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Evaluating the Impacts of Access to a Digital Jobs Platform and Skills Training on Female Domestic Workers

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Corresponding Author:	S ANUKRITI World Bank UNITED STATES
First Author:	S ANUKRITI
Order of Authors:	S ANUKRITI
	Maurizio Bussolo
	Jean Lee
	Siddharth Sharma
	Anaise Williams
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Evaluating the Impacts of Access to a Digital Jobs Platform and Skills Training on Female Domestic Workers*

S Anukriti
The World Bank[†]

Maurizio Bussolo
The World Bank[‡]

Jean Lee
The World Bank[§]

Siddharth Sharma
The World Bank[¶]

Anaise Williams
Johns Hopkins University[‖]

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Abstract

Many low-wage labor markets tend to be characterized by search frictions which give rise to employer monopsony power. We study the market for domestic work in Bangladesh, where the majority of workers are low-skilled women and employers wield significant market power. We implement a clustered randomized experiment in which women are given access to a digital app which provides new job opportunities, or skills training, or both. We test whether these interventions can improve worker compensation and work conditions. New job offers through the app may increase workers' wages by reducing search frictions. Skills training could also increase wages by improving productivity, although its impact may be muted when the returns from higher productivity are captured by monopsony employers. Finally, we anticipate workers to benefit the most when their skills improve along with a decline in employers' market power due to reductions in search frictions.

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JEL codes: J24, J31, J42, J46

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[†]Development Research Group and IZA. sanukriti@worldbank.org.

[‡]Office of the Chief Economist in the South Asia Region. mbussolo@worldbank.org.

[§]Office of the Chief Economist in the South Asia Region. jlee20@worldbank.org.

[¶]Office of the Chief Economist in the South Asia Region. ssharma1@worldbank.org.

[‖]Bloomberg School of Public Health. awill137@jhmi.edu.

1 Introduction

The informal sector accounts for 61 percent of all employment globally and 90 percent of total employment in developing countries (Bonnet et al., 2019). We focus on a specific group of informal workers, namely female domestic workers. According to ILO estimates, there are 75.6 million domestic workers aged 15 years and above globally, with women accounting for the vast majority (80 percent) (Hobden and Bonnet, 2021). Compared to most other wage workers, domestic workers tend to have lower wages, fewer benefits, and less legal or social protections. In many countries, they are completely excluded from labor law and social security protection.

Informal workers, especially female domestic workers, are usually low-skilled, and tend to operate in labor markets where employers wield significant market power and workers earn less than their marginal product (Chau et al., 2022). Although there may be multiple sources of monopsony in such settings, we focus on employer market power resulting from job search frictions. Specifically, we study the market for domestic work in Bangladesh and examine whether the labor market outcomes of female domestic workers can be improved through two interventions that seek to a) decrease search frictions and b) upskill workers.

We conduct a clustered randomized controlled trial with 238 domestic worker groups, each comprised of roughly 20 women who reside in Dhaka and who are either currently engaged in or interested in domestic work. Our first intervention enables women to access a digital jobs platform that allows households looking to hire a domestic worker to match with a female domestic worker searching for a job via a phone-based app in a manner similar to Handy or Uber. Our second intervention offers women participation in a skills training program developed by our partner NGO that seeks to improve life and occupational skills of women interested in paid domestic work. We have a 2x2 design and thus four arms in our experiment: a control arm, an arm that receives only the app intervention, an arm that receives only the training intervention, and an arm that receives both.

Skills training programs are one of the most commonly used policy interventions to improve worker outcomes in a wide variety of labor markets. The rationale behind these programs is that they correct for market failures that result in inefficient under-investment in workers' skill accumulation (Becker, 1962, 2009). When labor markets are imperfectly competitive, workers under-invest in their human capital because they cannot reap the full returns of their investment.[1] Although employers have an incentive to train workers in such markets, the level of investment undertaken by them in workers' *general* skills is also likely to be sub-optimal (Acemoglu, 1997).[2] Public provision of skills training, either directly or through subsidies to employers, is one way to potentially correct this market failure. In a similar manner, our skills

[1]There may be other reasons for under-investment in human capital, e.g., credit constraints and imperfect information about returns to skills that prevent workers from financing their own training.

[2]For example, due to the possibility of employee turnover and poaching and coordination problems between workers and firms.

training intervention, offered at no cost to the workers or their employers, seeks to increase workers' earnings by improving their productivity.

However, if employers wield substantial monopsony power, they may capture a disproportionate share of the productivity gains from skills training (Naidu and Sojourner, 2020). More generally, any intervention that ignores the market structure may be ineffective in improving workers' outcomes. The domestic work market is typically characterized by an imbalance in economic power between employers and workers. Our baseline data suggests that this is also likely to be true for our study area. At baseline, almost 90 percent of workers in our sample stated that their employers have more say in their compensation and 76 percent reported that their employers have more say in their work conditions than the workers themselves. The literature on monopsony suggests that employers in the domestic work market potentially derive market power due to the severe search frictions faced by workers, the lack of labor unions, and limited opportunities for credible skill assessment and certification. In fact, our baseline data reveals that 90 percent of the surveyed workers face difficulties in finding their desired amount of work and 88 percent find it difficult to get new information about work opportunities. At baseline, 71 percent of workers reported that it would be difficult for them to replace their current job if they left or were let go by their employer. Moreover, only eight percent of surveyed women are members of a women's organization, and merely two percent have a skill certification.

Therefore, in addition to skills training, we evaluate the impact of another intervention, i.e., the digital jobs platform, that has the potential to reduce employer monopsony power resulting from job search frictions. By enabling workers to receive offers for jobs in Dhaka on their personal phones at no monetary cost, we expect the jobs platform to increase the arrival rate of job offers relative to the baseline scenario where referrals are the predominant source of information about job vacancies. Consequently, we expect that access to the jobs platform should increase wages and bring them closer to a worker's marginal product.

To evaluate the joint and separate effects of the two interventions on earnings and employment conditions of female domestic workers, we will utilize the exogenous variation in treatment status generated by randomization. After conducting the baseline survey between August and November 2022, we randomly assigned the 238 groups of workers to either a control arm or one of three treatment arms. The first treatment arm is offered assistance with onboarding to the digital jobs platform; the second treatment arm is offered skills training; and the third treatment arm is offered both interventions. We will estimate intent-to treat treatment effects using an ANCOVA specification.

Our primary outcomes of interest are the respondent's earnings and non-monetary compensation received from domestic work, time spent doing paid domestic work, wage rate for domestic work, and a measure of workplace conditions experienced by the respondent, all measured at endline. We expect both interventions to result in an improvement in these dimensions relative to the control group. Moreover, we anticipate the joint effect of the two interventions to be larger than their individual effect. As mentioned above, the effect of the skills training program may be attenuated by the degree of employer market power at baseline—although it is possible that the group-based skills training may also reduce employer market power by easing the job search process for workers by broadening their social networks and by providing skills certification. We will also explore whether a reduction in employer monopsony power, via a decline in search frictions on the labor market, is potentially an underlying mechanism through which our interventions influence worker outcomes. In addition, we will examine alternate channels for any potential treatment effects that we may observe. For instance, we will attempt to examine whether our interventions, specifically the skills training program, improve workers' productivity.[3]

Our study makes a substantial contribution to three strands of the economics literature. First, our paper contributes to the growing literature on the impacts of digital labor market platforms on workers. Research on this topic has contrasted the potential benefits of flexibility and enhanced labor market participation of certain groups (e.g., women, students, youth) offered by such platforms against the reduced or absent social protection of "gig" workers and a general erosion of the bargaining power of labor (Collier et al., 2017). In recent years, many studies have examined the conditions of work on digital ride-hailing platforms, in particular the efficiency and flexibility entailed in these jobs relative to conventional taxi driving (Chen et al. (2019); Hall and Krueger (2018); Alexander et al. (2022); Cramer and Krueger (2016); Cook et al. (2020)) and their potential to displace traditional jobs (Berger et al., 2018). Another strand of this literature examines labor market information frictions (Pallais (2014); Agrawal et al. (2016); Stanton and Thomas (2015)) and monopsony power (Dube et al., 2020) on online task platforms. These studies are largely based in high-income countries, where "gig" platforms are seen as a force that is disrupting organized labor markets. However, there is little evidence on how digital labor platforms impact low-skilled workers in settings where the status quo is already informal and unorganized.

Second, we contribute to the literature on employer monopsony power. A large number of studies have shown that employer market power is an important feature of a wide variety of labor markets in both developed and developing countries (see Chau et al. (2022) and Naidu and Sojourner (2020) for a review of the evidence on employer power in labor markets in developing and developed countries, respectively).[4] Although our paper does not seek to directly estimate the elasticity of labor supply, if we find that our intervention to reduce search frictions leads to improvements in worker outcomes, our results will be consistent with the presence of and demonstrate the importance of monopsony power in the domestic work market. Such a finding

[3] In Section 2.4, we provide further details about the measurement of these variables.

[4] Also see, for example, Card and Krueger (1994); Dal Bó et al. (2013); Naidu et al. (2016); Sokolova and Sorensen (2021); Caldwell and Harmon (2019); Dube et al. (2018, 2019); Abel et al. (2019); Kroft et al. (2020); Amodio and de Roux (2023); Ashenfelter et al. (2022); Manning (2021); Brooks et al. (2021); Kondo et al. (2021); Bachmann et al. (2022); Bassier et al. (2022).

would also be consistent with Dinkelman and Ranchhod (2012) whose findings are indicative of monopsony power in the domestic work market in South Africa.[5] Moreover, we propose to test whether market-based interventions (such as our digital jobs platform) can counter employer market power and improve worker well-being in contexts where state enforcement capacity is weak and hence the potential for state-led corrective policies is somewhat limited.

Third, we contribute to the large literature on the impacts of skills training interventions that seek to improve workers' general, i.e., non-firm-specific, skills. Broadly speaking, these programs have not been very effective in improving earnings and employment outcomes, and even when they have positive impacts, they are rarely, if ever, cost-effective, given the high costs of delivering skills interventions (Piper, 2018).[6] Moreover, given the high opportunity costs of participating in these training programs, most studies have found negative impacts on at least one outcome of interest in the short run (J-PAL, 2023).[7] Naidu and Sojourner (2020) argue that training programs fail to deliver better outcomes for workers because employers with substantial market power may capture a disproportionate share of the productivity gains from skills training for workers. To our knowledge, no previous study has analyzed the interaction between skills training interventions and potential reductions in employer market power in improving labor market outcomes and well-being of workers, especially through a randomized controlled trial.

Lastly, a unique aspect of our study is its focus on the market for domestic work, which remains under-studied by economists despite its growing prevalence across countries. Dinkelman and Ranchhod (2012), Hertz (2005), and Yamada (2008) are some related papers that examine the impact of minimum wage laws in South Africa on the domestic work sector.

Timeline

Baseline survey: August - November 2022

Randomization: January 2023

Intervention period: January - December 2023

Endline survey: March - May 2024

2 Research Design

In this section, we describe our study sample, the interventions that are being implemented as part of the randomized experiment, the process of randomization, the outcomes of interest, and

[5] Dinkelman and Ranchhod (2012) show that binding minimum wage increases for domestic workers in South Africa led to wage improvements with no accompanying reductions in employment, implying that workers were paid less than their marginal product before the legal reform.

[6] Although some programs have led to long-term gains, such as in Colombia (Attanasio et al., 2017), more commonly, the initially positive impacts fade in the medium to long term (see, e.g., Alzúa et al. (2016)).

[7] A related literature has demonstrated that vocational training interventions when combined with career counseling or referrals to potential employers may improve labor market outcomes of young jobseekers (Atkin et al. (2021); Elsayed et al. (2018); Acevedo et al. (2020); Schochet et al. (2008); Bandiera et al. (2023). However, our population of interest is substantially different from those examined in this literature.

our power calculations.

2.1 Context and Study Sample

In 2022, the female labor force participation rate in Bangladesh was about 43 percent.[8] Although domestic work is one of the most common jobs for women in the country, and an estimated 80 percent of the 1.3 million domestic workers in Bangladesh are female (Oxfam, 2020), domestic workers are not protected under the 2006 Bangladesh Labor Act. In 2015, Bangladesh implemented a Domestic Workers' Protection and Welfare Policy that lays out domestic worker rights to welfare benefits, including sick pay and paid maternity leave, right to a decent wage, right to leisure time, provisions addressing violence at the workplace, and enforcement of no child labor (Oxfam, 2020). The policy was a key milestone towards registration and legal assistance for domestic workers. However, enforcement of this policy remains poor (Murshed et al., 2021).

In this context, we partnered with an NGO that seeks to improve the well-being of female domestic workers in Bangladesh through skills training and policy advocacy to recognize domestic work as a legal profession. The sample for this study was recruited by our partner NGO as part of their larger project. The only eligibility criteria for recruitment was that the participating woman had to be at least 18 years old at the time of enrollment. The project gives priority to domestic workers who have at least six months of work experience as a domestic worker; however, in cases where a non-domestic worker is eager to become a domestic worker, she is permitted to enroll.

The recruitment process followed by the NGO is as follows. The NGO collaborates with employees of other local organizations—termed as "social mobilizers"—to identify women residing in their communities who are currently participating in or interested in domestic work. After identification, social mobilizers visit these women at their homes and brief them about the NGO project. If a domestic worker shows interest, she fills out a registration form. Thereafter, once 20 domestic workers have registered within a geographic block or an adjacent block, a training group is formed through a group meeting. The group members elect a group leader who serves as the primary point of contact for the social mobilizers.

The NGO provided us with the contact information of 4,441 women who had already been assigned to groups with a designated group leader but had not received any training yet. We contracted with a Dhaka-based survey firm to conduct a baseline survey in the local language (Bangla) with each woman in the list provided to us by the NGO.[9] We were able to complete baseline surveys with 4,391 women divided into 238 groups. Note that our sample only constitutes a small portion of the female domestic workforce in this market.

[8]Source: Provisional report of the 2022 Quarterly Labor Force Survey, Bangladesh Bureau of Statistics.

[9]The baseline survey instrument included questions about individual and household demographics, work history, income, job search, workplace conditions and abuse, employer monopsony, negotiations, work preferences, measures of empowerment such as locus of control, agency, decision making, and economic control, mental health, and intimate partner violence.

Sample description. Women in our baseline sample are, on average, 34 years old and have low levels of educational attainment (Table A.1). Only 44 percent of the women can read and the average woman has 3.8 years of schooling. Almost the entire sample is Muslim, and 88 percent of women are married. Over 90 percent of the sample was born outside of Dhaka.[10] Most women in our sample are long-term or permanent migrants, with the average duration of residence in Dhaka being 19 years for migrants. About 75 percent own a phone for personal use and 91 percent of those who own a phone are comfortable using it. Although only 18 percent have a bank account, 78 percent are aware of mobile banking and about 42 percent have used mobile banking in the past.

At baseline, roughly three-quarters of sample women were engaged only in paid domestic work; eight percent were performing both paid domestic work and other types of paid work; and 12 percent were not working (Table A.2). The average domestic worker has worked in this occupation for 9 years. The most common task performed by domestic workers is cleaning (95 percent), followed by cooking (39 percent); childcare work is relatively uncommon (5 percent).

Income from domestic work. A National Domestic Women Workers' Union survey in Dhaka found that most domestic workers do not earn enough to cover basic needs and are not paid for frequent overtime (WIEGO, 2020). Smertnick and Bailur (2019) report that an average domestic worker earns USD 10-12 per month, with 10-12 hour working days, in 2019. In our baseline sample, the average monthly income from domestic work is Bangladeshi Taka (BDT) 4,637 (approximately USD 45) and the average wage per hour is BDT 44 (USD 0.42). The vast majority of domestic workers in our sample (89 percent) earn less than the minimum monthly wage set by the garment industry, Bangladesh's leading industry and a major employer of women.[11] However, about 67 percent of domestic workers also receive some additional non-monetary compensation for work, with the most common one being food (Table A.3). On average, a domestic worker in our sample spends a total of 30 hours per week working in about two houses (Table A.2). The average tenure of a worker per household is 30 months.

Work conditions. More than 80 percent of the women in our sample feel comfortable talking to their employers about personal and professional needs, and report having enough time to rest and eat at work (Table A.3). That said, nearly 20 percent of the women currently working as a domestic worker at the time of the baseline survey experienced some kind of poor workplace conditions in the last six months, of which the most common complaints were (in order): forced to work more hours than agreed upon; employer withheld salary; was fired unexpectedly; and forced to do extra tasks. About 15 percent of workers reported being forced to work longer hours than what was agreed upon; of these, 69 percent reported no additional compensation. Four percent of domestic workers surveyed at baseline reported being fired (or

[10] The most common reasons for moving to Dhaka (in order, not mutually exclusive) are for personal work, for spouse's work, with parents, and for marriage.

[11] The minimum wage in the garment industry was BDT 8,000 at the time of our baseline survey. It has been increased to BDT 12,500 as of December 1, 2023.

asked to leave the job unexpectedly) by an employer during the last six months. Domestic workers also typically lack employment contracts; in our sample, only 24 percent of worker-household relationships had a contract. About seven percent of women working as domestic workers also experienced workplace abuse, primarily verbal abuse, within the past six months.

Search frictions. The baseline data suggest that female domestic workers in Dhaka face difficulties in finding jobs and improving their job outcomes. Nearly 90 percent of the surveyed workers stated that it is difficult for them to find their desired amount of work and 88 percent stated that they find it difficult to get new information about work opportunities (Table A.4). In fact, 71 percent of workers reported that it would be difficult for them to replace their current job if they left or were let go by their employer. A large proportion of workers stated that their employers have more say than them in their compensation (90 percent) and work conditions (76 percent). In fact, more than half of the workers had never tried to negotiate their wages with their employers, and among the 46 percent of worker-household pairs where the worker reported ever negotiating about wages, only half of the requests were fully accepted. Among workers who had never negotiated their wage, 10 percent said that their reason for not negotiating was lack of confidence and 9 percent said that they did not negotiate due to fear. Furthermore, as we understand it, the search frictions experienced by workers are likely to be more severe than those experienced by employers for various reasons.

Adding to the evidence on job search frictions faced by domestic workers in Dhaka, the baseline data also indicate a reliance on social networks and referrals for job search. Only 22 percent of the women who were working as domestic workers at baseline reported that they found their jobs themselves; 66 percent reported that they found their jobs through friends; and 7 percent reported that they found their job through an employer (Table A.4). Referral-based job search is likely to be constrained by the size of a worker's social network: at baseline, an average worker reported speaking to only two friends for job advice or job referrals. Moreover, to the extent that workers' friendships are characterized by homophily and that workers may live in the same neighborhood as their friends, referral-based job search is bound to be relatively narrow in scope.

Wage dispersion. Consistent with the existence of monopsonistic labor market conditions, there is sizable unexplained wage dispersion among the domestic workers in our sample. Daily earnings per house worked are highly dispersed, ranging from BDT 58 at the 25th percentile to BDT 115 at the 75th percentile. Only 58 percent of the baseline variation in daily earnings per house worked can be explained by a saturated regression model that adjusts for a range of observed worker and job attributes including years of education, tenure, task type, commute time, in-kind compensation, total hours of work per daily visit, neighborhood-of-work fixed effects, and worker residence fixed effects, as shown in Table A.5. In fact, even the amount earned by the same worker in different employer households varies considerably, with a saturated regression model that includes worker fixed effects leaving 18 percent of the variation in daily earnings per house worked unexplained.

2.2 Interventions

We estimate the joint and separate effects of two treatments that are delivered to groups of female domestic workers. The first treatment facilitates **onboarding of female domestic workers to a digital jobs platform** operational in Dhaka. This platform allows households looking to hire a domestic worker to match with a female domestic worker searching for a job via a phone-based app. Domestic workers can use a feature phone or a smartphone to interact with this platform and find domestic work. The digital jobs platform was developed independently from our intervention and has been active in Dhaka for several years.

The app offers two types of jobs to domestic workers: i) instant jobs, in which an appointment is scheduled for a pre-specified amount of time for a fixed set of tasks, and ii) monthly jobs, in which the platform intermediates an agreement for domestic work between the job seeker and the employer household. Domestic workers can register on and interact with the job portal through an interactive voice response (IVR) system. At the time of registration, job seekers share with the app the designated neighborhoods (or "microareas") that they are willing to commute to for work. If an employer is interested in hiring a worker, they place a request on the portal (like a rider requests an Uber). The app uses a proprietary algorithm to match the employer with a registered domestic worker looking for work in the vicinity of the employer. The app then calls the worker on her feature phone through the IVR system. The worker is informed of the location of and compensation for the job at the time she is called and has an opportunity to accept or reject the offer, just like an Uber driver, by pressing a button on her phone. After completing the job, the worker is paid through digital phone-based banking. Note that the app does not operate like a typical job platform where the job seekers can view all available jobs and then apply to the jobs that meet their preferred criteria.

As of December 2023, the platform has approximately 100,000 registered employers, of which about 3,500 employers use the service at least once a month. As we understand it, the app is broadly advertised and is fairly well known among employers throughout middle- and upper-income neighborhoods of Dhaka. On the contrary, domestic workers in Dhaka do not appear to be as aware of or familiar with the app. In our baseline sample, 68 percent of the workers had not heard of the app. Consequently, it is not surprising that there are more employers than workers currently registered on the app. About 16,000 workers were registered on the app at baseline. In fact, the administrative data provided to us by the company suggest that many job requests from employers go unfulfilled, which is consistent with the currently low worker-employer ratio on the app and also why the app is interested in enrolling new workers on its platform. Note that although there are more employers than workers on the app, the opposite is true for Dhaka's domestic work market in general. Moreover, search and matching frictions can occur even if the ratio of workers to employers is balanced—this could happen due to, among other things, geographical concentration of employers and workers in different neighborhoods, and lack of education and lower access to digital job search methods among domestic workers. By informing workers about the app and facilitating their registration on it, we seek to overcome search and matching frictions experienced by the workers and also by the employers in this market.

The initial training to facilitate onboarding onto the jobs platform comprises of an in-person group-based orientation session where groups of domestic workers are introduced to the IVR system, followed by a practice phone call. The in-person training takes approximately three hours and is delivered by the partner NGO at four centralized locations in the neighborhoods where most of the workers reside. Workers receive BDT 350 ($\sim$ USD 3.50) as travel allowance for attending the training. The in-person training is followed by a phone-based IVR quiz to test workers' familiarity and comfort with the jobs platform. As part of the quiz, women receive calls from the IVR system during the evening time to complete five modules of questions. For each completed module, women receive BDT 50 in their personal mobile money account (i.e., a total of BDT 250 ($\sim$ USD 2.50) if they successfully complete all five modules). Workers become eligible to receive job offers through the platform after they successfully complete the IVR quiz. Moreover, all workers assigned to receive this treatment are provided an additional BDT 850 ($\sim$ USD 8.50) to support purchase of a personal mobile phone regardless of ownership at baseline.

The second intervention is a **group-based skills training program** developed by our partner NGO to improve skills of female domestic workers. The training consists of two parts: soft (life) skills training and hard (occupational) skills training. The soft skills training covers topics such as interpersonal skills, literacy and numeracy, financial literacy, issue-based life skills, workers' rights, gender equality, and gender-based violence. The soft skills training takes place in four sessions that last a total of 24 hours. Between two to three weeks after the soft skills training is completed, women receive the hard skills training with other workers in their group. The hard skills training teaches skills such as cooking, cleaning, effective communication with employers, occupational safety, and how to use certain household appliances.[12] The hard skills training is delivered through ten sessions lasting a total of 60 hours. Workers assigned to the skills training program receive BDT 5,000 ($\sim$ USD 50) as compensation for the time spent in training. The partner NGO was motivated to conduct this training due to a significant demand for worker upskilling in this market. A study conducted by Oxfam, Bangladesh found that the vast majority of live-in (84 percent) and live-out (93 percent) female domestic workers are interested in participating in skills training programs. Moreover, 98 percent of the surveyed domestic workers reported wanting to learn more about their rights. On the employer side, over 85 percent of surveyed employers expressed an interest in having their current domestic worker attend a skills training.

[12]In a study conducted by our partner NGO, employers cited workers' inability to use household appliances commonly used in middle- and upper-income households as an important skill gap that training could address.

Once the skills training and/or jobs platform onboarding is complete, the domestic worker groups convene for follow-up meetings approximately once every six weeks. The follow-up meetings are a way for the NGO to maintain contact with women, provide a refresher of training knowledge, address queries or concerns, and collect updated contact information. The NGO also remains in contact with women in the control arm through the follow-up meetings, allowing the women to stay connected while they wait for their training (which will take place once the impact evaluation is complete). Thus, women in all arms, including the control group, likely experience a boost in their social connectedness through these group meetings.

To ensure that workers' ability to participate in the digital jobs platform intervention is not hindered by the lack of a personal SIM card, we provide BDT 150 ($\sim$ USD 1.50), the approximate price of a SIM card, to all workers in our sample, regardless of whether they have a phone or a SIM card at baseline and regardless of treatment assignment. The BDT 150 cash transfer is provided at the first group meeting, which takes place at least five days before the start of the group's training. At the meeting, women are encouraged to purchase their own SIM card if they do not have one. All workers are also encouraged to and assisted with opening a mobile money account during this meeting.[13] Table 1 summarizes the payments received by women in each trial arm. Note that the endline survey will investigate the use of these funds by participants.

The inclusion of the skills training intervention was originally motivated by the need to get the cooperation and buy-in of the partner NGO for this project, who have a mandate to evaluate the training program. However, as suggested by Naidu and Sojourner (2020), including the training arm will potentially generate new evidence on how the degree of employer monopsony power alters the extent to which workers benefit from skills training programs. This is important because skills training programs have generally not been very effective in settings such as ours. A potential explanatory factor is that in the presence of significant employer monopsony, the gains from a skills training may be captured by employers, and workers would not necessarily be paid their new, higher marginal product (Naidu and Sojourner, 2020). If so, combining worker upskilling with access to the app could make the former more beneficial for the worker. As such, the skills training intervention is an integral part of the study. Since our experiment includes a training-only arm, an app-only arm, and an app plus training arm, the inclusion of the skills training intervention in our study will not confound our estimates of the effects of the app, which will be estimated by comparing the app-only arm with the control arm (more details on our estimation strategy are presented in Section 4).

[13]Our decision to provide these additional payments to the participants was based on findings from the baseline survey (which revealed that our respondents face substantial credit constraints) and a pilot of the onboarding assistance intervention that we conducted with 60 women. At the time of the pilot training, 12 women did not have a personal phone, 11 women did not have their own SIM card, and 29 women did not have their own mobile money accounts. However, after the training and the associated payments, all 12 women without a personal phone had acquired one; all 11 women without a personal SIM had acquired one; and all but one of the 29 women without mobile money accounts had opened such an account.

Table 1: Payments received by women in each trial arm

Payment in BDT for:	T1	T2	T3	C
Personal SIM card purchase	150	150	150	150
Personal phone purchase	850	-	-	-
Travel/time	350	$5,000$ *	$5,000$ *	-
IVR quiz completion	250 **	N/A	250 **	N/A

Notes: T1 refers to the arm that only receives onboarding assistance to the digital jobs platform; T2 refers to the arm that only receives skills training; T3 refers to the arm that receives both T1 and T2; C refers to the control arm. *Calculated as BDT 350 per day x 14 days of training. **This is the maximum possible amount received upon answering all questions correctly and is provided by the jobs platform. Nominal exchange rate: USD 1 $\sim$ BDT 100.

2.3 Randomization

We approached 4,441 women for the baseline survey which revealed that 44 women had either themselves used the digital jobs platform before or were in a group where most women had used the platform. In addition, six women did not consent to participate in the study after the baseline survey. We dropped these 50 women from our sample, which left us with a sample of 4,391 women across 238 groups, with an average group size of 18.4 women, for conducting randomization. We conducted randomization at the group level to contain spillovers within group since the neighborhoods where our sample women reside are densely populated and the group formation took place with women living in the same neighborhood or in close proximity.

Additionally, based on the baseline data, we stratified the randomization by three variables: 1) assignment of the group to one of the training hubs (four hubs); 2) whether the group size is above or below median; and 3) whether the share of women currently working as domestic workers in a group is above or below median. Within each of the 16 strata, we randomly allocated groups of workers into one of the following four trial arms:[14]

- Digital jobs platform onboarding assistance only (T1): 57 groups
- Skills training only (T2): 61 groups
- Digital jobs platform onboarding assistance plus skills training (T3 = T1 + T2): 59 groups
- Control (C): 61 groups

Table 2 shows that treatment assignment is quite balanced across a range of baseline characteristics of the woman including her demographic, household, and partner characteristics, labor market outcomes, agency and empowerment, health, digital skills, and measures of employer monopsony. See Appendix B for the definitions of the variables included in Table 2.

[14]Note that each woman in our sample will receive the full package of treatment (skills training plus assistance with onboarding to the digital jobs platform, or whichever intervention they had not received earlier) after the completion of our endline survey, from our partner NGO.

Table 2: Balance Table

	p-value (joint F-test)	Normalized differences[a]						Number of observations
		T1 v C	T2 v C	T3 v C	T1 v T2	T1 v T3	T2 v T3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	0.177	0.050	0.077	0.033	0.126	0.084	0.047	4,337
Currently married	0.474	0.060	0.031	0.062	0.029	0.000	0.030	4,391
Years of schooling	0.940	0.037	0.000	0.003	0.037	0.034	0.003	4,389
Husband age	0.114	0.047	0.088	0.048	0.138	0.099	0.043	3,847
Husband years of schooling	0.994	0.016	0.016	0.008	0.000	0.008	0.008	3,813
Husband works	0.430	0.000	0.063	0.063	0.063	0.063	0.000	3,842
Years in Dhaka	0.751	0.052	0.047	0.005	0.005	0.058	0.053	4,357
# Children under age 5	0.741	0.018	0.053	0.035	0.036	0.018	0.018	4,391
Household wealth index (z-score)	0.215	0.000	0.178	0.010	0.182	0.010	0.176	4,389
Household savings (log)	0.751	0.020	0.074	0.009	0.054	0.011	0.066	4,123
Total monthly income of respondent (log)	0.304	0.011	0.004	0.028	0.007	0.039	0.032	4,391
Years worked as DW	0.531	0.063	0.009	0.067	0.076	0.001	0.081	3,686
Years worked as DW consecutively	0.672	0.059	0.027	0.002	0.087	0.063	0.026	3,699
Monthly income from DW (log)	0.629	0.016	0.006	0.029	0.023	0.013	0.036	4,383
Hours worked as DW weekly	0.673	0.022	0.044	0.022	0.022	0.044	0.065	4,391
Wage rate for DW (log)	0.389	0.043	0.065	0.086	0.021	0.041	0.021	3,645
# Houses working at as DW	0.889	0.000	0.010	0.038	0.010	0.037	0.047	4,391
Non-wage compensation/amenities index (z-score)	0.259	0.089	0.049	0.010	0.147	0.103	0.041	3,653
Has a skill certification	0.068*	0.163	0.000	0.071	0.141	0.082	0.063	4,391
Worker-HH communications index (z-score)	0.493	0.090	0.095	0.020	0.000	0.067	0.070	4,361
Employer monopsony index (z-score)	0.517	0.092	0.020	0.010	0.111	0.079	0.029	4,336
Search frictions index (z-score)	0.327	0.050	0.100	0.069	0.153	0.121	0.030	4,194
Workplace conditions index (z-score)	0.052*	0.030	0.074	0.091	0.045	0.125	0.171	4,035
Workplace abuse index (z-score)	0.405	0.088	0.038	0.067	0.052	0.021	0.031	4,035
Bargaining power of DW	0.874	0.065	0.022	0.044	0.044	0.022	0.022	3,650
Physical health index (z-score)	0.593	0.030	0.040	0.089	0.010	0.060	0.050	4,385
Mental health index (z-score)	0.367	0.040	0.010	0.101	0.050	0.060	0.111	4,380
Experienced IPV in the last 6 months	0.425	0.063	0.000	0.062	0.063	0.125	0.062	4,391
Locus of control index (z-score)	0.425	0.128	0.000	0.029	0.135	0.097	0.030	4,380
Agency index (z-score)	0.207	0.159	0.088	0.094	0.058	0.062	0.000	4,222
Decision making index (z-score)	0.406	0.020	0.051	0.070	0.030	0.088	0.120	4,337
Economic control index (z-score)	0.344	0.050	0.172	0.020	0.122	0.030	0.151	3,864
Social connections index (z-score)	0.406	0.126	0.058	0.059	0.071	0.072	0.000	4,389
Has personal phone	0.974	0.023	0.023	0.045	0.000	0.023	0.023	4,391
Has bank account	0.827	0.000	0.026	0.026	0.026	0.026	0.000	4,391
Has used digital banking	0.831	0.061	0.040	0.020	0.020	0.041	0.020	4,391

Notes: $T1$ refers to the arm assigned to receive digital jobs platform onboarding assistance only; $T2$ refers to the arm assigned to receive skills training only; $T3 = T1+T2$ refers to the arm assigned to receive both interventions; C refers to the control arm. Column 1 presents the p-value from the F-test of joint orthogonality (i.e., the hypothesis that $T1 = T2 = T3 = 0$) from separate regressions of the variables of interest (leftmost column) on indicators of treatment groups and strata fixed effects, with standard errors clustered at the worker-group level. Columns 2-7 present the normalized differences between each pairwise combination from the four arms in our study, calculated as the absolute difference between the variable means of the two arms in the pair, divided by their joint standard deviation. Column 8 presents the number of observations. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$.
[a]Below 0.25 can be considered to be balanced (McKenzie, 2017).

2.4 Outcomes

Our primary outcomes of interest, as measured at endline, are: i) total earnings of the respondent from domestic work;[15] ii) an index of non-monetary compensation (e.g., food, lodging, etc.) or non-wage amenities (e.g., sick leave, weekly day off, etc.) received by the respondent from her employer(s); iii) time spent by the respondent doing paid domestic work; iv) domestic work wage rate earned by the respondent;[15] and v) an index of workplace conditions experienced by the respondent including, for example, whether the respondent was asked to leave her job unexpectedly and forced to work more than what was agreed upon.

Additionally, we will estimate treatment effects for several secondary outcomes. These include alternate measures of worker well-being, such as mental and physical health and level of empowerment (captured by decision-making power, agency, and locus of control, for instance), total earnings from any type of work, and participation in other types of work besides domestic work. To explore the mechanisms underlying the impacts on our primary outcomes of interest, we will also estimate treatment effects on several measures of employer monopsony power, search frictions in the domestic work market, respondents' skills, productivity, social connectedness, bargaining power, and experience of abuse or violence. See Appendix A for more details.

2.5 Statistical Power

We conducted power calculations for the following subset of our primary and secondary outcomes: i) monthly earnings from domestic work, ii) hours worked as domestic worker (weekly), iii) an employer monopsony index, and iv) a search frictions index. All power calculations are based on a hypothesis test with a 5 percent significance level and 80 percent power comparing two trial arms with balanced treatment assignment of 1,100 individuals within 55 groups each. Given that attrition is a concern with our study population, in Table 3 we present the minimum detectable effects (MDE) with varying degrees of individual-level attrition at endline. The estimates presented are conservative given that we will use an ANCOVA specification which controls for baseline values of the outcome variables and jointly estimate treatment effects at the individual level.

The employer monopsony index in Table 3 was constructed using the following variables: level of agreement (on a scale from 0 to 4) with the following statements i) "*If my employer reduced my hourly compensation by a quarter, I would leave the job" (reverse scale)* ii) "*The people I work for in this house have more say in my compensation than I do*;" iii) "*The people I work for in this house have more say on my working conditions and hours than I do.*" A higher value of the index indicates stronger employer monopsony power.

The search frictions index in Table 3 was constructed using the following variables: i) time spent searching for jobs over the past week in hours; level of agreement (on a scale from 0 to 4)

[15] We will utilize a log or an inverse hyperbolic sine transformation, if appropriate.

with the following statements ii) "*It is difficult to get new information about work opportunities*;" iii) "*It is difficult to find enough work to make the income I want to have*;" and iv) "*If I left this job or if this house let me go, it would be difficult for me to replace this job*."[16]

Table 3: Power Calculations

Individual-level attrition (1)	MDE (sd) (2)
A. Monthly earnings from domestic work[1]	
10% (18 per group)	0.2056 sd
20% (16 per group)	0.2099 sd
30% (14 per group)	0.2153 sd
B. Hours of domestic work per week[2]	
10% (18 per group)	0.1920 sd
20% (16 per group)	0.1967 sd
30% (14 per group)	0.2026 sd
C. Employer monopsony index (z-score)[3]	
10% (18 per group)	0.2423 sd
20% (16 per group)	0.2457 sd
30% (14 per group)	0.2501 sd
D. Search frictions index (z-score)[4]	
10% (18 per group)	0.2742 sd
20% (16 per group)	0.2770 sd
30% (14 per group)	0.2805 sd

Notes:

1. Total monthly income from domestic work during last 30 days: Mean (sd): BDT 3,910 (BDT 2,746), ICC: 0.098
2. Hours worked on domestic work in a typical week: Mean (sd): 25.2 (18.1), ICC: 0.078
3. Employer monopsony index (z-score; higher is stronger employer monopsony): Mean (sd): 0.00 (1.00), ICC: 0.159
4. Search frictions index (z-score; higher is stronger search friction): Mean (sd): 0.00 (1.00), ICC: 0.220
5. sd denotes standard deviations and MDE denotes minimum detectable effect. Based on a hypothesis test with 5 percent significance and 80 percent power comparing two trial arms with balanced treatment assignment of 1,100 individuals within 55 groups each, using baseline means and training group intra-class correlation (ICC) coefficients, at varying levels of individual-level attrition across groups.

We are powered to observe an MDE of 0.21 standard deviations (sd) for monthly earnings (about 14 percent of baseline average monthly earnings during the last 30 days), 0.19 sd for weekly hours spent on domestic work (about 14 percent of the baseline average), 0.24 sd for the

[16]Note that the employer monopsony index and the search frictions index in Table 3 are constructed using fewer variables than what we plan to include in our definitions of these indices at endline (described in Appendix A). This is because some of the survey questions mentioned in the Appendix were not included in the baseline survey instrument.

employer monopsony index (z-score), and 0.27 sd for the search frictions index (z-score) (Table 3). Given that our trial is a clustered-RCT, we did not find large differences in MDEs across individual attrition levels; we do not anticipate group-wise attrition.

Given the typical standardized MDE assumptions of 0.2 sd being a small effect and 0.5 sd being a medium effect, we anticipate being able to detect small to medium effects on our outcomes of interest (Cohen, 1988). Further, these effect sizes align with partner goals. The MDE for income in Table 3 is smaller than the effects found in the literature for workforce skills programming. For example, an evaluation of the Project QUEST workforce development program found a 20 percent increase in earnings (Naidu and Sojourner, 2020). Similarly, Per Scholas, a skills training program for underrepresented groups in the United States, was found to have an impact on earnings of 20-30 percent (Naidu and Sojourner, 2020).

3 Theory of Change

In this section, we briefly describe a conceptual framework for understanding the potential impact of our interventions on domestic worker outcomes. Our qualitative understanding of the market and the descriptive quantitative evidence from our baseline survey (presented earlier) suggest that the domestic work market in our setting is characterized by job search frictions and an imbalance in market power between employers and workers. To model this asymmetry in power, we assume that employers have monopsony power and face upward-sloping labor supply curves (Manning, 2021). This implies that employers can cut wages without immediately losing their domestic workers to competitors, unlike a perfectly competitive scenario. Although the literature has proposed multiple sources of employer monopsony power, including amenities (Sharma, 2023), our focus is on monopsony resulting from search-and-matching frictions due to the nature of the interventions being tested in this study (Burdett and Mortensen (1998); Postel-Vinay and Robin (2002)). In our framework, employers derive market power because of the fact that it takes time for workers to find jobs and that sometimes they lose their jobs.

Following models of labor market search-and-matching frictions with on the job search (Burdett and Mortensen (1998); Postel-Vinay and Robin (2002); Cahuc et al. (2006)), we conceptualize the market for domestic work as follows. Workers and employers (i.e., households) meet pairwise through a search process that is slow, random, and sequential. Job search is conducted not only by unemployed workers but also by employed workers seeking better or more jobs. Frictions in the search process are reflected in the low "arrival rates" of job offers to unemployed workers and employed workers searching for jobs. Workers differ in ability. Employers too differ in their productivity; in our setting, such differences in marginal product across employers could arise, for example, due to differences in household ability to manage domestic workers and economies of scale or scope in domestic work.

Given that it takes time for workers to generate competing job offers, employers do not need to pay workers their marginal revenue product, as they would in a perfectly competitive labor market. Employers offer unemployed workers their reservation wages because there is no immediate competitive pressure on them to offer higher wages. However, sequential on-the-job-search generates competition between pairs of employers. When an employed worker obtains a new job offer through on-the-job-search, the ensuing competition between the incumbent employer and the newly-located employer either leads to a wage increase in the current job or a job switch (Postel-Vinay and Robin, 2002).

In equilibrium, wages are below the marginal revenue product of workers. The equilibrium wage markdowns vary even among workers and employers of the same type because of random variation in job search histories, generating equilibrium wage dispersion—which we observe in our data (see Section 2.1). The extent of the wage markdown in steady-state equilibrium depends on the arrival rate of job offers relative to the rate at which workers lose their job. In other words, the relative arrival rate of job offers is a measure of the equilibrium elasticity of the labor supply to the employer: the higher the relative arrival rate, the more elastic the labor supply and the lower the wage markdown (Manning, 2021). As frictions vanish, with the arrival rate approaching infinity, the equilibrium wage approaches the competitive level and is close to the marginal revenue product of the worker (Burdett and Mortensen, 1998).

The jobs platform, by enabling workers to search for jobs across Dhaka on their personal phones at no monetary cost, has the potential to reduce monopsony power generated by search frictions.[17] In terms of the theoretical framework outlined above, this platform can be conceptualized as a technology that enables an increase in the arrival rate of job offers, causing an increase in wages and a reduction in wage dispersion in equilibrium.

Our framework also suggests that the history of job offers and job-to-job mobility matters for worker outcomes. Over time, as workers come into contact with more employers, their wages increase due to counteroffers made by incumbent employers or job mobility. To better understand this mechanism behind the jobs platform's impacts on domestic workers, we will examine the impact of the treatment on relevant secondary outcomes such as the number of job offers received and the occurrence of voluntary job switching.

Labor market competition also affects investments in workers' transferable or "general" occupational skills. In a competitive market, workers capture the full returns from investing in such skills because they are always paid their marginal product. Because workers fully capture

[17]The digital jobs platform may also affect the bargaining power of workers, which is relevant in a setting characterized by job search frictions. If workers have bargaining power, then the wage offers made to unemployed workers and incumbent workers who have located a new employer depend on the bargaining process. Hence, access to the platform may reduce the wage markdown by increasing both job search efficiency and worker bargaining power. While we will consider workers' perceptions of bargaining power as an intermediate outcome of interest, our experimental design does not aim to delineate the contributions of bargaining power and job search efficiency to the overall impact of providing workers assistance with onboarding to the digital jobs platform. The key predictions of the baseline model of search frictions do not change when a role for bargaining is introduced (Cahuc et al., 2006). Indeed, to the extent that workers derive power from their ability to find a new job more than their ability to negotiate wages with their employer, monopsony generated by search frictions may be more relevant to the domestic worker market than a bargaining model (Manning (2021); Brenčič (2012); Hall and Krueger (2012); Brenzel et al. (2014)).

the benefits of training via higher wages, they will undertake the efficient level of investment in general skills (Becker, 1962, 2009). However, in monopsonistic markets, workers may under-invest in skill accumulation because they cannot reap the full returns of their investment. In such a scenario, firms may be willing to bear some costs of providing general training to their workers if the increase in productivity induced by training is higher than the wage increase (Acemoglu (1997); Acemoglu and Pischke (1998, 1999); Autor (2001); Chang and Wang (1996); Katz and Ziderman (1990)). Nevertheless, there may still be an under-provision of training in imperfect labor markets if employee turnover is high (Acemoglu, 1997); in fact general training itself may increase turnover if training increases workers' outside option, or if training increases the probability that trained workers find a new job after losing or leaving their current job.

Our skills training intervention is expected to improve workers' productivity by building their general job-related skills, such as cooking skills, as well as life skills, such as interpersonal skills, at no cost to the workers or prospective employers.[18] The degree to which this improvement in skills translates into wage increase will depend on the degree to which increased worker productivity is captured by employers with monopsony power. Indeed, evidence from the United States suggests that the returns to training programs depend on institutional factors related to monopsony power, such as union density (Naidu and Sojourner, 2020). In the context of our interventions, this suggests that the wage gains from receiving skills training may be higher when it is combined with access to the digital jobs platform because the latter may enable trainees to capture a larger share of the productivity gains from training.

4 Empirical Strategy

Our study aims to answer three research questions. First, we will test whether enabling access to a digital jobs platform and offering the skills training program to female domestic workers improves their labor market outcomes, such as earnings, when offered jointly and separately, relative to the control arm. Second, we will evaluate whether access to the digital platform is more effective than skills training alone. Third, our trial will test whether the combined effect of both interventions is larger than the effect of each intervention alone.

The random allocation of groups of workers to treatment and control arms provides an exogenous source of variation in treatment status, allowing us to estimate causal effects of the interventions on our outcomes of interest. We will estimate treatment effects using the following ANCOVA specification for a worker i in group g, with one endline observation per worker:

$$Y_{ig} = \alpha + \beta_1 Training_{ig} + \beta_2 Portal_{ig} + \beta_3 Training_{ig} \times Portal_{ig} + \delta_s + X'_{ig} + \epsilon_{ig} \qquad (1)$$

where Y_{ig} is the outcome of interest; $Training_{ig}$ and $Portal_{ig}$ are indicator variables for

[18]For trainees who are currently employed as domestic workers, the training program can be thought of as a partial reduction in the cost of training for their employers because in many cases, trainees take leave from their employers to participate in the training.

being offered the skills training and jobs platform onboarding assistance, respectively; δ_s are fixed effects for strata (number of strata = 16); X'_{ig} is a vector of baseline controls that will include baseline measures of our outcomes (when they are available) and other controls that will be selected through a double-LASSO procedure. Standard errors will be clustered at the group level (number of groups = 238).

The coefficients β_1 and β_2 will capture the effect of the skills training program and jobs platform onboarding assistance on workers' labor market outcomes, whereas β_3 will estimate their combined effect. To test the relative effectiveness of the two interventions, we will compare β_1 and β_2. To test whether the combination of both treatments is more effective than each treatment alone, we will compare β_3 with β_1 and β_2, one at a time.

In addition to estimating intent-to-treat effects, we will estimate treatment-on-the-treated (TOT) effects to take into account potentially non-random compliance decisions of domestic workers. To do so, we will use the randomly assigned intent to treat as an instrumental variable for a worker's actual treatment status.

We anticipate that there will be attrition between baseline and endline data collection. We will check whether the level of attrition differs across treatment arms and whether "attritors" across arms differ in terms of their baseline characteristics. Should we find differential attrition across treatment arms, we will follow the literature and apply Lee bounds to correct for attrition (Kremer et al. (2009); Baird et al. (2011); Hidrobo et al. (2014); Cunha (2014); Drexler et al. (2014)). Lee bounds rely on a monotonicity assumption (assignment to treatment can only affect attrition in one direction). While we expect this to be the case, we can also use alternative methods such as those outlined in Millán and Macours (2017).

4.1 Mechanisms

To explore the mechanisms underlying treatment effects on our primary outcomes we will examine effects on relevant secondary outcomes and explore heterogeneous treatment impacts.

As discussed in Section 3, the main mechanism through which we expect the jobs platform to improve the labor market outcomes of domestic workers is a reduction in monopsony power generated by search frictions. A positive impact of the jobs platform intervention on the wage rate and number of hours of domestic work would be consistent with this mechanism. To provide supportive evidence for this channel, we will test whether the jobs platform intervention has a greater impact on workers experiencing higher baseline levels of self-reported employer monopsony power (as measured by a "monopsony index" variable which will include a hypothetical worker-reported proxy for labor supply elasticity that will measure the wage cut needed for the worker to leave the current employer).[19] In the event of a positive effect of the jobs platform on our primary outcomes, we will also examine whether the impacts on secondary outcomes related

[19] Conditional on budget, we will also explore the possibility of collecting supplementary information on the monopsony mechanism through an employer survey. For example, this survey could elicit employer perceptions of labor supply elasticity.

to job search are consistent with the predictions of a search frictions-based model of monopsony. Specifically, we will test if this intervention leads to a reduction in a “search frictions index,” which incorporates information on job search time, the number of job offers received, and the incidence of voluntary job switching.

However, any potential positive effects of the platform intervention may be driven by alternative factors besides a reduction in search frictions. Exposure to new employers on the platform may alter salary expectations of workers. Furthermore, improved job prospects on the platform may increase workers’ bargaining power with respect to their current employers. While it may be difficult to completely rule these mechanisms out in the event of a positive effect of the jobs platform on the primary outcomes, we plan to analyze them further by examining impacts on secondary outcomes related to bargaining and salary expectations.

Although our theory of change focuses on monopsony based on search frictions, the literature has proposed other explanations for monopsony that may be relevant for our empirical findings. For this reason, a null result in our study would not necessarily imply that there is no monopsony power in this market. In particular, monopsony power can also emerge when workers have idiosyncratic preferences for certain employers or the workplace amenities that they offer (Card et al. (2018); Manning (2021); Azar et al. (2022); Berger et al. (2023); Bazzi et al. (2021)). In recent work, Sharma (2023) examines the role of amenities as a driver of gender wage gaps in Brazil. Worker-firm-specific preferences or amenities may be relevant in our setting because domestic work entails heavy interpersonal interaction and has many non-pecuniary dimensions to it. It is possible that attachment to particular workplaces will attenuate the impact of the jobs platform intervention by diminishing incentives for active job search. Hence, in the event of a null effect of the platform intervention, we will examine the role of amenities as a possible explanation for the null result by collecting information on why job offers generated by the jobs platform may have been refused. Our endline survey will collect data on amenities, including coverage of clothing, lodging, child education, medical expenses, transportation, emergency loans, and employer treatment. Additionally, we will explore the possibility of collecting information on job refusals from the platform’s administrative data.

Relatedly, safety concerns, commuting costs, and spousal preferences may, in effect, tie female workers to certain employers. In this regard, we note that the app matches workers only to jobs in their preferred and specified “micro-areas” or neighborhoods of the city where they are willing to work because they are within walking distance of their homes. As previously noted, the incidence of abuse faced by domestic workers is low at baseline, and the app actually provides a better and more transparent process for reporting employers who engage in abuse than is available off the app, offering additional protections.

The existence of relational contracting could also help explain a null result on the platform, and hence we also propose to explore the predictions of relational contracting models in our setting (Levin, 2003). One prediction of such models, in which non-contractible outcomes are self-enforcing in long-term relationships through repeated interactions, is that workers may not change employers even when a superior offer arrives (see, for example, Hémous and Olsen (2022)). To test this, we will examine whether workers stay with employers that pay them less than the digital jobs platform would. Furthermore, in contrast to the monopsony model characterization of work conditions as static amenities which would not change within existing employer relationships (as in Berger et al. (2023)), in a model of relational contracting, the introduction of new opportunities through the digital jobs platform could lead to either improvement (via the improvement in outside options) or deterioration (via the reduced ability to commit to the relationship) in non-contractible job-related amenities, such as working conditions, within existing employer relationships. To examine this possibility, we will measure if there is an effect of the jobs platform intervention on workplace conditions and abuse within employment relationships, and the direction of the effect.

We hypothesize that the skills training intervention may improve earnings by compensating for the under-investment in job-relevant general skills that is expected in monopsonistic labor markets. In other words, this intervention is expected to increase the productivity of workers. A positive impact of the training intervention on the wage rate would be consistent with this mechanism.[20] Moreover, if the jobs platform intervention reduces monopsony power, workers in the combined arm may be able to capture a larger share of the training-induced productivity increase as higher wages than those in the training only arm. A positive interaction effect between the two interventions would support this hypothesis.

We will also consider other mechanisms that could explain a positive earnings impact of the skills training intervention. First, the skills training might make workers more effective at bargaining for better wages. We will explore this mechanism by examining the impact of the skills training intervention on secondary outcome variables related to bargaining. Second, being part of the skills training program might ease the job search process for workers by broadening their social networks and by providing skills certification. We will explore this possibility by examining the impact of the skills training intervention on secondary job search outcome variables, as well as on worker's number of social connections and whether they have a skill certification. We will also collect information on whether participants have acquired jobs though job information or referrals from friends, and compare its incidence across treatment arms. It is worth noting that all workers in our study sample, including those in the control arm, were already organized into groups by the NGO before our baseline survey and all groups have been participating in regular group meetings with their social organizer.

As noted in Section 2.2, following the usual practice of our partner NGO, workers assigned to receive the skills training intervention are provided BDT 5,000 to account for their opportunity

[20] Measuring the impact of the training on productivity directly, as opposed to inferring it from wages, would be useful. But measuring productivity is challenging in our setting. We plan to use a self-reported worker efficiency index, and are also exploring other ways to measure productivity. One possibility is to conduct a matched survey of employers and ask them to rate worker productivity.

cost of training time and transport expenses. This amount is of the same order of magnitude as the average earnings per month of a domestic worker (BDT 4,637). Given that the training lasts for a total of 14 days, this seems to be a reasonable compensation amount for the opportunity cost of training and is unlikely to act like an additional grant for workers who have to take unpaid leave from their jobs to attend the training. However, for women who are able to shift their work times to accommodate the training or those who were not working at baseline, the cash transfer could, in part, amount to a grant. Hence, we plan to ask our respondents whether they took unpaid leave to participate in the training and whether the training compensation supplemented their usual income or was a replacement for their usual income. In addition, we plan to ask questions on how the compensation was used.

Finally, we plan to test for heterogeneity in treatment effects in a data-driven way by leveraging machine-learning methods to elicit the relevant dimensions of heterogeneity, as in Chernozhukov et al. (2018) and others.

4.2 Multiple Hypothesis Testing

In order to account for multiple hypothesis testing, we will follow two protocols. First, individual outcome variables will be grouped into families or indices. In addition, within each table in the paper, Anderson's sharpened q-values will be calculated along with standard p-values for tests of significance.[21]

5 Administrative information

5.1 Ethics approval

This study received ethical approval from the Institute of Health Economics (IHE-IRB), University of Dhaka on July 16, 2022. IRB Approval Number: IHE/IRB/DU/26/2022/Final.

5.2 Funding

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[21] For additional information, see: `https://blogs.worldbank.org/impactevaluations/updated-overview-multiple-hypothesis-testing-commands-stata`.

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A Secondary Outcomes

I. Worker well-being

- Mental and physical health index: this index will be constructed from variables that measure generalized anxiety, perceived stress, self-perception of overall physical health, and experience of persistent pain and workplace injury for the respondent.

- Empowerment index: this index will capture the respondent's locus of control, agency, and decision-making power.

- Workplace abuse index: this index will capture whether the respondent has experienced verbal, physical, or sexual abuse at the workplace.

II. Employer monopsony power in the domestic work market

- Employer monopsony index: this index will be constructed from variables that measure the respondent's level of agreement with the following statements: i) "*If my employer reduced my hourly compensation by a [given percent reduction], I would leave the job (reverse scale)*; "[22] ii) "*The people I work for in this house have more say in my compensation than I do*;" iii) "*The people I work for in this house have more say on my working conditions and hours than I do.*" and iv) a variable that measures the wage cut needed for it to induce the worker to leave the current employer.

- Search frictions index: this index will be constructed from the following variables: i) the total time spent searching for a job; ii) the number of job offers received by the respondent (reverse scale); iii) whether the respondent has voluntarily switched out of any of the houses she was working with at baseline (reverse scale); the respondent's level of agreement with the statements: iv) "*It is difficult to get new information about work opportunities*;" v) "*It is difficult to find enough work to make the income I want to have*;" and vi) "*If I left this job or if this house let me go, it would be difficult for me to replace this job*."

- Social connections index: this index will be constructed from the following variables: i) respondents' number of friends; ii) the number of friends that the respondent speaks to for job advice or job referrals; and iii) whether the respondent is involved in any groups or associations.

III. Productivity

- Self-reported measure of efficiency or skills attainment[23]

IV. Alternate mechanisms

- Bargaining power: as a proxy for worker's bargaining power, we will measure the share of homes or employers that the worker has tried negotiating salary, benefits, or duration of work with.

V. Occupational choice

[22] We will try different 'percent reductions' in the endline survey to assess employer monopsony power.

[23] Measuring productivity is challenging in our setting and we are exploring alternate measures. One possibility is to conduct a matched survey of employers and ask them to rate worker productivity.

- Participation in other types of work besides domestic work.
- Total earnings from any type of work: the sum of monthly earnings from domestic work and non-domestic work [24]

B Definitions of baseline variables in the balance table

- **Age**: Respondent's self-reported age in years
- **Currently married**: Dummy variable that equals one if the respondent is currently married
- **Husband age**: Respondent's husband's age in years (missing for those not married)
- **Husband years of schooling**: Respondent's husband's years of schooling
- **Years in Dhaka**: Number of years the respondent has been living in or since she last moved to Dhaka
- **# Children under age 5**: Number of children in the respondent's household under age five
- **Household wealth index (z-score)**: Normalized sum of: i) the wall material of respondent's house; ii) the roof material of respondent's house; iii) the floor material of respondent's house; iv) whether the respondent's house has electricity; v) the type of toilet in respondent's house; vi) the type of water connection in respondent's house ; vii) whether any member of the respondent's household owns the house they live in or another house or both; viii) whether respondent's household owns a TV; and ix) whether respondent's household owns a fridge
- **Household savings (log)**: Log transformation of the total savings of the respondent's household to date
- **Total monthly income of respondent (log)**: Log transformation of the total income of the respondent (from both domestic and non-domestic work)
- **Years worked as DW**: Number of years since the respondent first started working as a domestic worker
- **Years worked as DW consecutively**: Number of years since the respondent has been working as a domestic worker consecutively
- **Monthly income from DW (log)**: Log transformation of the monthly earnings of the respondent from domestic work
- **Hours worked as DW weekly**: Number of hours spent by the respondent doing domestic work in the past week
- **Wage rate for DW (log)**: Log transformation of the earnings of the respondent from domestic work per hour spent doing domestic work

[24] We will utilize a log or an inverse hyperbolic sine transformation, if appropriate.

- **# Houses working at as DW**: Number of houses the respondent worked at as a domestic worker in the past week
- **Non-wage compensation/amenities index (z-score)**: Normalized sum of the number of houses where the respondent received: i) paid leave; ii) food; iii) clothing; iv) lodging; v) phone; vi) support for children's education expenditure; vii) support for medical expenses; viii) loan in case of emergency; ix) transportation; and x) something else
- **Has a skill certification**: Dummy variable which equals one if the respondent has a skill certification
- **Worker-HH communications index (z-score)**: Normalized sum of the respondent's level of agreement with i) *"I am comfortable talking to this home about my professional needs, such as taking breaks and compensation rates"*; ii) *"I am comfortable talking to this home about my personal needs, such as family issues or sickness that interferes with my work"*; and iii) *"I get enough time in my workday to rest, eat, drink water, and use the restroom while working at this home"*
- **Employer monopsony index (z-score)**:[25] Normalized sum of the worker's level of agreement with: i) *"If my employer reduced my hourly compensation by a quarter, I would leave the job"* (reverse scale); ii) *"The people I work for in this house have more say in my compensation than I do"*; and iii) *"The people I work for in this house have more say on my working conditions and hours than I do"*
- **Search frictions index (z-score)**[25]: Normalized sum of: i) the time spent by the worker on job searching for domestic work in the past week; and the worker's level of agreement with the statements: ii) *"It is difficult to get new information about work opportunities"*; iii) *"It is difficult to find enough work to make the income I want to have"*; iv) *"If I left this job or if this house let me go, it would be difficult for me to replace this job"*
- **Workplace conditions index (z-score)**: Normalized sum of whether the households the respondent has worked at have done the following: i) Withheld salary; ii) Asked her to leave the job unexpectedly; iii) Forced her to work more than she agreed; and iv) Forced her to do an activity not in her job description that she did not want to do
- **Workplace abuse index (z-score)**: Normalized sum of whether the respondent experienced the following at the households she has worked at in the last six months: i) Verbal abuse; ii) Physical abuse; and iii) Sexual abuse
- **Bargaining power of DW**: The number of houses where the respondent has tried to negotiate her salary or other benefits or duration of work divided by the number of houses she has worked at in the past week
- **Physical health index (z-score)**: Normalized sum of: i) the respondent's perception of her overall physical health in the last six months; ii) whether she has had persistent pain anywhere on her body (reverse scale); iii) whether she has experienced any workplace injury related to work activities (reverse scale)

[25]This definition differs from the one provided in our outcomes as one or more of the components of the index were not measured at baseline but will be measured at endline.

- **Mental health index (z-score)**: Normalized sum of how often the respondent has felt the following: i) unable to control the important things in her life in the last month (reverse scale); ii) confident about her ability to handle her personal problems in the last month; iii) things were going her way in the last month ; iv) difficulties were piling up so high that she could not overcome them in the last month (reverse scale); v) nervous, anxious or on edge in the last 2 weeks (reverse scale); and vi) not being able to stop or control worrying in the last two weeks (reverse scale)

- **Experienced IPV in the last 6 months**: Normalized sum of how often the respondent's partner has: i) slapped, punched, kicked, twisted her arm, or pulled her hair; ii) threatened her with a knife or other weapon; iii) attacked her with a knife or other weapon; iv) tried to strangle or burn her; v) forced her to have sex or sexual acts when she did not want to due to threats, pressure, or force; and vi) yelled at or insulted her to make her feel bad

- **Locus of control index (z-score)**: Normalized sum of the extent to which the respondent feels that the following apply to her: i) *"I'm my own boss"*; ii) *"If I work hard, I will succeed"*; iii) *"Whether at work or in my private life: What I do is mainly determined by others"* (reverse scale); and iv) *"Fate often gets in the way of my plans"* (reverse scale)

- **Agency index (z-score)**: Normalized sum of: i) the extent to which the respondent's unpaid domestic labor at home decreases if her paid workload increases; and ii) whether she sees herself working in the next ten years

- **Decision making index (z-score)**: Normalized sum of the participation of the respondent regarding the following decisions: i) Whether to access healthcare for herself (reverse scale); ii) Whether to visit a friend (reverse scale); iii) Whether to make major household purchases (reverse scale); iv) What to do with money she earns (reverse scale); and v) What to do with money her partner earns (reverse scale)

- **Economic control index (z-score)**: Normalized sum of the amount of control the respondent's partner has over: i) decisions about how to spend money she has earned (reverse scale); ii) decisions about whether she should take on more paid work (reverse scale); and iii) decisions about where she works (reverse scale)

- **Social connections index (z-score)**: Normalized sum of: i) the number of friends the respondent speaks to for job advice or job referrals; and ii) whether the respondent is involved in any women's organization/ team/ group/ association working for her rights

- **Has personal phone**: Dummy variable that equals one if the respondent has a personal phone that only she mainly uses

- **Has bank account**: Dummy variable that equals one if the respondent has a bank or savings account only in her name or if she has a joint bank or savings account with someone

- **Has used digital banking**: Dummy variable that equals one if the respondent is aware of and has used any of the mobile financial services or phone banking in the past

C Additional Tables

Table A.1: Sample descriptive stats

	Mean	SD	Median	Min.	Max.	N
Demographics						
Age (years)	34.22	6.81	35.00	15.00	60.00	4337
Literacy: Reading	0.44	0.50	0.00	0.00	1.00	4391
Literacy: Writing	0.41	0.49	0.00	0.00	1.00	4391
Years of education	3.83	3.24	4.00	0.00	17.00	4389
Muslim	0.98	0.13	1.00	0.00	1.00	4391
Married	0.88	0.33	1.00	0.00	1.00	4391
Skill certification	0.02	0.14	0.00	0.00	1.00	4391
# Children	1.86	1.05	2.00	0.00	7.00	4361
Husband age (if married)	39.38	8.76	39.00	19.00	82.00	3847
Husband yrs. of education (if married)	4.12	3.79	5.00	0.00	17.00	3813
Financial and technology access						
Own/joint bank a/c	0.18	0.38	0.00	0.00	1.00	4391
Personal phone	0.74	0.44	1.00	0.00	1.00	4391
Comfortable using phone (if has phone)	0.91	0.29	1.00	0.00	1.00	4270
Aware of mobile banking services	0.78	0.41	1.00	0.00	1.00	4391
Used mobile banking in past (if aware)	0.53	0.50	1.00	0.00	1.00	3422
Comfortable using mobile banking (if used)	0.92	0.27	1.00	0.00	1.00	1830
Migrant to Dhaka?						
Born in Dhaka	0.09	0.28	0.00	0.00	1.00	4391
If not born in Dhaka:						
Moved: Own work	0.48	0.50	0.00	0.00	1.00	4001
Moved: Spouse's work	0.38	0.48	0.00	0.00	1.00	4001
Moved: With parents	0.30	0.46	0.00	0.00	1.00	4001
Moved: No jobs in hometown	0.23	0.42	0.00	0.00	1.00	4001
Moved: Marriage	0.17	0.37	0.00	0.00	1.00	4001
Moved: For children's future	0.13	0.34	0.00	0.00	1.00	4001
Yrs. living in Dhaka	19.05	11.56	18.00	1.00	58.00	3967

Notes:

1. This table shows data at the worker level.
2. Data are unconditional unless specified.

Table A.2: Domestic and non-domestic work

	Mean	SD	Median	Min.	Max.	N
Occupational choice						
Does only domestic work	0.76	0.43	1.00	0.00	1.00	4391
Does only non-domestic work	0.04	0.18	0.00	0.00	1.00	4391
Does both	0.08	0.27	0.00	0.00	1.00	4391
Currently not working	0.12	0.33	0.00	0.00	1.00	4391
Domestic work						
Time measures						
Yrs. as a domestic worker	8.81	7.55	6.01	0.55	50.50	3686
Days worked (weekly)	6.39	1.26	7.00	0.00	7.00	3699
Hours worked (weekly)	29.91	15.16	28.00	0.00	105.00	3699
No. houses worked	1.74	0.91	2.00	0.00	5.00	3699
Avg. tenure at a house (months)	29.91	36.88	19.73	0.07	397.93	3653
Formality and nature of work						
Share of houses: contract	0.24	0.42	0.00	0.00	1.00	3650
Share of houses: paid leave	0.81	0.36	1.00	0.00	1.00	3650
Share of houses: fixed schedule	0.88	0.31	1.00	0.00	1.00	3650
Share of houses: on-call	0.29	0.39	0.00	0.00	1.00	3650
Monetary compensation						
Income (BDT, monthly)	4636.87	2353.45	4000.00	300.00	16800.00	3691
Log income (BDT, monthly)	8.31	0.53	8.29	5.71	9.73	3691
Wage per hour (BDT)	44.00	29.99	36.46	3.57	500.00	3645
Below garment sector min. wage (< 8000 BDT/mo.)	0.89	0.31	1.00	0.00	1.00	3691
Type of work						
Share of houses: cleaning	0.95	0.20	1.00	0.00	1.00	3650
Share of houses: cooking	0.39	0.44	0.00	0.00	1.00	3650
Share of houses: childcare	0.05	0.20	0.00	0.00	1.00	3650
Non-domestic work						
Occupation: Self-employed	0.43	0.50	0.00	0.00	1.00	506
Occupation: Regular paid employee	0.20	0.40	0.00	0.00	1.00	506
Occupation: Casual paid laborer	0.26	0.44	0.00	0.00	1.00	506
Days worked (weekly)	4.45	2.57	5.00	0.00	7.00	506
Hours worked (weekly)	18.36	21.11	10.00	0.00	112.00	506
Income (BDT, monthly)	3466.97	4023.21	2000.00	0.00	30000.00	458
Log income	7.54	1.22	7.60	0.00	10.31	458

Notes:

1. This table shows data at the worker level.
2. The domestic work subsection in the table only covers respondents working as a domestic worker at baseline and does not include respondents who were domestic workers in the past but were not at baseline.
3. The types of domestic work included in the table are not exhaustive, they are the three most common types of chores domestic workers perform at their employer households. Other types of work include shopping for the household, caring for elderly, gardening, driving, and others.

Table A.3: Workplace conditions, amenities, and worker preferences

	Mean	SD	Median	Min.	Max.	N
Non-monetary compensation						
Share of houses: receive food	0.58	0.44	0.67	0.00	1.00	3650
Share of houses: receive clothing	0.29	0.42	0.00	0.00	1.00	3650
Share of houses: receive medical exp. help	0.11	0.29	0.00	0.00	1.00	3650
Share of houses: receive emergency loan	0.13	0.31	0.00	0.00	1.00	3650
Share of houses: transportation	0.04	0.18	0.00	0.00	1.00	3650
Share of houses: children's education exp.	0.03	0.17	0.00	0.00	1.00	3650
Share of houses: lodging	0.01	0.08	0.00	0.00	1.00	3650
Share of houses: other assistance	0.08	0.25	0.00	0.00	1.00	3650
Share of houses: no assistance	0.33	0.42	0.00	0.00	1.00	3650
Worker-household communication						
Comfortable talking about personal needs	0.84	0.37	1.00	0.00	1.00	4365
Comfortable talking about professional needs	0.89	0.31	1.00	0.00	1.00	4365
Enough time to rest, eat, use restroom, etc.	0.81	0.39	1.00	0.00	1.00	4366
Poor workplace conditions (last 6 months)						
Any poor conditions?	0.19	0.39	0.00	0.00	1.00	4035
Withheld salary	0.04	0.20	0.00	0.00	1.00	4035
Fired unexpectedly	0.04	0.20	0.00	0.00	1.00	4035
Forced to work more	0.15	0.35	0.00	0.00	1.00	4035
Compensated (if forced to work more)	0.31	0.46	0.00	0.00	1.00	586
Forced to do extra tasks	0.02	0.14	0.00	0.00	1.00	4035
Workplace abuse (last 6 months)						
Any abuse?	0.07	0.26	0.00	0.00	1.00	4035
Verbal abuse	0.06	0.24	0.00	0.00	1.00	4035
Physical abuse	0.00	0.05	0.00	0.00	1.00	4035
Sexual abuse	0.00	0.03	0.00	0.00	1.00	4035
Work amenities considered important						
Job flexibility more important	0.22	0.41	0.00	0.00	1.00	4387
Job/wage security more important	0.27	0.45	0.00	0.00	1.00	4387
Job flexibility and security equally important	0.51	0.50	1.00	0.00	1.00	4387

Notes:

1. This table shows data at the worker level.
2. The non-monetary compensation subsection only covers respondents working as domestic workers at baseline and not those who were domestic workers in the past but not at baseline.
3. The worker-household communication, poor workplace conditions, and workplace abuse subsections cover respondents who had ever worked as a domestic worker at baseline.
4. The worker-household communication questions were asked for the worker's primary household where she spent the most time working most recently (or a random household if no primary household).
5. The poor workplace conditions and workplace abuse questions include all houses the worker has worked at in the 6 months preceding the baseline survey and workers who have worked during that time.
6. Questions on the work amenities considered important were asked of all respondents in the sample.

Table A.4: Employer monopsony, search frictions, and negotiations

	Mean	SD	Median	Min.	Max.	N
Employer monopsony and search frictions						
Difficult - find work	0.90	0.30	1.00	0.00	1.00	4340
Difficult - find info. about work opportunities	0.88	0.33	1.00	0.00	1.00	4368
Difficult - replace this job	0.71	0.45	1.00	0.00	1.00	4345
Employer has more say in compensation	0.88	0.33	1.00	0.00	1.00	4364
Employer has more say in work conditions	0.76	0.43	1.00	0.00	1.00	4367
Searching for job	0.27	0.44	0.00	0.00	1.00	4267
Time spent on job search (cond., weekly, hrs)	1.76	2.04	1.00	0.08	23.00	1160
Share of houses-job source: Friend	0.66	0.42	1.00	0.00	1.00	3650
Share of houses-job source: Self	0.22	0.37	0.00	0.00	1.00	3650
Share of houses-job source: Previous emp.	0.07	0.19	0.00	0.00	1.00	3650
Social connections						
# Friends	4.59	4.26	3.00	0.00	50.00	4391
# Friends: Talks about jobs	2.24	2.06	2.00	0.00	20.00	4391
Part of women's org.	0.08	0.27	0.00	0.00	1.00	4389
Negotiations						
Share of houses: Tried negotiating	0.46	0.46	0.50	0.00	1.00	3650
Outcome if tried negotiating:						
Share of houses: Employer accepted	0.51	0.48	0.50	0.00	1.00	2002
Share of houses: Both compromised	0.21	0.39	0.00	0.00	1.00	2002
Share of houses: Employer refused	0.27	0.42	0.00	0.00	1.00	2002
Reasons if never negotiated:						
Share of houses: Happy with conditions	0.70	0.44	1.00	0.00	1.00	2263
Share of houses: No opportunity/time to ask	0.15	0.34	0.00	0.00	1.00	2263
Share of houses: Lack confidence	0.08	0.25	0.00	0.00	1.00	2263
Share of houses: Unsure how/how much to ask	0.03	0.15	0.00	0.00	1.00	2263
Share of houses: Fear of losing job	0.07	0.23	0.00	0.00	1.00	2263
Share of houses: Fear of receiving poor rec.	0.02	0.14	0.00	0.00	1.00	2263
Share of houses: Fear of punishment	0.01	0.07	0.00	0.00	1.00	2263
Share of houses: None of the above	0.08	0.25	0.00	0.00	1.00	2263

Notes:

1. This table shows data at the worker level.

2. In the employer monopsony and search frictions subsection, the first five statements cover respondents who had ever worked as domestic workers at baseline; the dummy on searching for a job covers all respondents in the sample (and the time spent on job search is conditional on searching for a job); the job source variables only cover respondents who were working as domestic workers at baseline.

3. The job sources shown are the three most common ways of finding an employer household and are not exhaustive; other job sources include digital agency, non-digital agency, advertisement in the newspaper, and others.

4. The social connections subsection covers all respondents in the sample.

5. The negotiations subsection only covers respondents who were domestic workers at baseline.

Table A.5: Saturated regressions: Earnings from domestic work (worker-household level)

	(1) Daily earnings	(2) Daily earnings
Years as a domestic worker	0.609**	
	[0.244]	
Years as a domestic worker, squared	-0.018**	
	[0.007]	
Years of education	-0.169	
	[0.310]	
Can read	-2.584	
	[2.507]	
Can write	0.739	
	[2.475]	
Has a skill certification	2.923	
	[4.660]	
Hours per visit	27.710***	32.820***
	[1.439]	[2.452]
Hours per visit, squared	-0.590***	-1.703***
	[0.194]	[0.451]
Does cleaning	-6.627*	-4.035
	[3.594]	[4.081]
Does cooking	9.173***	5.005*
	[2.263]	[3.021]
Tenure at household (months)	0.113***	0.155***
	[0.037]	[0.048]
Tenure at household (months), squared	-0.000**	-0.000
	[0.000]	[0.000]
Has contract	3.927**	2.502
	[1.596]	[3.301]
R-squared	0.585	0.820
Worker Fixed Effects	No	Yes
N	6334	4628

Notes:

1. Daily earnings have been calculated from monthly earnings data using the average number of days that workers work in a month (26 days in a month on average) to make them comparable to the hours data which was collected on a per visit basis.

2. Limited baseline covariates have been shown in the regression table for clarity. The excluded list of baseline covariates at the worker level include: Married (Y/N), No. of children under age 5, Personal/joint bank a/c (Y/N), Phone for personal use (Y/N), Born in Dhaka (Y/N); Part of a women's organization (Y/N), No. of friends, No. of friends to talk about jobs with. The excluded list of baseline covariates at the worker-household level include: # Chores the worker does, Any paid leave (Y/N), Flexible/on-demand visit schedule (Y/N), Frequency of payment of wages (daily, weekly, monthly), Levels of negotiation with employer regarding salary/benefits/duration of work (never negotiated, negotiated and employer refused, negotiated and employer accepted, negotiated and both compromised), Agrees with the statement "I feel suitable and able to do the tasks asked of me by this house", Dummies for in-kind assistance received (food, clothing, medical expenses, emergency loan, lodging, phone, children's education expenditure, transportation, other, none), Source of job (digital placement agency, non-digital placement agency, through a friend/relative, through an advertisement, through a previous employer, by own searching, other), the neighborhood the worker works in, and the neighborhood the worker lives in.

3. Regressions using the logged version of the outcome are similar and available upon request.

4. Robust standard errors in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$