

Barsbai, Toman et al.

Working Paper

Picture This: Social Distance and the Mistreatment of Migrant Workers

CESifo Working Paper, No. 10177

Provided in Cooperation with:

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

Suggested Citation: Barsbai, Toman et al. (2022) : Picture This: Social Distance and the Mistreatment of Migrant Workers, CESifo Working Paper, No. 10177, Center for Economic Studies and Ifo Institute (CESifo), Munich

This Version is available at:

<https://hdl.handle.net/10419/271821>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Picture This: Social Distance and the Mistreatment of Migrant Workers

*Toman Barsbai, Vojtěch Bartoš, Victoria Licuanan, Andreas Steinmayr,
Erwin Tiongson, Dean Yang*

Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

An electronic version of the paper may be downloaded

- from the SSRN website: www.SSRN.com
- from the RePEc website: www.RePEc.org
- from the CESifo website: <https://www.cesifo.org/en/wp>

Picture This: Social Distance and the Mistreatment of Migrant Workers

Abstract

We experimentally study an intervention to reduce mistreatment of Filipino overseas domestic workers (DWs) by their employers. Encouraging DWs to show their employers a family photo while providing a small gift when starting employment reduced DW mistreatment, increased their job satisfaction, and increased the likelihood of contract extension. While generally unaware of the intervention, DWs' families staying behind become more positive about international labor migration. An online experiment with potential employers suggests that the effect operates through a reduction in employers' perceived social distance from their employees.

JEL-Codes: D900, J610.

Keywords: temporary labor migration, working conditions, contract enforcement, dictator game.

Toman Barsbai
University of Bristol / United Kingdom
toman.barsbai@bristol.ac.uk

Vojtěch Bartoš
University of Milan / Italy
vojtech.bartos@unimi.it

Victoria Licuanan
Asian Institute of Management
Manila / Philippines
vlicuanan.aim@gmail.com

Andreas Steinmayr
University of Innsbruck / Austria
andreas.steinmayr@uibk.ac.at

Erwin Tiongson
Georgetown University / Washington DC / USA
ert@georgetown.edu

Dean Yang
University of Michigan
Ann Arbor / MI / USA
deanyang@umich.edu

December 14, 2022

For helpful comments and discussions, we thank Michel Beine, Simone Bertoli, Jesus Fernandez-Huertas, Martin Fernandez Sanchez, André Gröger, Alice Mesnard, Joan Monras, Melanie Morten, as well as seminar and conference participants at LMU Munich, NYU Abu Dhabi, the University of Regensburg, the 11th Migration and Development Conference, the Conference of the German Economic Association, and the Economics of Migration Webinar. We received funding from the International Initiative for Impact Evaluation (3ie), grant number OW4/1171. The views expressed in this article are not necessarily those of 3ie or its members. IRB review was provided by the University of Michigan (HUM00087460). Our AEA RCT Registry number is AEARCTR-0003837 for the field experiment and AEARCTR-0003788 for the online experiment. We are grateful for the support we received from the Overseas Workers Welfare Administration (OWWA). We thank Anna Jennifer Umlas for excellent project management and Jakob Beuschlein and Yasar Ersan for superb research assistance.

1 Introduction

A major objective of public policy is to secure labor rights, counteracting inherent power asymmetries between employers and workers. Across societies, laws and regulations aiming to protect workers from abuse take a variety of approaches (OECD, 1996; ILO, 2006). Societies can ensure workers have freedom of association and the right to collective bargaining. Laws frequently prohibit forced or child labor. Regulations can set minimum health and safety standards, such as limitations on hours of work.

There is particular interest in reducing abuses of international migrant workers, since they often enjoy limited formal worker protections and are distant from their networks of social support. Migrants who work for private households as domestic workers (DWs) are considered especially vulnerable as they live in their employers' homes and are shielded from the public (Malhotra *et al.*, 2013). Out of 150 million international migrant workers in 2013, 11.5 million were DWs, and 73.4% of migrant DWs were women (ILO, 2015). In our own survey data, nontrivial shares of Filipino DWs report mistreatment by their household employers. The percentages of DWs experiencing sexual harassment are 7% in Saudi Arabia and 2% in Hong Kong. DWs in our sample also report high rates of experiencing physical violence (8% and 7%, respectively); not receiving salary on time (36% and 13%); and not having a weekly rest day (91% and 13%).¹

Policy approaches to reducing abuses of international migrant workers face significant challenges. The most prominent difficulty is that the governments most interested in protecting migrant workers – their origin countries (e.g., the Philippines) – cannot directly implement public policies in workers' destination countries (e.g., Saudi Arabia). Protecting migrant workers often requires bilateral or multinational agreements among governments, which are difficult to secure and even more difficult to enforce.²

In this paper, we study an intervention aimed at reducing mistreatment of international migrant workers that does not require changing public policies in destination countries. The intervention is motivated by prior findings in behavioral economics, and is aimed at changing the behavior of employers towards their migrant employees. Prior research has found that reducing perceived social distance increases prosociality and trust in others (Hoffman *et al.*, 1996; Bohnet and Frey, 1999; Charness and Gneezy, 2008; Leider *et al.*, 2009; List and Price, 2009; Ligon and Schechter, 2012; Binzel and Fehr, 2013).

We implemented a randomized controlled trial of a simple intervention: encouraging migrant workers to show a photo of their family while giving a small gift to their

¹These percentages are control group means in our DW survey. For details, see Section 3 below.

²Origin-country governments like the Philippines have on occasion simply banned international migrant labor to certain destinations when major abuses have garnered media attention (Theoharides, 2020).

employer upon starting employment. We hypothesized that when DWs show employers their family photo, it would lead employers to reduce their perceived social distance with the DW, making them more generous along a range of dimensions.³

Our sample is composed of nearly 2,000 Filipino women newly departing for work as DWs in Saudi Arabia and Hong Kong. The women in our sample were all departing alone (without their families) to live and work in a household in their destination country. We worked with the Philippine government to implement the subject enrollment and treatment assignment in the context of the study participants' pre-departure orientation seminar. Study participants randomly assigned to the treatment group were encouraged to show a photo of their family to the new employer while offering a small gift.⁴

We find that the intervention improved the treatment of DWs by their employers, as revealed by a number of measures two years after the start of employment. First, the intervention led DWs to report less mistreatment at the hands of their employers along a range of dimensions (an index incorporating verbal and physical abuse, working conditions, and timeliness of wage payments). Second, DWs report higher satisfaction in their relationship with their employer. Third, DWs are more likely to remain working for the same employer, and are less likely to have returned to the Philippines.

We also find that the intervention led the DW's household members (who remained behind in the Philippines) to have a more favorable view of international labor migration, thus affirming the DW's assessment. The positive effects on remaining with the same employer, and the left-behind household's more-favorable view of migration suggests that response bias is not driving the results. DWs' assessments of their treatment by the employer or their satisfaction with the employer may potentially be subject to experimenter-demand effects. By contrast, whether one remains with the same employer two years later can be seen as a revealed-preference measure of the overall quality of the employer-employee relationship. Also, since household members in the Philippines did not receive the intervention directly and are for the most part unaware of it, they should be less likely to exhibit response bias.

To better understand the mechanism behind these effects, we conducted an online experiment with potential household employers of DWs. We recruited a representative sample of 1,662 upper- and middle-income individuals from three countries (Hong

³While the small gift's main purpose is that of a conversation opener, it could also create feelings of reciprocity from employers to employees (Charness and Rabin, 2002; Falk and Fischbacher, 2006). To disentangle the potential mechanisms at play, we implemented an online experiment among potential employers, described below.

⁴They were provided a pack of dried mangoes (a Filipino delicacy) to give as the small gift. Compliance with the intervention was high, with 64% of those in the treatment group reporting that they followed the instructions on how to introduce themselves.

Kong, Saudi Arabia, and the United Arab Emirates).⁵ 66 percent of subjects were either past or current employers of DWs. Using a dictator game, we measure subjects' generosity towards Filipino women with characteristics similar to those of an average DW in the sample of our field experiment. We tested the impact on dictator-game allocations to DWs of showing a family photo alone, providing a small gift alone, and providing both a photo and gift (compared to a control group receiving neither photo nor gift). The impacts of these manipulations on dictator-game allocations are consistent with the family photo alone being the sole driver of increased employer generosity, with the gift providing no incremental positive effect on top of the photo. In sum, the results suggest that a reduction in social distance is the key driver of increased generosity of potential employers.

Finally, we assess whether and how the intervention affects remaining household members in the Philippines. Household members of DWs in the intervention assess the effects of the migration of the DW on the household more positively, particularly in the domains of the household's financial security, standard of living, education, family life, and social life. This subjective assessment is corroborated with more objective measures such as higher education expenditures and school enrollment of older children.

Our paper builds on the behavioral economics literature on social distance. In his seminal work, Schelling (1968) suggested that an identifiable victim receives more empathy than the same victim described in statistical, more distant terms. Social distance is associated with lower levels of altruism and trust in carefully designed studies using redistribution experiments in existing social networks (Hoffman *et al.*, 1996; Bohnet and Frey, 1999; Charness and Gneezy, 2008; Leider *et al.*, 2009; Ligon and Schechter, 2012; Binzel and Fehr, 2013). Chandrasekhar *et al.* (2018) show that social distance increases the need for contractual completeness. Observational studies document reduced public good provision and lower preferences for redistribution in more fragmented communities (i.e., those with greater social distance among members) (Alesina and La Ferrara, 2000; Dahlberg *et al.*, 2012). To study causal effects, laboratory studies manipulate social distance either by increasing identifiability through provision of recipients' first names (Charness and Gneezy, 2008), by manipulating perceptions of team identity (Eckel and Grossman, 2005), or by manipulating the salience of existing social identities (Chen *et al.*, 2014). All these studies document increased altruism or more effective cooperation rates with reduced social distance.⁶ Contextual information and familiarity seem to be im-

⁵Recruitment of online study participants from Saudi Arabia was difficult, leading us to supplement the online sample with households from the neighboring United Arab Emirates.

⁶Social psychologists use the term "social discounting" for the inverse relation between altruistic behavior and social distance (Jones and Rachlin, 2006; Jones, 2021). Economists use the term "targeted altruism" for the same phenomenon (Leider *et al.*, 2009; Ligon and Schechter, 2012).

portant drivers of prosocial behavior. Importantly, studies have documented that perceptions of social distance are malleable, and are affected by exogenous shocks (Shayo and Zussman, 2011; Voigtländer and Voth, 2012) and state-building efforts (Blouin and Mukand, 2019). Our study is the first, to our knowledge, to causally manipulate social distance to improve employer-employee relations in both field and online experimental settings.

Our work is also relevant for policy-making aimed at raising the gains from international labor migration. There is considerable evidence that international labor migration brings substantial economic gains for migrants (Gibson *et al.*, 2018), their origin households (Yang, 2008; Gibson *et al.*, 2011; Clemens and Tiongson, 2017), and their origin areas (Dinkelman *et al.*, 2020; Caballero *et al.*, 2021; Khanna *et al.*, 2022). At the same time, there is substantial concern that the social harms caused by mistreatment and abuse of migrant workers reduce the economic gains from international labor migration (IOM, 2020). In the worst case, the social harms may even outweigh the economic gains. Under such conditions, migrants may regret their decision to migrate in hindsight and potential migrants may be deterred from migration. These concerns are particularly prominent for female migrants (UN Women, 2018). Evidence on ways to reduce migration's social harms is thus relevant for improving migrants' welfare and policy decision-making related to international labor migration. In related work, Bazzi *et al.* (2021) show that providing migrants in Indonesia with information about the quality of intermediaries (recruitment agencies) reduces the use of low-quality intermediaries and improves migrants' experience abroad. Fernando and Singh (2021) show that the introduction of a rating system causes intermediaries to invest in rating criteria which subsequently also improves the migrants' situation.

2 Context

A substantial share of international migrant workers work for private households as domestic workers (DWs). According to the International Labor Organization (ILO, 2015), there were 11.5 million migrant domestic workers worldwide in 2013, out of a total of 150 million international migrant workers. Of all migrant DWs, 73.4% are women, 27.4% work in the Middle East (accounting for the largest share of migrant DWs across world subregions), 24.4% in East/Southeast Asia and the Pacific, and 19.2% in Europe.

We study a sample of Filipino women departing to work as DWs in Saudi Arabia and Hong Kong. International labor migration is a major phenomenon in the Philippines. At the start of our study period in 2015, 2.4 million Filipinos were working overseas

(out of a population of 100 million). 84% of Filipino migrant workers were in Asia and the Middle East. Saudi Arabia was the single largest destination country, accounting for 25% of workers. Among Asian countries, Hong Kong is the largest destination (6% of all Filipino migrant workers). Overall, women made up 51.1% of Filipino international migrant workers. Domestic worker is the most common occupation of female Filipino migrants (Government of the Philippines, 2015).

DWs work in private households and are typically tasked with household chores such as cleaning and cooking, as well as taking care of children and elderly. DWs find jobs overseas via Philippines-based recruitment agencies that serve as intermediaries between Filipino job-seekers and overseas household employers.

In the Philippines, two government agencies regulate international labor migration. The Philippine Overseas Employment Administration (POEA) regulates international migrant recruitment, issuing operating licenses to recruitment agencies and reviewing and approving migrant work contracts. The Overseas Workers Welfare Administration (OWWA) works to ensure the well-being of overseas Filipino workers (OFWs) and their families. It intercedes (via Philippine consulates worldwide) for workers experiencing abuse or contract violations, repatriates workers in conflict zones, assists OFW families in hardship, and facilitates the return and “reintegration” of OFWs to the Philippines. OWWA is our research partner on this project.⁷

Philippine government regulations mandate pre-departure trainings for international migrants, referred to as Pre-Departure Orientation Seminars (PDOS). All Filipinos leaving for abroad on migrant labor contracts are required to attend a PDOS.⁸ This requirement is strictly enforced; migrants failing to attend a PDOS will not be granted permission to depart for overseas, and can be denied clearance to board an international departing flight at airports.

The PDOS includes sections on working abroad (cultural insights and a code of discipline for overseas workers), the employment contract (rights and responsibilities of workers, and what to do in case of violations), government services for overseas workers, and advice on matters such as health, financial planning, and travel. The PDOS for outbound DWs (referred to as the Comprehensive Pre-Departure Education Program, or CPDEP) includes additional content compared to the standard PDOS, covering language training, culture, and stress management. The CPDEP is implemented by OWWA staff at

⁷Policies facilitating and regulating international labor migration, and agencies like the Philippines’ POEA and OWWA, can be found in the majority of developing countries. Khanna *et al.* (2022) tabulate data on government policies on outbound international migration. Out of the 70 developing countries with populations exceeding 1 million, 94% have a dedicated government agency implementing migration policy; 88% have a dedicated government agency for overseas employment, citizens abroad, or diaspora engagement; and 78% have policies promoting remittances.

⁸Attendance of a PDOS is also mandatory for permanent migrants. For an evaluation of the PDOS for permanent migrants, see Barsbai *et al.* (2020b).

OWWA training locations (unlike the standard PDOS for other occupations, whose implementation is typically outsourced to private firms). Our field experiment was among outbound DWs and was administered during a baseline survey conducted immediately prior to their CPDEP sessions. DWs attend CPDEP sessions after signing the contract and typically a few weeks before departure. Out of the DWs in our sample, 75% departed within two months and 90% did so within three months.

3 Field experiment, hypotheses, and empirical approach

Intervention

The intervention aims to reduce the social distance between the DW and the employer during their initial encounter and to create a more favorable trajectory of the relationship. In particular, the intervention aims at portraying the DW as a human being who comes with good intentions and has a family herself.

The intervention is motivated by insights from a preparatory survey and focus group discussions with returned DWs. Many reported that they had been treated poorly by members of the employer's household, had not felt respected as human beings, and had mostly been seen as cheap labor. Some felt they were considered "slaves" or "machines" by their employers.

DWs assigned to the intervention were encouraged to show their employer a photo of themselves together with their immediate family members (typically including their own children). The photo aims to signal to the employer's family that the DW is a human being with a family and a personal background, too. To create a pretext for showing the photo, the intervention provides the DW with a small gift, a pack of dried mangoes (a specialty in the Philippines worth about USD 2). Presenting the small gift should act as a conversation opener, create a friendly atmosphere, and eventually allow the DW to show the photo. The intervention suggests then placing the family photo in the DW's sleeping quarter. There it is easily visible to someone who enters the room, thus reinforcing its effect.

The intervention provided DWs with guidance on presenting the photo to avoid cultural misunderstandings. For example, the intervention recommended showing a photo where family members are appropriately dressed, and not directly comparing the DW's family with the employer's family. For DWs going to Saudi Arabia, the instructions recommended asking the recruitment agency whether the employer would accept a gift, and who the gift should be given to within the household. The instructions were also provided to DWs in written form (see Figure A.1 in Appendix A).

Baseline interviews and experimental design

The sample consists of 1,992 first-time DWs bound for Saudi Arabia (1,185) and Hong Kong (807), the two largest destinations for DWs in the Middle East and Asia, respectively.

The intervention was implemented from May 26 to August 15, 2014. In this range of dates, outbound DWs were surveyed face-to-face while waiting for the start of CPDEP sessions at OWWA's central training site in Manila. Sampling of participants and treatment assignment was based on CPDEP registration lists made available to our research team the day before each session. Randomization of the intervention was at the individual level, with no stratification. Details on the sampling protocol and the implementation of the randomization are discussed in Appendix A. Survey staff provided instructions and handed over the small gift (the pack of dried mangoes) to DWs randomly assigned to the intervention (the treatment group) at the end of the baseline interview.

Table 1 presents summary statistics and balance tests. DWs are, on average, about 30 years old. 51% of DWs bound for Hong Kong and 44% of DWs bound for Saudi Arabia are married. About three-quarters have at least one child. Levels of educational attainment differ by destination. 20% of DWs bound for Hong Kong have a college degree; 34% have at most a high school degree. DWs bound for Saudi Arabia are less educated, with only 11% having a college degree and 74% having at most a high school degree. About 40% of DWs have prior experience as a DW in the Philippines. The vast majority of DWs come from outside the National Capital Region, use the internet, and have no personal savings. DWs typically do not know any other person at the destination. Half of DWs bound for Hong Kong have already talked to their employers at the time of the baseline interview, while only 13% of DWs bound for Saudi Arabia have done so. Balance checks reveal no significant differences between observable characteristics of study participants across treatment and control groups. As pre-specified, we include these baseline characteristics as controls in all our regressions.

We now consider compliance with the randomized intervention. Treated DWs might not follow the instructions to introduce themselves as suggested. Moreover, control group members could have learned about the intervention from treated individuals, and followed the instructions themselves. Table D.1 in the appendix shows that there is indeed non-compliance. While almost all DWs in the treatment group remember the instructions at the time of the endline survey after two years (92%), only about two-thirds report having followed them. In addition, 17% of DWs in the control group report remembering the intervention instructions and 8% report having introduced themselves with the gift and photo. Thus, our estimates should be considered intention-to-treat

effects.

Follow-up interviews and attrition

In addition to the baseline interview with the DWs, we conducted a baseline interview with members of the household remaining in the Philippines. These interviews took place on average about twelve weeks after the interview with the migrant.

Outcomes were collected in a midline and an endline survey. Surveying migrant DWs comes with two distinct challenges. First, during the migration process, a large proportion of their contact information changes, with the new contact information usually not known at the time of the baseline survey. Second, DWs live in their employer's house and are tightly monitored. Thus, they have few possibilities to interact with interviewers freely, especially about sensitive topics like how they are treated.

To overcome the first challenge, we collected DWs' contact information for multiple channels (phone, email, social media, etc.) as well as the contact information of their household members remaining in the Philippines. Households' contact information is more stable over time and households will have updated contact information from their members abroad. To overcome the second challenge, we relied on a combination of direct phone interviews with DWs and proxy interviews with knowledgeable household members.

The midline survey was conducted via proxy interviews with household members in the Philippines between April 2015 and January 2016. We completed 1,426 interviews, which results in a re-interview rate of 72%. Most (96%) households report that they have been in contact with the DW at least once per week and a knowledgeable household member could thus provide information on the DW's situation. Respondents were instructed to answer questions only if they felt well-informed about the specific issue.

The endline survey took place about two years after the baseline interview, around the end of the initial contract duration. It was organized primarily as direct interviews with the DWs, with proxy interviews only in instances where a direct interview was not possible. For direct interviews, we paid utmost attention to privacy and safety of the respondents. To set up direct interviews with DWs, we asked household members to reach out to the DW to inform her about two ways to conduct the interview: (i) provide the survey company with a suitable time for a call, or (ii) call the survey company directly at any time convenient for them. This procedure ensured that the interview took place at a time when the DWs could talk freely. Interviews were conducted in Tagalog or another Philippine dialect to further increase privacy. DWs who had returned to the Philippines were interviewed in person as part of the household interview.

This strategy led to 986 direct interviews. In case a direct interview was not possible, we conducted a proxy interview (441 cases). In total, we completed 1,427 interviews, which results in a re-interview rate of 72%.⁹ In case we could not collect endline data, we replace a missing endline value with the corresponding midline value to minimize attrition. We also conducted an endline survey with households in the Philippines, which took place at about the same time.

We test for selective attrition in a number of ways. Table D.2 in the appendix shows that assignment to the intervention is not correlated with whether an endline interview with a DW was completed via either interview method (column 1), via a direct interview (column 2), or via a proxy interview (column 3). Likewise, assignment to the intervention is not correlated with the likelihood of re-interviewing the DW’s household in the Philippines (column 4). Throughout, baseline characteristics have little power to predict re-interview status. The R-squared of the corresponding regressions is low. There is hence no indication that our sample systematically loses specific types of DWs over time.

Outcomes and hypotheses

We examine outcomes and hypotheses as specified in our pre-analysis plan (PAP).¹⁰ We group outcomes by domain and construct an aggregate index or standardized treatment effect (STE) for most domains. For the construction of the STE, we follow Kling *et al.* (2007).¹¹ Details on the construction of indices are in Appendix B.

Our primary hypothesis is that the intervention leads to better treatment by the employer, which we measure as the STE of several measures of mistreatment of the DW by members of the employer’s household. We introduce this measure in more detail below. As secondary hypotheses, we test whether the intervention leads to a better subjective assessment of the relationship with the employer, affects the DW’s plans for staying with the employer, has a positive effect on the DW’s subjective wellbeing, and affects

⁹The re-interview rate is comparable or even higher to those of other studies that have tracked migrants from their origin to their destination countries and over a similar period of time. Ambler (2015) successfully re-interviewed 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, Gibson *et al.* (2018) 64 percent of migrants from Tonga to New Zealand, and Barsbai *et al.* (2020b) 61 percent of migrants from the Philippines to the U.S.

¹⁰ This study is registered with the AEA RCT Registry. Our AEA RCT Registry number is AEARCTR-0003837 for the field experiment and AEARCTR-0003788 for the online experiment. The PAPs are available for download from the registry. For the field experiment, we specified a PAP for midline results and a slightly updated PAP for endline results that also specifies hypotheses regarding household outcomes.

¹¹ 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 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 means in the treatment and control groups in standard deviation units.

whether the DW thinks it would be good for her children to become migrant workers themselves. In addition, we test how the intervention affects household members in the Philippines. Our outcomes of interest include the subjective assessment of the effect of DW's migration on the household, and the intention of household members to become migrant workers themselves.

We also consider a few additional outcomes for exploratory analysis to better understand the effects of the intervention. We clearly highlight such exploratory analysis in the description of our results below. The PAP also details the hypotheses tested for the other interventions evaluated as part of the larger research project. More information on and results from the larger research project can be found in the project report for the funding agency *3ie* (Barsbai *et al.*, 2020a).

Measuring mistreatment

Our main outcome of interest is an index that aggregates different dimensions of the DW's treatment by the employer. It is defined as a Standardized Treatment Effect (STE) of the following mostly binary indicators that aim to capture different dimensions of mistreatment and contract violations: (i) not having been shouted at, (ii) not having experienced physical violence, (iii) not having been threatened, (iv) not having experienced sexual harassment, (v) having had enough food, (vi) having had a weekly rest day, (vii) not having been forced to work when sick, (viii) having been allowed to leave the employer's house, (ix) having received salary on time, (x) not having experienced salary deductions, and (xi) hours of daily spare time.¹²

DWs in the control group report mistreatment and contract violations for many of these dimensions, with conditions generally being worse in Saudi Arabia than in Hong Kong (Table 3). 44% of non-treated DWs were shouted at, 14% did not have enough food, 60% had no weekly rest day (13% in Hong Kong and 91% in Saudi Arabia), 21% had to work when sick, 65% were not allowed to leave the house (19% in Hong Kong, 95% in Saudi Arabia), and 27% did not receive their salary on time (13% in Hong Kong and 36% in Saudi Arabia). 8% of non-treated DWs even experienced physical violence, and 5% sexual harassment (2% in Hong Kong and 7% in Saudi Arabia).

DW treatment by the employer is closely linked to DWs' subjective wellbeing. Table D.3 in the appendix shows that better treatment of the DW by the employer correlates

¹²These indicators were chosen with reference to standardized employment contracts negotiated between the government of the Philippines and destination-country governments. These contracts regulate issues like working time, salary and mode of payment, provision of food and accommodation. For Hong Kong, such a contract can be found here: <https://www.immd.gov.hk/eng/forms/forms/fdhcontractterms.html>. A description of the contract content for Saudi Arabia can be found here: <https://www.dole.gov.ph/news/dole-welcomes-new-employment-protection-measures-for-saudi-bound-hsws-to-stem-abuse-and-exploitation/>.

with higher subjective wellbeing of the DW. A one standard deviation increase in the STE of the treatment of the DW is associated with a 0.61 standard deviation increase in the DW’s subjective wellbeing (see Appendix B for the definition). The high correlation suggests that our aggregate index is a meaningful measure of treatment by the employer.

Empirical analysis

We use the following equation to estimate the effect of the intervention:

$$Y_i = \beta_0 + \beta_1 Intervention_i + X_i' \theta + \varepsilon_i \quad (1)$$

where Y_i is the outcome of interest for DW i and $Intervention_i$ an indicator for being randomly assigned to the treatment group. Thus, the coefficient β_1 is the intent-to-treat (ITT) effect of assignment to the treatment group. X_i' is a vector of pre-treatment covariates as specified in the PAP. Their inclusion in the model should improve precision and address chance imbalances. In the standard specification, we include age, education, an indicator for having a child, an indicator for no internet use, an indicator for having a personal bank account, time elapsed since the baseline interview (log days), and an indicator for working in Saudi Arabia.¹³

To minimize attrition in our outcome data, we replace information not available from direct interviews with information from endline (second priority) or midline proxy interviews (third priority). In specifications with outcomes based on both direct and proxy interviews, we also include an indicator for whether the data point comes from a proxy interview. For individual hypotheses, we add further control variables – in particular, pre-treatment measures of the respective outcomes – as described in the PAP and in the respective table notes.¹⁴ We report Huber-White robust standard errors.

4 Results

We present our results in three steps. First, we present evidence from the field experiment on the effect of the intervention on the treatment of DWs by their employer. Second, we summarize results from the online experiment that shed light on how the intervention changes employers’ behavior towards DWs. Third, we return to the field experiment and show how the intervention affects household members of DWs who stay behind in the Philippines.

¹³The Saudi Arabia indicator is dropped when the Hong Kong and Saudi Arabia samples are analyzed separately.

¹⁴ X_i' also includes indicators for participating in one of the two group interventions that were randomized at the classroom level. These two interventions were randomized independently and are orthogonal to the intervention studied here. See Barsbai *et al.* (2020a) for more details on these interventions.

Effects of the intervention on the treatment of DWs

The intervention triggered overwhelmingly positive reactions by employers. 88.5% of DWs who said they followed the intervention instructions report that their employers reacted positively; 11% report a neutral reaction. Only two DWs report a skeptical reaction. Overall, 14 employers refused to accept the gift.¹⁵

Table 2 presents estimates of the impact of being assigned to the intervention (coefficient β_1 in Equation 1) on the treatment of DWs by their employers and related outcomes. We present results for the full sample (top row), as well as separately for DWs in Hong Kong and Saudi Arabia (middle and bottom rows).

We first focus on the aggregate index that summarizes different dimensions of better treatment by the employer (column 1). Being assigned to the intervention increases the aggregate index in the full sample by 0.035 standard deviations. The effect is close to being statistically significant (p-value 0.108). The treatment effect is larger and statistically significant in the Hong Kong sample (0.066 standard deviations) and close to zero in Saudi Arabia.^{16 17}

Table 3 shows the effect on the individual components of the index for insights into the dimensions that drive the overall effect. The overall effect on the aggregate index masks considerable heterogeneity. In Hong Kong, the intervention helps to reduce mistreatment in the form of shouting (column 1; p-value 0.136). It also improves working conditions. Treated DWs are more likely to have enough food (column 6) and be allowed to leave the employer's house (column 8). In Saudi Arabia, the intervention helps to reduce sexual harassment (column 4) and treated DWs are substantially more likely to receive their salary on time (column 9). But we also find that treated DW are less likely to have enough food (column 6). The intervention hence affects different aspects of a DW's treatment by their employer in the two countries, which offer quite different working environments for DWs.

In column 2 of Table 2, we consider a DW's satisfaction with the relationship with the

¹⁵A few DWs shared more details about the reaction of their employers: "They prayed for my child in the Philippines." "They were happy. They looked at my children's pictures. They had gifts sent to my child." "When I went home to the Philippines, my employer asked me to buy a lot of dried mangoes. They liked it."

¹⁶We correct for testing multiple hypotheses in the subgroups formed by the two destinations using the *mhtreg* Stata package (Steinmayr, 2020). It modifies the procedure introduced by List *et al.* (2019) to control for the family-wise error rate (for more details see Barsbai *et al.* (2020b)). The adjusted p-values are 0.047 for Hong Kong and 0.67 for Saudi Arabia.

¹⁷In a robustness check, Appendix Table D.4, column 2 shows that results change very little when no controls are included. The same is true when we drop cases where interviewers deviated from the randomized assignment (column 3). Our findings also hold when we restrict the analysis to directly reported data from DWs, which might be more reliable (column 4). If anything, the coefficients become slightly larger. The treatment effect for DWs in Hong Kong increases from 0.06 to 0.09 standard deviations, suggesting that DWs might not fully share their experiences of mistreatment with household members in the Philippines. Column 5 shows results from an instrumental variable regression using assignment to the intervention as instrument for having followed the instructions of the intervention. In this analysis, we find a treatment effect of 0.135 standard deviations for DWs in Hong Kong. The treatment effect amounts to 0.062 standard deviations for DWs in Saudi Arabia and 0.08 standard deviations for the entire sample, but these are not precisely estimated.

employer as an alternative outcome variable. This variable is measured as the average of respondent ratings (1-5 scale; higher is better) of the relationship with the employer initially (retrospective report of satisfaction in first month of job) and at the time of the endline survey. In line with the previous findings, the intervention makes DWs more satisfied with the relationship with their employers. Being assigned to the intervention increases a DW's satisfaction by 0.102 in the entire sample (or 2.9 percent when compared to the control-group mean of 3.47). The increases in satisfaction are larger for DWs in Hong Kong (0.167 or 4.6 percent when compared to the control-group mean of 3.60) and smaller for DWs in Saudi Arabia (0.064 or 1.9 percent when compared to the control-group mean of 3.37; not statistically significant).

So far, our outcome variables have relied on self-reported measures of treatment by the employer. In column 3, we look at whether the DW still works for the initial employer at the time of the endline interview or is planning to return to the employer soon (as opposed to returning to the Philippines or working for a different employer abroad). At this time, the initial contract is already completed. The decision to renew the contract therefore comes close to revealed preferences (of both parties), and helps mitigate experimenter-demand effects. An additional advantage of this outcome is that it implicitly captures dimensions of mistreatment and wellbeing that the previous set of variables are unable to measure. Assignment to the intervention increases the likelihood of a DW remaining with the initial employer by 3.8 percentage points (or 21 percent when compared to the control-group mean of 0.18). The treatment effect is particularly large for DWs in Saudi Arabia (7.5 percentage points or 44 percent when compared to the control-group mean of 0.17 percent), while it is close to zero and not statistically significant for DWs in Hong Kong. This finding might reflect the fact that working conditions are more favorable in Hong Kong than in Saudi Arabia to start with.

Consistent with the above findings, treated DWs are more likely to agree with the statement that it will be good for their children if they become an Overseas Filipino Worker (OFW) when they grow up (column 4). Being assigned to the intervention increases the likelihood of agreeing with this statement by 5.4 percentage points (or 23 percent of the control mean of 0.23). Again, the treatment effect is more pronounced for DWs in Saudi Arabia (7.3 percentage points or 37 percent of the control mean of 0.20) and closer to zero for DWs in Hong Kong.

Overall, the results in Table 2 show that the intervention improves the treatment of the DW by the employer. For DWs in Hong Kong, the intervention improves the relationship between the DW and the employer and the working conditions. For Saudi Arabia, the intervention does not affect these "softer" dimensions but helps to prevent more severe

forms of mistreatment including sexual harassment and not receiving salary on time. As a result, treated DWs in Saudi Arabia are more likely to remain with their initial employer and consider overseas working as a favorable choice for their own children.

Online experiment

We complement the field study with an online experiment. The online experiment helps us understand whether the treatment affects the employer’s behavior towards the domestic worker, and which underlying mechanism is driving that change. It closely mimics the asymmetric bargaining power between employer and employee in the field experiment. We measure behavior of potential employers – dictators – using an incentivized dictator game. We manipulate the contents of messages potential employees – receivers – communicate prior to the dictator’s decision. On top of shedding light on dictator’s behavior, the design also allows us to separate the effects of the intervention: Prior to the dictator’s decision, a receiver either shows a photo of her family, sends a small gift, does both, or does nothing. The online experiment measures the immediate behavioral responses of dictators, while shutting down any endogenous reactions from the receivers. Full wording of the instructions is presented in Appendix C.¹⁸

Dictators sample

We collected data from 1,662 participants in the dictator role from Saudi Arabia, United Arab Emirates, and Hong Kong.¹⁹ The sample is representative of upper- and middle-income populations of each of the countries, who are likely to employ a DW. Indeed, 66% of the participants reported ever having employed a DW. Table D.5 provides summary statistics by country.²⁰

Receivers sample

Dictators’ choices affect payoffs of real receivers. We recruited ten female participants in the receiver role in the Philippines. The participants all agreed in a pre-screening survey to provide information that would allow us to construct the treatment manipulations.

¹⁸Here we focus on the primary outcome defined in the pre-analysis plan. We also specified several secondary hypotheses. However, we do not present these results as they are inconclusive.

¹⁹The survey firm was unable to recruit a large enough sample for Saudi Arabia. For this reason, we recruited additional participants from the United Arab Emirates, which like neighboring Saudi Arabia is an oil-producing country and major destination of Filipino DWs. Our main results remain robust when focusing on the Saudi Arabia sample alone. Our final sample consists of 407 individuals from Saudi Arabia, 255 from the United Arab Emirates, and 1,000 from Hong Kong.

²⁰We focus on a sample of individuals that is different from the sample of employers in the main field experiment for ethical reasons. We did not want to reveal to the employers that the DW working in their household participates in a research study to protect DWs from unintended consequences.

The receivers were aware that their information could be shared with some dictators and that we might share only a subset of this information. Participants were compensated with a show-up fee of \$5 during the first meeting to compensate them for their time. Their earnings from the dictator game were delivered with a delay.

Dictator game

Dictators redistribute money between themselves and the matched Filipino receiver. We use a dictator game with a restricted choice set where dictators receive an endowment of \$20 and can transfer up to \$10 to the matched receiver. Any integer value between \$0 and \$10 in \$1 increments is possible. The receivers are not endowed and are passive in the game. The skewed bargaining power closely resembles the relationship between employers and DWs.

We explained the complete rules of the dictator game including the recruitment process of dictators to the receivers. The dictators were told they would be matched with one of the receivers and that their choices had a positive probability of being payoff relevant. At the end of the experiment, we randomly selected one payoff-relevant choice from Hong Kong and one from one of the Middle Eastern countries to be the actual payoff for the recipient. We describe details of the online experiment in Appendix C.

Interventions

Before dictators made their decisions, we exogenously manipulated whether additional information was provided to them by the receivers. The experimental conditions were the following:

- **No info.** No additional information was provided about the receiver, beyond their being around 30 years old, married, female, and from the Philippines, the profile of a typical DW. This manipulation mimics that of the control treatment in the field experiment.
- **Photo.** On top of the no info treatment information, receivers reveal photos of their families together with a short description of their family in a instant messaging application-like environment.²¹ This intervention is aimed at reducing social distance between the dictator and the receiver.

²¹A standardized photo of a family with the wife, husband, and their child or children was taken inside of their home. The background was chosen to be neutral and we made sure the photos do not signal the neediness of the respective families. We also asked the adult women to cover their bare shoulders so that conservative Muslims are not offended. The accompanying message was standardized and said: "Hello. My name is FIRST NAME WIFE, I am X years old. Next to me is my husband, FIRST NAME HUSBAND who is Y years old. There is also our [son/daughter], FIRST NAME CHILD. [S/he] is Z years old. We live in the Philippines."

- **Gift.** On top of the no info treatment information, receivers send a small online gift, a photo of a Filipino landscape with a personal note, to the dictator. This intervention is aimed at generating feelings of reciprocity on the part of dictators towards receivers. We designed the gift to mimic the original gift used in the main study – a pack of dried mangoes. It satisfies the following features of a non-monetary gift (Falk, 2007; Kube *et al.*, 2012): (i) It can be perceived as an intentional act of receiver’s kindness, (ii) it comes at a – small and non-monetary but positive effort – cost to the receiver, and (iii) it is easy to process by the dictator. We also made sure that the gift does not signal a perception of neediness or economic status of the receiver.
- **Photo and gift.** Both the photo and gift treatments were provided.

The experimental design allows us to causally estimate whether dictators’ behavior changes with the treatment manipulations.²²

To further separate mechanisms underlying dictators’ behavior, we also manipulate the default allocation of money in the dictator game. The default either proposes an unequal allocation in which the dictator keeps the entire endowment, or splitting the money equally between the two parties (non-egalitarian and egalitarian default, respectively). The choice set remains constant across the two default allocations. Given the restricted choice set allowing maximum transfers of USD 10 to the recipient, any active change by the dictator under the egalitarian default results in a reduction of the receiver’s payoff. The opposite is true for the non-egalitarian default. This experimental design allows us to study the effects of exogenously changing the moral costs of causing harm (as in List, 2007), without it being confounded by the photo and gift treatment variation.

We build the default manipulation on evidence from Krupka and Weber (2013) and Cox *et al.* (2016) that manipulating the default allocation in dictator games establishes different social norms. Imagine a situation in which the total endowment is fixed, and the initial allocation is an equal split between the dictator and the receiver. A dictator’s choice to “take” even a small amount from receiver’s initial allocation would be perceived as a norm violation. In contrast, imagine a different scenario in which the initial endowment is allocated entirely to the dictator. A dictator’s action resulting in the same final payoff as in the previous case would be perceived as an act of altruism. The reason is that now the money is perceived as being “passed” to the receiver. The cost of selfish action increases when a non-egalitarian choice stems from own action rather than from

²²Table D.5 also presents summary statistics by treatment and balance tests document successful randomization.

a default allocation where no active choice needed to be made.

Empirical analysis

The analysis follows a pre-analysis plan.²³ As pre-specified, we conduct the analysis on the full sample of participants across the three countries. We estimate the following equation to test our primary hypothesis that the treatments affect dictators' prosocial behavior:

$$Transfer_i = \beta_0 + \beta_1 Photo_i + \beta_2 Gift_i + \beta_3 Photo\&Gift_i + \beta_4 default_i + X_i' \theta + \varepsilon_i \quad (2)$$

where $Transfer_i \in [0,10]$ is the transfer of dictator i in US dollars. The coefficients of interest are β_1 , β_2 , and β_3 that represent the photo, gift, and photo and gift treatment effects, respectively. The excluded category is the no info treatment. We also include a dummy for egalitarian default $default_i$, that stands for a default allocation being (10,10). X_i' is a vector of individual demographic characteristics,²⁴ country dummies, a set of dummy variables for an assigned matched receiver, and characteristics of the hardware respondents used. We report Huber-White robust standard errors.

Results

Table 4 summarizes our results. Column 1 presents the pooled results across all countries. Relative to the control group, dictators seeing the photo of the family of the matched receiver transfer 83 cents more (14 percent more when compared to the control-group mean of USD 6.07). Neither the gift treatment, nor the combined photo and gift treatment have statistically significant effects, despite having positive point estimates. The effect of the egalitarian default is substantial, increasing transfers by USD 3.20 relative to the non-egalitarian default. This is consistent with the increased moral cost of causing harm under the egalitarian treatment where dictators need to "take" money away from receivers when deviating from the default allocation.

Columns 2 and 3 present results for the Hong Kong sample and the Middle Eastern sample separately. For both samples, the photo treatment coefficient remains positive and statistically significant, while the gift and the photo and gift treatments remain

²³ The AEA RCT Registry number is AEARCTR-0003788.

²⁴We include a set of dummies for age range (9 dummies), education level (5 dummies), employment status (6 dummies), marital status (3 dummies), number of children (5 dummies), and a variable for income converted to 2018 PPP US dollars.

insignificant.²⁵

Columns 4 and 5 split the sample by the orthogonal manipulation of the default allocation. Interestingly, the coefficient is highest and significant only for the non-egalitarian default (OLS $p=0.06$), i.e. the setting where moral costs of causing harm are lower as opposed to the egalitarian default. The photo treatment seems particularly effective in relationships with high initial inequalities, such as employer-DW relationships examined in the field experiment.²⁶ To summarize, the positive effects of the photo treatment suggest that reduced social distance between the dictator and the receiver seems to be the driving behavioral mechanism on the side of the employers.

A potential concern is that the effect of the photo treatment on increased altruism towards Filipina recipients is driven by factors other than reduced social distance. The treatments may have increased salience of the matched recipient. This may have resulted in treatment-specific beliefs about whether the recipient is a real person. 86 percent of participants report that they believe that the matched receiver was a real person, while the remaining 14 percent believe otherwise. This share is not treatment specific. Our results hold when restricting the sample to those who believe the matched receiver was real. Finally, our results are robust to alternative specifications such as when we include no control variables, when we pool the three treatments into a single treatment category, and when we restrict the sample to participants for whom we have complete survey data (Appendix Table D.6).

Effects of the intervention on household members in the Philippines

We now return to the field experiment and examine how the intervention affects household members in the Philippines. As the photo aims to illustrate to employers, DWs are part of transnational households with partners, children, and other family members who stay behind in the Philippines. Indeed, the situation of the DW abroad is reflected in Philippine households' assessment of how migration has affected their household. As columns 1 and 2 of Table D.7 in the appendix show, better treatment of the DW by the employer is associated with how positively households assess the effects of DWs' employment abroad on the household. A one-standard-deviation increase in the STE of the treatment of the DW (as reported by the DW) is associated with a 0.092 standard-deviation increase in the STE of the household's subjective assessment of the effects of migration on the household (as reported by the household).

²⁵Restricting the sample to Saudi Arabia respondents only, the point estimate for the photo treatment is 1.15 ($p=0.099$). Results are not shown but available on request.

²⁶This effect may also be driven by ceiling effects. The egalitarian default control mean is USD 7.83, not far from the maximum transfer of USD 10 the experimental design allows.

At the same time, household members in the Philippines are much less aware of the intervention than DWs themselves. Only 18 percent of those with a DW in the treatment group know about the assignment of the intervention (column 3 of Table D.7 in the appendix, relative to the control group). Experimenter-demand effects are hence unlikely to explain household-level effects of the intervention.

In Table 5, we present regression results for the effect of the DW being assigned to the intervention on the household's subjective assessment of the effects of migration on the household. On a range of dimensions, household respondents assessed whether migration of the DW led to improvement, had no effect, or led to a worsening. The DW's assignment to the intervention leads to an overall more positive subjective assessment by the household (column 1).²⁷ This effect is similar for households with DWs in Hong Kong and Saudi Arabia but is only marginally significant for the first group. As the individual component variables are ordinal, we estimate treatment effects using an ordered probit model (columns 2-8). Overall, the intervention leads to a more positive assessment of the effect of migration on a household's financial security (column 2), standard of living (column 3), education (column 6), family life (column 7), and social life (column 8). Again, the treatment effects differ by destination of DWs. For households with a DW in Hong Kong, the effects are concentrated on standard of living and social life. For households with a DW in Saudi Arabia, the effects are concentrated on financial security, education, and family life. Results are robust to using binary variables for more positive subjective assessment (see Table D.8 in the appendix).

To follow-up on the effects on the subjective assessment of education outcomes, particularly of households with a DW in Saudi Arabia, we look at education expenditures and school enrollment of children in exploratory (not pre-specified) analyses. Table 6 presents regression results. Assignment to the intervention increases annual education expenditures of households with a DW in Saudi Arabia by USD 73 (column 1, the control group mean is USD 282) or 43 percent when we use a log specification (column 2). The substantial effect on education expenditures also translates into higher enrollment (columns 3-5). The enrollment effect is concentrated among older children, aged 13-20, who largely attend secondary school (column 5). Assignment to the intervention increases the likelihood of enrollment in this age group by 7.2 percentage points (or 9 percent when compared to the control-group mean of 0.79). The enrollment effect is positive but smaller in magnitude for younger children, as primary school attendance is mandatory and therefore very high to start with (column 4). Consistent with

²⁷Column 1 shows the effect on the mean of the subjective assessment of the effect of migration by the household respondent (across columns 2-8). These variables are coded as 1 "worse", 2 "same", and 3 "better".

higher secondary school enrollment, we also find that older children are less likely to be employed (column 6). We do not observe significant treatment effects on educational outcomes for households with a DW in Hong Kong, which might reflect our earlier finding that these households do not experience subjective improvements in the domain of education.

Table D.9 in the appendix explores whether remittances can link migrant-level with household-level outcomes. Consistent with the previous finding that treated DWs in Saudi Arabia are substantially more likely to receive their salary on time (column 9 of Table 3), the intervention increases the amount of remittances received by households with a DW in Saudi Arabia by 21 percent (column 1). However, this result does not hold when we look at the amount of remittances sent as reported by the migrant (column 2). There are no treatment effects on the amount of remittances for households with a DW in Hong Kong. In addition, the intervention does not affect the frequency of remittance flows (columns 3 and 4) and the variance of remittances (columns 5). While the evidence is mixed, remittances may account for the treatment effect on educational outcomes of households with a DW in Saudi Arabia. By helping the DW to receive her salary on time, the intervention could bolster remittance flows, which in turn might allow households to increase their education expenditures and send more children to school. In addition, the intervention might drive human capital investments by increasing the likelihood the migrant continues working overseas (column 3 of Table 2).

5 Conclusion

We study the impacts of a simple behavioral intervention that aimed to reduce mistreatment of international migrant workers by their employers. The study participants were Filipino women about to depart for overseas work as domestic workers (DWs) in private households in Hong Kong and Saudi Arabia. We implemented a randomized controlled trial in which the treatment encouraged DWs to show a picture of their family while giving their employer a small gift at the start of employment. The treatment had positive impacts on a range of dimensions. Compared to the control group, treated DWs reported less mistreatment by their employers (measured by an index of behaviors such as verbal and physical abuse, working conditions, and timely salary payments) and higher satisfaction with the employment relationship. They also were more likely to have remained with the same employer after two years and were less likely to have returned to the Philippines. What's more, their family members back in the Philippines also viewed international labor migration more positively.

To understand the mechanisms behind these effects, we implemented an additional online experiment with participants in Hong Kong and the Middle East. The online experiment aimed to determine whether the photo itself, the gift itself, or the combination of the two was responsible for the treatment effect. The results suggest that the photo alone could explain the entirety of the treatment effect. We interpret the online experiment's results to indicate that a reduction in perceived social distance stemming from seeing the photo leads employers to be more generous towards their DW employees.

Compared to other approaches to improving the working conditions of workers, a novel feature of the intervention we study is that it does not require any public policy changes in migrant workers' destinations. While it can be facilitated and encouraged by origin-country governments or NGOs, it can actually be implemented unilaterally by migrants themselves due to its simplicity and low cost. However, it is important to stress that such an intervention should not substitute for other policies aiming to reduce mistreatment of migrant workers, such as legal protections for workers in destination countries.

There are a number of promising avenues for future research. First, it would be worth exploring approaches to reducing social distance that might go beyond simply sharing a family photo. In addition, it would be worth using a similar research design to investigate whether an intervention to reduce social distance between workers and employers would have similar effects in other types of jobs: are such effects limited to household service workers, or would they also emerge among employees in different types of firms? Finally, the difference in our results across our Saudi Arabia and Hong Kong subsamples suggests that future studies should explore how the effects of interventions to reduce social distance vary in different social contexts.

References

- ALESINA, A. and LA FERRARA, E. (2000). Participation in heterogeneous communities. *The Quarterly Journal of Economics*, **115** (3), 847–904.
- AMBLER, K. (2015). Don't tell on me: Experimental evidence of asymmetric information in transnational households. *Journal of Development Economics*, **113**, 52 – 69.
- ASHRAF, N., AYCINENA, D., MARTINEZ, C. and YANG, D. (2015). Savings in transnational households: A field experiment among migrants from El Salvador. *Review of Economics and Statistics*, **97** (2), 332–351.
- BARSBAL, T., BARTOS, V., LICUANAN, V., STEINMAYR, A., TIONGSON, E. and YANG, D. (2020a). *Harnessing the Development Benefits of International Migration: A Randomized*

- Evaluation of Enhanced Pre-Departure Orientation Seminars for Labor Migrants from the Philippines to Hong Kong and Saudi Arabia*. Grantee final report, 3ie.
- , LICUANAN, V., STEINMAYR, A., TIONGSON, E. and YANG, D. (2020b). *Information and the Acquisition of Social Network Connections*. NBER Working Paper 27346, National Bureau of Economic Research.
- BAZZI, S., CAMERON, L., SCHANER, S. and WITOELAR, F. (2021). *Information, Intermediaries, and International Migration*. NBER Working Paper 29588, National Bureau of Economic Research.
- BINZEL, C. and FEHR, D. (2013). Social distance and trust: Experimental evidence from a slum in cairo. *Journal of Development Economics*, **103**, 99–106.
- BLOUIN, A. and MUKAND, S. (2019). Erasing ethnicity? Propaganda, nation building, and identity in Rwanda. *Journal of Political Economy*, **127** (3), 1008–1062.
- BOHNET, I. and FREY, B. S. (1999). Social distance and other-regarding behavior in dictator games: Comment. *American Economic Review*, **89** (1), 335–339.
- CABALLERO, M. E., CADENA, B. C. and KOVAK, B. K. (2021). *The International Transmission of Local Economic Shocks Through Migrant Networks*. NBER Working Paper 28696, National Bureau of Economic Research.
- CHANDRASEKHAR, A. G., KINNAN, C. and LARREGUY, H. (2018). Social networks as contract enforcement: Evidence from a lab experiment in the field. *American Economic Journal: Applied Economics*, **10** (4), 43–78.
- CHARNESS, G. and GNEEZY, U. (2008). What’s in a name? Anonymity and social distance in dictator and ultimatum games. *Journal of Economic Behavior & Organization*, **68** (1), 29–35.
- and RABIN, M. (2002). Understanding social preferences with simple tests. *The Quarterly Journal of Economics*, **117** (3), 817–869.
- CHEN, Y., LI, S. X., LIU, T. X. and SHIH, M. (2014). Which hat to wear? Impact of natural identities on coordination and cooperation. *Games and Economic Behavior*, **84**, 58–86.
- CLEMENS, M. A. and TIONGSON, E. R. (2017). Split decisions: Household finance when a policy discontinuity allocates overseas work. *The Review of Economics and Statistics*, **99** (3), 531–543.
- COX, J. C., LIST, J. A., PRICE, M., SADIRAJ, V. and SAMEK, A. S. (2016). *Moral Costs and Rational Choice: Theory and Experimental Evidence*. NBER Working Paper 22234, National Bureau of Economic Research.
- DAHLBERG, M., EDMARK, K. and LUNDQVIST, H. (2012). Ethnic diversity and preferences for redistribution. *Journal of Political Economy*, **120** (1), 41–76.

- DINKELMAN, T., KUMCHULESI, G. and MARIOTTI, M. (2020). *Labor migration, capital accumulation, and the structure of rural labor markets*. Working Paper 22, GLM|LIC.
- ECKEL, C. C. and GROSSMAN, P. J. (2005). Managing diversity by creating team identity. *Journal of Economic Behavior & Organization*, **58** (3), 371–392.
- FALK, A. (2007). Gift exchange in the field. *Econometrica*, **75** (5), 1501–1511.
- and FISCHBACHER, U. (2006). A theory of reciprocity. *Games and Economic Behavior*, **54** (2), 293–315.
- FERNANDO, A. N. and SINGH, N. (2021). Regulation by reputation? Quality revelation of labor intermediaries in international migration. *Working Paper*.
- GIBSON, J., MCKENZIE, D., ROHORUA, H. and STILLMAN, S. (2018). The long-term impacts of international migration: Evidence from a lottery. *World Bank Economic Review*, **32** (1), 127–47.
- , — and STILLMAN, S. (2011). The impacts of international migration on remaining household members: Omnibus result from a migration lottery program. *Review of Economics and Statistics*, **93** (4), 1297–1318.
- GOVERNMENT OF THE PHILIPPINES (2015). *Survey on Overseas Filipinos*. Manila: Philippine Statistical Authority.
- HOFFMAN, E., McCABE, K. and SMITH, V. L. (1996). Social distance and other-regarding behavior in dictator games. *The American Economic Review*, **86** (3), 653–660.
- ILO (2006). *Declaration on Fundamental Principles and Rights at Work*. International Labor Organization.
- (2015). *ILO Global Estimates on International Migrant Workers: Special Focus on Migrant Domestic Workers*. Geneva: International Labor Organization, Department of Statistics.
- IOM (2020). *World Migration Report 2020*. Geneva: International Organization for Migration, United Nations.
- JONES, B. and RACHLIN, H. (2006). Social discounting. *Psychological Science*, **17** (4), 283–286.
- JONES, B. A. (2021). A review of social discounting: The impact of social distance on altruism. *The Psychological Record*, **72**, 511–515.
- KHANNA, G., MURATHANOGLU, E., THEOHARIDES, C. and YANG, D. (2022). *Abundance from Abroad: Migrant Income and Long-Run Economic Development*. NBER Working Paper 29862, National Bureau of Economic Research.
- KLING, J. R., LIEBMAN, J. B. and KATZ, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, **75** (1), 83–119.
- KRUPKA, E. L. and WEBER, R. A. (2013). Identifying social norms using coordination

- games: Why does dictator game sharing vary? *Journal of the European Economic Association*, **11** (3), 495–524.
- KUBE, S., MARÉCHAL, M. A. and PUPPE, C. (2012). The currency of reciprocity: Gift exchange in the workplace. *American Economic Review*, **102** (4), 1644–62.
- LEIDER, S., MÖBIUS, M. M., ROSENBLAT, T. and DO, Q.-A. (2009). Directed altruism and enforced reciprocity in social networks. *The Quarterly Journal of Economics*, **124** (4), 1815–1851.
- LIGON, E. and SCHECHTER, L. (2012). Motives for sharing in social networks. *Journal of Development Economics*, **99** (1), 13–26.
- LIST, J. A. (2007). On the interpretation of giving in dictator games. *Journal of Political Economy*, **115** (3), 482–493.
- and PRICE, M. K. (2009). The role of social connections in charitable fundraising: Evidence from a natural field experiment. *Journal of Economic Behavior & Organization*, **69** (2), 160–169.
- , SHAIKH, A. M. and XU, Y. (2019). Multiple hypothesis testing in experimental economics. *Experimental Economics*, **22** (4), 773–793.
- MALHOTRA, R., ARAMBEPOLA, C., TARUN, S., DE SILVA, V., KISHORE, J. and OSTBYE, T. (2013). Health issues of female foreign domestic workers: A systematic review of the scientific and gray literature. *International Journal of Occupational and Environmental Health*, **19** (4), 261–277.
- OECD (1996). *Trade, Employment and Labour Standards: A Study of Core Workers' Rights and International Trade*. Organization for Economic Cooperation and Development.
- SCHELLING, T. C. (1968). The life you save may be your own. *Problems in Public Expenditure*, pp. 127–162.
- SESHAN, G. and YANG, D. (2014). Motivating migrants: A field experiment on financial decision-making in transnational households. *Journal of Development Economics*, **108**, 119–127.
- SHAYO, M. and ZUSSMAN, A. (2011). Judicial ingroup bias in the shadow of terrorism. *The Quarterly Journal of Economics*, **126** (3), 1447–1484.
- SHRESTHA, S. and YANG, D. (2019). Facilitating worker mobility: A randomized information intervention among migrant workers in Singapore. *Economic Development and Cultural Change*, **68** (1), 63–91.
- STEINMAYR, A. (2020). MHTREG: Stata module for multiple hypothesis testing controlling for FWER. Statistical Software Components S458853, Boston College Department of Economics.

- THEOHARIDES, C. (2020). The unintended consequences of migration policy on origin-country labor market decisions. *Journal of Development Economics*, **142**, 102271.
- UN WOMEN (2018). *Fund for Gender Equality Annual Report 2017-2018*. New York: United Nations Women's Fund for Gender Equality.
- VOIGTLÄNDER, N. and VOTH, H.-J. (2012). Persecution perpetuated: the medieval origins of anti-semitic violence in nazi germany. *The Quarterly Journal of Economics*, **127** (3), 1339–1392.
- YANG, D. (2008). International migration, remittances, and household investment: Evidence from Philippine migrants' exchange rate shocks. *Economic Journal*, **118**, 591–630.

Table 1: Baseline characteristics of DWs by treatment status

	Hong Kong				Saudi Arabia			
	(1) Control	(2) Treated	(3) Total	(4) p-value	(5) Control	(6) Treated	(7) Total	(8) p-value
Age	30.4	30.4	30.4	0.99	31.2	31.1	31.1	0.63
Married (0/1)	0.51	0.52	0.51	0.82	0.43	0.45	0.44	0.57
Has children (0/1)	0.75	0.72	0.73	0.35	0.80	0.80	0.80	0.92
Speaks Tagalog at home (0/1)	0.41	0.41	0.41	0.95	0.48	0.48	0.48	0.96
Highschool degree or less (0/1)	0.36	0.31	0.34	0.098	0.73	0.74	0.74	0.71
College degree (0/1)	0.19	0.22	0.20	0.34	0.12	0.11	0.11	0.77
Worked 6 months ago 0/1)	0.50	0.53	0.51	0.35	0.47	0.49	0.48	0.52
Worked as domestic helper in PH (0/1)	0.39	0.38	0.38	0.69	0.43	0.42	0.43	0.73
Born in Pangasinan province (0/1)	0.093	0.12	0.10	0.29	0.051	0.041	0.046	0.40
Born in National Capital Region (0/1)	0.060	0.038	0.050	0.14	0.056	0.052	0.054	0.79
Does not use internet (0/1)	0.14	0.11	0.12	0.20	0.31	0.33	0.32	0.44
Has personal savings (0/1)	0.17	0.17	0.17	0.96	0.079	0.095	0.087	0.34
Has personal bank account (0/1)	0.30	0.33	0.31	0.39	0.13	0.12	0.13	0.59
Knows someone at destination (0/1)	0.13	0.11	0.12	0.31	0.070	0.064	0.067	0.64
Salary deduction (0/1)	0.18	0.19	0.18	0.56	0.083	0.10	0.091	0.30
Has talked to employer (0/1)	0.52	0.50	0.51	0.63	0.13	0.13	0.13	0.71
Knows the name of the language spoken at destination (0/1)	0.88	0.88	0.88	0.78	0.91	0.91	0.91	0.88
Mental health index	23.2	23.4	23.3	0.37	22.5	22.7	22.6	0.44
Household size	4.47	4.16	4.33	0.034	4.73	4.74	4.74	0.96
Destination city is Riyadh (0/1)	0.43	0.42	0.42	0.60
Observations	432	373	805	.	590	592	1182	.

Notes: Columns 1-3 and 5-7 report means based on data from baseline interviews with DWs. Columns 4 and 8 report p-values from a two-sided t-test for equality of means for the control and the treatment group. Columns 1-4 use data for the Hong Kong subsample, columns 5-8 use data for the Saudi Arabia subsample. Due to separate randomizations for the Hong Kong and the Saudi Arabia subsamples, we do not show a balance test for the full sample.

Table 2: Effect of the intervention on the treatment of the DW by the employer

	(1) Index of treatment of DW (STE)	(2) Satisfaction with relationship with employer	(3) With initial employer	(4) DW: Good for children to become OFW
	<i>Full</i>			
Intervention	0.035 (0.022)	0.102* (0.056)	0.038* (0.019)	0.054* (0.028)
Control mean	0.00	3.47	0.18	0.23
Observations	1667	1377	1430	934
	<i>Hong Kong</i>			
Intervention	0.066** (0.029)	0.167** (0.082)	-0.015 (0.029)	0.027 (0.048)
Control mean	0.20	3.60	0.19	0.27
Observations	697	584	602	363
	<i>Saudi Arabia</i>			
Intervention	0.013 (0.031)	0.064 (0.076)	0.075*** (0.026)	0.073** (0.036)
Control mean	-0.16	3.37	0.17	0.20
Observations	970	793	828	571

Notes: The table reports OLS estimates. The column title shows the dependent variable. These are an index of the treatment of the DW by the employer measured as a standardized treatment effect across the indicators listed in Table 3 in column 1, satisfaction of the DW with the relationship with the employer measured as the average of DW ratings (1-5 scale; higher is better) of the relationship with the employer initially (retrospective report of satisfaction in first month of job) and at the time of the endline survey in column 2, an indicator whether the DW still works for the initial employer at the time of the endline interview or is planning to return to the employer soon (as opposed to returning to the Philippines or working for a different employer abroad) in column 3, an indicator for whether the DW agrees with the statement that it would be good for her children to become an Overseas Filipino Worker (OFW) in column 4. In columns 1 and 2, we replace information not available from direct interviews with information from endline (second priority) or midline proxy interviews (third priority). In column 4, we use data from direct interviews only. The explanatory variable indicates whether the DW was assigned to the intervention. All regressions include the standard set of baseline covariates as outlined in the PAP. These are age, indicators of having children, having at most a high school degree, having a college degree, not using the internet, having a personal bank account, migrating to Saudi Arabia, indicators of missing baseline covariates (missing values are replaced with the sample mean), an indicator of the outcome being measured in a direct interview, and the time elapsed since the baseline interview (log days). We also include indicators of being assigned to the savings module and the experience-sharing module, which were randomized independently. In addition, we include hypotheses-specific covariates as outlined in the PAP. These are an indicator of whether the DW had talked to the employer before the baseline interview (columns 1 and 2), an indicator of whether the DW planned to stay abroad for more than two years at the time of the baseline interview (column 3), and an indicator of whether the DW agreed with the statement that it would be good for her children to become OFWs at the time of the baseline interview (column 4). Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level. For our primary outcome in column 1, we correct for multiple testing in the subgroups formed by the two destinations. We use the *mhtreg* Stata package (Steinmayr, 2020) that modifies the procedure introduced by List *et al.* (2019) to control for the family-wise error rate. The adjusted p-values are 0.047 for Hong Kong and 0.67 for Saudi Arabia.

Table 3: Effect of the intervention on the treatment of DW by the employer (individual components)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	No shouting	No violence	No threats	No sexual harassment	No work when sick	Have enough food	Weekly rest day	Leave the employer's house	Salary on time	No salary deduction	Spare time (hours)
	<i>Full</i>										
Intervention	0.013	-0.009	0.017	0.020	0.021	-0.005	0.009	0.022	0.059**	0.019	0.206
Control mean	(0.032)	(0.018)	(0.021)	(0.013)	(0.026)	(0.022)	(0.020)	(0.018)	(0.027)	(0.015)	(0.223)
Observations	943	944	943	942	944	938	944	944	940	937	875
	<i>Hong Kong</i>										
Intervention	0.076	0.003	0.032	-0.001	-0.005	0.084**	0.031	0.064*	0.032	0.015	0.249
Control mean	(0.051)	(0.026)	(0.028)	(0.015)	(0.037)	(0.039)	(0.034)	(0.038)	(0.033)	(0.024)	(0.323)
Observations	365	366	366	365	366	362	366	366	363	365	345
	<i>Saudi Arabia</i>										
Intervention	-0.020	-0.015	0.012	0.033*	0.043	-0.056**	-0.002	-0.003	0.077**	0.019	0.199
Control mean	(0.042)	(0.024)	(0.029)	(0.020)	(0.036)	(0.027)	(0.024)	(0.018)	(0.038)	(0.019)	(0.302)
Observations	578	578	577	577	578	576	578	578	577	572	530

Notes: The table reports OLS estimates. The column title shows the dependent variable. Outcomes in columns 1-10 are binary indicators of whether the DW states that the statement corresponds to her experience with the employer. Columns 1-5: Responded to "Did you experience the following from your last employer or employer's household members? [Shouting from employer or household members / Any kind of threats / Any form of physical violence / Sexual harassment / Having to work when sick]" with a "No"; column 6: Responded to "Does your employer provide enough food or food allowance?" with a "Yes"; column 7: Responded to "Do you have a rest day every week?" with a "Yes"; column 8: Responded to "During your rest day, are you allowed to leave the house on your own and without the company of your employer or employer's household?" with a "Yes"; the variable is set to zero if the DW does not have a rest day; column 9: Responded to "Do you receive your monthly salary on time?" with a "Yes"; column 10: Responded to "Have there been any deductions from your monthly salary?" with a "No"; Column 11: Hours of spare time (calculated as 24 - (work start time - work end time) - break). The explanatory variable indicates whether DW was assigned to the intervention. All regressions include the same covariates as in column 1 of Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table 4: Results from the online dictator game

	Full	Hong Kong	Saudi Arabia and United Arab Emirates	Default: egalitarian	Default: not egalitarian
	(1)	(2)	(3)	(4)	(5)
Photo treatment	0.83*** (0.32)	0.79* (0.40)	0.98* (0.52)	0.52 (0.36)	1.05** (0.52)
Gift treatment	0.46 (0.29)	0.41 (0.36)	0.75 (0.48)	0.43 (0.35)	0.64 (0.45)
Photo + gift treatment	0.38 (0.31)	0.42 (0.38)	0.39 (0.55)	0.23 (0.39)	0.73 (0.47)
Default egalitarian	3.20*** (0.20)	3.28*** (0.26)	3.14*** (0.34)		
<i>F-test (p-values)</i>					
Photo = Gift	0.33	0.44	0.71	0.84	0.50
Photo + gift = Photo	0.26	0.46	0.39	0.54	0.61
Photo + gift = Gift	0.84	0.98	0.58	0.66	0.88
Control mean	6.07	6.32	5.68	7.83	4.50
Observations	1662	1000	662	789	873

Notes: The table reports OLS estimates. The dependent variable is a dictator game transfer in USD that can take integer values from 0 to 10. All regressions control for gender, age categories (9 dummies), educational attainment (5 dummies), employment status (6 dummies), monthly household income converted from local currencies to 2018 PPP USD, marital status (3 dummies), number of children (5 dummies), binary variables for countries, matched partner indicators, and the type of device used. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level. We correct for multiple testing as there are three treatment groups. We use the *mhtreg* Stata package (Steinmayr, 2020) that modifies the procedure introduced by List *et al.* (2019) to control for the family-wise error rate. The adjusted p-values are 0.03, 0.26, and 0.84 for the photo, gift, and photo and gift treatments.

Table 5: Effects of the intervention on the subjective assessment of migration by households

	(1) HH: Overall effect	(2) HH: Effect on financial security	(3) HH: Effect on standard of living	(4) HH: Effect on housing	(5) HH: Effect on health	(6) HH: Effect on education	(7) HH: Effect on family life	(8) HH: Effect on social life
<i>Full</i>								
Intervention	0.056** (0.022)							
Worse		-0.009* (0.005)	-0.009** (0.004)	-0.004 (0.003)	-0.001 (0.004)	-0.008** (0.004)	-0.011** (0.005)	-0.015** (0.007)
Same		-0.044** (0.022)	-0.056** (0.023)	-0.030 (0.025)	-0.008 (0.023)	-0.060** (0.025)	-0.051** (0.023)	-0.029** (0.014)
Better		0.053* (0.027)	0.065** (0.027)	0.034 (0.028)	0.009 (0.028)	0.068** (0.028)	0.062** (0.027)	0.044** (0.020)
Control mean	2.40							
Observations	1173	1167	1169	1169	1170	1148	1169	1100
<i>Hong Kong</i>								
Intervention	0.054 (0.034)							
Worse		-0.005 (0.008)	-0.012* (0.007)	-0.005 (0.005)	0.001 (0.007)	-0.005 (0.004)	-0.009 (0.007)	-0.019* (0.011)
Same		-0.020 (0.034)	-0.080** (0.034)	-0.043 (0.038)	0.004 (0.035)	-0.044 (0.040)	-0.049 (0.037)	-0.044** (0.022)
Better		0.025 (0.042)	0.092** (0.040)	0.048 (0.042)	-0.005 (0.042)	0.049 (0.044)	0.058 (0.043)	0.063** (0.032)
Control mean	2.40							
Observations	495	492	494	494	493	481	492	471
<i>Saudi Arabia</i>								
Intervention	0.056* (0.030)							
Worse		-0.012** (0.006)	-0.005 (0.005)	-0.003 (0.005)	-0.002 (0.005)	-0.010** (0.005)	-0.012 (0.007)	-0.010 (0.010)
Same		-0.065** (0.030)	-0.035 (0.031)	-0.024 (0.032)	-0.016 (0.031)	-0.069** (0.032)	-0.049* (0.029)	-0.019 (0.017)
Better		0.077** (0.036)	0.041 (0.036)	0.028 (0.037)	0.018 (0.036)	0.079** (0.036)	0.060* (0.036)	0.029 (0.027)
Control mean	2.40							
Observations	678	675	675	675	677	667	677	629

Notes: The table reports OLS estimates in column 1 and marginal effects at the mean from an ordered probit in columns 2-8. The column title shows the dependent variable. In column 1, the dependent variable is the mean of the subjective assessment of the household of the effect of migration on different aspects (see columns 2-8). These variables are coded as 1 "worse" 2 "same" 3 "better"; In columns 2-8, the dependent variable is the response to the question "Do you think that through [name of DW] employment in [Hong Kong/Saudi Arabia] the situation of your household has gotten better, stayed the same or got worse with regard to [aspect stated in column 2-8]?" All columns use data from household interviews. The explanatory variable indicates whether the DW was assigned to the intervention. All regressions include the standard set of baseline covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table 6: Effect of the intervention on household education expenditures and enrolment

	Education expenditure		Enrolled			Employed
	(1) USD	(2) Log (+1)	(3) All	(4) Age 3-12	(5) Age 13-20	(6) Age 16-25
	<i>Full</i>					
Intervention	44.478 (27.299)	0.261** (0.132)	0.040** (0.016)	0.025* (0.013)	0.068** (0.034)	-0.079* (0.042)
Control mean	309.73	4.15	0.90	0.96	0.80	0.18
Observations	1199	1212	1336	851	485	310
	<i>Hong Kong</i>					
Intervention	-2.454 (47.140)	-0.005 (0.233)	0.023 (0.020)	0.013 (0.013)	0.029 (0.051)	-0.013 (0.068)
Control mean	346.84	3.93	0.92	0.98	0.80	0.11
Observations	496	502	483	321	162	94
	<i>Saudi Arabia</i>					
Intervention	72.886** (33.207)	0.430*** (0.157)	0.044** (0.021)	0.030 (0.021)	0.072* (0.042)	-0.119** (0.054)
Control mean	281.64	4.31	0.89	0.95	0.79	0.22
Observations	703	710	853	530	323	216

Notes: The table reports OLS estimates. The column title shows the dependent variable. These are total education expenditures (e.g., tuition, bus fares, allowance for members studying away from home, books, school supplies) of the household in the previous 12 months in USD in column 1 (we drop outliers exceeding the 99th percentile of educational expenditures), logged education expenditures in column 2 (not outliers dropped), an indicator of whether a given child in the household is currently enrolled in an educational institution in column 3-5, and an indicator of whether a young hh member is currently employed. Observations in columns 3-5 correspond to children aged 3-20 living in the household at baseline. Observations in column 6 correspond to young adults aged 16-25 living in the household at baseline. All columns use data from household interviews. The explanatory variable indicates whether the DW was assigned to the intervention. All regressions control for the standard set of covariates described in Table 2. In addition, we control for household size in columns 1 and 2, age, sex, and enrolment status of the child at baseline in column 3-6, and employment status at baseline in column 6. Standard errors clustered at the level of DWs in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Online Appendix

A Further details on the field experiment

Randomized assignment of the intervention

The intervention was administered during the baseline interviews with DWs immediately prior to their CPDEP sessions. For every classroom, we created an assignment sheet based on the registration list for this classroom that we received the day before the session. Every DW had a 50% chance of being randomly assigned to the intervention. As we discovered ex-post, the seed chosen for the randomized assignment in the Hong Kong sample assigned only about 45% to the intervention if about 40-70 DWs were fed into the algorithm. Since the seed was left constant over the intervention period, this resulted in a treated share below 50% in the Hong Kong sample. While this particular seed affected the unconditional probability of being treated, it did not introduce any correlation between individual characteristics and the probability of being treated. Interviewers implemented the intervention as indicated on the assignment sheet. In 4.6% of cases (92), however, the assignment as recorded by the interviewers does not match the originally assigned status. This discrepancy could be the result of deviations from the originally assigned status or mere misrecording of the implemented assignment. Since our estimates have to be interpreted as intent-to-treat (ITT) effects in any case, we use the originally assigned status for the analysis to avoid selection problems. Column 3 of Table D.4 shows that our main result holds when we exclude these observations are excluded.

The intervention was administered as part of a larger evaluation project to investigate the effect of pre-departure interventions on migrants. Two additional group-level interventions, a savings module and an experience-sharing module aimed at improving financial decision-making and expectation management, respectively. Another individual-level intervention consisted of sending text messages with finance-related advice. It was administered to only a small sub-sample. These interventions were randomized independently and are orthogonal to the intervention studied here. Details on these interventions as well as results are provided in the project report (Barsbai *et al.*, 2020a).

DRIED MANGOES

INSTRUCTIONS:

1. BRING THIS PACKAGE OF DRIED MANGOES IN YOUR SUITCASE TO YOUR DESTINATION COUNTRY
2. IF YOU ARE GOING TO **KSA**: ASK THE RECRUITER (OR THE EMPLOYER IF HE MEETS YOU), IF YOU CAN GIVE THE DRIED MANGOES TO YOUR EMPLOYER'S FAMILY AS A GIFT FROM THE PHILIPPINES. ALSO ASK WHEN AND WHO TO GIVE IT TO.

IF YOU ARE GOING TO **HONGKONG**: GIVE THE GIFT TO MADAM WHEN YOU MEET HER.

3. WHEN YOU GIVE THE MANGOES, PLEASE SAY: "THIS IS A GIFT FROM MY HOME COUNTRY, THE PHILIPPINES, FOR YOU AND YOUR FAMILY. I HOPE YOU WILL ENJOY IT. I AM HAPPY TO COME TO SERVE YOU AND WILL DO MY BEST TO DO MY JOB WELL."

1. DALHIN ANG DRIED MANGOES SA IYONG PUPUNTAHANG BANSA. ILAGAY ITO SA IYONG MALETA.

2. KUNG IKAW AY PUPUNTA SA **KSA**: KAPAG NAGKITA KAYO NG RECRUITER O EMPLOYER, TANUNGIN ANG RECRUITER O EMPLOYER KUNG PWEDE MONG IBIGAY ANG DRIED MANGOES BILANG REGALO MULA SA PILIPINAS PARA SA PAMILYA NG IYONG EMPLOYER.

KUNG IKAW AY PUPUNTA SA **HONG KONG**: IBIGAY KAY MADAM ANG DRIED MANGOES KAPAG NAGKITA NA KAYO.

3. KAPAG BINIGAY MO ANG DRIED MANGOES, SABIHIN: "ITO AY REGALO MULA SA PILIPINAS PARA SA INYO AT SA INYONG PAMILYA. SANA AY MAGUSTUHAN NIYO. MASAYA AKONG PUMUNTA DITO AT PAGBUBUTIHAN KO ANG AKING TRABAHO SA ABOT NG AKING MAKAKAYA

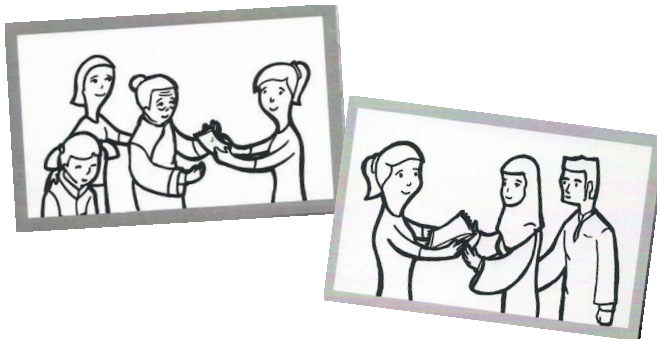


PHOTO / LARAWAN

INSTRUCTIONS:

1. BRING A PICTURE OF YOUR FAMILY.
 - YOUR FAMILY MUST BE IN CONSERVATIVE, DECENT AND CLEAN CLOTHES.
 - DO **NOT** USE PHOTOS WHERE A FAMILY MEMBER IS WEARING SWIMWEAR (BATHING SUITS), SLEEVELESS TOP, SHORTS, MINI-SKIRTS OR TIGHT CLOTHES .
2. DISPLAY THE PHOTO INSIDE YOUR ROOM WHERE YOU COULD EASILY SEE IT.
3. TALK ABOUT YOUR FAMILY PARTICULARLY ABOUT YOUR CHILDREN TO MADAM **IF** SHE SHOWS INTEREST. FOR EXAMPLE, MADAM ASKS ABOUT YOUR FAMILY OR CHILDREN
 - TALK ONLY ABOUT HAPPY AND POSITIVE TOPICS. FOR EXAMPLE, YOUR CHILDREN ARE HEALTHY, ARE DOING WELL IN SCHOOL, LEARNING A NEW HOBBY
 - **NEVER** TALK ABOUT YOUR FAMILY OR MONEY PROBLEMS WITH MADAM.
 - DO **NOT** MAKE COMPARISONS BETWEEN YOUR FAMILY AND MADAM'S FAMILY.

1. MAGDALA NG LARAWAN NG IYONG PAMILYA
 - DAPAT AY NAKASUOT NG KONSERBATIBO, DISENTE AT MAAYOS NA DAMIT.
 - HUWAG MAGDALA NG LARAWAN KUNG SAAN MAY NAKASUOT NG PANG-SWIMMING TULAD NG BATHING SUIT O TRUNKS, WALANG MANGGAS NA PANG-ITAAS, SHORTS, MAIKLING PALDA O MASISIKIP NA DAMIT.
2. I-DISPLAY ANG LARAWAN SA LOOB NG IYONG KWARTO KUNG SAAN MADALI ITONG MAKITA.
3. IKWENTO NG IYONG PAMILYA KAY MADAM **KUNG** NAGPAKITA SIYA NG INTERES. HALIMBAWA KUSANG TINANONG NI MADAM KUNG KUMUSTA NA ANG PAMILYA MO O MGA ANAK.
 - KAPAG KAUSAP SI MADAM, IKWENTO LANG ANG MGA MASASAYA AT POSITIBONG MGA PAKSA TULAD NG MAAYOS NA KALUSUGAN AT NAKAKAPAG-ARAL ANG MGA ANAK
 - HUWAG IKWENTO ANG MGA PROBLEMA SA PAMILYA O PERA
 - HUWAG IKUMPARA ANG IYONG PAMILYA SA PAMILYA NI MADAM.



Figure A.1: Instructions provided to treatment group

Data collection

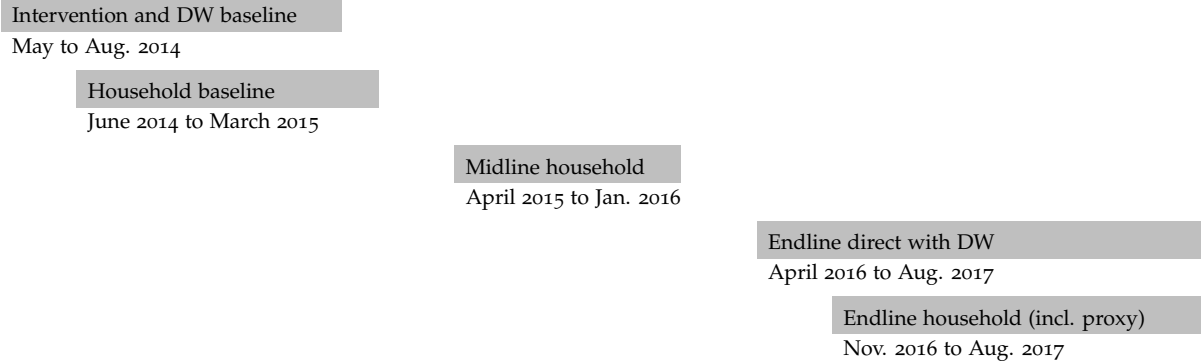
We hired the Philippine branch of TNS, a large international survey firm, to conduct the fieldwork of the project. TNS could provide field staff in all parts of the Philippines and the infrastructure needed for phone interviews.

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. Except for the baseline interviews with DWs, during which the intervention was administered, field staff was blind to the treatment status of each respondent. All interviews were computer-assisted and administered on tablets. 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.

For the direct endline interviews with DWs, we placed highest priority on ensuring that interviews were conducted only when the DW could talk in private and without interruption from the employer. We therefore refrained from reaching out to DWs directly to avoid contacting them in an inconvenient moment. Instead, TNS reached out to families in the Philippines who then contacted the DW to inform them about two possible ways to conduct the interview: (i) providing us with a suitable time for a call or (ii) calling TNS directly at any time convenient for them. To ensure high participation rates but also to compensate DWs for potential fees for calling TNS, we provided a token worth USD 21, half of which in the form of an electronic phone credit sent shortly after the interview with the DW and half of which in the form of a gift voucher provided during the household interview. In case a direct interview was still not possible, we conducted a proxy interview with a knowledgeable family member. Proxy interviews were part of the regular household interviews. In case TNS was unable to conduct a direct interview with a DW prior to the household interview but the DW was present during the household interview, a personal interview with the DW was conducted.

Figure A.2 shows the timeline of the field experiment and related data collection.

Figure A.2: Timeline field experiment



B 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 PAP). We also reprint the exact survey questions and answer options in italics. Survey questions shown here are for DWs currently working abroad. We adjusted survey questions for DWs who had already returned to the Philippines at the time of the interview and for proxy interviews.

Index of treatment of DW Standardized treatment effect¹ (STE) of (i) not having been shouted at, (ii) not having experienced physical violence, (iii) not having been threatened, (iv) not having experienced sexual harassment, (v) having enough food, (vi) having a weekly rest day, (vii) not having been forced to work when sick, (viii) being allowed to leave the employers house², (ix) receiving the salary on time, (x) not having experienced salary deductions, and (xi) the amount of daily spare time³.

Did you experience the following from your current employer or employer's household members: (i) Shouting from employer or household members, (ii) was forced to work when sick, (iii) any form of physical violence, (iv) any kind of threats, (v) sexual harassment?
[Yes / No]

¹We 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, let μ_k be the control-group mean, and let σ_k be the control group standard deviation. The normalized outcome is $Y_k^* = (Y_k - \mu_k) / \sigma_k$. The summary index is $Y^* = 1/K \sum_K Y_k^*$. 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.

²This variable is coded as zero if the DW reported not having a rest day at all.

³The variable is constructed as end time - start time - rest time. In some instances start and end times or a.m. and p.m. times seem to have been confused. We correct for unrealistic values. Our results also hold if we calculate the STE without the amount of daily spare time

Does your employer provide enough food or food allowance? [Yes, employer provides enough food or food allowance / No, employer did not provide enough food or food allowance]

What time do you usually start working? hh:mm

What time do you usually end working? hh:mm

Do you have breaks or rest in between your working hours? [Yes / No]

How many hours do you rest in between? hh:mm

Do you have a rest day every week? [Yes, I have a rest day once a week / Yes, I have a rest day, but only 2-3 times a month / Yes, I have a rest day, but only once a month / No, I do not have a rest day]

During your rest day, are you allowed to leave the house on your own and without the company of your employer or employer's household? [Yes / No]

Do you receive your monthly salary on time? [Yes, I receive my salary on time / No, I usually receive my salary late by 1-7 days / No, I usually receive my salary late by more than 7 days / No, I do not receive my salary at all]

Have there been any deductions from your monthly salary? [Yes, I was deducted full amount of my monthly salary / Yes, I was deducted part of my monthly salary / No, there was no salary deduction]

Wellbeing index Standardized treatment effect of (i) mental health index and (ii) migrant wellbeing index. The mental health 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 in Hong Kong/Saudi Arabia.

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 downhearted and blue, (v) did you feel physical pain, (vi) did you feel homesick, (vii) did you feel overwhelmed by the challenges you face in Hong Kong/Saudi Arabia? [None of the time / A little of the time / Some of the time / Most of the time / All of the time]

Communication index Standardized treatment effect of (i) number of days per week with an exchange of text messages⁴, (b) number of weekly phone calls, (c) length

⁴Since text frequency was reported in categories, we use the midpoints of the categories.

of last phone conversation in minutes.

In the last four weeks, how often did you and your family exchange SMS/text/chat? [Daily / 4-6 times a week / 2-3 times a week / Once a week / Never]

In the past week, how many times did you talk with your family on the phone or using Skype? Number of times

How long was your last (voice) conversation with your family? hh:mm

C Further details on the online experiment

This section presents details on the procedure for both dictators and receivers in the online experiment. While dictators participated entirely online and were recruited exclusively from existing panels of the survey firms Nielsen (Hong Kong) and Ipsos (Saudi Arabia and United Arab Emirates), respondents were recruited by our research assistant by randomly visiting households in low-income neighborhoods of Manila, the Philippines. The dictators' sample is representative of the upper and upper-middle income households in the respective countries.

C.1 Dictator instructions

Experiments were conducted online using a tailor-made website. The following instructions were translated from English to Chinese (Hong Kong) and Arabic (Saudi Arabia and United Arab Emirates).

Introduction. Welcome to the research study in which we examine attitudes of [Hong Kong / Saudi] population towards foreigners. There will be two parts. Each part will take at most 20 minutes of your time. One part will take place right now and the other part will take place two weeks from now, also online. As a token of appreciation, we will give you USD 5 for completion of both parts, but you will be able to earn much more money, depending on the decisions you will make.

Data protection. Your participation in this study is voluntary and you can decide to leave anytime. If you do not finish all parts of the study, we will delete data for all the decisions and will only keep your basic demographic characteristics. But at the same time, you will not receive any money from us. We will use the data we collect only for the purposes of this research study. We will never learn your true identity and would record all of your decisions under an anonymized ID number. In order to distribute the payments, we would then match your ID number to your [IPSOS / NIELSEN] account so that the company can deliver your rewards. We may share the anonymized data with

a wider community of researchers. However, these data would never allow anyone to identify your person. Sincerely, Yours research team from the Asian Institute of Management, Georgetown University, University of Michigan, University of Munich, and the University of St. Andrews. [LOGOS OF OUR UNIVERSITIES]

Initial survey. Many [Hongkongers / Saudis] employ foreign domestic workers. We will now ask you several questions regarding such workers. The following questions will ask about the typical employment contract of foreign domestic workers in your country. Please, answer what you think is correct.

1. How long does a typical foreign domestic worker contract last? [*6 months / 1 year / 2 years / 5 years / Indefinite*]
2. How much is a minimal monthly salary of a foreign domestic worker? [*100 US Dollars / 400 US Dollars / 700 US Dollars / 1000 US Dollars*]
3. What rights do the foreign domestic workers have? (Please, check all that applies) [*Right to basic medical care in the host country / Right to contact their family / Right to two rest days per week / Right to terminate the contract without giving a reason (1 month in advance)*]
4. What rights do the employers of foreign domestic workers have? (Please, check all that applies) [*Right to terminate the contract without any reason (1 month in advance) / Right to delay or reduce payment to the domestic worker in case of misconduct / Right to terminate the contract immediately in case of frequent absenteeism*]
5. List three countries from which most of the foreign domestic workers typically come from?
6. Now let me ask you few questions regarding your own experience with foreign domestic workers. Did your household ever employ a foreign domestic worker? [*Yes, in the past / Yes, currently employing one / Yes, currently employing more than one / No, never*]
7. Would you consider hiring a foreign domestic worker in the future? [*Yes / No*]
8. IF 6 ANSWERED WITH A YES (ANY): Was any of the domestic workers you employed from the Philippines?

Task instructions. Next, I will introduce you to a task. Please, make sure to read the details carefully. You will be able to earn money during this task and the instructions will tell you how.

You will make choices over allocation of real money between yourself and another person. You will be given a chance to redistribute real money between yourself and another person. The money used is provided by a research grant our universities obtained. It is separate from the USD 5 that we offer you for completing this study. If you complete this study, you will keep the USD 5 for sure. However, all choices you will make during this task will affect your own and the other person's monetary earnings beyond the USD 5.

What choices will you be making. There will be two activities in the task. In the second activity, there will also be several sub-parts. In both activities we will ask you to make a choice about how to allocate money between yourself and the matched person. Since you will make several choices and you will also make similar choices in the next part of this study two weeks later, the computer will then randomly pick one of the activities that will determine the amounts that you and the person matched with you will earn.

Since you won't know which activity will be paid until after making all decisions, please, make sure to take all choices as seriously as if every one of them matters.

In the second part of the study two weeks later, after you have made all the choices, we will inform you which activity is the one that will count.

You will be paid as follows:

- USD 5 for participation in the study +
- the amount that you have decided to keep for yourself in the selected activity

The person you are matched with will be paid as follows:

- USD 5 for participation in the study +
- the amount that you have decided to send to her in the selected activity.

You should know that there are no right or wrong choices in either of the activities. The choices you will make will determine how much money you and this person earn.

Description of the other person follows. The person you are matched with is a woman, around 30 years old, from the Philippines. Her profile is similar to many domestic workers in [Hong Kong / Saudi Arabia]. Although you will never learn her true identity, you should trust us that the person is real and the team in the Philippines working on this research project is in touch with her. We also gave her USD 5 to participate in the study. She will keep this USD 5 regardless of any choices you make in this task. Your choices over the remaining money in this task, however, will affect her monetary earnings.

Photo treatment / photo and gift treatment. *The other person would like to introduce you to her family.* She decided to introduce her family to you. [Photo from Figure C.3 here]

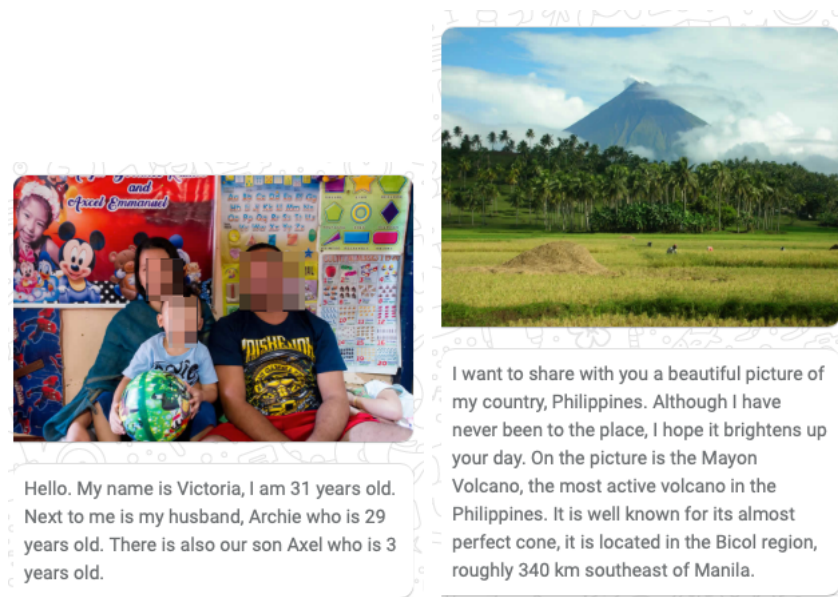


Figure C.3: Photo and gift treatment sample screenshots. Faces in the photo are blurred here to protect the privacy of the family (but not in the experiment).

Gift treatment / photo and gift treatment. *The other person would like to share a small present from the Philippines with you.* She [PHOTO AND GIFT: "also"] decided to share a small present with you. She selected a picture of her favorite landscape in the Philippines that she wants you to see. You can see the picture below. If you wish, you can download the picture by clicking on the picture. It will automatically open in a separate tab. The person you are matched with also wrote a short note accompanying the picture that we translated for you. [Gift from Figure C.3 here]

You should also note two more things.

1. The person you are matched with will never learn your identity. But when we met her, we told her that she would be matched with a person from [Hong Kong / Saudi Arabia].
2. We gave the person the same detailed description of the task as you will receive it. And she knows that we told you this.

Activity 1 (Dictator game). Let's begin with the first activity. In this activity you will allocate money between yourself and the person you are matched with. There is USD 20 in total to be allocated. Below, you see a slider bar [See Figure C.4]. It ranges from USD

10 to USD 20. The amount selected represents how much money you keep for yourself. We indicate this below the slider bar. The remaining part of the USD 20 will be allocated to the matched person.

After you answer few questions below, you will be able to drag the slider bar to reallocate the money. By dragging the slider by one unit to the right:

- The amount of money for you increases by USD 1, while
- The amount of money for the matched person decreases by USD 1.

You can choose any allocation that you want and then confirm the allocation by clicking the "Submit" button below. After clicking the "Submit" button, you will not be able to change your decision.

If this activity is the one selected for payment, you will receive the amount that you selected to keep for yourself and the matched partner will receive the amount that you selected to send to her.

Comprehension questions. Let me ask you few questions before the actual task to check that I have explained the instructions well [NEED TO BE ANSWERED CORRECTLY BEFORE PROCEEDING]:

1. If you keep the entire amount for yourself, how many USD will the matched person get?
2. How many USD will you keep for yourself if you slide the bar all the way to the left?

Thank you, the answers are correct. Now let us proceed to the choice. Please, allocate the money using the slider bar. Again, remember that there is no right or wrong answer. After you are satisfied with the allocation, click the "Submit" button.

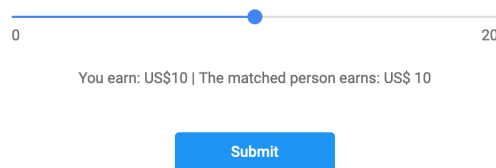


Figure C.4: Dictator game slider

Activity 2 (Binary dictator games). Perfect. Now let me explain the second activity. To remind you, you won't be paid for all the four activities. A computer will randomly select one of the activities at the end of the second round and you will be paid for that activity only. Since you won't know which activity will be paid until after making all

choices, including those in the second round of the study, please, make your decisions in all activities carefully. In the second activity you are matched with the same person as in the previous activity.

In this activity, you will be making choices about allocating money between yourself and the matched person. You will do this several times, in a series of similar sub-parts. You will be presented with two options showing:

1. How much money you would earn for yourself and
2. How much money would the other person earn.

We will present the two options in the following way:

Option	A	B
You earn	USD 1	USD 4
The matched person earns	USD 2	USD 3

Then you will be asked to select either A or B.

If this activity gets selected for payment, the computer will randomly choose one of the sub-parts. You and your matched partner will then be paid accordingly based on the choices in that selected sub-part. In the example above, by choosing A, you would earn USD 1 and your partner would earn USD 2.

Comprehension questions. Let me ask you few questions before the actual task to check that I have explained the instructions well [NEED TO BE ANSWERED CORRECTLY BEFORE PROCEEDING]:

1. How much money will the matched person earn, if you choose option B?
2. If this activity is selected for payment, will you get paid for your choices in all of the sub-parts within this activity or just for one randomly selected? [I will be paid for all sub-parts / I will be paid for one randomly selected sub-part]
3. Can you select neither of the two options? [Yes / No]

Thank you, the answers are correct. Now let us proceed to the choice itself. Please, choose one of the two options below. Again, remember that there is no right or wrong answer. After you are satisfied with your choice, click the "Submit" button and a next sub-part will follow until you finish the last one.

Choices. Random ordering. (10,10) vs. (11,13), (10,8), (9,6), (8,4), (7,2), or (6,0).

Thank you. You have completed both activities. Before we finish this part of the study, let me ask you few quick questions. After you are satisfied with your responses, click the "Submit" button.

Final survey.

- Is the person you are matched with a real person? *[Yes / No]*
- In which country does the person matched with you live?
- What else do you remember about the person matched with you?

Conclusion. This completes the first part of the study. Thank you for all your responses. We will be in touch with you with the second part of the study in two weeks. [IPSOS / NIELSEN] will send you a reminder email with a link. Please, remember that we will deliver your payments via [IPSOS / NIELSEN] after you successfully complete the second part of the study. Kind regards. Research team from Asian Institute of Management, Georgetown University, University of Michigan, University of Munich, and the University of St. Andrews. [LOGOS OF OUR UNIVERSITIES]

Second round instructions. Same as in round 1. Adds an Implicit association task and a final survey. If no info treatment in round 1, random assignment of either of the four treatments in round 2. If photo / gift / photo and gift treatments in round 1, no info treatment in round 2.

C.2 Receiver instructions

Ten female married receivers with at least one child, around 30 years old were recruited from low-income neighborhoods of Manila, the Philippines. The following instructions were translated from English to Tagalog.

Introduction. Hello, I am XXX and am employed by the Asian Institute of Management. We are doing a research study in which we try to understand relationships between different nationalities. Thank you for agreeing to participate. Before we start, please let me confirm that you meet the criteria for the people we are looking for. Are you between 28 and 33 years old and live with your husband in the same house? Do you have children that are currently present?

[IF PERSON MEETS CRITERIA, CONTINUE]

Your profile matches that of the people we are looking for in our study. Now let me give you some details so that you can decide whether you participate or not.

Data protection and consent. Let me stress out that your participation in this study is voluntary and you can decide to leave at any point of time. We will use any data that

you provide us with only for the purposes of the given research study. We may share the data with a wider community of researchers, however, this data would never allow them to identify your person or your family.

Show-up fee and incentives. For the participation in this survey, you will receive USD 5 as a token of appreciation for your time. You will be able to make some additional money. I will tell you how later.

Procedures. We will match you with an adult person in Hong Kong or in Saudi Arabia. You will not learn any details about the person. The person will be doing some choices I will explain later. These choices will influence how much money you will get on top of the USD 5 you get for participation. Multiple people in Hong Kong or in Saudi Arabia might be matched with you, but choices of only one person selected by chance will be relevant for your payment. You will receive the payments in about two months.

These persons will see a card similar to the following ones. A chance will decide which one of these cards the person would see. We will randomly decide which card is shown. [Stylized screenshots as in Figure C.3 shown]

We will need two things from you.

Photo treatment input. First, we will need you to allow us to take a photo of you and your family that we would potentially share with the matched person (husband and your child). We will also need to get the name of your husband, his age, and the name and age of your child. Our photographer will take the photo. As a token of appreciation, we will give you a print copy of the photo in a frame once we deliver the payment. Note that the matched person will only be given a chance to see the photo on the screen of his or her computer once and will not have an option to store the photo in any way.

Gift treatment input. Second, we will need you to choose from a range of pictures of famous places of the Philippines. We will present the photos to you. Then we would ask you to add a personal note for why you have picked the photo and why you feel a person from a foreign country might like this place. One, maximally two short sentences would be enough.

Decision to proceed. Will you be willing to do these two things for us? You can either decide right now or I can explain the whole purpose of the study and you can decide later. [*Yes / No / Decide later*]

Activities (Dictator games). Now let me explain the task in more detail. In the task, the matched person will be able to transfer some money to you. This is in addition to the USD 5 that each of you will get for participation in this study. There will be two activities in which the person will be making the decisions.

In the first activity, we will give the person USD 20 in one dollar bills from our

research budget. The person will have a chance to send any number of bills between USD 0 and USD 10 to you. The person will keep the remaining money for him or herself. To give you an example [SHOW GRAPHICALLY USING DOLLAR BILLS].

- *Example 1:* If the person sends USD 5 to you, the person will keep USD 15 for him or herself.
- *Example 2:* If the person sends USD 10 to you, the person will keep USD 10 for him or herself.

The second activity is similar, except that we restrict the options the other person can choose from. But again, the other person has to decide how to split the money between you and herself. One of the options will always be an allocation that gives USD 10 to the person and USD 10 to you. The other options will differ. It would either be [DESCRIBE OPTIONS USING VISUAL AIDS: (11,13), (10,8), (9,6), (8,4), (7,2), (6,0)]. Again, this money comes from our research budget.

The first and second activity will be repeated in two weeks. After that, one of the four activities will be selected by chance and you will receive the amount that the other person has decided to give you in this activity.

Payoff. After receiving the money, the task is complete. You will finish the task with the USD 5 that you begin the task with, plus the amount that the matched person sends in the activity selected for payment. The matched person will end this task with the USD 5 that he or she begin with, plus the amount that he or she chooses to keep for him or herself.

Consent photo and gift. Now I have explained to you the entire procedure. Are you willing to allow us to take the photo of your family that we would potentially share with the matched person and to select a picture from the Philippines with an added personal note? [Yes / No]

D Appendix tables and figures

Table D.1: Awareness of and compliance with assignment to the intervention

	(1)	(2)
	Remembers assignment	Followed instructions
	<i>Full</i>	
Intervention	0.752*** (0.021)	0.567*** (0.025)
Control mean	0.17	0.08
Observations	944	944
	<i>Hong Kong</i>	
Intervention	0.774*** (0.033)	0.634*** (0.039)
Control mean	0.16	0.07
Observations	366	366
	<i>Saudi Arabia</i>	
Intervention	0.745*** (0.028)	0.533*** (0.033)
Control mean	0.17	0.08
Observations	578	578

Notes: The table reports OLS estimates. The column title shows the dependent variable. These are an indicator of whether the DW states that she remembers being given instructions for the intervention in column 1 and an indicator of whether the DW showed the photo and gave the gift to the employer in column 2. All columns use data from direct interviews. The explanatory variable indicates whether the DW was assigned to the intervention. The regressions include the standard set of covariates as in Table 2. The reported share of employers responding positively to the gift (conditional on giving it) is 89.16% in the full sample, 88.88% in the Hong Kong sample, and 89.37% in the Saudi Arabia sample. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table D.2: Effect of the intervention on the probability to be re-interviewed

	Successful re-interview with DW			... with household
	(1)	(2)	(3)	(4)
	Direct or proxy	Direct	Proxy	
	<i>Full</i>			
Intervention	-0.001 (0.020)	0.010 (0.022)	-0.011 (0.019)	-0.034 (0.022)
Control mean	0.72	0.49	0.23	0.63
R2	0.05	0.03	0.02	0.03
Observations	1987	1987	1987	1987
	<i>Hong Kong</i>			
Intervention	-0.003 (0.030)	0.029 (0.035)	-0.032 (0.032)	-0.034 (0.034)
Control mean	0.74	0.46	0.29	0.64
R2	0.04	0.02	0.02	0.02
Observations	805	805	805	805
	<i>Saudi Arabia</i>			
Intervention	0.001 (0.026)	-0.005 (0.029)	0.006 (0.023)	-0.034 (0.028)
Control mean	0.70	0.52	0.19	0.62
R2	0.07	0.04	0.01	0.05
Observations	1182	1182	1182	1182

Notes: The table reports OLS estimates. The column title shows the dependent variable. These are an indicator of any successful endline interview with the DW in column 1, a successful direct interview with the DW in column 2, a successful proxy interview for a DW in column 3, and an indicator of a successful endline interview with the household in column 4. The explanatory variable indicates whether the DW was assigned to the intervention. The regressions include the standard set of covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table D.3: Correlation between treatment of the DW by the employer and her wellbeing

	(1)	(2)	(3)
	Full	Hong Kong	Saudi Arabia
Index of treatment of DW (STE)	0.613*** (0.069)	0.656*** (0.129)	0.594*** (0.082)
R2	0.15	0.13	0.13
Observations	944	366	578

Notes: The table reports OLS estimates. The dependent variable is an index of subjective wellbeing measured as standardized treatment effect across six different indicators (see Appendix B for the definition). The explanatory variable is the index of the treatment of the DW by the employer measured as a standardized treatment effect. All regressions do not control for any additional variables and use data from direct interviews. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table D.4: Effect of the intervention on the treatment of the DW by the employer (robustness checks)

	(1) Baseline full sample	(2) No controls	(3) No assignment deviations	(4) Direct interviews	(5) IV direct interviews
			<i>Full</i>		
Intervention	0.035 (0.022)	0.030 (0.023)	0.032 (0.022)	0.049 (0.032)	0.087 (0.055)
Control mean	0.00	0.00	0.00	-0.09	
Observations	1667	1667	1591	944	944
			<i>Hong Kong</i>		
Intervention	0.066** (0.029)	0.061** (0.030)	0.064** (0.030)	0.086* (0.045)	0.135* (0.070)
Control mean	0.20	0.20	0.21	0.14	
Observations	697	697	665	366	366
			<i>Saudi Arabia</i>		
Intervention	0.013 (0.031)	0.008 (0.033)	0.011 (0.032)	0.033 (0.043)	0.062 (0.080)
Control mean	-0.16	-0.16	-0.16	-0.24	
Observations	970	970	926	578	578

Notes: The table reports OLS estimates in column 1-4 and IV estimates in column 5. The outcome in all columns is the index of the treatment of the DW by the employer measured as a standardized treatment effect. In columns 1-3, we replace information not available from direct interviews with information from endline (second priority) or midline proxy interviews (third priority). In columns 4 and 5, we use data from direct interviews only. Column 1 replicates the main results from Table 2, column 1. Column 2 shows a specification without any controls, except an indicator migrating to Saudi Arabia in the full sample. Column 3 is identical to column 1 but excludes observations from the sample, where the assignment recorded by the survey firm deviated from the original assignment list. Column 5 shows results from an instrumental variable regression, using the assignment to the intervention as instrument for having followed the instructions of the intervention. The first stage corresponds to Table D.1, column 2. The regressions include the standard set of covariates as in Table 2. Robust standard errors are in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table D.5: Online experiment: Summary statistics, by country and treatment

	Full (1)	Hong Kong (2)	Saudi Arabia (3)	United Arab Emirates (4)	No info (5)	Photo (6)	Gift (7)	Photo and gift (8)
Female (0/1)	0.43 (0.50)	0.49 (0.50)	0.33 (0.47)	0.35 (0.48)	0.42 (0.49)	0.38 (0.49)	0.44 (0.50)	0.50 (0.50)
Age range 18-34 (0/1)	0.48 (0.50)	0.42 (0.49)	0.58 (0.49)	0.56 (0.50)	0.49 (0.50)	0.48 (0.50)	0.48 (0.50)	0.48 (0.50)
Age range 35-49 (0/1)	0.42 (0.49)	0.43 (0.50)	0.39 (0.49)	0.44 (0.50)	0.42 (0.49)	0.42 (0.50)	0.39 (0.49)	0.43 (0.50)
Age range 50+ (0/1)	0.10 (0.30)	0.14 (0.35)	0.03 (0.18)	0.00 (0.06)	0.09 (0.29)	0.09 (0.29)	0.13 (0.33)	0.09 (0.28)
Some college (0/1)	0.98 (0.12)	0.98 (0.13)	0.98 (0.13)	0.99 (0.09)	0.98 (0.12)	0.98 (0.12)	0.98 (0.14)	0.99 (0.11)
Employed (0/1)	0.88 (0.32)	0.95 (0.22)	0.77 (0.42)	0.81 (0.39)	0.88 (0.33)	0.92 (0.27)	0.87 (0.33)	0.89 (0.32)
Monthly household income	5808.04 (3709.49)	5072.13 (3104.24)	7538.18 (4312.05)	5932.53 (3926.63)	5825.53 (3711.03)	6047.52 (3712.10)	5554.59 (3659.20)	5805.91 (3761.65)
Single or not married (0/1)	0.35 (0.48)	0.42 (0.49)	0.24 (0.43)	0.22 (0.41)	0.36 (0.48)	0.32 (0.47)	0.35 (0.48)	0.33 (0.47)
Married (0/1)	0.63 (0.48)	0.56 (0.50)	0.73 (0.44)	0.76 (0.43)	0.63 (0.48)	0.67 (0.47)	0.63 (0.48)	0.65 (0.48)
Any children (0/1)	0.53 (0.50)	0.34 (0.47)	0.84 (0.36)	0.79 (0.41)	0.52 (0.50)	0.55 (0.50)	0.52 (0.50)	0.56 (0.50)
Observations	1662	1000	407	255	971	200	254	237

Notes: The table reports means and standard deviations in parentheses. Monthly household income is measured in 2018 PPP USD for the minimum values in an income range.

Table D.6: Online dictator game (robustness checks)

	Pre-specified specification (1)	No controls (2)	Pooled treatment (3)	Complete survey data (4)	Matched person is real (5)
Photo treatment	0.83*** (0.32)	0.85*** (0.31)		0.88** (0.41)	0.65* (0.37)
Gift treatment	0.46 (0.29)	0.47 (0.28)		0.09 (0.38)	0.24 (0.34)
Photo + gift treatment	0.38 (0.31)	0.36 (0.31)		0.08 (0.41)	0.08 (0.37)
Treatments pooled			0.54*** (0.20)		
Default egalitarian	3.20*** (0.20)	3.21*** (0.20)	3.21*** (0.20)	3.06*** (0.27)	2.88*** (0.24)
Constant	5.56*** (1.50)	4.56*** (0.18)	4.56*** (0.18)	4.83*** (1.69)	
Controls	Yes	No	Yes	Yes	Yes
Control mean	6.07	6.07	6.07	6.26	6.07
Observations	1,662	1,662	1,662	946	1,145

Notes: The table reports OLS estimates. The dependent variable is a dictator game transfer in USD that can take integer values from 0 to 10. All regressions control for gender, age categories (9 dummies), educational attainment (5 dummies), employment status (6 dummies), monthly household income converted from local currencies to 2018 PPP USD, marital status (3 dummies), number of children (5 dummies), binary variables for countries, matched partner indicators, and the type of device used. Column 1 presents the pre-specified specification, column 4 restricts the sample to individuals with complete survey data, column 5 restricts the sample to participants who responded that the matched participant is a real person for whom the data is recorded. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table D.7: Correlation between the treatment of the DW by the employer and the overall subjective assessment of migration by households

	(1) Index of treatment of DW (STE), all interviews	(2) Index of treatment of DW (STE), direct interviews only	(3) Aware of intervention
		<i>Full</i>	
HH: Overall effect	0.075** (0.033)	0.092* (0.048)	
Intervention			0.183*** (0.021)
Control mean			0.14
Observations	1120	687	1512
		<i>Hong Kong</i>	
HH: Overall effect	0.057 (0.044)	0.048 (0.070)	
Intervention			0.202*** (0.033)
Control mean			0.12
Observations	477	259	606
		<i>Saudi Arabia</i>	
HH: Overall effect	0.093* (0.048)	0.117* (0.064)	
Intervention			0.177*** (0.028)
Control mean			0.15
Observations	643	428	906

Notes: The table reports OLS estimates. The column title shows the dependent variable. These are the index of the treatment of the DW by the employer in columns 1 and 2 and an indicator of whether the household respondent is aware that the DW was assigned to the intervention in column 3. The explanatory variable in columns 1 and 2 is the subjective assessment of the household of the effect of migration (see column 1 of Table D.8). The explanatory variable in column 3 indicates whether the DW was assigned to the intervention. In column 1, we use answers from both direct and proxy interviews. In column 2, we use answers from direct interviews only. The assessment of the treatment of the DW by the employer and the subjective assessment of the effect of migration are hence from different individuals. In the upper panel of columns 1 and 2, we control for an indicator of working in Saudi Arabia. In column 1, we also control for an indicator of the outcome being measured in a direct interview. In column 3, we include the standard set of covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table D.8: Effect of the intervention on the subjective assessment of migration by households (binary)

	(1) HH: Overall effect	(2) HH: Effect on financial security	(3) HH: Effect on standard of living	(4) HH: Effect on housing	(5) HH: Effect on health	(6) HH: Effect on education	(7) HH: Effect on family life	(8) HH: Effect on social life
	<i>Full</i>							
Intervention	0.042** (0.021)	0.036 (0.029)	0.051* (0.029)	0.037 (0.029)	0.009 (0.029)	0.070** (0.029)	0.064** (0.029)	0.031 (0.024)
Control mean	0.44	0.53	0.41	0.47	0.44	0.51	0.50	0.17
Observations	1173	1167	1169	1169	1170	1148	1169	1100
	<i>Hong Kong</i>							
Intervention	0.034 (0.031)	-0.006 (0.045)	0.061 (0.045)	0.028 (0.045)	-0.017 (0.045)	0.055 (0.046)	0.071 (0.045)	0.040 (0.037)
Control mean	0.44	0.54	0.40	0.46	0.42	0.53	0.52	0.18
Observations	495	492	494	494	493	481	492	471
	<i>Saudi Arabia</i>							
Intervention	0.048* (0.027)	0.071* (0.038)	0.038 (0.039)	0.046 (0.039)	0.027 (0.039)	0.081** (0.038)	0.055 (0.039)	0.023 (0.031)
Control mean	0.44	0.52	0.43	0.48	0.45	0.50	0.48	0.17
Observations	678	675	675	675	677	667	677	629

Notes: The table, which is a binarized version of Table 5, reports OLS estimates in all columns. The column title shows the dependent variable. In column 1, the dependent variable is the mean of the subjective assessment of the household of the effect of migration on different aspects (see columns 2-8). These variables are coded as 1 "worse" 2 "same" 3 "better"; In columns 2-8, the dependent variable is the response to the question "Do you think that through [name of DW] employment in [Hong Kong/Saudi Arabia] the situation of your household has gotten better, stayed the same or got worse with regard to [aspect stated in column 2-8]?" All columns use data from household interviews. The explanatory variable indicates whether the DW was assigned to the intervention. All regressions include the standard set of baseline covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table D.9: Effect of the intervention on remittances

	(1) Amount received by HH (log)	(2) Amount sent by DW (log)	(3) Remittances frequency, reported by HH	(4) Remittances frequency, reported by DW	(5) Remittances variance (log)
<i>Full</i>					
Intervention	0.141* (0.077)	-0.061 (0.083)	0.162 (0.146)	0.065 (0.153)	-0.111 (0.076)
Control mean	4.90	5.11	4.39	4.90	4.66
Observations	1126	918	1148	933	535
<i>Hong Kong</i>					
Intervention	0.024 (0.119)	0.027 (0.128)	0.328 (0.233)	0.163 (0.284)	-0.207 (0.128)
Control mean	4.80	5.06	4.76	5.29	4.50
Observations	465	353	479	361	181
<i>Saudi Arabia</i>					
Intervention	0.214** (0.100)	-0.121 (0.107)	0.041 (0.186)	0.031 (0.167)	-0.057 (0.093)
Control mean	4.97	5.14	4.11	4.64	4.73
Observations	661	565	669	572	354

Notes: The table reports OLS estimates. The column title shows the dependent variable. These are the log of remittances received (+ 1) as reported by the household in column 1, the log of remittances received (+1) as reported by the DW in column 2, the number of times the DW sent remittances in the six months before the interview as reported by the household in column 3, the number of times the DW sent remittances in the six months before the interview as reported by the DW in column 4, the log of the variance of remittances received (calculated over the amounts stated in the midline, the household endline and the DW endline interview survey) in column 5. Columns 2 and 4 use data from direct interviews with DWs, other columns use data from interviews with households. The explanatory variable indicates whether the DW was assigned to the intervention. The regressions include the standard set of covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

E Additional results

This section summarizes a few additional results from both pre-specified and exploratory analysis. Table E.10 shows how the intervention changes the way the DW describes the employer. DWs in both Hong Kong and Saudi Arabia describe their employers quite favorably. Most DWs agree that their employer is good (column 2), not short-tempered (column 3), no slaver (column 4), not violent (column 5), and not strict (column 6). We do not find that the intervention has a meaningful effect on any of these descriptions. The same is true if we aggregate them in an index (column 1).

As shown in the main text, treated DWs in Saudi Arabia are more likely to remain with their initial employer and consider overseas working as a favorable choice for their own children (columns 3 and 4 of Table 2). However, Table E.11 shows that the intervention does not change the intention of household respondents in the Philippines to become an Overseas Filipino Worker themselves.

In exploratory analysis, we analyze whether the timing of the endline interview has an influence on the magnitude of the treatment effect on our main outcome, the aggregate index that summarizes different dimensions of how the DW is treated by the

employer. Endline interviews were scheduled to take place about two years after the baseline interview, around the end of the initial work contract. While the average interview happened about 736 days after the baseline interview, some interviews happened considerably later. However, as Figure E.5 shows, the timing of the endline interview does not affect the estimated treatment effect.

In addition to the financial mechanisms explored in the main text, the intervention could also affect households in the Philippines through communication or psychological mechanisms.

Table E.12 shows that the intervention does not affect the amount of communication between the DW and her household members. Thus, the intervention does not seem to affect household-level outcomes via better communication or coordination between the DW and her household members in the Philippines (Seshan and Yang, 2014).

Table E.13 shows the results for psychological mechanisms. Better treatment of the DW by the employer abroad might increase the subjective wellbeing of the DW abroad and thus make it easier for the household to sustain the physical separation (which in turn might lead to a better assessment of the effects of migration on the household). However, the intervention does not affect the overall index of subjective wellbeing of the DW (column 1), the indices of mental health (column 2), and migrant-specific wellbeing (column 7) as well as most of their components (remaining columns). Only when it comes to not feeling pain (column 6) and not feeling overwhelmed by the challenges faced abroad (column 9) do we find statistically significant treatment effects for DWs in Saudi Arabia. They are, however, small in magnitude. There is hence little evidence for the effects on household members being driven by psychological mechanisms.

Table E.10: Effect of the intervention on the way the DW describes the employer

	(1) Average employer description	(2) Employer good	(3) Employer no short-temper	(4) Employer no slaver	(5) Employer not violent	(6) Employer not strict
	<i>Full</i>					
Intervention	0.006 (0.010)	0.006 (0.019)	0.023 (0.016)	0.015 (0.010)	-0.008 (0.008)	-0.005 (0.016)
Control mean	0.92	0.85	0.88	0.96	0.98	0.91
Observations	1408	1408	1407	1407	1407	1407
	<i>Hong Kong</i>					
Intervention	0.025 (0.015)	0.044 (0.028)	0.041* (0.025)	0.024* (0.014)	0.005 (0.012)	0.012 (0.024)
Control mean	0.91	0.84	0.88	0.96	0.98	0.91
Observations	591	591	591	591	591	591
	<i>Saudi Arabia</i>					
Intervention	-0.005 (0.014)	-0.017 (0.025)	0.012 (0.022)	0.008 (0.013)	-0.016 (0.011)	-0.014 (0.021)
Control mean	0.92	0.86	0.89	0.96	0.98	0.91
Observations	817	817	816	816	816	816

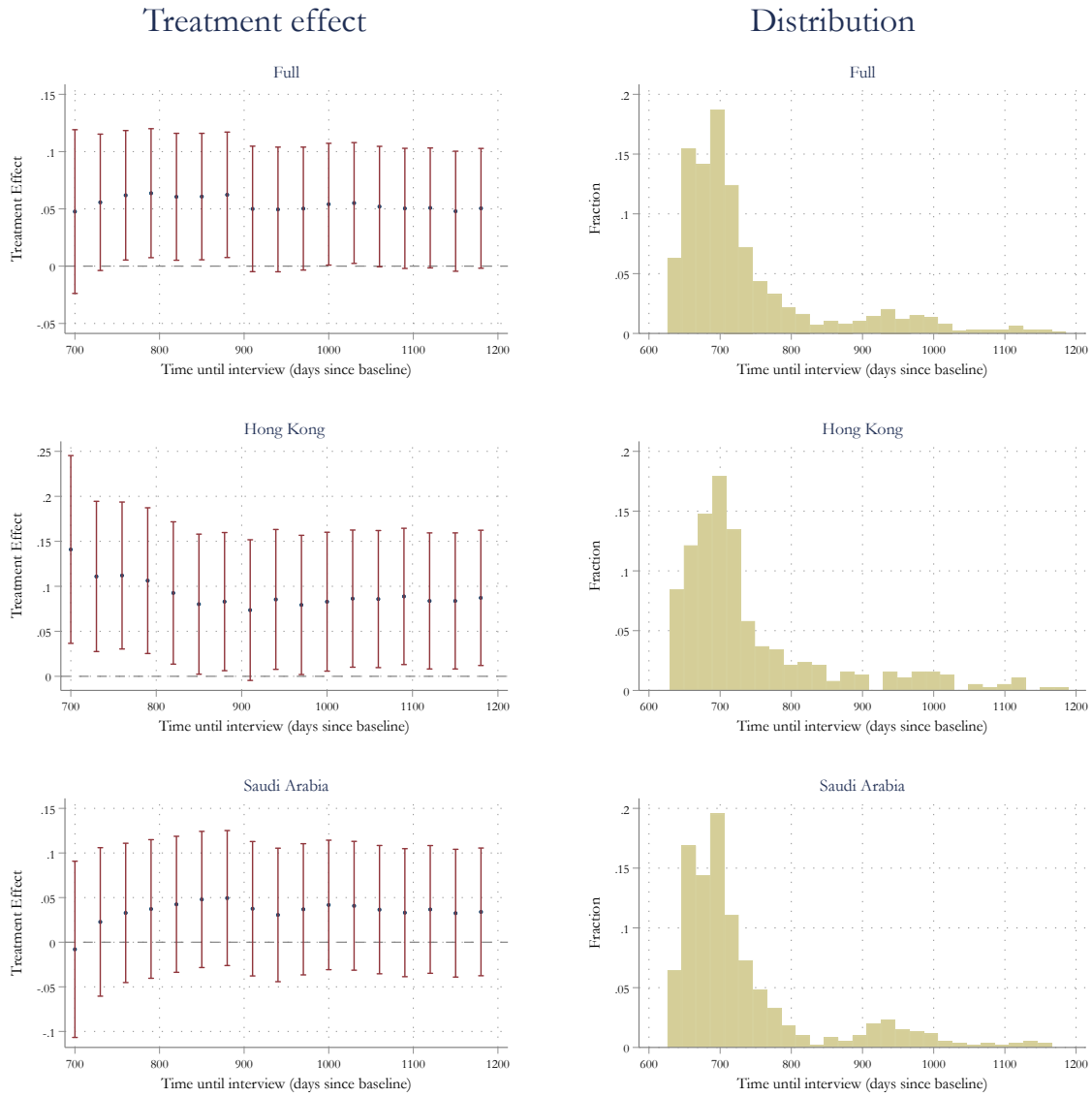
Notes: The table reports OLS estimates. The column title shows the dependent variable. These are binary indicators of whether the DW describes the employer with the respective attitudes in columns 2-6, and the mean description across the different attitudes in column 1. The regressions include the standard set of covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table E.11: Effect of the intervention on intentions of household respondents to become an Overseas Filipino Worker

	(1) Full	(2) Hong Kong	(3) Saudi Arabia
Intervention	0.028 (0.024)	0.042 (0.039)	0.024 (0.032)
Control mean	0.20	0.22	0.19
Observations	1133	480	653

Notes: The table reports OLS estimates. The dependent variable is an indicator of whether the household respondent answers yes to the question "Do you have any intentions of being an OFW?". The explanatory variable indicates whether the DW was assigned to the intervention. The regressions include the standard set of covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Figure E.5: Treatment effect by timing of endline interview



Notes: The left-hand side of each panel shows a coefficient plot of the effect of the intervention on the index of the treatment of the DW by the employer. The sample is restricted to direct interviews. The specification corresponds to the one in Table D.4, column 4. The individual coefficients are from regressions using sub-samples of DWs completing the endline interview by different times since the baseline interview (the horizontal axis). E.g., the coefficient for the day 800 uses the sample of individuals who completed the endline interview on the 800th day since the baseline interview or earlier. Robust standard errors. Error bars represent 90 percent confidence intervals. The right-hand side of each panel shows the distribution of time elapsed between the baseline and the endline interview. Bins represent 20 days.

Table E.12: Effect of the intervention on the communication between the DW and her household in the Philippines

	(1) Communication index (STE)	(2) Text frequency	(3) Call frequency	(4) Call length	(5) Confiscated phone
	<i>Full</i>				
Intervention	0.017 (0.037)	0.028 (0.146)	0.040 (0.374)	1.801 (1.761)	0.000 (0.019)
Control mean	0.00	3.13	5.42	23.59	0.14
Observations	1413	1403	1332	1340	1329
	<i>Hong Kong</i>				
Intervention	-0.015 (0.062)	-0.069 (0.236)	-0.211 (0.669)	0.443 (3.230)	-0.007 (0.020)
Control mean	0.16	3.61	6.86	27.84	0.06
Observations	558	556	524	533	575
	<i>Saudi Arabia</i>				
Intervention	0.041 (0.045)	0.103 (0.189)	0.148 (0.435)	3.134 (2.094)	0.010 (0.030)
Control mean	-0.11	2.79	4.44	20.66	0.21
Observations	855	847	808	807	754

Notes: The table reports OLS estimates. All regressions are based on data from midline proxy interviews. The column title shows the dependent variable. These are the communication index as described in Appendix B in column 1, the number of days per week that the DW and the household exchange text messages in column 2, the number of calls in the past four weeks in column 3, and the length of the last call (in minutes) in column 4. The explanatory variable indicates whether the DW was assigned to the intervention. The regressions include the standard set of covariates as in Table 2. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.

Table E.13: Effect of the intervention on the subjective wellbeing of the DW

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Wellbeing index (STE)	Mental health index	Happy	Nervous	Downhearted and blue	Pain	Migrant specific well-being	Homesick	Overwhelmed
	<i>Full</i>								
Intervention	0.026 (0.039)	0.019 (0.046)	0.062 (0.067)	-0.016 (0.072)	-0.062 (0.073)	0.093 (0.070)	0.048 (0.065)	-0.028 (0.084)	0.125 (0.076)
Control mean	-0.00	3.61	3.70	3.43	3.49	3.81	3.27	2.81	3.74
Observations	944	944	944	943	944	944	944	944	944
	<i>Hong Kong</i>								
Intervention	0.063 (0.061)	0.074 (0.073)	0.083 (0.105)	0.111 (0.111)	0.073 (0.115)	0.035 (0.112)	0.070 (0.103)	0.091 (0.137)	0.049 (0.121)
Control mean	0.10	3.73	3.94	3.46	3.52	4.02	3.34	2.87	3.82
Observations	366	366	366	365	366	366	366	366	366
	<i>Saudi Arabia</i>								
Intervention	0.014 (0.052)	-0.005 (0.060)	0.053 (0.088)	-0.086 (0.095)	-0.134 (0.096)	0.148 (0.092)	0.055 (0.084)	-0.081 (0.106)	0.190* (0.097)
Control mean	-0.06	3.52	3.54	3.41	3.47	3.67	3.22	2.76	3.68
Observations	578	578	578	578	578	578	578	578	578

Notes: The table reports OLS estimates. The column title shows the dependent variable. These are an index of subjective wellbeing measured as standardized treatment effect (see Appendix B for the definition) in column 1, a mental health index, defined as the mean across the indicators in columns 3-6 in column 1, and a migrant-specific wellbeing index, defined as the mean across the indicators in columns 8 and 9. Columns 3-6, 8, and 9 are based on responses to "During the past month, how much of the time ... [were you a happy person / a very nervous person / did you feel downhearted and blue / physical pain / homesick / overwhelmed by the challenges you face in Hong Kong/Saudi Arabia]?". Answer options go from "None of the time" to "All of the time" on a five-point scale. Higher values imply more positive outcomes. The explanatory variable indicates whether the DW was assigned to the intervention. Regressions are based on data from direct interviews only and include the standard set of covariates as in Table 2 and the baseline mental health index. Robust standard errors in parentheses. */**/** indicate statistical significance at the 10/5/1%-level.