a scale from 1 (very poor) to 5 (excellent). On average, both the old and new PDOS receive very positive feedback. The figure therefore focuses on the share of migrants who give the best possible rating.

Table E.1: Average baseline characteristics and main short- and long-term outcomes by treatment status (old PDOS vs new PDOS)

	Old PDOS	New PDOS	All migrants	p-value
Baseline characteristics				
Age	33.073	33.391	33.266	0.558
Male	0.415	0.478	0.453	0.027
Vocational degree	0.072	0.082	0.078	0.521
College degree	0.465	0.473	0.470	0.787
Migrates alone	0.511	0.491	0.499	0.485
Migrates to California	0.383	0.429	0.411	0.106
Migrates to Hawaii	0.186	0.154	0.167	0.147
Daily internet user	0.641	0.601	0.617	0.153
English skills (0-1)	0.728	0.728	0.728	0.971
Has job in the US	0.202	0.170	0.182	0.156
Outcomes at first follow-up interview (after about 7 months in the US)				
Re-interviewed at first follow-up	0.866	0.868	0.867	0.934
Proxy interview (share among those re-interviewed)	0.387	0.415	0.404	0.332
Log days in the US	5.210	5.222	5.217	0.601
Travel-related problems (0-1)	0.020	0.008	0.013	0.025
Settlement index (0-1)	0.590	0.611	0.603	0.314
Employment index (STE)	-0.000	-0.057	-0.035	0.468
Network size index (STE)	0.000	-0.175	-0.105	0.002
Subjective wellbeing index (STE)	0.000	-0.022	-0.013	0.769
Outcomes at endline interview (after about 30 months in the US)				
Re-interviewed at endline	0.625	0.605	0.613	0.477
Proxy interview (share among those re-interviewed)	0.493	0.512	0.504	0.516
Log days in the US	6.429	6.455	6.445	0.440
Settlement index (0-1)	0.797	0.793	0.795	0.855
Employment index (STE)	-0.027	-0.119	-0.082	0.265
Network size index (STE)	-0.067	-0.188	-0.139	0.033
Subjective wellbeing index (STE)	-0.009	0.026	0.012	0.502

Note: The last column provides p-values from an F-test on equality of means of the respective variable between the two groups in columns 1 and 2.

Table E.2: Average baseline characteristics and main short-term and long-term outcomes by treatment status (old PDOS vs new PDOS with and without employment module)

	Old PDOS	New PDOS with emp. module	New PDOS without emp. module	All migrants	p-value
Baseline characteristics					
Age	33.073	33.193	33.620	33.266	0.704
Male	0.415	0.467	0.490	0.453	0.072
Vocational degree	0.072	0.070	0.095	0.078	0.408
College degree	0.465	0.504	0.437	0.470	0.177
Migrates alone	0.511	0.521	0.457	0.499	0.164
Migrates to California	0.383	0.424	0.435	0.411	0.258
Migrates to Hawaii	0.186	0.143	0.167	0.167	0.216
Daily internet user	0.641	0.603	0.599	0.617	0.359
English skills (0-1)	0.728	0.736	0.719	0.728	0.364
Has job in the US	0.202	0.174	0.164	0.182	0.339
Outcomes at first follow-up interview (after about 7 months in the US)					
Re-interviewed at first follow-up	0.866	0.850	0.889	0.867	0.273
Proxy interview (share among those re-interviewed)	0.387	0.397	0.435	0.404	0.362
Log days in the US	5.210	5.206	5.238	5.217	0.422
Travel-related problems (0-1)	0.020	0.007	0.010	0.013	0.073
Settlement index (0-1)	0.590	0.610	0.612	0.603	0.602
Employment index (STE)	-0.000	-0.096	-0.014	-0.035	0.527
Network size index (STE)	0.000	-0.150	-0.204	-0.105	0.004
Subjective wellbeing index (STE)	0.000	-0.058	0.019	-0.013	0.697
Outcomes at endline interview (after about 30 months in the US)					
Re-interviewed at endline	0.625	0.588	0.624	0.613	0.470
Proxy interview (share among those re-interviewed)	0.493	0.508	0.515	0.504	0.796
Log days in the US	6.429	6.439	6.473	6.445	0.521
Settlement index (0-1)	0.797	0.803	0.782	0.795	0.654
Employment index (STE)	-0.027	-0.146	-0.090	-0.082	0.474
Network size index (STE)	-0.067	-0.162	-0.218	-0.139	0.063
Subjective wellbeing index (STE)	-0.009	0.030	0.022	0.012	0.788

Note: The last column provides p-values from an F-test on equality of means of the respective variable between the three groups in columns 1-3.

Table E.3: Average baseline characteristics and main short-term and long-term outcomes by treatment status (old PDOS vs new PDOS with and without association email)

	Old PDOS	New PDOS with ass. email	New PDOS without ass. email	All migrants	p-value
Baseline characteristics					
Age	32.060	31.598	33.062	32.192	0.208
Male	0.413	0.474	0.478	0.451	0.178
Vocational degree	0.084	0.100	0.063	0.083	0.287
College degree	0.483	0.488	0.530	0.498	0.487
Migrates alone	0.503	0.467	0.498	0.490	0.639
Migrates to California	0.466	0.557	0.498	0.504	0.071
Migrates to Hawaii	0.198	0.137	0.190	0.176	0.083
Daily internet user	0.684	0.698	0.648	0.678	0.456
English skills (0-1)	0.742	0.735	0.758	0.744	0.247
Has job in the US	0.168	0.179	0.138	0.163	0.404
Outcomes at first follow-up interview (after about 7 months in the US)					
Re-interviewed at first follow-up	0.866	0.876	0.838	0.861	0.436
Proxy interview (share among those re-interviewed)	0.383	0.395	0.415	0.396	0.725
Log days in the US	5.204	5.225	5.232	5.218	0.630
Travel-related problems (0-1)	0.023	0.005	0.012	0.014	0.012
Settlement index (0-1)	0.586	0.596	0.656	0.607	0.059
Employment index (STE)	0.011	-0.118	0.022	-0.026	0.385
Network size index (STE)	0.004	-0.136	-0.184	-0.091	0.055
Subjective wellbeing index (STE)	-0.050	0.040	-0.133	-0.041	0.331
Outcomes at endline interview (after about 30 months in the US)					
Re-interviewed at endline	0.606	0.608	0.569	0.596	0.584
Proxy interview (share among those re-interviewed)	0.483	0.464	0.502	0.482	0.675
Log days in the US	6.414	6.428	6.439	6.425	0.880
Settlement index (0-1)	0.804	0.796	0.813	0.804	0.807
Employment index (STE)	-0.059	0.003	-0.090	-0.046	0.730
Network size index (STE)	-0.023	-0.145	-0.196	-0.109	0.096
Subjective wellbeing index (STE)	-0.031	-0.027	0.057	-0.005	0.426

Note: The last column provides p-values from an F-test on equality of means of the respective variable between the three groups in columns 1-3.

Table E.4: Relationship between the network size index and other outcome indices

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Subjective wellbeing index (STE)
All migrants	0.056	0.142	0.027
	(0.013)	(0.051)	(0.041)
Old PDOS	0.055	0.140	0.068
	(0.018)	(0.078)	(0.068)
New PDOS	0.059	0.139	0.007
	(0.018)	(0.068)	(0.054)

Note: Coefficient estimates from regressions of the settlement, employment, and subjective wellbeing indices on the network size index. Each cell in table shows coefficient (standard error) on network size index in a separate regression. In first row, estimates are from the full sample. In second and third rows, estimates are from regressions run separately in control group and treatment group, respectively. No other variables included in regression. Data come from the long-term survey. Standard errors clustered at the PDOS session level in parentheses.

Table E.5: First follow-up interview: Attrition and mode of re-interview

	(1) Successful re-interview	(2) Successful re-interview	(3) Successful re-interview	(4) Direct re-interview	(5) Direct re-interview	(6) Direct re-interview	(7) Proxy re-interview	(8) Proxy re-interview	(9) Proxy re-interview
New PDOS (either version)	0.001	0.019	-0.030	-0.025	-0.047	-0.034	0.025	0.047	0.034
	(0.021)	(0.024)	(0.027)	(0.028)	(0.035)	(0.041)	(0.028)	(0.035)	(0.041)
New PDOS with emp. module		-0.034			0.042			-0.042	
		(0.029)			(0.039)			(0.039)	
New PDOS with ass. email			0.033			0.028			-0.028
			(0.027)			(0.037)			(0.037)
Age	-0.004	-0.004	-0.015	0.009	0.009	0.008	-0.009	-0.009	-0.008
	(0.008)	(0.008)	(0.011)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)	(0.014)
Age squared	0.000	0.000	0.000	-0.000	-0.000	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Male	-0.002	-0.003	-0.002	0.001	0.002	-0.000	-0.001	-0.002	0.000
	(0.018)	(0.018)	(0.022)	(0.029)	(0.029)	(0.033)	(0.029)	(0.029)	(0.033)
Vocational education	0.061	0.060	0.119	-0.023	-0.021	-0.047	0.023	0.021	0.047
	(0.029)	(0.029)	(0.026)	(0.055)	(0.055)	(0.061)	(0.055)	(0.055)	(0.061)
College education	0.006	0.007	0.019	-0.008	-0.009	0.008	0.008	0.009	-0.008
	(0.022)	(0.022)	(0.026)	(0.031)	(0.031)	(0.035)	(0.031)	(0.031)	(0.035)
Migrates alone	-0.015	-0.014	-0.022	-0.015	-0.016	-0.023	0.015	0.016	0.023
	(0.018)	(0.018)	(0.023)	(0.025)	(0.025)	(0.031)	(0.025)	(0.025)	(0.031)
California	0.002	0.002	0.019	0.011	0.012	0.016	-0.011	-0.012	-0.016
	(0.021)	(0.021)	(0.028)	(0.029)	(0.029)	(0.034)	(0.029)	(0.029)	(0.034)
Hawaii	0.010	0.009	0.024	0.118	0.120	0.097	-0.118	-0.120	-0.097
	(0.029)	(0.029)	(0.035)	(0.037)	(0.037)	(0.043)	(0.037)	(0.037)	(0.043)
Daily internet use	-0.000	-0.001	-0.004	0.013	0.013	0.028	-0.013	-0.013	-0.028
	(0.019)	(0.019)	(0.024)	(0.036)	(0.036)	(0.042)	(0.036)	(0.036)	(0.042)
English skills	-0.010	-0.010	-0.010	0.001	0.000	-0.001	-0.001	-0.000	0.001
	(0.005)	(0.005)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)
Has job in US	0.033	0.033	0.037	-0.078	-0.079	-0.060	0.078	0.079	0.060
	(0.027)	(0.027)	(0.034)	(0.040)	(0.040)	(0.050)	(0.040)	(0.040)	(0.050)
Constant	1.026	1.025	1.176	0.467	0.468	0.454	0.533	0.532	0.546
	(0.150)	(0.150)	(0.193)	(0.216)	(0.215)	(0.253)	(0.216)	(0.215)	(0.253)
F-statistic treatment variables=0	0.001	0.758	0.942	0.754	0.963	0.396	0.754	0.963	0.396
p-value	0.975	0.471	0.393	0.387	0.385	0.674	0.387	0.385	0.674
R2	0.011	0.012	0.024	0.014	0.015	0.011	0.014	0.015	0.011
Observations	1273	1273	902	1273	1273	902	1273	1273	902

Note: The table reports OLS estimates. The column title shows the dependent variable. Standard errors clustered at the PDOS session level in parentheses. */**/*** denote statistical significance at the 10/5/1 percent level.

Table E.6: First follow-up interview: Attrition by outcome domain

	(1) Successful re-interview	(2) Travel-related problems observed	(3) Settlement observed	(4) Employment observed	(5) Networks observed	(6) Wellbeing observed
PANEL A						
New PDOS (either version)	0.001	0.002	-0.033	-0.010	-0.018	-0.035
	(0.021)	(0.010)	(0.026)	(0.025)	(0.028)	(0.030)
F-statistic treatment variables=0	0.001	0.030	1.687	0.170	0.391	1.395
p-value	0.975	0.862	0.197	0.681	0.533	0.240
R2	0.011	0.014	0.017	0.121	0.017	0.019
Observations	1273	1104	1104	1104	1104	1104
PANEL B						
New PDOS (either version)	0.019	0.003	-0.044	-0.017	-0.039	-0.046
	(0.024)	(0.012)	(0.035)	(0.031)	(0.037)	(0.036)
New PDOS with emp. module	-0.034	-0.003	0.020	0.013	0.041	0.021
	(0.029)	(0.012)	(0.040)	(0.034)	(0.040)	(0.042)
F-statistic treatment variables=0	0.758	0.040	0.901	0.159	0.635	0.841
p-value	0.471	0.961	0.409	0.853	0.532	0.434
R2	0.012	0.014	0.017	0.121	0.018	0.019
Observations	1273	1104	1104	1104	1104	1104
PANEL C						
New PDOS (either version)	-0.030	-0.010	-0.079	-0.011	-0.038	-0.056
	(0.027)	(0.016)	(0.046)	(0.037)	(0.044)	(0.045)
New PDOS with ass. email	0.033	0.012	0.050	-0.019	0.045	0.049
	(0.027)	(0.015)	(0.049)	(0.038)	(0.043)	(0.044)
F-statistic treatment variables=0	0.942	0.321	1.465	0.388	0.572	0.840
p-value	0.393	0.726	0.236	0.680	0.566	0.435
R2	0.024	0.016	0.020	0.121	0.014	0.018
Observations	902	777	777	777	777	777

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach.

Table E.7: First follow-up interview: Direct interview for observed outcome domain

	(1) Direct re-interview among re-interviewed	(2) Direct re-interview among travel-related problems observed	(3) Direct re-interview among settlement observed	(4) Direct re-interview among network observed
PANEL A				
New PDOS (either version)	-0.029	-0.035	0.000	-0.028
	(0.031)	(0.030)	(0.032)	(0.021)
F-statistic treatment variables=0	0.893	1.355	0.000	1.771
p-value	0.347	0.247	0.996	0.186
R2	0.018	0.019	0.013	0.032
Observations	1104	1077	728	614
PANEL B				
New PDOS (either version)	-0.044	-0.049	-0.008	-0.006
	(0.037)	(0.037)	(0.036)	(0.021)
New PDOS with emp. module	0.028	0.026	0.016	-0.039
	(0.042)	(0.041)	(0.039)	(0.029)
F-statistic treatment variables=0	0.716	0.903	0.081	1.360
p-value	0.491	0.408	0.922	0.261
R2	0.019	0.019	0.013	0.035
Observations	1104	1077	728	614
PANEL C				
New PDOS (either version)	-0.054	-0.050	0.001	-0.031
	(0.046)	(0.046)	(0.051)	(0.029)
New PDOS with ass. email	0.048	0.042	0.044	0.004
	(0.043)	(0.042)	(0.049)	(0.033)
F-statistic treatment variables=0	0.792	0.655	0.780	0.921
p-value	0.456	0.521	0.461	0.401
R2	0.018	0.017	0.009	0.053
Observations	777	755	515	436

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. Employment and wellbeing outcomes were only collected in direct interviews. Hence, we cannot report results for these outcomes.

Table E.8: Short-term effects (after about seven months in the U.S.), direct interviews only

	(1) Travel-related problems (0-1)	(2) Settlement index (0-1)	(3) Employment index (STE)	(4) Network index (STE)	(5) Subjective wellbeing index (STE)
PANEL A					
New PDOS (either version)	-0.010	0.038	-0.012	-0.181	-0.020
	(0.007)	(0.019)	(0.070)	(0.059)	(0.076)
MHT-adjusted p-value	0.632	0.302	0.965	0.027	0.981
Mean outcome control group	0.021	0.535	-0.000	0.053	0.000
R2	0.021	0.144	0.130	0.083	0.072
Observations	579	565	362	570	578
PANEL B					
New PDOS (either version)			0.016		
			(0.090)		
New PDOS with emp. module			-0.053		
			(0.095)		
MHT-adjusted p-value treatment			0.868		
MHT-adjusted p-value interacted treatment			0.927		
R2			0.130		
Observations			362		
PANEL C					
New PDOS (either version)				-0.232	
				(0.081)	
New PDOS with ass. email				0.076	
				(0.081)	
MHT-adjusted p-value treatment				0.033	
MHT-adjusted p-value interacted treatment				0.844	
R2				0.091	
Observations				406	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.9: Short-term effects (after about 7 months in the U.S.): Components of travel-related-problems index

	Index	Index components				
	(1)	(2)	(3)	(4)	(5)	(6)
	Travel-related problems (0-1)	Missed flight	Luggage problem	Customs problem	PH authorities problem	US authorities problem
PANEL A						
New PDOS (either version)	-0.012	-0.020	-0.015	-0.009	-0.009	-0.005
	(0.006)	(0.011)	(0.010)	(0.007)	(0.005)	(0.005)
MHT-adjusted p-value		0.283	0.336	0.316	0.286	0.364
Mean outcome control group	0.020	0.038	0.024	0.019	0.012	0.009
R2	0.021	0.024	0.017	0.014	0.017	0.020
Observations	1077	1077	1077	1077	1077	1077

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.10: Short-term effects (after about 7 months in the U.S.): Components of the network size index and alternative network measures

	Index	Index components		Alternative network measures			
	(1) Network size index (STE)	(2) IHS nr of friends and acquaintances	(3) Received support from association	(4) Logged nr of friends and acquaintances	(5) Nr of friends and acquaintances	(6) Nr of friends and acquaintances winsored at p90	(7) Contacted association
PANEL A							
New PDOS (either	-0.169	-0.295	-0.032	-0.256	-3.437	-2.417	-0.050
version)	(0.056)	(0.124)	(0.014)	(0.102)	(1.035)	(0.757)	(0.025)
MHT-adjusted p-value		0.104	0.123				
Mean outcome control group	0.000	2.424	0.049	1.976	12.988	11.348	0.123
R2	0.166	0.203	0.053	0.199	0.124	0.147	0.056
Observations	614	614	614	614	614	614	608
PANEL C							
New PDOS (either	-0.223	-0.444	-0.035	-0.375	-3.677	-2.973	-0.106
version)	(0.078)	(0.181)	(0.021)	(0.149)	(1.286)	(0.991)	(0.029)
New PDOS with ass.	0.092	0.256	0.004	0.210	1.219	1.360	0.062
email	(0.077)	(0.170)	(0.021)	(0.141)	(1.370)	(1.080)	(0.029)
MHT-adjusted p-value treatment		0.093	0.256				
MHT-adjusted p-value interacted treatment		0.233	0.848				
R2	0.165	0.206	0.064	0.202	0.134	0.156	0.102
Observations	436	436	436	436	436	436	431

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D. IHS is short for inverse hyperbolic sine.

Table E.11: Short-term effects (after about 7 months in the U.S.): Impact heterogeneity by college degree

	(1) Travel-related problems (0-1)	(2) Settlement index (0-1)	(3) Employment index (STE)	(4) Network index (STE)	(5) Subjective wellbeing index (STE)
PANEL A					
New PDOS (either version)	-0.012	-0.010	-0.086	-0.116	-0.186
	(0.007)	(0.024)	(0.099)	(0.066)	(0.094)
New PDOS x college degree	0.002	0.082	0.166	-0.122	0.379
	(0.011)	(0.038)	(0.155)	(0.099)	(0.147)
MHT-adjusted p-value interaction	0.892	0.216	0.710	0.682	0.097
Mean outcome control group	0.020	0.590	-0.000	0.000	0.000
R2	0.021	0.228	0.133	0.168	0.083
Observations	1077	728	362	614	578
PANEL B					
New PDOS (either version)			-0.123		
			(0.130)		
New PDOS with emp. module			0.076		
			(0.126)		
New PDOS x college degree			0.303		
			(0.204)		
New PDOS with emp. module x college degree			-0.267		
			(0.180)		
MHT-adjusted p-value interaction 1			0.523		
MHT-adjusted p-value interaction 2			0.570		
R2			0.138		
Observations			362		
PANEL C					
New PDOS (either version)				-0.143	
				(0.111)	
New PDOS with ass. email				0.053	
				(0.094)	
New PDOS x college degree				-0.163	
				(0.157)	
New PDOS with ass. email x college degree				0.076	
				(0.144)	
MHT-adjusted p-value interaction 1				0.626	
MHT-adjusted p-value interaction 2				0.851	
R2				0.167	
Observations				436	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.12: Short-term effects (after about 7 months in the U.S.): Impact heterogeneity by gender

	(1) Travel-related problems (0-1)	(2) Settlement index (0-1)	(3) Employment index (STE)	(4) Network index (STE)	(5) Subjective wellbeing index (STE)
PANEL A					
New PDOS (either version)	-0.007	0.043	-0.004	-0.155	-0.054
	(0.008)	(0.024)	(0.088)	(0.069)	(0.101)
New PDOS x male	-0.010	-0.034	-0.022	-0.032	0.079
	(0.011)	(0.038)	(0.133)	(0.102)	(0.155)
MHT-adjusted p-value interaction	0.961	0.973	0.877	0.985	0.994
Mean outcome control group	0.020	0.590	-0.000	0.000	0.000
R2	0.022	0.224	0.130	0.166	0.072
Observations	1077	728	362	614	578
PANEL B					
New PDOS (either version)			-0.007		
			(0.116)		
New PDOS with emp. module			0.006		
			(0.111)		
New PDOS x male			0.047		
			(0.142)		
New PDOS with emp. module x male			-0.132		
			(0.157)		
MHT-adjusted p-value interaction 1			0.992		
MHT-adjusted p-value interaction 2			0.945		
R2			0.132		
Observations			362		
PANEL C					
New PDOS (either version)				-0.255	
				(0.118)	
New PDOS with ass. email				0.109	
				(0.116)	
New PDOS x male				0.073	
				(0.165)	
New PDOS with ass. email x male				-0.034	
				(0.155)	
MHT-adjusted p-value interaction 1				0.990	
MHT-adjusted p-value interaction 2				0.970	
R2				0.166	
Observations				436	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.13: Short-term effects (after about 7 months in the U.S.): Impact heterogeneity by baseline knowledge

	(1) Travel-related problems (0-1)	(2) Settlement index (0-1)	(3) Employment index (STE)	(4) Network index (STE)	(5) Subjective wellbeing index (STE)
PANEL A					
New PDOS (either	-0.011	0.056	0.002	-0.268	-0.015
version)	(0.008)	(0.029)	(0.114)	(0.092)	(0.115)
New PDOS x	-0.000	-0.049	-0.025	0.171	-0.010
below-median baseline knowledge	(0.009)	(0.036)	(0.161)	(0.107)	(0.143)
MHT-adjusted p-value					
interaction	0.980	0.683	0.998	0.605	0.996
Mean outcome control group	0.020	0.590	-0.000	0.000	0.000
R2	0.021	0.226	0.135	0.174	0.075
Observations	1077	728	362	614	578
PANEL B					
New PDOS (either			0.056		
version)			(0.141)		
New PDOS with emp.			-0.100		
module			(0.145)		
New PDOS x			-0.067		
below-median baseline knowledge			(0.190)		
New PDOS with emp.			0.078		
module x below-median baseline knowledge			(0.193)		
MHT-adjusted p-value					
interaction 1			0.990		
MHT-adjusted p-value					
interaction 2			0.998		
R2			0.136		
Observations			362		
PANEL C					
New PDOS (either				-0.263	
version)				(0.129)	
New PDOS with ass.				-0.023	
email				(0.113)	
New PDOS x				0.064	
below-median baseline knowledge				(0.173)	
New PDOS with ass.				0.203	
email x below-median baseline knowledge				(0.144)	
MHT-adjusted p-value					
interaction 1				0.996	
MHT-adjusted p-value					
interaction 2				0.683	
R2				0.178	
Observations				436	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.14: Short-term effects (after about 7 months in the U.S.): Mechanisms

	(1) Job search index (STE)	(2) Job search knowledge index (STE)	(3) Network establishment index (STE)	(4) Agreed with hh on amount of remittances
PANEL A				
New PDOS (either version)	-0.133	-0.029	-0.082	-0.023
	(0.103)	(0.062)	(0.043)	(0.028)
Mean outcome control group	-0.000	0.000	-0.000	0.213
R2	0.163	0.156	0.064	0.036
Observations	280	579	788	1077
PANEL B				
New PDOS (either version)	-0.033	-0.125		
	(0.127)	(0.070)		
New PDOS with emp. module	-0.184	0.179		
	(0.110)	(0.071)		
R2	0.172	0.163		
Observations	280	579		
PANEL C				
New PDOS (either version)			-0.081	
			(0.064)	
New PDOS with ass. email			-0.003	
			(0.055)	
R2			0.077	
Observations			552	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach.

Table E.15: Endline interview: Attrition and mode of re-interview

	(1) Successful re-interview	(2) Successful re-interview	(3) Successful re-interview	(4) Direct re-interview	(5) Direct re-interview	(6) Direct re-interview	(7) Proxy re-interview	(8) Proxy re-interview	(9) Proxy re-interview
New PDOS (either version)	-0.017	-0.010	-0.031	-0.036	-0.037	-0.025	0.022	0.025	0.027
	(0.015)	(0.018)	(0.022)	(0.025)	(0.033)	(0.036)	(0.030)	(0.035)	(0.044)
New PDOS with emp. module		-0.013			0.003			-0.005	
		(0.022)			(0.036)			(0.037)	
New PDOS with ass. email			0.015			0.004			-0.041
			(0.024)			(0.035)			(0.040)
Age	0.013	0.013	0.004	0.011	0.011	0.008	0.005	0.005	0.005
	(0.007)	(0.007)	(0.009)	(0.012)	(0.012)	(0.014)	(0.012)	(0.012)	(0.014)
Age squared	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Male	-0.020	-0.021	-0.017	0.004	0.004	0.003	-0.052	-0.052	-0.048
	(0.014)	(0.014)	(0.019)	(0.027)	(0.027)	(0.031)	(0.028)	(0.028)	(0.035)
Vocational degree	0.019	0.019	0.044	-0.007	-0.007	-0.018	-0.012	-0.012	-0.013
	(0.019)	(0.019)	(0.022)	(0.050)	(0.050)	(0.056)	(0.058)	(0.058)	(0.067)
College degree	-0.017	-0.016	-0.016	0.008	0.008	0.015	-0.066	-0.066	-0.055
	(0.017)	(0.017)	(0.021)	(0.029)	(0.029)	(0.035)	(0.030)	(0.031)	(0.037)
Migrates alone	-0.004	-0.003	-0.017	-0.026	-0.026	-0.028	0.034	0.034	0.031
	(0.015)	(0.015)	(0.019)	(0.025)	(0.025)	(0.031)	(0.031)	(0.030)	(0.034)
California	0.019	0.019	0.032	0.027	0.027	0.026	-0.010	-0.010	-0.016
	(0.017)	(0.017)	(0.024)	(0.025)	(0.025)	(0.030)	(0.029)	(0.029)	(0.037)
Hawaii	0.038	0.038	0.055	0.095	0.095	0.076	-0.049	-0.049	-0.025
	(0.021)	(0.021)	(0.030)	(0.036)	(0.036)	(0.045)	(0.045)	(0.045)	(0.054)
Daily internet use	-0.021	-0.021	-0.019	-0.021	-0.021	-0.020	-0.036	-0.036	-0.023
	(0.016)	(0.016)	(0.021)	(0.032)	(0.032)	(0.037)	(0.036)	(0.036)	(0.043)
English skills	-0.097	-0.096	-0.093	-0.007	-0.007	-0.054	-0.095	-0.095	-0.058
	(0.055)	(0.055)	(0.074)	(0.088)	(0.088)	(0.107)	(0.099)	(0.098)	(0.124)
Has job in US	0.040	0.040	0.044	-0.043	-0.043	-0.019	0.037	0.037	0.034
	(0.020)	(0.020)	(0.025)	(0.034)	(0.034)	(0.036)	(0.037)	(0.037)	(0.042)
Constant	0.798	0.798	0.911	0.557	0.557	0.598	0.525	0.525	0.503
	(0.125)	(0.125)	(0.164)	(0.204)	(0.203)	(0.239)	(0.192)	(0.192)	(0.240)
F-statistic treatment variables=0	1.249	0.709	0.961	2.105	1.053	0.310	0.530	0.283	0.531
p-value	0.266	0.495	0.386	0.150	0.352	0.734	0.468	0.754	0.590
R2	0.024	0.025	0.027	0.011	0.011	0.008	0.013	0.013	0.008
Observations	1273	1273	902	1273	1273	902	1273	1273	902

Note: The table reports OLS estimates. The column title shows the dependent variable. Standard errors clustered at the PDOS session level in parentheses. */**/*** denote statistical significance at the 10/5/1 percent level.

Table E.16: Endline interview: Attrition by outcome domain

	(1) Successful re-interview	(2) Settlement observed	(3) Employment observed	(4) Network size observed	(5) Wellbeing observed
PANEL A					
New PDOS (either	-0.017	-0.019	-0.021	-0.015	-0.005
version)	(0.015)	(0.021)	(0.024)	(0.026)	(0.024)
F-statistic treatment variables=0	1.249	0.851	0.785	0.325	0.037
p-value	0.266	0.358	0.378	0.570	0.848
R2	0.024	0.008	0.249	0.010	0.021
Observations	1273	1176	1176	1176	1176
PANEL B					
New PDOS (either	-0.010	-0.038	-0.019	-0.016	0.006
version)	(0.018)	(0.027)	(0.031)	(0.035)	(0.027)
New PDOS with emp.	-0.013	0.035	-0.003	0.003	-0.020
module	(0.022)	(0.029)	(0.036)	(0.036)	(0.027)
F-statistic treatment variables=0	0.709	1.045	0.399	0.163	0.276
p-value	0.495	0.355	0.672	0.850	0.759
R2	0.025	0.009	0.249	0.010	0.021
Observations	1273	1176	1176	1176	1176
PANEL C					
New PDOS (either	-0.031	0.001	-0.056	-0.031	-0.011
version)	(0.022)	(0.033)	(0.035)	(0.041)	(0.033)
New PDOS with ass.	0.015	-0.040	0.074	0.035	0.006
email	(0.024)	(0.035)	(0.035)	(0.039)	(0.034)
F-statistic treatment variables=0	0.961	0.996	2.397	0.429	0.050
p-value	0.386	0.373	0.096	0.652	0.951
R2	0.027	0.009	0.235	0.013	0.023
Observations	902	823	823	823	823

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach.

Table E.17: Endline interview: Direct interview for observed outcome domain

	(1) Direct re-interview among re-interviewed	(2) Direct re-interview among settlement observed	(3) Direct re-interview among employment observed	(4) Direct re-interview among network size observed	(5) Direct re-interview among wellbeing observed
PANEL A					
New PDOS (either version)	-0.046	-0.023	-0.031	-0.018	-0.040
	(0.026)	(0.025)	(0.026)	(0.014)	(0.024)
F-statistic treatment variables=0	3.193	0.859	1.513	1.621	2.775
p-value	0.077	0.356	0.221	0.206	0.099
R2	0.017	0.012	0.020	0.016	0.010
Observations	1176	989	601	751	917
PANEL B					
New PDOS (either version)	-0.045	0.003	-0.011	-0.000	-0.042
	(0.034)	(0.033)	(0.029)	(0.015)	(0.031)
New PDOS with emp. module	-0.002	-0.048	-0.038	-0.033	0.005
	(0.037)	(0.034)	(0.033)	(0.018)	(0.036)
F-statistic treatment variables=0	1.616	1.618	1.309	2.049	1.390
p-value	0.203	0.203	0.274	0.134	0.253
R2	0.017	0.014	0.022	0.019	0.010
Observations	1176	989	601	751	917
PANEL C					
New PDOS (either version)	-0.039	-0.039	-0.008	-0.006	-0.028
	(0.038)	(0.037)	(0.038)	(0.020)	(0.035)
New PDOS with ass. email	0.010	0.055	-0.024	-0.012	-0.007
	(0.040)	(0.039)	(0.045)	(0.026)	(0.040)
F-statistic treatment variables=0	0.639	1.010	0.433	0.434	0.719
p-value	0.530	0.368	0.650	0.649	0.490
R2	0.015	0.018	0.018	0.027	0.010
Observations	823	696	428	533	637

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach.

Table E.18: Long-term effects (after about 30 months in the U.S.), direct interviews only

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Subjective wellbeing index (STE)
PANEL A				
New PDOS (either version)	0.018	0.050	-0.134	0.051
	(0.021)	(0.094)	(0.065)	(0.062)
MHT-adjusted p-value treatment	0.877	0.844	0.264	0.805
Mean outcome control group	0.750	0.031	-0.021	0.005
R2	0.337	0.113	0.092	0.044
Observations	527	399	526	527
PANEL B				
New PDOS (either version)		0.057		
		(0.107)		
New PDOS with emp. module		-0.015		
		(0.098)		
MHT-adjusted p-value treatment		0.847		
MHT-adjusted p-value interacted treatment		0.876		
R2		0.113		
Observations		399		
PANEL C				
New PDOS (either version)			-0.252	
			(0.103)	
New PDOS with ass. email			0.139	
			(0.105)	
MHT-adjusted p-value treatment			0.105	
MHT-adjusted p-value interacted treatment			0.669	
R2			0.121	
Observations			381	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.19: Long-term effects (after about 30 months in the U.S.): Components of the network size index and alternative network measures

	Index	Index components		Alternative network measures		
	(1)	(2)	(3)	(4)	(5)	(6)
	Network size index (STE)	IHS nr of friends and acquaintances	Contacted association	Logged nr of friends and acquaintances	Nr of friends and acquaintances	Nr of friends and acquaintances winsored at p90
PANEL A						
New PDOS (either version)	-0.136	-0.176	-0.015	-0.160	0.373	-4.377
	(0.053)	(0.097)	(0.036)	(0.086)	(9.081)	(2.554)
MHT-adjusted p-value		0.336	0.892			
Mean outcome control group	-0.067	4.001	0.657	3.398	55.350	46.238
R2	0.108	0.383	0.192	0.383	0.089	0.280
Observations	751	751	751	751	751	751
PANEL C						
New PDOS (either version)	-0.238	-0.206	-0.038	-0.193	12.190	-7.468
	(0.080)	(0.122)	(0.050)	(0.110)	(21.668)	(3.401)
New PDOS with ass. email	0.095	-0.079	0.014	-0.053	-15.175	4.254
	(0.079)	(0.137)	(0.056)	(0.122)	(17.220)	(3.114)
MHT-adjusted p-value treatment		0.368	0.857			
MHT-adjusted p-value interacted treatment		0.925	0.813			
R2	0.139	0.405	0.221	0.405	0.105	0.303
Observations	533	533	533	533	533	533

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.20: Long-term effects (after about 30 months in the U.S.): Impact heterogeneity by college degree

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Subjective wellbeing index (STE)
PANEL A				
New PDOS (either version)	0.002	-0.128	-0.067	-0.074
	(0.024)	(0.106)	(0.069)	(0.061)
New PDOS x college degree	-0.023	0.148	-0.154	0.247
	(0.029)	(0.146)	(0.113)	(0.105)
MHT-adjusted p-value interaction	0.669	0.683	0.644	0.139
Mean outcome control group	0.797	-0.027	-0.067	-0.009
R2	0.235	0.136	0.110	0.038
Observations	989	601	751	917
PANEL B				
New PDOS (either version)		-0.113		
		(0.123)		
New PDOS with emp. module		-0.030		
		(0.123)		
New PDOS x college degree		0.149		
		(0.171)		
New PDOS with emp. module x college degree		-0.000		
		(0.158)		
MHT-adjusted p-value interaction 1		0.750		
MHT-adjusted p-value interaction 2		0.997		
R2		0.136		
Observations		601		
PANEL C				
New PDOS (either version)			-0.103	
			(0.113)	
New PDOS with ass. email			-0.015	
			(0.114)	
New PDOS x college degree			-0.278	
			(0.169)	
New PDOS with ass. email x college degree			0.225	
			(0.176)	
MHT-adjusted p-value interaction 1			0.472	
MHT-adjusted p-value interaction 2			0.610	
R2			0.144	
Observations			533	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.21: Long-term effects (after about 30 months in the U.S.): Impact heterogeneity by gender

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Subjective wellbeing index (STE)
PANEL A				
New PDOS (either version)	0.014	-0.152	-0.209	0.075
	(0.021)	(0.113)	(0.076)	(0.070)
New PDOS x male	-0.051	0.228	0.172	-0.092
	(0.032)	(0.160)	(0.114)	(0.107)
MHT-adjusted p-value interaction	0.535	0.510	0.562	0.770
Mean outcome control group	0.797	-0.027	-0.067	-0.009
R2	0.236	0.137	0.111	0.033
Observations	989	601	751	917
PANEL B				
New PDOS (either version)		-0.128		
		(0.131)		
New PDOS with emp. module		-0.044		
		(0.134)		
New PDOS x male		0.200		
		(0.203)		
New PDOS with emp. module x male		0.051		
		(0.204)		
MHT-adjusted p-value interaction 1		0.773		
MHT-adjusted p-value interaction 2		0.808		
R2		0.137		
Observations		601		
PANEL C				
New PDOS (either version)			-0.360	
			(0.119)	
New PDOS with ass. email			0.129	
			(0.111)	
New PDOS x male			0.267	
			(0.176)	
New PDOS with ass. email x male			-0.054	
			(0.140)	
MHT-adjusted p-value interaction 1			0.529	
MHT-adjusted p-value interaction 2			0.912	
R2			0.144	
Observations			533	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.22: Long-term effects (after about 30 months in the U.S.): Impact heterogeneity by baseline knowledge

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Subjective wellbeing index (STE)
PANEL A				
New PDOS (either	0.019	0.005	-0.118	0.080
version)	(0.021)	(0.136)	(0.093)	(0.083)
New PDOS x	-0.048	-0.121	-0.033	-0.081
below-median baseline knowledge	(0.028)	(0.169)	(0.111)	(0.109)
MHT-adjusted p-value				
interaction	0.483	0.929	0.764	0.958
Mean outcome control group	0.797	-0.027	-0.067	-0.009
R2	0.239	0.135	0.111	0.036
Observations	989	601	751	917
PANEL B				
New PDOS (either		-0.010		
version)		(0.149)		
New PDOS with emp.		0.031		
module		(0.131)		
New PDOS x		-0.066		
below-median baseline knowledge		(0.195)		
New PDOS with emp.		-0.107		
module x below-median baseline knowledge		(0.204)		
MHT-adjusted p-value				
interaction 1		0.934		
MHT-adjusted p-value				
interaction 2		0.974		
R2		0.136		
Observations		601		
PANEL C				
New PDOS (either			-0.160	
version)			(0.137)	
New PDOS with ass.			0.054	
email			(0.118)	
New PDOS x			-0.144	
below-median baseline knowledge			(0.171)	
New PDOS with ass.			0.075	
email x below-median baseline knowledge			(0.147)	
MHT-adjusted p-value				
interaction 1			0.937	
MHT-adjusted p-value				
interaction 2			0.938	
R2			0.144	
Observations			533	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.23: Long-term effects (after about 30 months in the U.S.) following long-term PAP

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Network type index (STE)	(5) Diaspora engagement index (STE)	(6) Subjective wellbeing index (STE)	(7) Financial decision-making index (STE)
PANEL A							
New PDOS (either	-0.003	-0.059	-0.154	-0.031	0.019	0.030	0.009
version)	(0.019)	(0.087)	(0.063)	(0.046)	(0.055)	(0.044)	(0.058)
MHT-adjusted p-value	0.983	0.991	0.209	0.998	1.000	0.990	0.881
Mean outcome control group	0.765	-0.038	-0.072	0.013	-0.033	-0.014	-0.022
R2	0.266	0.121	0.083	0.108	0.069	0.036	0.072
Observations	965	705	751	692	585	881	464
PANEL B							
New PDOS (either		-0.048					
version)		(0.101)					
New PDOS with emp.		-0.021					
module		(0.093)					
MHT-adjusted p-value treatment		0.996					
MHT-adjusted p-value interacted treatment		1.000					
R2		0.121					
Observations		705					
PANEL C							
New PDOS (either			-0.267	-0.052	0.016		
version)			(0.094)	(0.069)	(0.078)		
New PDOS with ass.			0.142	-0.043	-0.119		
email			(0.083)	(0.064)	(0.091)		
MHT-adjusted p-value treatment			0.072	0.997	0.996		
MHT-adjusted p-value interacted treatment			0.615	0.995	0.888		
R2			0.119	0.091	0.084		
Observations			533	494	412		

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D. See long-term PAP for definition of indices.

Table E.24: Long-term effects (after about 30 months in the U.S.): Components of the network size index following long-term PAP

	Index	Index components		
	(1)	(2)	(3)	(4)
	Network size index (STE)	IHS nr of Filipino friends and acquaintances	IHS nr of non-Filipino friends and acquaintances	IHS nr of close friends
PANEL A				
New PDOS (either version)	-0.154	-0.168	-0.110	-0.182
	(0.063)	(0.077)	(0.102)	(0.090)
MHT-adjusted p-value		0.199	0.295	0.224
Mean outcome control group	-0.072	3.903	3.453	2.951
R2	0.083	0.109	0.123	0.113
Observations	751	591	590	474
PANEL C				
New PDOS (either version)	-0.267	-0.257	-0.263	-0.315
	(0.094)	(0.124)	(0.135)	(0.153)
New PDOS with ass. email	0.142	0.160	0.195	0.301
	(0.083)	(0.121)	(0.145)	(0.135)
MHT-adjusted p-value treatment		0.201	0.199	0.187
MHT-adjusted p-value interacted treatment		0.351	0.460	0.209
R2	0.119	0.132	0.156	0.130
Observations	533	419	417	341

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D. IHS is short for inverse hyperbolic sine.

Table E.25: Long-term effects (after about 30 months in the U.S.): Components of the network type index following long-term PAP

	Index	Index components				
	(1) Network type index (STE)	(2) College degree two closest contacts in US	(3) Non-Filipino ethnicity two closest contacts in US	(4) Visited people of US origin at home	(5) Received visitors of US origin at home	(6) Frequency of favours received from non-Filipinos
PANEL A						
New PDOS (either	0.012	0.022	-0.003	0.006	-0.032	-0.068
version)	(0.049)	(0.031)	(0.024)	(0.044)	(0.047)	(0.068)
MHT-adjusted p-value		0.997	1.000	1.000	0.997	0.979
Mean outcome control group	-0.012	0.767	0.134	0.549	0.539	1.455
R2	0.115	0.099	0.067	0.081	0.050	0.033
Observations	584	604	672	668	666	522
PANEL C						
New PDOS (either	-0.018	-0.017	-0.004	-0.004	-0.048	-0.144
version)	(0.071)	(0.042)	(0.031)	(0.063)	(0.064)	(0.101)
New PDOS with ass.	-0.038	-0.001	-0.018	-0.003	-0.059	0.047
email	(0.063)	(0.039)	(0.032)	(0.058)	(0.053)	(0.097)
MHT-adjusted p-value treatment		0.998	1.000	0.667	0.750	0.995
MHT-adjusted p-value interacted treatment		0.989	0.997	0.961	0.979	0.959
R2	0.105	0.088	0.050	0.078	0.052	0.048
Observations	423	433	470	471	471	370

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.26: Long-term effects (after about 30 months in the U.S.): Spillovers on household members in the Philippines following long-term PAP

	(1) Perception of how well migrant does in US (0-1)	(2) Share of hh members intending to travel to US	(3) Share of hh members intending to migrate to US	(4) Intends to migrate to US (main respondent)	(5) Young hh members should live in US	(6) Perceived ease of making the transition to US oneself (0-1)	(7) Perceived benefits of migration for HH (0-1)	(8) IHS amount of remittances received by migrant
PANEL A								
New PDOS (either version)	-0.013	0.000	-0.001	-0.004	0.028	-0.024	-0.012	-0.160
	(0.012)	(0.027)	(0.032)	(0.031)	(0.036)	(0.038)	(0.032)	(0.104)
MHT-adjusted p-value	0.924	0.995	1.000	0.998	0.967	0.972	0.992	0.645
Mean outcome control group	0.857	0.240	0.331	0.270	0.785	0.719	0.435	6.204
R2	0.098	0.111	0.239	0.222	0.144	0.375	0.128	0.143
Observations	838	733	712	834	898	167	730	535

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables as well as household-specific control variables as specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. P-values are adjusted for multiple hypothesis testing within each panel. They are computed using the procedure described in Appendix D. IHS is short for inverse hyperbolic sine.

Table E.27: Long-term effects (after about 30 months in the U.S.): Secondary outcomes and mechanisms following long-term PAP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Use of any type of public welfare	Employment quality index (STE)	IHS reservation wage	Initiated the process of having qualifications recognized	Qualifications successfully recognized	Has had contact with a US association	Has had contact with a Filipino association
PANEL A							
New PDOS (either version)	0.048	-0.008	0.332	0.025	0.022	0.062	-0.017
	(0.040)	(0.054)	(0.233)	(0.028)	(0.028)	(0.066)	(0.031)
Mean outcome control group	0.674	-0.025	8.289	0.454	0.374	0.642	0.313
R2	0.061	0.066	0.590	0.128	0.131	0.092	0.103
Observations	660	476	35	925	917	855	864
PANEL B							
New PDOS (either version)	0.017	-0.028	0.146	0.008	0.007		
	(0.048)	(0.068)	(0.251)	(0.031)	(0.031)		
New PDOS with emp. module	0.057	0.040	0.357	0.032	0.029		
	(0.046)	(0.071)	(0.266)	(0.031)	(0.026)		
R2	0.063	0.066	0.637	0.128	0.131		
Observations	660	476	35	925	917		
PANEL C							
New PDOS (either version)						0.075	-0.055
						(0.095)	(0.048)
New PDOS with ass. email						-0.058	0.058
						(0.109)	(0.045)
R2						0.105	0.127
Observations						590	592

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. IHS is short for inverse hyperbolic sine.

Table E.28: Medium-term effects (after about 15 months in the U.S.)

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Subjective wellbeing index (STE)
PANEL A				
New PDOS (either version)	-0.001	-0.082	-0.090	0.132
	(0.016)	(0.087)	(0.052)	(0.065)
MHT-adjusted p-value	0.997	0.646	0.401	0.266
Mean outcome control group	0.774	-0.037	-0.031	-0.028
R2	0.190	0.106	0.119	0.052
Observations	899	525	697	660
PANEL B				
New PDOS (either version)		-0.005		
		(0.096)		
New PDOS with emp. module		-0.147		
		(0.106)		
MHT-adjusted p-value treatment		0.958		
MHT-adjusted p-value interacted treatment		0.547		
R2		0.110		
Observations		525		
PANEL C				
New PDOS (either version)			-0.142	
			(0.084)	
New PDOS with ass. email			0.100	
			(0.085)	
MHT-adjusted p-value treatment			0.403	
MHT-adjusted p-value interacted treatment			0.615	
R2			0.143	
Observations			490	

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D.

Table E.29: Medium-term effects (after about 15 months in the U.S.): Main outcomes following medium-term PAP

	(1) Settlement index (0-1)	(2) Employment index (STE)	(3) Network size index (STE)	(4) Network type index (STE)	(5) Diaspora engagement index (STE)	(6) Subjective wellbeing index (STE)	(7) Financial decision-making index (STE)
PANEL A							
New PDOS (either version)	0.004	-0.087	-0.147	-0.032	-0.035	0.088	-0.107
	(0.018)	(0.082)	(0.065)	(0.064)	(0.064)	(0.055)	(0.131)
MHT-adjusted p-value	0.969	0.930	0.307	0.988	0.994	0.676	0.994
Mean outcome control group	0.719	-0.031	-0.031	-0.000	-0.000	-0.027	-0.000
R2	0.221	0.117	0.101	0.088	0.096	0.047	0.052
Observations	875	607	697	550	496	660	335
PANEL B							
New PDOS (either version)		0.007					
		(0.095)					
New PDOS with emp. module		-0.179					
		(0.106)					
MHT-adjusted p-value treatment		0.942					
MHT-adjusted p-value interacted treatment		0.679					
R2		0.121					
Observations		607					
PANEL C							
New PDOS (either version)			-0.163	-0.046	-0.060		
			(0.101)	(0.098)	(0.087)		
New PDOS with ass. email			0.037	0.055	0.140		
			(0.098)	(0.095)	(0.083)		
MHT-adjusted p-value treatment			0.697	0.979	0.988		
MHT-adjusted p-value interacted treatment			0.976	0.994	0.705		
R2			0.133	0.088	0.104		
Observations			490	390	346		

Note: The table reports OLS estimates. The column title shows the dependent variable. All regressions include the standard set of baseline control variables. Additional outcome-specific control variables are specified in the PAP. Standard errors clustered at the PDOS session level in parentheses. Panel A/B/C refer to specifications based on equations 1/2/3, which we present in our empirical approach. P-values adjusted for multiple hypothesis testing are computed using the procedure described in Appendix D. See mid-term PAP for definition of indices.