



## Regular article

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## ABSTRACT

We study a randomly-assigned program providing information on U.S. settlement for new Filipino immigrants. The intervention, a 2.5-hour pre-departure training and an accompanying paper handbook, has no effect on employment, settlement, and subjective wellbeing, but leads immigrants to acquire substantially fewer social network connections. We rationalize these findings with a simple model, showing that information and social network links are substitutes under reasonable assumptions. Consistent with the model, the treatment reduces social network links more when costs of acquiring network links are lower. Offsetting reductions in the acquisition of social network connections can hence reduce the effectiveness of information interventions.

## 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 (Calvo-Armengol, 2004), and financial literacy (Lusardi and Mitchell, 2014), among other areas. Imperfect information is a particular concern for immigrants who have just arrived in their country of destination as they typically have to

learn about their new environment. Imperfectly informed immigrants may not be able to make optimal choices, which arguably worsens their integration outcomes.

We implemented a randomized controlled trial on the impact of reducing imperfect information problems among immigrants. In collaboration with the Philippine government, we evaluate an information intervention for new immigrants to the U.S.: an enhanced “pre-departure orientation seminar” (PDOS) and an accompanying comprehensive paper handbook. The enhanced PDOS provides much more

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detailed information on an extended set of topics related to longer-term socio-economic integration, such as traveling to the U.S., getting settled (securing housing, enrolling in schools, opening bank accounts, etc.), navigating the U.S. labor market, managing one's finances, building a support network, and maintaining ties with the Philippines. It aims to provide practical information and also points migrants to additional resources, assisting migrants throughout the migration and settlement process. We randomly assigned the enhanced PDOS to Filipinos about to depart for the U.S. as new lawful 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 via the phone and proxy interviews with family member in the Philippines about seven and 30 months after arrival.

The new PDOS is better rated by participants, reduces the number of travel-related problems, and improves job-search related knowledge. However, it has no effects on the key outcomes it aimed to improve, such as settlement,<sup>1</sup> employment, or subjective wellbeing. Yet, it leads to considerable 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 a reduction of 0.14 to 0.17 standard deviations of the network size index, and is stable across the short- and longer-run (i.e., about seven and 30 months after arrival in the U.S.). 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. We did not anticipate the negative effect on social network links. Because the new PDOS explicitly encourages migrants to make new friends and join Filipino associations in the U.S., we hypothesized a positive treatment effect on social network connections in our pre-analysis plan.

We rationalize these findings with a simple model that explains how the negative effect on social network links can account for the null effects on other post-arrival outcomes. We consider individuals with imperfect information deciding on the optimal number of first-degree network links ("friends").<sup>2</sup> Friends are costly to acquire, but reduce information imperfections. We consider the impact of exogenously reducing information imperfections in the context of this model. For decreasing or constant returns to information, information and friends are substitutes, meaning additional information provided by the treatment reduces the marginal benefit of friends, and correspondingly reduces friend acquisition. Our empirical results are consistent with information and friends being substitutes: improved information leads to offsetting reductions in the acquisition of network links, which in turn reduces the effects of improved information on other outcome domains. At the same time, the model suggests that migrants may still benefit from the intervention. They are able to obtain similar outcomes, while investing less in acquiring social network connections to reduce information imperfections.

In exploratory analyses, we examine the heterogeneity of the treatment effect with respect to a proxy for the cost of finding friends, the

<sup>1</sup> 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.

<sup>2</sup> 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).

size of the local Filipino community. We test a theoretical prediction: the lower the cost of acquiring friends, the stronger the degree of substitutability between information and friends. 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. All in all, our results highlight the information-providing role of social networks and suggest a high degree of substitutability between information and social network connections.

Our work contributes to several literatures. First, we 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).

Second, we contribute to studies showing how the intended impacts of social policies can be attenuated 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 et al. (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 the possibility of 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 effects on final outcomes (e.g., labor market outcomes) could be attenuated.

Third, we contribute to the literature on social networks (Sacerdote, 2014; Chuang and Schechter, 2015). Social networks facilitate flows of information about new agricultural technologies (Foster and Rosenzweig, 2010; Carter et al., 2021), 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 et al., 2019), and business opportunities (Cai and Szeidl, 2018). Substantial past research documents the important role of social networks for immigrants.<sup>3</sup> Immigrants frequently live and work with compatriots in ethnic enclaves, motivated in part by eased sharing of information that comes with geographic proximity (Portes and Jensen, 1989; Beaman, 2012). We add to this literature by studying the causal impact of an information intervention on social network links. Few studies examine factors influencing strategic network formation. Comola and Mendola (2015) and Barr et al. (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 (2021), Banerjee et al. (2024) and Cecchi et al. (2016) find that savings, credit, and insurance interventions (respectively) reduce social network connections. Heß

<sup>3</sup> Key citations include Massey (1988), Borjas (1992), Carrington et al. (1996), Munshi (2003), Calvo-Armengol and Jackson (2004), Orrenius and Zavodny (2005), Amuedo-Dorantes and Mundra (2007), Dolfin and Genicot (2010), Docquier et al. (2014), Mahajan and Yang (2020).

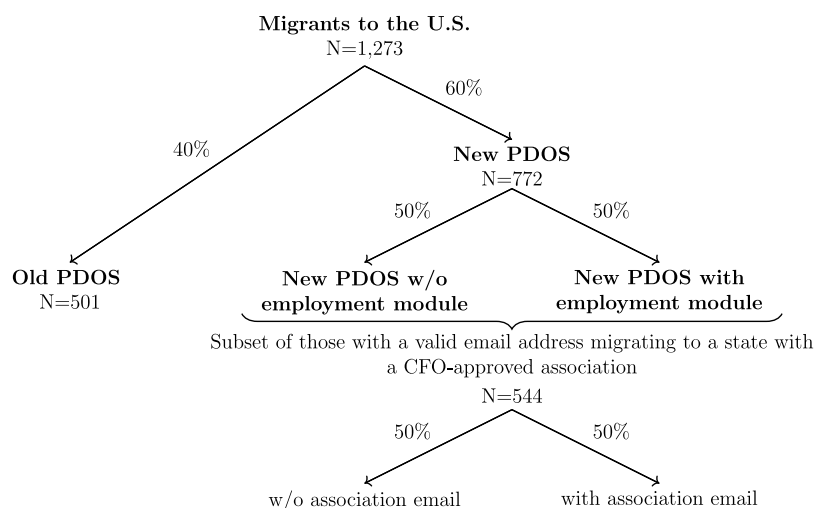


Fig. 1. Treatment conditions.

et al. (2021) find that a community-driven development program in Gambia reduces social network connections. Caria et al. (2018) show that a job-search assistance intervention in Ethiopia reduces social interactions between treated and untreated individuals.

In addition, we provide a new Stata command that adjusts p-values for multiple hypothesis testing. It modifies the List et al. (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.<sup>4</sup> Many governments and NGOs in lower-income countries implement trainings of migrants (IOM, 2011), but prior to our study there has been no well-identified assessment of their impacts (Rinne, 2013; McKenzie and Yang, 2015). We show that the intervention has no effect on key outcome domains, such as employment, settlement, or subjective wellbeing. More generally, our results suggest that the effectiveness of information interventions might be attenuated due to offsetting reductions in social network links. Our results do not suggest, however, that information interventions are necessarily ineffective. In the context of our model, in which building and maintaining a social network is costly, migrants benefit as they can achieve similar outcomes at lower levels of investment in social networks. Furthermore, results from our heterogeneity analysis show that the magnitude of the offsetting effects depends on the costs of acquiring social network links. Information interventions may be more effective in settings where such costs are high as information and social network links are less likely to be substitutes in this case. For instance, providing migrants with information might be more effective for more marginalized immigrant groups that cannot access large networks of compatriots.

## 2. 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 percent 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 et al., 2017).

<sup>4</sup> 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).

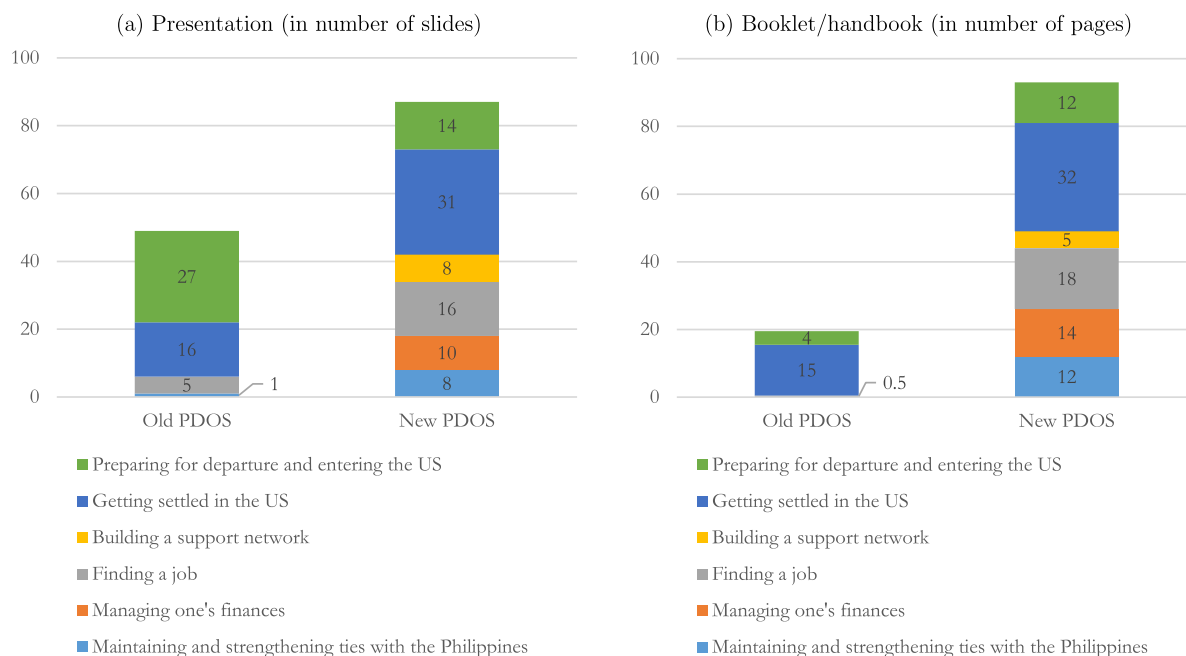
The Philippine government implements a number of policies related to international migration of its citizens. Our collaborator on this study, the Commission on Filipinos Overseas (CFO), enacts policies related to permanent migrants. Pre-departure orientation seminars (PDOS) are one of the government's 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 will 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

Fig. 1 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,



**Fig. 2.** Differences in coverage of topics between the old and new PDOS (in terms of quantity).  
 Note: The figure shows the topics provided in the new PDOS with employment module. The new PDOS without employment module provides exactly the same information, but omits all information on “Finding a job”. 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 significantly expands the information provided on topics, which are relevant for longer-term socio-economic integration. Another major difference is that the new PDOS offers much richer and practical information. Figures B.2 and B.3 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>.

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 is much more comprehensive, significantly expanding the information provided on topics (ii)-(vi). Fig. 2 documents these differences in coverage. 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.2 and B.3 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.

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.) that encouraged migrants to 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 (Fig. 1). 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 (71 percent of the sample). Our protocols were designed to minimize spillover of information from treatment to control study participants. 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 ( $324/2639 = 12$  percent).

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 percent are female. They are positively selected in terms of education levels, with 47 percent having college or more education. 18 percent have a job waiting for them in the U.S. Half migrated alone, and the remainder migrated with family members. California (41 percent) and Hawaii (17 percent) were the two most important destination states. The vast majority of study participants (93.5 percent) obtained their green cards via family sponsorship, i.e. they have family already in the U.S.<sup>5</sup> and typically join existing households.<sup>6</sup> Overall, family migrants constitute the single largest group of green card holders, accounting for 65 percent of all persons obtaining lawful permanent resident status in the U.S. in 2015 (Office of Immigration Statistics, 2016). They are hence an important immigrant group to study.

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 and 30 months after arrival in the U.S. We refer to these interviews as the short-term and long-term survey in the following. For further details on survey implementation, see Appendix B.

#### Pre-analysis plan

This study is registered with the AEA RCT Registry.<sup>7</sup> 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)<sup>8</sup> and final survey data (submitted July 28, 2016). These latter two PAPs add additional hypotheses related to employment and network characteristics.

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

<sup>5</sup> Of the 6.5 percent 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.

<sup>6</sup> Own calculations using administrative data from the Philippine government on the universe of family migrants from the Philippines to the U.S. reveal that this is the case for 98.5 percent of migrants.

<sup>7</sup> <https://www.socialscienceregistry.org/trials/1389/>

<sup>8</sup> The mid-term survey was conducted about 15 months after arrival in the U.S. It plays only a minor role in our analysis as we focus on the short-term and long-term results.

are provided in Appendix E. All conclusions are robust to estimating longer-run impacts using methods from longer-run PAPs.

We make two notable deviations from the pre-analysis plan. First, we adjust test statistics for multiple testing, making our inferences (correctly) more conservative. Second, 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, 90th 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). For the construction of STEs, we follow Kling et al. (2007). Details on the 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.)<sup>9</sup>

**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.)

<sup>9</sup> It is important to note that the old PDOS provided some limited employment-related content, whereas the new PDOS without employment module does not cover this topic at all (see Fig. 2). The new PDOS without employment module hence provides a more relevant comparison group to identify the effects of the employment module.

**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].)

It is worth noting that **Hypothesis 4A** – 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.

### 3. Empirical analyses

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

$$Y_i = \alpha + \beta T_i + X_i' \theta + \varepsilon_i \quad (1)$$

$T_i$  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. For each outcome domain, we also pre-specified that we would include controls relevant to the specific domain.<sup>10</sup>  $\beta$  is the average treatment effect for migrants satisfying our screening criteria. It is the average effect of the different sub-treatments, and will be the basis for testing **Hypotheses 1, 2, 3A, 4A** and **5**.

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

$$Y_i = \alpha + \gamma T_i + \delta T_{Emp_i} + X_i' \theta + \varepsilon_i \quad (2)$$

This regression equation modifies Eq. (1) by adding  $\delta T_{Emp_i}$ , an indicator for being assigned to the new PDOS with employment module. The coefficient  $\gamma$  is the treatment effect of the new PDOS *without* the employment module, and the coefficient  $\delta$  is the incremental impact of adding the employment module to the new PDOS. The total effect of the new PDOS with employment module (compared to the control group) is  $\gamma + \delta$ . The coefficient  $\delta$  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 association email:

$$Y_i = \alpha + \phi T_i + \lambda T_{Assoc_i} + X_i' \theta + \varepsilon_i \quad (3)$$

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

#### 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

<sup>10</sup> Baseline controls consist of 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 or California, an indicator for daily internet use, self-assessed English skills, an indicator for having a U.S. job prior to departure, and an indicator that the outcome was reported in a proxy interview with a family member in the Philippines. As an example for domain-specific controls, 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.

the method of [List et al. \(2019\)](#) 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 et al. \(2019\)](#) 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 (i.e., about seven month after departure) and 50 percent in the long-term survey (i.e., about 30 months after departure). Our results hold when we restrict the analysis to directly reported data from migrants (Appendix Tables E.11 and E.23), which might be more reliable ([Baseler, 2020](#)).

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.

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.20). Because attrition is specific to given outcome measures, we also examine this outcome by outcome (Appendix Tables E.6 and E.21).<sup>11</sup> 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.22). 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. Finally, if at least one of the index components is observed, we impute missing values of other index components at the random assignment group mean ([Kling et al., 2007](#)). This procedure substantially increases the number of observations. Our results hold (Appendix Tables E.12 and E.24).

<sup>11</sup> 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).

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. 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.8 reveal that there is a positive and statistically significant relationship between the network size index and the settlement and employment indices. 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 positive, but less pronounced and not statistically significant. Coefficients are similar in the full sample, and in regressions run separately in the control and treatment groups. Appendix Figure E.2 provides scatter plots that visualize these correlations and arrive at the same conclusions. 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 size index is economically meaningful and not simply noise.

#### 4. Main results

Table 1 presents regression results for our primary hypothesis tests, using data from the short-term survey. Panel A presents coefficients from Eq. (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 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. While the new PDOS featured considerably less travel-related content than the old PDOS in the presentation, it featured considerably more such content in the handbook (see Fig. 2).

The new PDOS has no effect on settlement, employment, and subjective wellbeing, some of the key outcome domains the intervention aimed to improve. The coefficients on the treatment indicator in regressions for these outcomes are small in magnitude, and none are statistically significantly different from zero. However, the treatment has a negative effect on the network size index (column 4). The effect is substantial in magnitude, amounting to 0.17 standard deviations of the network size index. This is the sole outcome that is statistically significant after multiple-hypothesis correction ( $p$ -value 0.03). Appendix Table E.15 shows treatment effects on the component variables of the network size index. The treatment has large negative effects on both components. Treatment causes the number of friends to fall by 28 percent,<sup>12</sup> the rate of receiving support from associations to fall by 3.2 percentage points (control mean 4.9 percent). It also lowers the rate of contacting an association by 5 percentage points (control mean 12.3 percent).

<sup>12</sup> We use the method of Bellemare and Wichman (2019) to convert IHS coefficients into percentage changes.

Panel B presents coefficients from estimating Eq. (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.<sup>13</sup> Panel C presents coefficients from estimating Eq. (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).

Before proceeding with the results from the long-term survey, we analyze whether the new PDOS improved migrants' knowledge. We have two measures of knowledge: the share of correct answers to questions about the U.S. more generally<sup>14</sup> and the share of correct answers to questions about which items to include in an American CV.<sup>15</sup> We also construct an overall measure that combines the two. Unfortunately, we did not collect more comprehensive measures of knowledge. Appendix Table E.9 summarizes the exploratory and not pre-specified analysis. The new PDOS does not improve general knowledge about the U.S (column 1). This result is not surprising as the new PDOS does not cover any content that should improve knowledge about the U.S. as measured by our questions. We should hence not expect an effect on this measure. The effect on knowledge about CV items depends on whether the employment module is part of the new PDOS (column 2). The new PDOS with employment module improves knowledge about CV items (column 2), whereas the new PDOS without the employment module reduces it. This result is consistent with the fact that the employment module of the new PDOS explicitly covers which items to include in a CV.<sup>16</sup> By contrast, the new PDOS without the employment module does not cover any employment-related content. This is even less than the old PDOS which rudimentary covers how to find a job (see Fig. 2). Our results, while restricted to knowledge about CV items, are hence consistent with the idea that the information provided by the PDOS improves knowledge. In line with this conclusion, we also find that the new PDOS received higher ratings on almost every aspect, in particular

<sup>13</sup> Appendix Table E.14 shows the effects on the individual components of the employment index. The new PDOS, including the version with the employment module, has no effect on the likelihood of being employed (column 1). We find a substantial negative coefficient of  $-0.30$  on income (transformed by the inverse hyperbolic sine, column 2). However, with a standard error of 0.28 the coefficient is not precisely measured and hard to interpret. Columns 3 and 4 evaluate the effects on the perceived chances of being employed in the near future and of having a job that matches one's qualifications (which we only measure in the short-term survey). The results are insignificant, with coefficients close to zero across the different specifications. These results also hold when we look at employment outcomes measured in the long-term survey (Appendix Table E.25). Overall, the results for the individual components corroborate the result for the aggregate index, indicating no significant effect on employment outcomes.

<sup>14</sup> Knowledge about the U.S. is based on seven questions: (i) What is the name of the President of the U.S.?, (ii) What is the name of the Vice-President of the U.S.?, (iii) What does the U.S. celebrate on July 4?, (iv) What is the capital of the U.S.?, (v) What are the two major political parties in the U.S.?, (vi) Can you use a state driver's license anywhere in the US?, (vii) Suppose you want to send US\$ 200 from the US to your family in the Philippines. What do you think would cost you more, sending it all at once as US\$ 200, or sending it at two different times at US\$ 100 each, or is the cost the same either way?

<sup>15</sup> Knowledge about CV items is based on six questions: Do you include or are you going to include the following information on your resume: (i) age, (ii) education, (iii) marital status, (iv) religion, (v) work experience, (vi) photo?

<sup>16</sup> Presentation slides 66 and 67 and pp. 62–64 in the handbook. Both can be downloaded at <https://sites.google.com/view/tomanbarsbai/pdos>.

**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 size index (STE)	(5) Subjective wellbeing index (STE)
<b>PANEL A</b>					
New PDOS (either version)	–0.012** (0.006)	0.028 (0.017)	–0.012 (0.070)	–0.169*** (0.056)	–0.020 (0.076)
MHT-adjusted <i>p</i> -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
<b>PANEL B</b>					
New PDOS (either version)			0.016 (0.090)		
New PDOS with emp. module			–0.053 (0.095)		
MHT-adjusted <i>p</i> -value treatment			0.967		
MHT-adjusted <i>p</i> -value interacted treatment			0.939		
R2			0.130		
Observations			362		
<b>PANEL C</b>					
New PDOS (either version)				–0.223*** (0.078)	
New PDOS with ass. email				0.092 (0.077)	
MHT-adjusted <i>p</i> -value treatment				0.052	
MHT-adjusted <i>p</i> -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. The number of observations varies across the outcomes because an index can only be generated if all components are observed. However, proxy interviews only fully cover travel-related problems, settlement, and social networks, but not employment and subjective wellbeing. In addition, proxy interviews record a considerable number of don't-know responses to questions about settlement and in particular about social networks. The number of observations is especially low for the employment index as quite a few migrants refuse to report their income in addition to the reasons stated above. 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.

on the usefulness of various topics and the quality of the slides and the written material (Appendix Figure E.1).

In a second step, we also analyze whether knowledge is associated with better settlement and employment outcomes. We find suggestive evidence that knowledge matters (Appendix Table E.10). All three measures of knowledge are significantly associated with better settlement outcomes (columns 1–3). The strength of the association is economically meaningful. A one-standard-deviation increase in overall knowledge about the U.S. (about 17 percent) is associated with an increase in the settlement index by 0.03 units or 0.13 standard deviations (column 3). We also find positive correlations with the employment index. However, they are not precisely estimated.

We now proceed with the results from the long-term survey. Table 2 presents the 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. The magnitude of the effect, amounting to 0.14 standard deviations of the network size index, is comparable to the short-term effect reported above. The same is true when we look at the components of the network size index (Appendix Table E.26). In Panels B and C of Table 2, 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.<sup>17</sup> Our results also hold when we exclude proxy reports from household members and restrict the analysis to directly reported data from migrants. Using directly reported data from the short-term survey, the size and statistical significance of the treatment effect on the network size index does not change (Appendix Table E.11). Using directly reported data from the long-term survey, the coefficient on the network size index remains stable but is no longer statistically significant after correcting *p*-values for multiple testing (Appendix Table E.23). Our sample size, however, is reduced by about 30 percent, which might explain why we lose precision.

Density plots of the number of friends provide an alternate view of the treatment effects on network formation. Fig. 3 presents probability

<sup>17</sup> Short-term results are also 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.26). We also show robustness to defining the network measure as specified in the long-term PAP (Appendix Table E.30).



**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)
<b>PANEL A</b>				
New PDOS (either version)	–0.009 (0.016)	–0.065 (0.087)	–0.136** (0.053)	0.035 (0.049)
MHT-adjusted <i>p</i> -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
<b>PANEL B</b>				
New PDOS (either version)		–0.050 (0.098)		
New PDOS with emp. module		–0.028 (0.088)		
MHT-adjusted <i>p</i> -value treatment		0.830		
MHT-adjusted <i>p</i> -value interacted treatment		0.751		
R2		0.135		
Observations		601		
<b>PANEL C</b>				
New PDOS (either version)			–0.238*** (0.080)	
New PDOS with ass. email			0.095 (0.079)	
MHT-adjusted <i>p</i> -value treatment			0.032	
MHT-adjusted <i>p</i> -value interacted treatment			0.726	
R2			0.139	
Observations			533	
<b>PANEL D</b>				
New PDOS (either version)	–0.007 (0.015)	–0.042 (0.092)	–0.127** (0.053)	0.041 (0.051)
IHS nr of Filipinos in county (demeaned)	–0.001 (0.005)	–0.015 (0.026)	0.043** (0.018)	0.026 (0.017)
New PDOS x IHS nr of Filipinos in county	–0.001 (0.006)	0.010 (0.032)	–0.042** (0.020)	–0.044** (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. IHS is short for inverse hyperbolic sine.

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 did not collect these outcomes in the short-term survey). Appendix Table E.31 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.32). 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. In addition, the new PDOS does not affect how frequently the migrant is in touch with the two closest new contacts in the U.S. and whether they have a

different education level than the migrant. Overall, our results suggest a reduction in the number of network links across the board with few changes in the type of links.

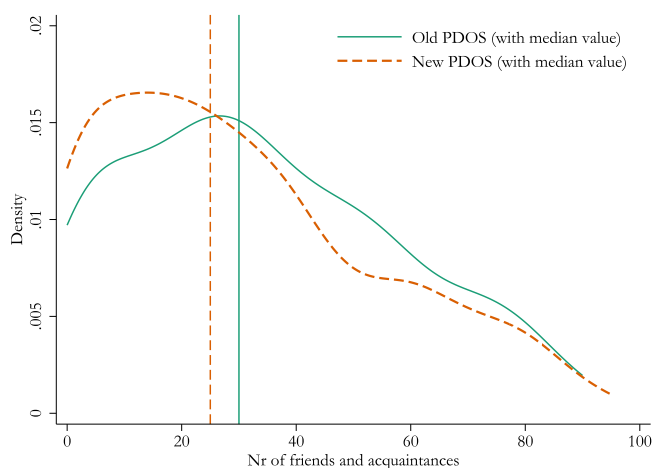
The treatment might also induce migrants to maintain links to the Philippines at the expense of building new social network links in the U.S. We assess this possibility by analyzing whether the new PDOS affects migrants' strength of ties to the Philippines. The exploratory and not pre-specified analysis in Appendix Table E.33 does not suggest this is the case. The new PDOS does not make migrants more likely to communicate with their family in the Philippines, consume news about the Philippines, or visit the Philippines after their arrival in the U.S.

In additional 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 less likely to have found their current job through social networks than migrants who attended the new PDOS without employment module (column 5). This finding potentially reflects that the employment module significantly improves migrants' job-search knowledge (see column 2 of Appendix Table E.19), which reduces their reliance on social networks. By contrast, migrants who received the association email, which explicitly encourages them to

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Has a job	Has a job	Has a job	Found job through network	Found job through network	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.



**Fig. 3.** Density plot of number of friends after 30 months in the U.S. by treatment status.

Note: Based on data from long-term survey. Missing data are replaced with values from mid-term survey or short-term survey (in that order).

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).

**5. A model of information and social network links as substitutes**

*Model setup*

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, and no impact on other post-arrival outcomes. We are interested in the interplay between information imperfections and individual efforts to increase social network links. In particular, we are interested in the impact of interventions alleviating information imperfections.<sup>18</sup>

Individuals (in our case immigrants) 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. Individuals also have social network

<sup>18</sup> 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 et al., 2011; Canen et al., 2019; Currarini et al., 2009).

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, but making friends is costly. 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, which the network literature typically refers to as *cooperation capital*, such as various forms of assistance (transfers, informal insurance, and psychological support).<sup>19</sup>

Utility depends on baseline or starting-point information imperfections (prior to any reduction in information imperfections resulting from friend investments),  $\theta$ , and the number of endogenous friends  $f \geq 0$ . Individuals choose  $f$  to maximize the benefits from friends  $B(\theta, f)$  net of 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  $\theta$  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  $e \geq 1$ .<sup>20</sup> Let an individual’s amount of information  $I$  be a function of information imperfections  $\theta$ , 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  $\theta$  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  $cf$ .<sup>21</sup>

<sup>19</sup> 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.

<sup>20</sup> For new immigrants, the exogenous friend could be the individual who officially sponsors their immigration visa.

<sup>21</sup> 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.

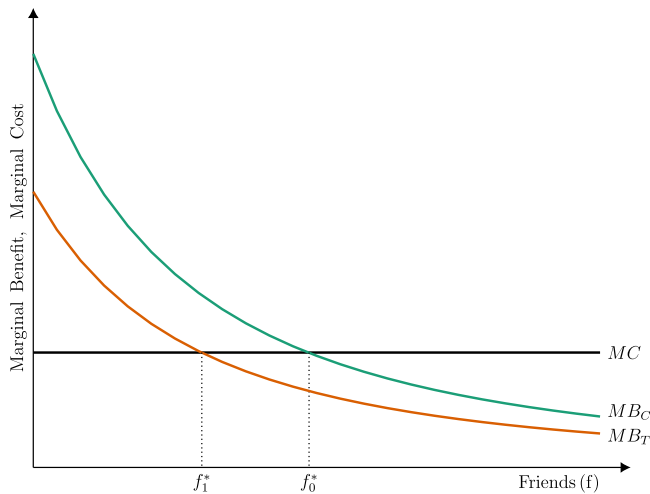


Fig. 4. Constant returns to information. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Model predictions

We can now show that a reduction in information imperfections  $\theta$  (e.g., our information treatment for new immigrants) always reduces friend acquisition as long as returns to information  $I$  (in utility) are either constant or decreasing. We flesh out the case of increasing returns to information, for which the impact of reducing information imperfections is ambiguous, in Appendix A.

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.)

We can then take the partial derivative of  $f^*$  with respect to  $\theta$  to understand the effect of baseline information imperfections on the optimal number of friends:

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

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

Fig. 4 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 black 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, which in turn lowers the optimal number of friends.

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:

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

The first order condition is:

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

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. As in Fig. 4, the reduction in information imperfections due to treatment lowers the marginal benefit of friends and therefore the optimal number of friends (the orange curve is always lower than the green curve).<sup>22</sup>

We are thus able to explain why the information treatment substantially reduces the size of the social networks that immigrants build in the U.S. and why it does not affect other post-arrival outcomes: improved information leads to offsetting reductions in the acquisition of network links, which in turn reduces the effects of improved information on other outcomes. The magnitude of the treatment effect points to a relatively high degree of substitutability between information and social network links. 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 not attenuate the effects on travel-related problems.

Further empirical results

The model allows us to derive another theoretical prediction: when friend-acquisition costs are lower, the degree of substitutability between information and friends is higher. This can be seen by shifting the marginal cost function in Fig. 4 (black line) downwards. Due to the convexity of the marginal benefit function, the treatment would lead to an even greater reduction in friends for lower friend-acquisition costs. In this case, the treatment would have a less positive impact on wellbeing because utility gains from better treatment-provided information would be more strongly offset by reductions in friend-provided information.<sup>23</sup>

We test this theoretical possibility in additional analyses (not pre-specified). We estimate Eq. (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). The main effect

<sup>22</sup> It is also possible for information and friends to be complements, in that reducing information imperfections leads to higher friend acquisition. This can occur in the case of increasing returns to information, which we discuss in Appendix A.

<sup>23</sup> In the case of increasing returns to information (discussed in Appendix A), information and friends are more likely to be substitutes (rather than complements) in places with lower friend acquisition costs (such as places with high Filipino populations). In Figure A.1 in Appendix A, the information treatment leads to fewer friends (a reduction from  $f^*$  to  $f^{**}$ ) if the marginal cost of making friends is low ( $MC_L$ ), but a positive effect (an increase from 0 to  $f^*$ ) if the marginal cost of making friends is high ( $MC_H$ ).

of number of Filipinos is also included in the regression. To mitigate endogeneity concerns, we use the intended U.S. destination county stated by the study participant in their baseline interview, ignoring any subsequent moves. The intended U.S. destination county is usually predetermined by the location of the immigrant’s visa sponsor and thus exogenous.<sup>24</sup> 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 Fig. 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 percent confidence intervals of the nonparametric regression estimate, based 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. The intervention targets Filipino immigrants to the U.S., who largely obtain their green card via family sponsorship. The Philippines-U.S. migration corridor is one of the most important ones in the world. At the same time, family migrants constitute the largest group of green card holders, accounting for 65 percent of all persons obtaining lawful permanent resident status in the U.S.

The information intervention has no effect on key dimensions of immigrant success abroad, such as settlement, employment, or subjective wellbeing. At the same time, we find that when new immigrants are better-informed, they acquire substantially fewer new social network connections. In the context of a simple model, in which the acquisition of social network connections is costly, 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 it may not affect final outcomes if individuals respond to the information provided by reducing their acquisition of information from social networks. Nevertheless, if building and maintaining a social network is costly, individuals may still benefit as they achieve similar outcomes, but at lower levels of investment in

<sup>24</sup> Indeed, we find no evidence that the number of Filipinos in one’s intended destination county is endogenous to treatment. When estimating equation (1) with the inverse hyperbolic sine of number of Filipinos in the intended destination county as the dependent variable, the coefficient on treatment is small in magnitude and is not statistically significantly different from zero. Similarly, we do not find that migrants who move to counties with larger Filipino communities (i.e., above median number of Filipinos) differ from those who move to counties with smaller Filipino communities (i.e., below median number of Filipinos). The balance check in Appendix E.4 reveals no significant differences in individual characteristics. Composition effects are hence unlikely to confound the analysis.

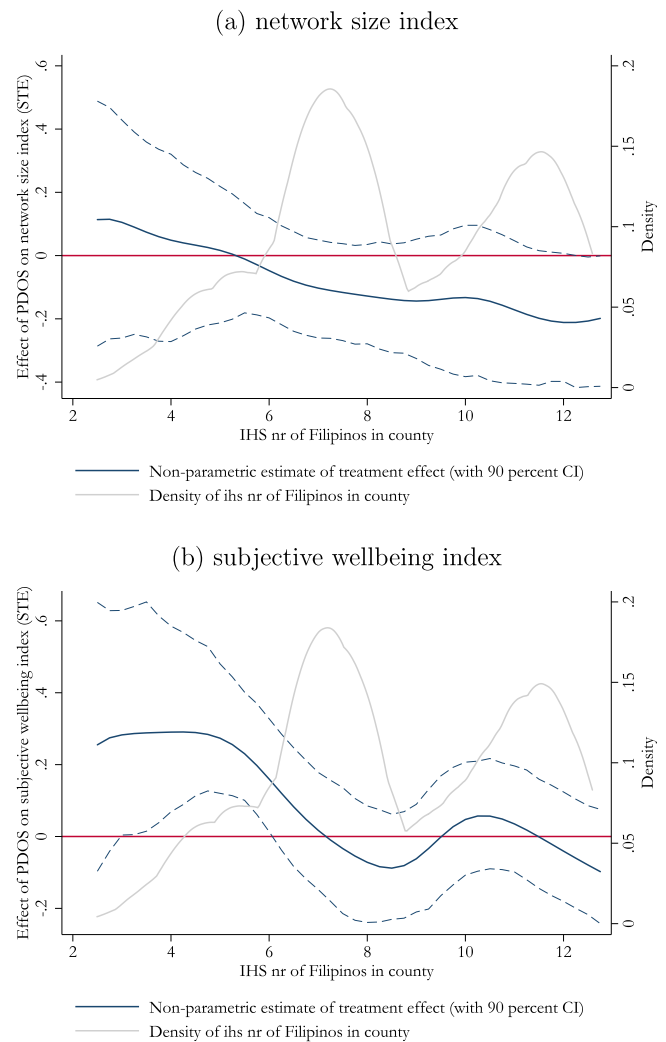


Fig. 5. Nonparametric treatment effects of PDOS on network size index and subjective wellbeing index by size of Filipino community. Note: Gaussian kernel. Bootstrapped standard errors with 200 replications. IHS is short for inverse hyperbolic sine.

social network connections. However, it is difficult to fully assess the welfare implications as social networks also provide non-information benefits such as informal insurance, which are beyond the scope of our analysis.

Our empirical findings are consistent with this interpretation. However, we cannot rule out the possibility that the information provided by the new PDOS eased the initial migration experience and reduced migrants’ reliance on social networks, but was not instrumental to job search and subsequent labor market outcomes. Not finding a treatment effect on labor market outcomes would then simply be a null result, and not a positive treatment effect that was attenuated by reduced investments in 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 et al., 2014). For the specific intervention we study, we find no effects on relevant outcome domains. In the context of our model, we attribute this finding to the substitutability of the information provided by the PDOS with information obtained from social

networks. In future research, it will be important to examine whether information interventions also lead to reductions in social networks in other contexts and whether they can improve relevant outcomes in contexts where information acquisition, in particular through social networks, is difficult.

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 wellbeing, 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 model, the higher the cost of acquiring social network connections, the lower the degree of substitutability between information and social network connections, and the more positive can be the impact of the information intervention on wellbeing. This finding extends the external validity of our results and has a policy implication. Information interventions may have the highest positive impacts on the wellbeing 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).

In recent decades, the proliferation of digital technologies has revolutionized how individuals access information. Our findings suggest that easy access to information might have diminished the traditional role of social networks as sources of information and changed the formation of social networks. Migrants may hence need to rely less on social ties to navigate new environments. This conclusion may not only apply to migrants but also extend to other contexts.

#### CRediT authorship contribution statement

**Toman Barsbai:** Conceptualization, Methodology, Software, Formal analysis, Investigation, Writing – review & editing, Supervision, Project administration, Funding acquisition. **Victoria Licuanan:** Conceptualization, Investigation, Supervision, Project administration, Funding acquisition. **Andreas Steinmayr:** Conceptualization, Methodology, Investigation, Project administration, Funding acquisition, Software, Formal analysis. **Erwin Tiongson:** Conceptualization, Investigation, Writing – review & editing, Funding acquisition. **Dean Yang:** Conceptualization, Methodology, Formal analysis, Investigation, Writing – original draft, Writing – review & editing, Funding acquisition.

#### Data availability

Data will be made available on request.

#### Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jdeveco.2024.103305>.

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