Picture This: Social Distance and the Mistreatment of Migrant Workers

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For helpful comments and discussions, we thank Michel Beine, Simone Bertoli, Jesus Fernandez-Huertas, Martin Fernandez Sanchez, André Gröger, David McKenzie, Alice Mesnard, Joan Monras, Melanie Morten, and Alexandra Scacco, as well as seminar and

Electronically published March 27, 2026

Journal of Political Economy Microeconomics, volume 4, number 2, May 2026.

https://doi.org/10.1086/736930

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, treated DWs' families who remained in the Philippines came to view international labor migration more positively. An online experiment with potential employers suggests that the effect operates through a reduction in employers' perceived social distance from their employees.

I. Introduction

Inherent power asymmetries between employers and workers pose a severe challenge to securing labor rights and protecting workers from abuse. There is particular concern regarding 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 (International Labor Organization 2015).

In our own survey data from Filipino DWs working in Saudi Arabia and Hong Kong, nontrivial shares 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

conference participants at Ludwig Maximilian University Munich, New York University Abu Dhabi, the University of Regensburg, the eleventh 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 OW4/1171. Bartoš acknowledges financial support from the Italian Ministry of University and Research under the Department of Excellence 2023–27 grant agreement "Centre of Excellence in Economics and Data Science." The views expressed in this article are not necessarily those of 3ie or its members. The institutional review board review was provided by the University of Michigan (HUM00087460). Our American Economic Association Randomized Controlled Trial (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. We thank Anna Jennifer Umlas for excellent project management and Jakob Beuschlein and Yasar Ersan for superb research assistance. This paper was edited by John List.

[1] These percentages are control group means in our DW survey. For details, see sec. III.

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] Similarly, employers in destination countries may not share the same interest in protecting migrant workers, as they may not see any immediate benefits from such policies for themselves.

In this paper, we study an intervention that aims to reduce mistreatment of international migrant workers by changing the behavior of their employers toward them. The intervention is implemented directly by the workers and complements difficult public policy changes in destination countries. It builds on evidence from behavioral economics that reducing perceived social distance increases prosociality and trust in others (Hoffman, McCabe, and Smith 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). The intervention is simple: encouraging migrant workers to show a photo of their family while giving a small gift to their employer upon starting employment. We hypothesize that showing the photo reduces employers' perceived social distance from the DW.[3]

We implemented the intervention as part of a natural field experiment (Harrison and List 2004) with 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' predeparture 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, inspired by the Filipino tradition of *pasalubong*, that is, the traveler bringing a gift.[4]

[2] Origin-country governments such as the Philippines have on occasion simply banned international migrant labor to certain destinations in response to major abuses (Theoharides 2020).

[3] While the small gift's main purpose is that of a conversation opener, it could also create feelings of reciprocity from employers (Charness and Rabin 2002; Falk and Fischbacher 2006). In an employer-employee relationship with large power asymmetries, reducing social distance may benefit the DW in a range of domains. This may include a reduction in exploitation and harassment of the DW, a reduction in authoritarian employer practices, and an increase in employer support for the DW's work-life balance. To disentangle the potential mechanisms at play, we implemented an online experiment among potential employers, described below.

[4] They were provided a pack of dried mangoes (a Filipino delicacy) to give as the small gift. Compliance with the intervention was high, with 57% of those in the treatment group reporting that they followed the instructions on how to introduce themselves.

We find that the intervention improved the treatment of DWs by their employers, as revealed by a number of measures 2 years after the start of employment. First, the intervention led DWs to report less mistreatment by their employers (an index incorporating verbal and physical abuse, working conditions, and timeliness of wage payments). Second, treated DWs report higher satisfaction in their relationship with their employer. Third, treated DWs are more likely to remain working for the same employer, and are less likely to have returned to the Philippines. Fourth, the intervention affects different domains of mistreatment in Saudi Arabia and Hong Kong, suggesting that the effects are context specific.

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, suggest 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 2 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.

In an exploratory—not preregistered—analysis, we show that all effects are exclusively driven by the subgroup of DWs who had not talked to their employer prior to their departure. This result suggests that DWs or employers who are more socially distant to start with or those with weaker social skills benefit more from the intervention.

We also 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.

To better understand the mechanism behind these effects from the demand side of the market, 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 Kong, Saudi Arabia, and the United Arab Emirates),[5] and 66% of

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

subjects were either past or current employers of DWs. Using a dictator game, we measure subjects' generosity toward 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 accompanied by a personalized well-wishing message 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, we provide suggestive evidence that a reduction in social distance drives the increased generosity of potential employers.

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. Increased social distance is associated with lower levels of altruism (Hoffman, McCabe, and Smith 1996; Bohnet and Frey 1999) and trust (Binzel and Fehr 2013) in carefully designed studies using redistribution experiments. Studies manipulating social distance in random encounters (Charness and Gneezy 2008) and in existing social networks of peers (Leider et al. 2009; Ligon and Schechter 2012) arrive at the same finding. Chandrasekhar, Kinnan, and Larreguy (2018) show that increased 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, Edmark, and Lundqvist 2012). To study causal effects, laboratory studies manipulate social distance either by increasing identifiability through the 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 important 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. It is far from obvious

[6] Social 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).

whether previous findings from lab or lab-in-the-field experiments on the role of social distance apply to real-world employer-employee relations that involve cross-cultural encounters and enormous power asymmetries. Such relations are notably transactional in nature and may be impervious to attempts at reducing social distance.

Our work is also relevant for policymaking 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, McKenzie, and Stillman 2011; Clemens and Tiongson 2017), and their origin areas (Caballero, Cadena, and Kovak 2023; Dinkelman, Kumchulesi, and Mariotti 2024; Khanna et al., forthcoming). 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 (International Organization for Migration 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). Other than the first-order ethical importance, improving migrants' work experience may thus also have economic implications. Limited evidence aimed at informing prospective migrants about quality of recruitment agencies (Bazzi et al., forthcoming) or improving intermediary service quality by introducing a rating system (Fernando and Singh, forthcoming) improves migrants' experience abroad. The effect is likely driven by improved employer-worker matching. Keeping the match fixed, our study complements this emerging literature by focusing on improving the employer-worker relationship. We show that this approach reduces migration's social harms and improves migrants' welfare.

In addition, we provide first evidence on the working conditions and the extent of mistreatment in a representative sample of an important segment of migrant workers. Migrant workers, and DWs in particular, are notoriously difficult to sample and survey, especially when it comes to sensitive topics such as working conditions. This is why the few existing studies are based on nonrepresentative samples and often use qualitative evidence (e.g., Human Rights Watch 2006; Naufal and Malit 2018; Hall, Garabiles, and Latkin 2019; Amnesty International 2020; Chung and Mak 2020; Blaydes 2023). We survey a representative sample from the population of Filipino women departing to work as DWs in two of the most important destinations. Our survey design included various safeguards to maximize the safety of the respondents and, at the same time, minimize attrition, such as scheduling interviews with DWs at the end of the initial contract period, interviewing them only at times when they could talk freely, and conducting proxy interviews with knowledgeable household

members remaining in the Philippines if necessary. As stated above, significant shares of DWs report mistreatment and poor working conditions, suggesting that the topic deserves more attention.

II. Context

A substantial share of international migrant workers work for private households as domestic workers (DWs). According to the International Labor Organization (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 Filipino migrant 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]

[7] Policies facilitating and regulating international labor migration, and agencies such as the Philippines' POEA and OWWA, can be found in the majority of developing countries. Khanna

Philippine government regulations mandate predeparture training for international migrants, referred to as Pre-Departure Orientation Seminars (PDOSs). All Filipinos going 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 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. Of the DWs in our sample, 75% departed within 2 months, and 90% did so within 3 months.

III. Field Experiment, Hypotheses, and Empirical Approach

A. 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

et al. (forthcoming) tabulate data on government policies on outbound international migration. 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.

[8] Attending a PDOS is also mandatory for permanent migrants. For an evaluation of the PDOS for permanent migrants, see Barsbai et al. (2024b).

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). A traveler bringing a gift, most often food, is a Filipino tradition known as *pasalubong*. 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 in which 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 fig. A.1 [figs. A.1–F.5 are available online]). We discuss ethical considerations in appendix section D.1 (apps. A–F are available online).

B. Baseline Interviews and Experimental Design

The sample consists of 1,987 first-time DWs bound for Saudi Arabia (1,182) and Hong Kong (805), 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. The 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. The 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, roughly half are married, and about three-quarters

TABLE 1
Baseline Characteristics of DWs by Treatment Status

	Hong Kong			Saudi Arabia		
	Control (1)	Treated (2)	*p*-value (3)	Control (4)	Treated (5)	*p*-value (6)
Age	30.4	30.4	.99	31.2	31.1	.63
Married (0/1)	.51	.52	.82	.43	.45	.57
Has children (0/1)	.75	.72	.35	.80	.80	.92
Speaks Tagalog at home (0/1)	.41	.41	.95	.48	.48	.96
High school degree or less (0/1)	.36	.31	.098	.73	.74	.71
College degree (0/1)	.19	.22	.34	.12	.11	.77
Worked 6 months ago (0/1)	.50	.53	.35	.47	.49	.52
Worked as domestic helper in Philippines (0/1)	.39	.38	.69	.43	.42	.73
Born in Pangasinan province (0/1)	.093	.12	.29	.051	.041	.40
Born in National Capital Region (0/1)	.060	.038	.14	.056	.052	.79
Does not use internet (0/1)	.14	.11	.20	.31	.33	.44
Has personal savings (0/1)	.17	.17	.96	.079	.095	.34
Has personal bank account (0/1)	.30	.33	.39	.13	.12	.59
Knows someone at destination (0/1)	.13	.11	.31	.070	.064	.64
Salary deduction (0/1)	.18	.19	.56	.083	.10	.30
Has talked to employer (0/1)	.52	.50	.63	.13	.13	.71
Knows the name of the language spoken at destination (0/1)	.88	.88	.78	.91	.91	.88
Mental health index	23.2	23.4	.43	22.5	22.7	.47
Household size	4.47	4.16	.034	4.73	4.74	.96
Destination city is Riyadh (0/1)				.43	.42	.60
Observations	432	373		590	592	

Note.—Columns 1–2 and 4–5 report means based on data from baseline interviews with DWs. Columns 3 and 6 report *p*-values from a two-sided *t*-test for equality of means for the control and the treatment group. Columns 1–3 use data for the Hong Kong subsample; cols. 4–6 use data for the Saudi Arabia subsample. Because of separate randomizations for the Hong Kong and the Saudi Arabia subsamples, we do not show a balance test for the full sample. Table E.1 shows corresponding balance tests in a sample with completed endline interviews.

have at least one child. DWs bound for Saudi Arabia are considerably less educated than DWs bound for Hong Kong (74% vs. 34% have at most a high school degree). Another noteworthy difference is that half of those going to Hong Kong have already talked to their employers at the time of the baseline interview, while only 13% of those going to Saudi Arabia have done so. Balance checks reveal no significant differences between observable characteristics of study participants across treatment and control groups.[9] As prespecified, 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 individuals could have learned about

[9] Table E.1 (tables E.1–F.15 are available online) shows corresponding balance tests in a sample with completed endline interviews.

the intervention from treated individuals and followed the instructions themselves. Table E.2 shows that there is indeed noncompliance. While almost all DWs in the treatment group remember the instructions at the time of the endline survey after 2 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.

C. 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 12 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 such as 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 reinterview rate of 74%. 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 2 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 cases in which a direct interview was not possible, we conducted a proxy interview (441 cases). In total, we completed 1,427 interviews, which resulted in a reinterview rate of 74% (49.5% without the proxy interviews).[10] In cases in which we could not collect endline data, we replaced 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 E.3 shows that assignment to the intervention is not correlated with whether an endline interview with a DW was completed via either interview method (col. 1), via a direct interview (col. 2), or via a proxy interview (col. 3). Likewise, assignment to the intervention is not correlated with the likelihood of reinterviewing the DW's household in the Philippines (col. 4). Throughout, baseline characteristics have little power to predict reinterview status. The R^2 of the corresponding regressions is low. In addition, we find no evidence that attrition affects different types of DWs in the treatment and control group. Table E.4 shows that interaction terms between intervention indicator and baseline characteristics cannot predict reinterview status. High p-values from F-tests on joint significance do not point to systematic differences in attrition by treatment status. Overall, there is no indication that our sample systematically loses specific types of DWs over time.

D. Outcomes and Hypotheses

We examine outcomes and hypotheses as specified in our preanalysis plan (PAP).[11] We group outcomes by domain and construct an aggregate

[10] The reinterview rate is comparable with other studies that have tracked migrants from their origin to their destination countries and over a similar period of time (and with the proxy interviews we achieve even higher reinterview rates). Ambler (2015) successfully reinterviewed 73% of migrants from El Salvador to Washington, DC, Ashraf et al. (2015) 57% of migrants from El Salvador to Washington, DC, Shrestha and Yang (2019) 60% of Filipino maids to Singapore, Gibson et al. (2018) 64% of migrants from Tonga to New Zealand, and Barsbai et al. (2024b) 61% of migrants from the Philippines to the United States.

[11] 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

index or standardized treatment effect (STE) for most domains. For the construction of the STE, we follow Kling, Liebman, and Katz (2007).[12] Details on the construction of indexes 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 well-being, and affects 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 the 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. 2020).

E. 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

specified a PAP for midline results and a slightly updated PAP for endline results that also specifies hypotheses regarding household outcomes.

[12] 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 kth 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 $Y^* = \Sigma_K Y_k^*/K$. 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.

on time, (x) not having experienced salary deductions, and (xi) hours of daily spare time.[13]

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 nontreated 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). Eight percent of nontreated 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 well-being. Table E.5 shows that better treatment of the DW by the employer correlates with higher subjective well-being of the DW. A 1 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 well being (see app. B for the definition). The high correlation suggests that our aggregate index is a meaningful measure of treatment by the employer.

F. Empirical Analysis

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

$$Y_i = \beta_0 + \beta_1 \text{Intervention}_i + X_i'\theta + \varepsilon_i, \tag{1}$$

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 effect of assignment to the treatment group. X_i' is a vector of pretreatment 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.[14]

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

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

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, pretreatment measures of the respective outcomes—as described in the PAP and in the respective table notes.[15] We report Huber-White robust standard errors.

IV. 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 show how the intervention affects household members of DWs who stay behind in the Philippines. Third, we summarize results from the online experiment that shed light on how the intervention changes employers' behavior toward DWs.

A. Effects of the Intervention on the Treatment of DWs

The majority of employers reacted positively to the intervention: 88.5% of DWs who said they followed the intervention instructions report a positive reaction; 11% report a neutral reaction. Only two DWs report a skeptical reaction. Overall, 14 employers refused to accept the gift.[16]

Table 2 presents estimates of the impact of being assigned to the intervention (coefficient β_1 in eq. [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 (second and third rows). We discuss the results of an exploratory analysis presented in the bottom two rows later when discussing potential mechanisms.

We first focus on the aggregate index that summarizes different dimensions of better treatment by the employer (col. 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 .108). The treatment effect is larger and statistically significant

[15] 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. (2020) for more details on these interventions.

[16] A 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 them."

TABLE 2
EFFECT OF INTERVENTION ON TREATMENT OF DW BY EMPLOYER

	Index of Treatment of DW (STE) (1)	Satisfaction with Relationship with Employer (2)	With Initial Employer (3)	DW: Good for Children to become OFWs (4)
Full:				
Intervention	.035	.102	.038	.054
	[−.01, .08]	[−.01, .21]	[−.00, .08]	[−.00, .11]
Control mean	.00	3.47	.18	.23
Observations	1,667	1,377	1,430	934
Hong Kong:				
Intervention	.066	.167	−.015	.027
	[.01, .12]	[.01, .33]	[−.07, .04]	[−.07, .12]
Control mean	.20	3.60	.19	.27
Observations	697	584	602	363
Saudi Arabia:				
Intervention	.013	.064	.075	.073
	[−.05, .07]	[−.09, .21]	[.02, .13]	[.00, .14]
Control mean	−.16	3.37	.17	.20
Observations	970	793	828	571
Did not talk to employer:				
Intervention	.051	.123	.063	.077
	[.00, .10]	[−.01, .26]	[.02, .11]	[.01, .14]
Control mean	−.06	3.43	.17	.21
Observations	1,173	964	1,003	679
Talked to employer:				
Intervention	−.009	.082	−.021	.005
	[−.09, .07]	[−.11, .27]	[−.09, .05]	[−.11, .12]
Control mean	.14	3.57	.19	.28
Observations	494	413	427	255

NOTE.—The table reports OLS estimates. The column heads give the dependent variables. 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 col. 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 col. 2, an indicator of 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 col. 3, and an indicator of whether the DW agrees with the statement that it would be good for her children to become overseas Filipino workers (OFWs) in col. 4. In cols. 1 and 2, we replace information not available from direct interviews with information from endline (second priority) or midline proxy interviews (third priority). In col. 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 (cols. 1 and 2), an indicator of whether the DW planned to stay abroad for more than 2 years at the time of the baseline interview (col. 3), and an indicator of whether

in the Hong Kong sample (0.066 standard deviations) and close to zero in Saudi Arabia.[17]

For two reasons, our estimates are likely lower-bound estimates of the true treatment effects. First, our results are somewhat stronger when we restrict the analysis to directly reported data from DWs, which might be more reliable (col. 4 of table E.6). The treatment effect for DWs in Hong Kong increases to 0.086 standard deviations, suggesting that DWs might not fully share their experiences of mistreatment with household members in the Philippines or that some experiences are difficult to fully comprehend by those not directly affected. Second, our estimates are intent-to-treat effects. Compliance with the intervention was imperfect, with 64% of those in the treatment group reporting that they followed the instructions on how to introduce themselves. Using assignment to the intervention as an instrument for having followed the instructions uncovers 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 0.087 standard deviations for the entire sample, but these are not precisely estimated (col. 5 of table E.6).

Table 3 shows the effect on the individual components of the index for insights into the dimensions that might drive the overall effect. The overall effect on the aggregate index masks considerable heterogeneity. In Hong Kong, the intervention is associated with less mistreatment in the form of shouting (col. 1) and better working conditions. Treated DWs are more likely to have enough food (col. 6) and be allowed to leave the employer's house (col. 8). In Saudi Arabia, the intervention is associated with less sexual harassment (col. 4). Treated DWs are also more likely to receive their salary on time (col. 9). However, we also find that treated DW are less likely to have enough food (col. 6). The intervention seems to affect different aspects of a DW's treatment by their employer in the two countries. We should stress, however, that the table examines 11 different outcomes across two different destinations. Correcting for multiple testing is too demanding an exercise with our sample size and would render all coefficients statistically insignificant. The evidence should, therefore, be considered suggestive as we cannot rule out chance findings for individual indicators.

[17] Table E.6 shows that our results are robust to not including control variables (col. 2) or dropping cases where interviewers deviated from the randomized assignment (col. 3).

the DW agreed with the statement that it would be good for her children to become OFWs at the time of the baseline interview (col. 4). Square brackets represent 95% confidence intervals, based on robust standard errors. For our primary outcome in col. 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, Shaikh, and Xu (2019) to control for the family-wise error rate. The adjusted *p*-values are .047 for Hong Kong and .67 for Saudi Arabia.

TABLE 3
EFFECT OF INTERVENTION ON TREATMENT OF DW BY EMPLOYER (INDIVIDUAL COMPONENTS)

	No Shouting (1)	No Violence (2)	No Threats (3)	No Sexual Harassment (4)	No Work When Sick (5)	Have Enough Food (6)	Weekly Rest Day (7)	Leave the Employer's House (8)	Salary on Time (9)	No Salary Deduction (10)	Spare Time (Hours) (11)
Full:											
Intervention	.013	−.009	.017	.020	.021	−.005	.009	.022	.059	.019	.206
	[−.05, .08]	[−.04, .03]	[−.02, .06]	[−.01, .05]	[−.03, .07]	[−.05, .04]	[−.03, .05]	[−.01, .06]	[.01, .11]	[−.01, .05]	[−.23, .64]
Control mean	.56	.92	.87	.95	.79	.86	.40	.35	.73	.93	10.38
Observations	943	944	943	942	944	938	944	944	940	937	875
Hong Kong:											
Intervention	.076	.003	.032	−.001	−.005	.084	.031	.064	.032	.015	.249
	[−.02, .18]	[−.05, .05]	[−.02, .09]	[−.03, .03]	[−.08, .07]	[.01, .16]	[−.04, .10]	[−.01, .14]	[−.03, .10]	[−.03, .06]	[−.39, .88]
Control mean	.59	.93	.91	.98	.86	.79	.87	.81	.87	.93	10.13
Observations	365	366	366	365	366	362	366	366	363	365	345
Saudi Arabia:											
Intervention	−.020	−.015	.012	.033	.043	−.056	−.002	−.003	.077	.019	.199
	[−.10, .06]	[−.06, .03]	[−.05, .07]	[−.01, .07]	[−.03, .11]	[−.11, −.00]	[−.05, .04]	[−.04, .03]	[.00, .15]	[−.02, .06]	[−.39, .79]
Control mean	.54	.92	.85	.93	.74	.90	.09	.05	.64	.93	10.54
Observations	578	578	577	577	578	576	578	578	577	572	530

Did not talk to employer:											
Intervention	.030	.016	.025	.034	.061	−.022	.002	−.003	.080	.039	.338
	[−.04, .10]	[−.02, .06]	[−.02, .07]	[.00, .06]	[.00, .12]	[−.07, .03]	[−.04, .04]	[−.04, .03]	[.02, .14]	[.00, .07]	[−.17, .85]
Control mean	.54	.92	.86	.94	.77	.88	.30	.26	.70	.92	10.33
Observations	686	687	686	686	687	683	687	687	686	681	635
Talked to employer:											
Intervention	−.049	−.085	−.013	−.022	−.090	.028	.036	.095	.011	−.038	−.140
	[−.17, .08]	[−.16,−.01]	[−.09, .07]	[−.08, .03]	[−.19, .01]	[−.07, .13]	[−.05, .12]	[.01, .18]	[−.08, .10]	[−.10, .02]	[−1.01, .72]
Control mean	.59	.94	.89	.97	.84	.79	.65	.57	.82	.95	10.51
Observations	257	257	257	256	257	255	257	257	254	256	240

NOTE.—The table reports OLS estimates. The column heads give the dependent variables. Outcomes in cols. 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 threat / 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 to work end time) − break). The explanatory variable indicates whether DW was assigned to the intervention. All regressions include the same covariates as in col. 1 of table 2. Square brackets represent 95% confidence intervals, based on robust standard errors.

In column 2 of table 2, we consider a DW's satisfaction with the relationship with the 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 the first month of the 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% 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% when compared to the control-group mean of 3.60) and smaller for DWs in Saudi Arabia (0.064 or 1.9% 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. Assignment to the intervention increases the likelihood of a DW remaining with the initial employer by 3.8 percentage points (or 21% 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% when compared to the control-group mean of 0.17), while it is close to zero and not statistically significant for DWs in Hong Kong.

This finding may have several interpretations. First, contract renewal may capture different dimensions of mistreatment and well-being that the previous set of variables are unable to measure. Second, the outcome may give different implicit weights to the different components that we use to create the aggregate index. The aggregate index follows Kling, Liebman, and Katz (2007) and weighs each component equally, which may not be a plausible assumption in our setting. Third, working conditions are more favorable in Hong Kong than in Saudi Arabia to start with. At the same time, the cultural distance between the Philippines and Hong Kong is likely smaller, because of their shared cultural Chinese heritage. In addition, the more educated DWs in Hong Kong might have different outside options, making them less dependent on a contract renewal. Finally, the finding may reflect institutional differences in the contract-renewal process. In particular, contract renewal in Saudi Arabia is a more straightforward and shorter process that is led by the employer and can be completed online. By contrast, contract renewal in Hong Kong is led by the DW and involves the submission of required forms to Hong

Kong's Immigration Department as well as the submission of required forms and payment of fees to the Migrant Workers Office of the Philippines (formerly known as the Philippine Overseas Labor Office).

One may be concerned that a contract extension, especially in the employer-led process in Saudi Arabia, is not a result of DWs' free will but rather of increased employer coercion power. Three arguments speak against this. First, even in Saudi Arabia, both the employer and the DW need to mutually agree on the contract extension. Second, our results documenting reduced mistreatment and increased satisfaction with the employer speak against increased coercion. Third, when asking DWs about their plan to stay with their employer, the treatment effect is also positive,[18] suggesting that both sides agree on the contract extension.

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 overseas Filipino workers (OFWs) when they grow up (col. 4). Being assigned to the intervention increases the likelihood of agreeing with this statement by 5.4 percentage points (or 23% 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% 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.

The last two rows of table 2 present results of an exploratory—not preregistered—analysis that splits the sample into DWs who had talked to their employer prior to the baseline interview and those who had not. We hypothesize that DWs who had not communicated with their employer before departure are more likely to benefit from the intervention. There are two possible mechanisms. First, the mere act of communicating signals higher interpersonal skills on either the side of the employer or the DW. Such skills allow for the formation of better relationships and better treatment in the workplace to begin with. This interpretation aligns with the observation that individuals in the control group who had communicated with their employer have higher means across all outcomes compared to those who did not. In addition, DWs who had communicated

[18] The coefficient is 0.056 (standard error 0.03) in the combined sample.

with their employer have higher levels of education than those who had not done so (22% vs. 12% have a college degree, and 42% vs. 64% have at most a high school degree or less). Higher levels of education might be associated with higher interpersonal skills of DWs, which could mediate the effect of the intervention. Second, DWs who had communicated with their employer prior to their departure may have already reduced social distance, and hence, the marginal effect of the intervention would be lower. Our data do not allow us to distinguish between the two mechanisms, nor do they allow us to say whether any effect is driven by the employer or the DW. Nevertheless, the analysis is still illustrative.

In line with the hypothesis, we find that the positive result of the intervention is driven exclusively by the DWs who had not communicated with their employer prior to their departure (fourth row). Our results are similar when focusing on the individual components of the mistreatment index (table 3), although we see considerable heterogeneity between individual coefficients due to the small sample sizes. Effect heterogeneity by communication with the employer before departure cannot explain why we find generally different treatment effects for DWs in Hong Kong and Saudi Arabia. A bit more than half of DWs in Hong Kong talked to their employer before departure, while only 13% of those in Saudi Arabia did so. Nevertheless, the intervention affects the aggregate index for DWs in Hong Kong but not in Saudi Arabia. Unfortunately, the small sample size prevents us from meaningfully analyzing effect heterogeneity within destination countries.

B. Effects of the Intervention on Household Members in the Philippines

We now 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 E.7 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 1 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).

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

In table 4, 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 (col. 1).[19] 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 (cols. 2–8). Overall, the intervention leads to a more positive assessment of the effect of migration on a household's financial security (col. 2), standard of living (col. 3), education (col. 6), family life (col. 7), and social life (col. 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 E.8). The bottom two rows of table 4 document that, as for the DW-level results above, the effect is concentrated among households with DWs who had not communicated with the employer prior to their departure.

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 prespecified) analyses. Table 5 presents regression results. Assignment to the intervention increases annual education expenditures of households with a DW in Saudi Arabia by USD 73 (col. 1; the control group mean is USD 282) or 43% when we use a log specification (col. 2). The substantial effect on education expenditures also translates into higher enrollment (cols. 3–5). The enrollment effect is concentrated among older children, aged 13–20, who largely attend secondary school (col. 5). Assignment to the intervention increases the likelihood of enrollment in this age group by 7.2 percentage points (or 9% 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 (col. 4). Consistent with higher secondary school enrollment, we also find that older children are less likely to be employed (col. 6). The results for households with DWs in Saudi Arabia may reflect an increase in expected returns to education as DWs exhibit

[19] Column 1 of table 4 shows the effect on the mean of the subjective assessment of the effect of migration by the household respondent (across cols. 2–8). These variables are coded as 1 = worse, 2 = same, and 3 = better.

TABLE 4
EFFECTS OF INTERVENTION ON HOUSEHOLDS' SUBJECTIVE ASSESSMENT OF MIGRATION

	Household: Overall Effect (1)	Household: Effect on Financial Security (2)	Household: Effect on Standard of Living (3)	Household: Effect on Housing (4)	Household: Effect on Health (5)	Household: Effect on Education (6)	Household: Effect on Family Life (7)	Household: Effect on Social Life (8)
Full:								
Intervention	.056							
	[.01, .10]							
Worse		−.009	−.009	−.004	−.001	−.008	−.011	−.015
		[−.02, .00]	[−.02, −.00]	[−.01, .00]	[−.01, .01]	[−.01, −.00]	[−.02, −.00]	[−.03, −.00]
Same		−.044	−.056	−.030	−.008	−.060	−.051	−.029
		[−.09, −.00]	[−.10, −.01]	[−.08, .02]	[−.05, .04]	[−.11, −.01]	[−.10, −.01]	[−.06, −.00]
Better		.053	.065	.034	.009	.068	.062	.044
		[−.00, .11]	[.01, .12]	[−.02, .09]	[−.04, .06]	[.01, .12]	[.01, .12]	[.00, .08]
Control mean	2.40							
Observations	1,173	1,167	1,169	1,169	1,170	1,148	1,169	1,100
Hong Kong:								
Intervention	.054							
	[−.01, .12]							
Worse		−.005	−.012	−.005	.001	−.005	−.009	−.019
		[−.02, .01]	[−.02, .00]	[−.01, .00]	[−.01, .01]	[−.01, .00]	[−.02, .00]	[−.04, .00]
Same		−.020	−.080	−.043	.004	−.044	−.049	−.044
		[−.09, .05]	[−.15, −.01]	[−.12, .03]	[−.06, .07]	[−.12, .03]	[−.12, .02]	[−.09, −.00]
Better		.025	.092	.048	−.005	.049	.058	.063
		[−.06, .11]	[.01, .17]	[−.04, .13]	[−.09, .08]	[−.04, .14]	[−.03, .14]	[.00, .13]
Control mean	2.40							
Observations	495	492	494	494	493	481	492	471
Saudi Arabia:								
Intervention	.056							
	[−.00, .12]							
Worse		−.012	−.005	−.003	−.002	−.010	−.012	−.010
		[−.02, −.00]	[−.02, .00]	[−.01, .01]	[−.01, .01]	[−.02,−.00]	[−.03, .00]	[−.03, .01]
Same		−.065	−.035	−.024	−.016	−.069	−.049	−.019
		[−.12, −.01]	[−.10, .03]	[−.09, .04]	[−.08, .05]	[−.13, −.01]	[−.10, .01]	[−.05, .01]

Better		.077	.041	.028	.018	.079	.060	.029
		[.01, .15]	[−.03, .11]	[−.04, .10]	[−.05, .09]	[.01, .15]	[−.01, .13]	[−.02, .08]
Control mean	2.40							
Observations	678	675	675	675	677	667	677	629
Did not talk to employer:								
Intervention	.076							
	[.02, .13]							
Worse		−.015	−.009	−.003	−.005	−.011	−.013	−.016
		[−.03, −.00]	[−.02, .00]	[−.01, .00]	[−.01, .00]	[−.02, −.00]	[−.03, −.00]	[−.03, .00]
Same		−.079	−.061	−.030	−.032	−.080	−.056	−.029
		[−.13, −.03]	[−.12, −.01]	[−.09, .03]	[−.09, .02]	[−.14, −.02]	[−.11, −.00]	[−.06, .00]
Better		.094	.070	.033	.037	.092	.069	.045
		[.03, .16]	[.01, .13]	[−.03, .10]	[−.03, .10]	[.03, .16]	[.00, .13]	[−.00, .09]
Control mean	2.40							
Observations	817	813	813	814	815	804	814	762
Talked to employer:								
Intervention	.015							
	[−.07, .10]							
Worse		.006	−.008	−.007	.008	−.001	−.007	−.012
		[−.01, .03]	[−.02, .01]	[−.02, .01]	[−.01, .02]	[−.01, .01]	[−.02, .01]	[−.04, .01]
Same		.023	−.046	−.039	.046	−.012	−.037	−.028
		[−.06, .10]	[−.13, .03]	[−.12, .04]	[−.04, .13]	[−.10, .08]	[−.12, .04]	[−.08, .02]
Better		−.029	.054	.046	−.054	.014	.045	.040
		[−.13, .07]	[−.04, .15]	[−.05, .14]	[−.15, .04]	[−.09, .11]	[−.05, .14]	[−.03, .11]
Control mean	2.41							
Observations	356	354	356	355	355	344	355	338

NOTE.—The table reports OLS estimates in col. 1 and marginal effects at the mean from an ordered probit in cols. 2–8. The column heads give the dependent variables. In col. 1, the dependent variable is the mean of the subjective assessment of the household of the effect of migration on different aspects (see cols. 2–8). These variables are coded as 1 = worse, 2 = same, and 3 = better. In cols. 2–8, the dependent variable is the response to the question "Do you think that through the employment of [name of DW] in [Hong Kong/Saudi Arabia] the situation of your household has gotten better, stayed the same, or gotten worse with regard to [aspect stated in cols. 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. Square brackets represent 95% confidence intervals, based on robust standard errors.

TABLE 5
EFFECT OF INTERVENTION ON HOUSEHOLD EDUCATION EXPENDITURES, ENROLLMENT, AND EMPLOYMENT

	Education Expenditure		Enrolled			Employed
	USD (1)	Log (+1) (2)	All (3)	Age 3–12 (4)	Age 13–20 (5)	(Age 16–25) (6)
Full:						
Intervention	44.478	.261	.040	.025	.068	−.079
	[−9.08, 98.04]	[.00, .52]	[.01, .07]	[−.00, .05]	[.00, .13]	[−.16, .00]
Control mean	309.73	4.15	.90	.96	.80	.18
Observations	1,199	1,212	1,336	851	485	310
Hong Kong:						
Intervention	−2.454	−.005	.023	.013	.029	−.013
	[−95.07, 90.17]	[−.46, .45]	[−.02, .06]	[−.01, .04]	[−.07, .13]	[−.15, .12]
Control mean	346.84	3.93	.92	.98	.80	.11
Observations	496	502	483	321	162	94
Saudi Arabia:						
Intervention	72.886	.430	.044	.030	.072	−.119
	[7.69, 138.08]	[.12, .74]	[.00, .08]	[−.01, .07]	[−.01, .15]	[−.23,−.01]
Control mean	281.64	4.31	.89	.95	.79	.22
Observations	703	710	853	530	323	216
Did not talk to employer:						
Intervention	79.455	.454	.046	.039	.061	−.070
	[16.60, 142.31]	[.15, .76]	[.01, .08]	[.00, .08]	[−.02, .14]	[−.17, .03]
Control mean	281.22	4.06	.89	.94	.81	.19
Observations	837	846	944	599	345	220
Talked to employer:						
Intervention	−35.159	−.211	.028	−.005	.071	−.072
	[−137.89, 67.58]	[−.70, .28]	[−.02, .08]	[−.03, .01]	[−.05, .20]	[−.20, .06]
Control mean	376.66	4.35	.92	.99	.77	.17
Observations	362	366	392	252	140	90

NOTE.—The table reports OLS estimates. The column heads give the dependent variables. 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 col. 1 (we drop outliers exceeding the 99th percentile of educational expenditures), logged education expenditures in col. 2 (outliers not dropped), an indicator of whether a given child in the household is currently enrolled in an educational institution in cols. 3–5, and an indicator of whether a young household member is currently employed. Observations in cols. 3–5 correspond to children aged 3–20 living in the household at baseline. Observations in col. 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 cols. 1 and 2, age, sex, and enrollment status of the child at baseline in cols. 3–6, and employment status at baseline in col. 6. Square brackets represent 95% confidence intervals, based on standard errors clustered at the level of DWs.

an increased desire for their children to become a DW when they grow up (table 2, col. 4). 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 E.9 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 (col. 9 of table 3), the intervention increases the amount of remittances received by households with a DW in Saudi Arabia by 21% (col. 1). However, this result does not hold when we look at the amount of remittances sent as reported by the migrant (col. 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 (cols. 3 and 4) and the variance of remittances (col. 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 (col. 3 of table 2).

C. 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 toward the domestic worker, and which underlying mechanism is driving that change. 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. A receiver either shows a photo of her family, sends a small gift accompanied by a personalized well-wishing message, does both, or does nothing. The online experiment measures the immediate behavioral responses of dictators, while shutting down any endogenous reactions from the receivers.

1. Dictator Game

We collected data from 1,662 participants in the dictator role from Saudi Arabia, United Arab Emirates, and Hong Kong.[20] The sample is

[20] The 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

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 E.10 provides summary statistics by country.[21]

Dictators' choices affect the payoffs of real receivers. We recruited 10 female participants in the receiver role in the Philippines. The participants all agreed to provide information that would allow us to construct the treatment manipulations. 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.

We use a dictator game with a restricted choice set in which 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 can be selected using a slider. The receivers are passive in the game. The skewed bargaining power closely resembles the relationship between employers and DWs.

We told dictators that 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 including the full set of instructions in appendix C.

2. Interventions

Before dictators made their decisions, we exogenously manipulated which additional information was provided to them by the receivers.

- *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 an instant-messaging-application-like environment.[22]

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neighboring Saudi Arabia is an oil-producing country and a major destination for Filipino DWs. Our final sample consists of 407 individuals from Saudi Arabia, 255 from the United Arab Emirates, and 1,000 from Hong Kong.

[21] We focus on a sample of individuals that is different from the sample of employers in the main field experiment for ethical reasons (see app. sec. D.2 for a detailed discussion of ethical considerations). 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.

[22] A standardized photo of a family with the wife, husband, and their child or children was taken inside their home. The background was chosen to be neutral, and we made sure

This intervention is aimed at reducing the social distance between the dictator and the receiver.

- *Gift.*—On top of the no-info treatment information, receivers send a small hand-picked online gift, a photo of a Philippine landscape accompanied by a personal well-wishing note written by the receivers themselves, to the dictator. This intervention is aimed at generating feelings of reciprocity on the part of dictators toward 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 nonmonetary gift (Falk 2007; Kube, Maréchal, and Puppe 2012): (i) It can be perceived as an intentional act of the receiver's kindness, (ii) it comes at a—small and nonmonetary but positive effort and time—cost to the receiver, and (iii) it is easy to process by the dictator. We also made sure that the gift does not signal the economic status of the receiver.[23]
- *Photo and gift.*—Both the photo and gift treatments were provided.

3. Empirical Analysis

Our analysis follows a preanalysis plan. We use 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:

$$\text{Transfer}_i = \beta_0 + \beta_1 \text{Photo}_i + \beta_2 \text{Gift}_i + \beta_3 \text{Photo\&Gift}_i + X_i'\theta + \varepsilon_i, \qquad (2)$$

where $\text{Transfer}_i \in [0, 10]$ is the transfer of dictator i in US dollars. The coefficients of interest are β_1, β_2, and β_3, which represent the photo, gift, and photo and gift treatment effects, respectively. The excluded category is the no-info treatment. X_i' is a vector of control variables.[24] We report 95% confidence intervals based on Huber-White robust standard errors.

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the photos did not signal the economic status of the 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."

[23] Previous studies highlight the effectiveness of small nonmonetary gifts. Falk (2007) shows that solicitation letters to potential donors were substantially more effective when they were accompanied by postcards showing paintings drawn by children. Similarly, Kube, Maréchal, and Puppe (2012) show that monetary gifts from employers to workers led to higher increases in worker productivity when the money was decoratively folded in the form of origami (thus signaling that the employer had invested more effort and time).

[24] We include a set of individual characteristics including dummies for dictator's age range (nine dummies), education level (five dummies), employment status (six dummies), marital status (three dummies), number of children (five dummies), and a variable for

4. Results

Table 6 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% 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. 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 insignificant.[25] 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.

The ineffectiveness of the combined photo and gift treatment may stem from the fact that attention to the double message was attenuated. Relative to the control group (who spent 55 seconds on the page on average), participants took considerably more time to inspect the information under the photo treatment (83 seconds), the same amount of time under the gift treatment (54 seconds), but less time under the combined treatment (40 seconds).

A potential concern is that the effect of the photo treatment on increased altruism toward Filipina recipients is driven by factors other than reduced social distance. The treatments may have increased the salience of the matched recipient. This may have resulted in treatment-specific beliefs about whether the recipient is a real person: 86% of participants reported that they believed that the matched receiver was a real person, while the remaining 14% believed otherwise. This share is not treatment specific. Our results hold when restricting the sample to those who believe the matched receiver was real. 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. Finally, our results are strongest when restricting the sample to the nonegalitarian default allocation (table E.11). This suggests that the photo treatment is particularly effective in relationships with high initial

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income converted to 2018 purchasing power parity (PPP) US dollars. We also add a dummy for the default allocation being (10, 10). We randomly manipulate at the individual level whether the slider is initially set at an egalitarian allocation of (10, 10) or at an unequal allocation of (20, 0). Finally, we include country dummies, a set of dummy variables for an assigned matched receiver, and characteristics of the hardware respondents used.

[25] Restricting the sample to Saudi Arabia respondents only, the point estimate for the photo treatment is 1.15 ($p = .099$).

TABLE 6
Results from Online Dictator Game

	Full (1)	Hong Kong (2)	Saudi Arabia and United Arab Emirates (3)
Photo treatment	.831	.787	.977
	[.21, 1.45]	[−.01, 1.58]	[−.05, 2.00]
Gift treatment	.460	.408	.746
	[−.10, 1.02]	[−.31, 1.12]	[−.19, 1.68]
Photo + gift treatment	.382	.420	.389
	[−.23, .99]	[−.32, 1.17]	[−.69, 1.47]
F-test (*p*-values):			
Photo = gift	.33	.44	.71
Photo + gift = photo	.26	.46	.39
Photo + gift = gift	.84	.98	.58
Control mean	6.07	6.32	5.68
Observations	1,662	1,000	662

Note.—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 (nine dummies), educational attainment (five dummies), employment status (six dummies), monthly household income converted from local currencies to 2018 PPP USD, marital status (three dummies), and number of children (five dummies). We also add a dummy for the default allocation being (10, 10). We randomly manipulate on an individual level whether the slider initiates at either of the two extremes of the allocation: an egalitarian default allocation of (10, 10), or at an unequal allocation of (20, 0). Finally, we include binary variables for countries, matched partner indicators, and the type of device used. Square brackets represent 95% confidence intervals, based on robust standard errors. 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, Shaikh, and Xu (2019) to control for the family-wise error rate. The adjusted *p*-values are .03, .26, and .84 for the photo, gift, and photo and gift treatments.

inequalities, such as the employer-DW relationships examined in the field experiment.[26]

The gift in the online experiment may not perfectly mimic a physical gift, and the personal well-wishing note accompanying the photo in the online experiment may also have contributed to reducing social distance. Still, the null results for the photo and the combined photo and gift treatments suggest that reciprocity, if at all, may be sensitive to the specific details of behavioral interventions inspired by our field experiment. Overall, the main takeaway from the online experiment is that the family photo is effective, suggesting that a reduction in social distance is an important driver of the main result.

V. Conclusion

We study the impacts of a simple behavioral intervention aimed at reducing mistreatment of international migrant workers by their employers. Our

[26] We discuss the results for the nonegalitarian default in more detail in app. sec. C.3.

randomized control trial evaluates an intervention encouraging Filipino domestic workers (DWs) to show a picture of their family while giving their employer a small gift at the start of their employment in private households in Hong Kong and Saudi Arabia. Treated DWs reported less mistreatment by their employers and higher satisfaction with the employment relationship. They were also more likely to have remained with the same employer after 2 years. Their family members back in the Philippines also viewed international labor migration more positively. Although the treatment effects may seem modest in magnitude, they are meaningful considering the virtually cost-free nature of the intervention. Evidence from an additional online experiment with potential employers in Hong Kong and the Middle East shows that the effect is likely driven by the photo alone. This result suggests that the effect operates through a reduction in employers' perceived social distance from their DW employees.

Compared to other approaches to improving the working conditions of migrants, 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 by origin-country governments or NGOs, it can be implemented unilaterally by migrants themselves because of its simplicity and negligible cost. In principle, interventions could also target employers directly. For example, increasing the salience of the details of DW rights and employer responsibilities as specified in work contracts could be an alternative strategy. In a different context, Bertrand and Crépon (2021) document imperfect knowledge of labor laws by small and medium-sized firms in South Africa. Whether this approach is promising in the context of international migrant workers, to whom destination-country governments and, hence, employers are arguably less accountable, remains to be tested. However, it is important to stress that any individual-level intervention should not substitute for other system-wide policies aiming to reduce mistreatment of migrant workers, in particular, legal protections for workers in destination countries and their enforcement.

One might wonder why DWs have not used the intervention without active encouragement. After all, the observed effects on reduced mistreatment are meaningful, and the intervention is virtually cost-free. However, individuals and firms are consistently off the production frontier in different domains. They do not focus on all components of complex production functions (Hanna, Mullainathan, and Schwartzstein 2014), and social learning in settings with multidimensional production functions is difficult (Conley and Udry 2010). Increasing awareness of the effects of the intervention during predeparture training or through information campaigns is thus an important prerequisite for widespread take-up. Moreover, we show in an exploratory analysis that the intervention is effective for individuals who had not communicated with their employer prior to their departure. Individuals with potentially weaker interpersonal skills

may find the encouragement provided by our intervention particularly beneficial.

Do our findings generalize to other settings? List (2020) proposes a useful set of SANS conditions (selection, attrition, naturalness, and scaling) to evaluate this question. Regarding selection, the field experiment draws a random sample from the population of Filipino women departing to work as DWs in two of the most important destinations (the Philippines itself is one of the largest migrant-sending countries). Our setting should, therefore, be relevant for many migrant domestic workers. Regarding attrition, we find no evidence that the intervention led to any systematic attrition. Our estimation sample should thus remain representative of the overall population. Regarding the naturalness of the task, setting, and time frame, the intervention is implemented naturally during the predeparture training organized by a government agency. Helping departing DWs to interact with the host family fits the nature of the training, and presenting a gift upon arrival is a Filipino tradition. In addition, the intervention is suitable for scaling because of its negligible implementation cost and no evidence of potential harm. The effect heterogeneity across the two settings suggests that the effects are context specific. Nevertheless, the cost-benefit analysis speaks in favor of implementing the intervention broadly, even if it proves ineffective in some settings and outcome domains.

Data Availability

Data and code replicating the tables and figures in this article can be found in Barsbai et al. (2024a) in the Harvard Dataverse, https://doi.org/10.7910/DVN/STFGQB.

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