Screening and Signaling Non-Cognitive Skills: Experimental Evidence from Uganda*

Vittorio Bassi† **Aisha Nansamba**‡

July 12, 2018

Abstract

We study how employers and job-seekers respond to credible information on skills that are difficult to observe. To do so, we experimentally vary whether certificates on workers' non-cognitive skills are disclosed to both sides of the market during 1,200 real job interviews between young labor market entrants and small firms in Uganda. We elicit the beliefs of managers on the skills of workers after each job interview, and track the workers' labor market expectations and realized labor market outcomes for two years. Our results show that the certificates impact beliefs on both sides of the market: the certificates cause workers to increase their expectations, such as expected earnings, while managers of higher ability, who value non-cognitive skills more, revise their assessments of the workers' skills upwards. The updating of workers is driven by a reduction in their perceived difficulty of signaling skills in the market, rather than by workers learning about their own skills. The two-sided reaction to the certificates in terms of beliefs leads to a more efficient allocation of workers to jobs, which then causes earnings to increase, as employed workers with a certificate earn 11% more in the two years after the intervention.

*We are grateful to Imran Rasul, Richard Blundell and Costas Meghir for their invaluable advice and support, and to everyone at BRAC Uganda who made this project possible. We also thank Dan Bennett, Michael Callen, Lorenzo Casaburi, Alan Crawford, Erika Deserranno, Christian Dustmann, Jan Eeckhout, Andrea Ichino, Michele Fioretti, Sarojini Hirshleifer, Matt Kahn, Asim Khwaja, Leon Matagi, David McKenzie, Monica Morlacco, Paul Niehaus, Emily Nix, Paulina Oliva, Tommaso Porzio, Nick Ryan, Simone Schaner, Uta Schönberg, John Strauss, Miri Stryjan, Anna Vitali, Alessandra Voena, Jeff Weaver and numerous seminar participants for valuable comments. Brian Kule provided excellent research assistance. This project was funded by GLM|LIC, grant GA-C3-RA2-346. This document is an output from a project funded by the UK Department for International Development (DFID) and the Institute for the Study of Labor (IZA) for the benefit of developing countries. The views expressed are not necessarily those of DFID or IZA. A previous draft of this paper was circulated under the title "Information Frictions in the Labor Market: Evidence from a Field Experiment in Uganda". Vittorio Bassi acknowledges funding from IFS and ESRC. All errors are our own.

†University of Southern California, IFS and INET. Email: vbassi@usc.edu.

‡BRAC NGO. Email: nansamba.aisha@gmail.com.

1 Introduction

Matching the right workers to the right jobs is important for productivity. Models of job matching emphasize that the efficiency of the matching process depends on the information available to both workers and firms (Jovanovic, 1979; Chade, 2006; Chade and Eeckhout, 2017). On the one hand, difficulties in *screening* workers can affect the ability of firms to select the right employees. On the other hand, difficulties in *signaling* skills to employers can impact the ability of workers to match with the right jobs, or even their ex ante decision to acquire human capital (Spence, 1973). Understanding information frictions on skills is thus important, as they can affect the efficient allocation of labor, and ultimately productivity and output.

The information problem could be even more relevant in developing countries, where labor markets tend to be informal, and access to credible screening and signaling technologies is hindered. A key question for policy is thus to what extent these frictions contribute to explaining low productivity and low wages in developing countries, as well as what policy-makers can do to address them. However, the empirical evidence on how information frictions on skills affect labor markets in low income countries, and in particular on their consequences for both workers and firms through mismatch, is at best limited (McKenzie, 2017).[1]

In this paper, we contribute to this literature by studying whether lack of information on the skills of workers at recruitment reduces productivity by distorting the efficient allocation of workers to jobs. To do so, we design a field experiment in the Ugandan labor market that has two key components: (i) a *matching* component, whereby firms and job seekers are matched together for real job interviews, and (ii) a *signaling* component, by introducing experimental variation in whether a credible signal on skills that are difficult to observe is shown to both sides of the market during the recruitment process, through the provision of certificates.

Our main contribution is to study how both firms and workers respond to the credible signal on skills, and how this two-sided reaction affects the matching process. On the firm side, we identify whether managers revise their beliefs on the skills of workers. On the worker side, we study whether the certificates impact the workers' perceptions about what they can achieve in the labor market, such as their expected earnings. We then develop a job search model with uncertainty on the skills of workers to link the updating of workers and firms to labor market outcomes, and compare the model predictions to reduced form impacts of the signal on the allocation of labor, overall employment and worker earnings in the two years post intervention.

Our sample includes young workers fresh out of vocational training and looking for jobs,

[1]Important exceptions are Abel et al. (2016), who show that reference letters raise employment for female job seekers in South Africa; and Abebe et al. (2017a), who find that a job application workshop in Ethiopia (which includes the provision of a skill certificate) raises employment and earnings for workers particularly disadvantaged at baseline. None of these studies explicitly focuses on the implications of the information friction on skills for mismatch and productivity. We further discuss our contribution to these studies below.

as well as Small and Medium Enterprises (SMEs) looking for workers. Young workers can be particularly affected by difficulties in signaling their skills, given their lack of work experience. Similarly, SMEs do not have access to sophisticated screening technologies, and so might be less able to screen workers, compared to larger firms.

We focus the information revelation on non-cognitive or "soft" skills, such as trustworthiness or reliability, which are hard to observe by nature, and have been shown to have high returns in the labor market both in high and low income countries.[2] In our context, we find that managers report difficulties in observing the soft skills of job applicants to be among their most important concerns. For example, stealing by their own employees is reported as one of the most important problems SMEs face. However, there is substantial variation in the importance given to soft skills: we show that managers with higher cognitive ability, who are also more profitable, value soft skills more in production.

To identify the specific soft skills to be revealed on the certificates, we use the stated preferences of firm owners over the skills they would find most useful to observe during recruitment.[3] We focus on five skills: attendance, communication skills, creativity, trustworthiness and willingness to help others. These are measured through assessments at the vocational institutes where the workers are enrolled, before graduation. We validate our measures of soft skills by showing that they predict earnings over the two-year study period in our Control group of workers, who do not receive a certificate.

We schedule and observe over 1,200 face-to-face real job interviews between our sample of SMEs and vocational training graduates. In a randomly selected half of these interviews, the soft skills certificates are revealed to both the worker and the firm, while in the other half, neither the worker nor the firm get to observe the certificate. We collect information on: (i) stated beliefs of firm owners on the skills of each applicant, (ii) job offers and hires as a result of the job interviews, (iii) outcomes of both workers and firms in the two years post intervention, regardless of whether the job interview turned into a hire, including information on workers' labor market expectations, preferences and job search behavior.

We document that our experimental sample of workers are *positively* selected on soft skills, relative to the eligible population of trainees at the vocational institutes. All eligible workers were informed that the results of their skills assessments could be disclosed to firm owners during the job interviews, and so this positive selection indicates that workers with higher skills understand they have more to benefit from participating.

Our first set of results relate to whether firm owners and workers respond to the certificates by updating their beliefs. We find that managers of higher ability, who value soft skills more,

[2]On the importance of non-cognitive skills in the US see, for instance, Bowles et al. (2001), Heckman et al. (2006) and Deming (2017). Adhvaryu et al. (2017) show that non-cognitive have high returns in India.

[3]In most cases the firm owner also acts as the hiring manager in the SMEs in our sample, so for the purpose of this paper we use the terms "firm owners" and "managers" interchangeably.

revise *upwards* their beliefs on the skills of the matched workers. The positive effect is stronger for workers with higher skills, but we find no evidence of negative updating for workers with relatively lower skills. The positive selection of workers into the experiment, along with low expectations of managers about the workers' soft skills at baseline, can explain why the certificates create mostly *positive news* for high ability owners. On the contrary, low ability owners, who value soft skills less, do not react at all to the information in terms of their beliefs.

We show that workers also react to the certificates by revising upwards their labor market expectations: in the two years post intervention workers with a certificate report 7% higher expected earnings, 5% higher expected employment probability, a higher intention to bargain for wages and a larger ideal firm size. The certificates also lead workers to pull away from poorly paid casual work. We interpret these findings as evidence that the certificates raise the perceived outside option of workers, who try to transition to better jobs. We further show that the updating of workers is driven by a reduction in their perceived difficulty to signal skills in the market, rather than by workers learning about their skills or about the returns to skills.

The results on firm and worker updating are then linked together through a partial equilibrium search model with heterogeneous workers and firms. In the model, soft skills have higher returns when matched to higher ability managers, and so the efficient allocation exhibits *positive sorting* on soft skills/ability. However, difficulties in signaling and screening increase search frictions, which results in mismatch and loss of output. We model the intervention as an increase in the precision of the signal on skills, and we show how the provision of certificates affects (and improves) the allocation of labor. Workers with higher skills earn higher wages as certificates increase their outside option, by making them able to precisely signal their type to the firms where they are most productive. On the contrary, the model makes clear that workers with lower skills can be *negatively* affected by the certificates, as these reduce their employment probability at higher ability firms.

The reduced form evidence on employment and earnings confirms the model predictions. We find that the certificates cause an increase in sorting, by leading to an increase in the probability of employment between our experimental sample of workers, who are positively selected on soft skills, and higher ability managers. So it is precisely those managers who value soft skills more, and who react to the certificates by revising their beliefs, who also react in terms of their hiring choices. On the contrary, there is no change in probability of employment between our experimental workers and lower ability managers. While the certificates lead to a change in the allocation of labor, they do not result in an overall increase in probability of employment, neither at the initially matched firms, nor in other firms that workers match to later on. However, in line with the allocation of labor being more efficient, experimental workers with a certificate earn 11% more while employed in the two years post intervention.

The estimated earnings benefits from the certificates outweigh the program costs. In line

with the average experimental worker benefitting from the certificates, we show that workers in the Control group would be willing to pay over 40% of their monthly earnings to obtain one. Interestingly, their willingness to pay is very close to the cost of the certificates, and so we discuss potential reasons why similar certificates are not already provided by the market.

Finally, we discuss the implications of introducing a mandatory certification policy on soft skills. This is an interesting exercise because we showed that those workers who decided not to participate in the intervention are negatively selected on soft skills, and so, as highlighted by the model, could be *negatively* affected by the certificates. In particular, we show how the cost-effectiveness of the intervention for the average *eligible* worker deteriorates as a function of the (negative) conjectured impact on earnings for those workers who selected out. We find that if the negative impact on the non-participants is larger than a 20% decrease in earnings, the intervention is no longer cost-effective for the average worker. This highlights how mandatory certification policies, while raising efficiency through the improved allocation of labor, could also have important redistributive effects among workers.

Our study extends a very recent but growing literature on how labor market frictions in developing countries affect workers and firms (Abel et al., 2016; Abebe et al., 2017a,b; Groh et al., 2015; Alfonsi et al., 2017; Hardy and McCasland, 2017; De Mel et al., 2018). Exploiting our two-sided design, our contribution is to focus on information frictions on skills, and to show how this friction affects the allocation of labor, and consequently, productivity and earnings.

We also contribute to an established literature on employer learning and information frictions on skills (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Schönberg, 2007; Pallais, 2014; Kahn and Lange, 2014).[4] A closely related paper is Pallais (2014), who shows that disclosing more information about worker abilities increases total hiring and welfare in an online labor market. We contribute by highlighting the role of *mismatch* between workers and jobs as one important channel through which information frictions on skills can reduce output and earnings.

Finally, there is an established literature on wage determination in informal labor markets (Jayachandran, 2006; Foster and Rosenzweig, 2008; Kaur, 2017; Muralidharan et al., 2017; Bandiera et al., 2017). This literature emphasizes the role of the worker outside option, such as opportunities to move to different sectors of the economy, in wage-setting. We contribute by showing that information frictions on skills can effectively reduce the outside option of workers, and so can lead to lower and more compressed wages.

The rest of the paper is organized as follows: Section 2 describes the sample. Section 3 presents the experiment. Section 4 shows the impacts on beliefs. Section 5 presents a model to map the impacts on beliefs to employment and earnings outcomes. Section 6 shows the impacts on labor market outcomes. Section 7 discusses policy implications, and Section 8 concludes.

[4] A related literature studies the signaling value of post-secondary educational credentials in the labor market. See, for example, Deming et al. (2016), and MacLeod et al. (2017).

2 Setting, Sample Selection and Descriptives

The project was implemented in partnership with an NGO, BRAC Uganda.[5] This section describes the sample of both workers and firms and presents descriptive evidence.

2.1 Firm Census and Selection into the Experimental Sample

As shown in Figure 1, we began the study by identifying the sample of firms and workers for the intervention. Firms were initially identified through a census of SMEs conducted in 17 urban areas of Uganda, covering all four regions of the country.[6] To be in the census, firms had to: (i) operate in either carpentry, catering, hairdressing, motor-mechanics, tailoring, welding; and (ii) employ at least two workers in addition to a firm owner.[7] We identified 1,086 eligible SMEs through the census. Table 1 reports summary statistics from the census. The median firm employs 4 workers and has been operating for 5 years. So the typical owner has experience recruiting and managing workers. Hairdressing is the most common trade, making up 30% of the sample, and most firms are located in the region of Kampala, the capital city.[8]

We decided to focus on SMEs in these sectors for two reasons. First, the majority of the labor force in Uganda and in developing countries more broadly is employed in small firms (Hsieh and Olken, 2014). SMEs operating in our six sectors provide an important source of employment for young workers in Uganda, as shown by the fact that vocational training institutes (VTIs) typically offer courses in these sectors. Second, SMEs might be particularly affected by information frictions at recruitment given their limited access to screening technologies and formal recruitment agencies.

At the end of each interview in the census, firm owners were asked their interest to participate in the BRAC Job Placement Program: they were told that BRAC would facilitate job interviews with recent graduates from VTIs, looking for employment in their sector and region. Importantly, firm owners were not informed about the signaling component of the intervention.[9] Information was recorded on interest to participate in the program, and all interested firms were administered a baseline survey: the 422 firms which confirmed their interest in the program and completed the survey form our experimental sample.[10]

[5]BRAC is a large non-profit organization founded in Bangladesh in 1972, currently active in 12 developing countries in Asia, Sub-Saharan Africa and the Caribbean. BRAC was launched in Uganda in 2006 and is currently one of the largest development organizations and micro-finance institutions in the country.

[6]The census took place within a 4km radius from the local BRAC branch in each urban area. This is a separate, and non overlapping census than the one discussed in Alfonsi et al. (2017).

[7]We set the minimum firm size at two workers to restrict the sample to firms used to hiring workers. We did not impose eligibility conditions of workers based on family linkages.

[8]This is in line with findings from other censuses of SMEs in urban Uganda (e.g. Alfonsi et al., 2017).

[9]Details about the VTIs workers could come from were not disclosed at this stage. BRAC is well known in Uganda for its programs targeting youths and firms, and so concerns related to its credibility are not first order.

[10]Table A1 in the Appendix uses data from the census to study selection into the final sample: we notice that

2.2 Worker Census and Selection into the Experimental Sample

We defined as eligible for the intervention all trainees currently enrolled at 15 partner VTIs in one of the six business sectors covered by the project, and expected to graduate in time for the placement intervention.[11] We conducted an initial survey of all the 1,011 eligible trainees. The survey was administered before any information was given to trainees about the BRAC Job Placement Program. Table 2 shows that the median eligible trainee is 20 years old, has completed 11 years of education before enrolling at the VTI, and is undertaking a 2-year course. Training focuses on practical skills, which are also certified at the end of training. On the other hand, VTIs do no provide formal training nor certificates on soft skills. Over 60% of workers plan to look for *wage* employment – as opposed to self-employment – as their first job, and the ideal firm size is less than 20 employees for about the same fraction of trainees. So we can expect the typical trainee to look for jobs in SMEs after training.[12]

We focus on young trainees for two reasons. First, the share of young workers is higher in developing countries, and this is particularly true for Uganda, which has one of the youngest populations in world (UN, 2017). Second, young workers can be particularly affected by information frictions at recruitment given their lack of work experience (Pallais, 2014). It is important to note that our sample of workers are representative of the population of VTI students, and so are more educated than the average Ugandan youth.[13] In addition, since the census was conducted *before* graduation, we did not restrict the sample to individuals who were unemployed or were facing particular challenges in finding employment.[14]

In addition to socio-demographics, the worker census included two measures of skills: (i) a cognitive test, and (ii) a Big 5 questionnaire.[15]After completing the survey, all eligible trainees were informed about *both* the matching and signaling components of the intervention. Specifically, they were told that BRAC would schedule job interviews with potential employers among SMEs, but also that BRAC would conduct additional skills measurements – on both cognitive and soft skills – and that the information from the assessments might be disclosed to potential

firm size is negatively correlated with interest in the program, while the dummies for carpentry and welding are positive and significant predictors, relative to hairdressing (the omitted category). Interest in the program can be interpreted as an indicator of unmet demand for labor, and so Table A1 suggests that this might be higher in smaller firms and in firms in male-dominated sectors. No information was collected on whether the firm had a vacancy at the time of the census, so it is not possible to study how this predicts selection.

[11]The selection and summary statistics of the VTIs are described in the Online Appendix.

[12]Job placement activities by VTIs are very limited and informal.

[13]The median youth aged 18-25 in the 2012/13 Ugandan National Household Survey has 6 years of formal education, while the median level of formal education is 11 years in our sample.

[14]Our sample is therefore different from the ones in related studies such as Abebe et al. (2017a), Abel et al. (2016) and Alfonsi et al. (2017), who instead specifically target disadvantaged unemployed youth.

[15]The Big 5 are five basic dimensions of personality: agreeableness, conscientiousness, extraversion, neuroticism and openness to experience. See John and Srivastava (1999) for a review of the main concepts and methods related to the definition and measurement of the Big 5 traits. More details on the measurement and distribution of these skills are given in the Online Appendix.

employers during the matching process. All trainees were then asked their interest to participate.[16] All interested trainees were administered a baseline survey and were included in the skills assessments: the 787 trainees that confirmed their interest in the intervention and completed the main skills assessments form our final experimental sample.

Since workers were informed about the signaling component of the intervention, a natural question is to what extent this affected their selection into the experiment: if workers are aware of their skills, then those with higher skills should have higher perceived returns from participation, and so a higher propensity to sign up. Table 3 provides evidence on selection into the experimental sample. Column 1 shows that while there is no selection on cognitive skills, there is significant selection on the Big 5 traits, as confirmed by the joint *F-test* of significance of the Big 5 variables which has associated *p-value* = .000. The selection is *positive*, so that workers with higher soft skills are more likely to be in the final sample.[17] The Big 5 variables remain jointly significant at the 1% level in column 2, once a number of controls are added. Figure 2 plots the distributions of the three Big 5 traits that remain significant in column 2 of Table 3: agreeableness, conscientiousness and neuroticism, and shows that the positive selection is *all along* the skill distribution. This implies that the experimental workers are *positively* selected on soft skills. So if firm owners expect the matched workers to be a random sample of VTI graduates, then the revelation of information might provide more positive news than it would do if the sample of graduates was representative of the VTI population. This selection implies that we will not be able to estimate the impacts of the intervention for workers at the lower end of the distribution, who are selecting out. However, we will use evidence on the importance of soft skills in our sample of firms, along with a model, to characterize what the impacts of the information revelation would be for these workers in a counter-factual exercise.

2.3 Key Facts about SMEs at Baseline

We conducted a baseline survey of the SMEs in our experimental sample, to learn about the key features of the labor market and the way these firms operate. We now present five key facts that emerge from the survey. These inform our research design and empirical strategy. More details on these key facts, as well as the supporting evidence, can be found in Appendix A.

1. Soft skills are perceived by firm owners as having relatively high returns, and are reported as more important than numeracy, literacy or theoretical skills. This matches evidence that non-cognitive skills have high returns in labor markets in both developing countries and the US (Adhvaryu et al., 2017; Heckman et al., 2006; Deming, 2017).

[16] Also on the worker side, we do not believe concerns related to the credibility of BRAC as an implementing agency to be first order given the scope of BRAC's work on youth programs in Uganda.

[17] Neuroticism is recoded so that a higher value corresponds to lower Neuroticism (i.e. to more self-control).

2. Firm owners report stealing by their own employees and difficulties in observing workers' soft skills among their main perceived constraints. Employee stealing is ranked as the most important constraint, among a list that includes also access to finance, one of the main constraints the literature has focused on.[18] Difficulties in assessing the soft skills of workers are reported as more important than lack of demand, access to electricity, difficulties in finding workers, or screening on practical skills.

3. Firm owners have relatively low expectations on the distribution of soft skills among workers: 80% of them think that workers with good soft skills are relatively less common than workers with good practical skills. This result is in line with other studies in similar contexts: for example, Caria and Falco (2016) document that firm owners in Ethiopia tend to underestimate the trustworthiness of employees.

4. It is common for firms to recruit applicants who have no prior connection or referral: over one-third of employees in our baseline sample of firms were hired in this way.

5. There is substantial heterogeneity in the cognitive ability of firm owners, and we find that managers of higher ability: i) have higher profits; ii) value soft skills relatively more and iii) are better able to delegate tasks and invest less time in supervising employees. However, they recruit through similar channels and do not report fewer problems in screening workers nor different expectations on the skill distribution of potential employees.

The first and second key facts justify our focus on non-cognitive skills. The second key fact in particular makes clear one reason why soft skills are important: workers with low soft skills can create a *loss* to the firm by, for example, stealing.[19] Since soft skills are reported as difficult to observe, as made precise later in the theoretical model, this can limit the propensity of managers to hire and can reduce wages. Given the relatively low expectations of firm owners on the distribution of soft skills, the third key fact suggests that the signaling intervention might be especially beneficial for those workers that can now credibly signal they are not among those types that firm owners are afraid of hiring. The fourth key fact indicates that an intervention that aims to introduce workers to firms they have no prior contact with is likely to be informative of the regular hiring process in the labor market. The final key fact suggests that the cognitive ability of firm owners might be an important source of heterogeneity in the results. Firm owners of higher ability value soft skills more, and this is consistent with their management style which relies more on delegation and less on direct supervision (potentially because their opportunity cost of time is higher). Therefore, hiring workers with good soft skills is especially important for this group of managers, who might then be particularly responsive to the signaling intervention.

[18] See, for instance, De Mel et al. (2008) and Banerjee and Duflo (2014).

[19] The weak institutional environment in Uganda severely limits the ability of firm owners to prosecute workers who steal, creating *de facto* limited liability for workers.

3 Intervention, Experimental Design and Data

3.1 Intervention and Experimental Design

The intervention we implemented has three components: i) a screening component, whereby information was collected on the soft skills of workers while they were still enrolled at the VTIs; ii) a matching component, whereby job interviews were scheduled between workers and firms; iii) a signaling component, by introducing *experimental variation* in whether information from the screening assessments was disclosed to *both* workers and firms during the job interview process, through the provision of skills certificates.

Worker Screening Our screening activities targeted seven soft skills identified as relevant in initial focus groups with firm owners: creativity, communication skills, willingness to help others/pro-sociality, pro-activity, trustworthiness, discipline, and attendance/time-keeping.[20] Creativity, to be intended both as the ability to generate new ideas, and the ability to come up with creative solutions to problems, is relevant for *all* sectors in our study, as workers are often asked to use in a creative way the limited tools available. Because employees are often asked to work in teams, and to take care of customers, skills such as communication, willingness to help others and pro-activity were mentioned as relevant in the focus groups. We discussed in the previous section how trustworthiness is important to firms. Existing research further suggests that firms value discipline, and that absenteeism is widespread in developing countries.[21]

We used teacher surveys to measure those skills that are easier to assess for an external examiner, namely attendance, discipline, communication, pro-sociality and pro-activity. Conversely, in order to measure creativity and trustworthiness we created our own assessments: for creativity, we developed a set of questions with the help of a local psychologist; for trustworthiness, we made trainees play trust games with real money.[22]

We chose to limit the amount of information on certificates to only five skills, to address concerns related to attention constraints of firm owners and workers.[23] We selected the five soft skills to be revealed based on the stated preferences of managers in the baseline survey. To do so, we asked firm owners to rate on a 0-10 scale how much they would value additional information on the seven soft skills in our assessments, if they were to interview workers fresh

[20] We chose to focus on skills such as communication skills, rather than on the Big 5 traits, because the latter are more difficult to understand for firm owners and workers, as revealed by our piloting exercise. We discuss the correlation between our chosen soft skills and the Big 5 later in this section.

[21] For example, Bowles and Gintis (1976) show that dependability is among the skills most valued by employers in the US. Chaudhury et al. (2006) document high absence rates of 19% and 35% for workers in the health and education sector respectively, across a number of developing countries including Uganda.

[22] The Online Appendix reports more details on the skills assessment procedures.

[23] Beaman et al. (2014) document significant attention constraints among micro-entrepreneurs in Kenya.

out of VTIs.[24] We then selected the top four skills: creativity, trustworthiness, communication and willingness to help others, plus attendance, as a placebo skill.[25] To facilitate the reporting, we followed the Ugandan education system, and graded each skill on a A-E scale. Grades were given using an absolute scale, and were *not* curved within our experimental sample.[26]

To provide further evidence on selection into the experimental sample, we verify how our grades are correlated to the Big 5 traits. Table A3.2 in the Appendix regresses the five soft skills on the Big 5 measures.[27] We document a positive correlation between the Big 5 and most of the other soft skills. In particular, conscientiousness and agreeableness, which predict selection in the final sample, are positively correlated with trustworthiness and creativity, respectively. This suggests that our sample of workers is positively selected not just on the Big 5, as documented in Section 2, but also on some of the soft skills revealed during the intervention. Appendix Figure A8 reports the distribution of grades for the five soft skills: very few workers in our sample were awarded the lowest grade of "E" on any of the skills, and only 2% of workers have a grade of D or below (which would be considered "Fail" grades in Uganda) in all the soft skills.

Validating the Soft Skills Measures To validate our measures of skills, we check whether they predict employment and earnings in our sample of workers. We use observations from the Control group, where information on skills was never revealed to workers, and report employment and earnings regressions estimated by pooling the two worker follow-up surveys conducted 12 and 26 months post intervention. To account for the correlation among soft skills, we aggregate them by creating a dummy equal to one if the worker scored C or above (that is, had a "Pass" grade) on all five skills.[28]

Appendix Table A4 shows that soft skills are a strong predictor of earnings, even within our positively selected sample. This holds true conditional on observables such as gender, age and education. For instance, column 6 shows that conditional on employment workers with a Pass grade on all skills earn $10.3 more per month, corresponding to an increase of 16% over the group who had at least one Fail mark. Such increase in earnings is equivalent to having 2.5 additional years of education. While not causal, these results show that our measurements capture skills that are associated with labor market success, and that cannot be fully inferred from observable characteristics such as education or age. Interestingly, columns 1 and 2 show that soft skills do not predict the extensive margin of employment, nor does education, another

[24] In Figure A7 in the Appendix, we report the average importance given to each skill.

[25] This design allows us to check whether attendance is then given a lower weight during recruitment, relative to the other skills. In practice however, as shown in Appendix Table A3.1, the five soft skills are correlated, which makes it hard to separately identify the effect of revealing information on each skill.

[26] More details on the grading procedure are given in the Online Appendix.

[27] The regressions control for dummies for VTI and sector of training, since the identity of the teachers and enumerators who conducted the skills measurements varied across courses and VTIs.

[28] 41% of workers had a Pass grade on all skills.

strong predictor of earnings. This can in part be explained by the fact that employment rates in the post-intervention period are already relatively high overall at 75%.[29]

Treatment Assignment and Matching The second component of the intervention involved scheduling job interviews. This was done in two steps: first, workers and firms were randomly assigned to a Treatment and a Control group. The randomization was conducted at the individual worker and firm level, and was stratified by submarkets, where a submarket is a sector-BRAC branch combination. Workers and firms were then randomly matched for job interviews within each submarket and treatment group. Figure 3 shows a summary of our experimental design.[30] The random assignment to Treatment and Control groups produced a balanced sample. Panel A of Appendix Table A5.1 reports balance checks on the firm side, showing a balanced sample on nine of the ten variables considered.[31] Panel A of Table A5.2 shows that the randomization produced a balanced sample also for workers. Importantly, the sample is well balanced on all soft skills.

The matching allocations produced a total of 1,230 scheduled job interviews: 616 in Treatment, and 614 in Control.[32] The median firm (worker) was matched with 3 workers (1 firm). There were no cross-treatment matches: Treatment firms (workers) only met Treatment workers (firms). On the other hand, Control firms (workers) only met Control workers (firms). Both Treatment and Control got the matching component, while the two groups differ in the signaling component, as discussed below.

Skills Signaling We created certificates reporting the grades of Treatment workers on the five soft skills. Panel A of Appendix Figure A9 shows an example. The order of the skills on the certificates was randomized. On the back page, the certificates reported a brief description of the skills assessment procedure, as well as guidelines to interpret the grades.[33] To stress the credibility of the certificate, the front page reported the signatures of two high BRAC officials.[34] To control for any potential effects of simply releasing any new document, a placebo

[29]The fact that we document returns to skills in terms of earnings rather than employment is consistent with the reality of informal labor markets in developing countries, where access to remunerative and stable income generating activities is a challenge for young workers (ILO, 2017).

[30] The Online Appendix reports more details on the treatment assignment and matching procedure.

[31]To limit concerns related to potential imbalances, we note that the normalized difference for the other variable (i.e. whether the owner has received training from a VTI) is small (.12), and we further control for this variable and other firm characteristics in our main regression specifications. Also, we are not able to reject the null hypothesis that the ten variables considered for the balance checks are all jointly not significant in predicting treatment assignment, as shown by the *p-value* on the joint *F-test* in column 2 (*p-value* = .393).

[32]The Online Appendix reports balance checks where the unit of observation is the match, and shows that the sample remains well balanced.

[33]The description made clear that trainees had not received any soft skills training as part of this project.

[34]The scope of BRAC work in Uganda limits concerns related to the credibility of the certificates, both within and outside our research sample.

certificate was produced for workers in Control. An example is shown in Panel B of Figure A9: the document simply states that the trainee was willing to be put in contact with potential employers, which is something that both the worker and the matched firms already knew, while it does not report information on the grades on the skills.[35] The certificate is otherwise identical to the one in Treatment. Any treatment effects will thus be due to the *content* of the certificate, rather than from just having an additional document in their application files.[36]

The timing of the revelation of the certificates was the same in Treatment and Control. On the interview day, the worker was first met by the BRAC staff, who showed the certificate and explained its content to the worker. The worker was informed that the firm owner would be shown the same certificate at the start of the interview. The worker then had the option to pull out, in case she had changed her mind about wanting to meet the firm owner. In practice, only two workers met the BRAC staff but decided not to proceed to the job interviews. The worker was then introduced to the matched firm by the BRAC staff, who made sure that the firm owner was also shown the transcript. The transcript was then left to the worker to keep. After the initial introduction, the firm owner and the worker were left to interact as they pleased, and the BRAC staff played no further role in the interview.[37]

3.2 Data Collection, Compliance and Attrition

As shown in Figure 1, we collected four post-intervention surveys. First, a "matching survey" was conducted while the matching intervention was taking place. For each scheduled job interview, we have information on: (i) whether the job interview took place (and the reasons why it did not take place in case); (ii) basic interview descriptives such as whether the trainee brought additional documents to the interview; (iii) the beliefs of managers on the skills of the matched workers. The firm follow-up survey was conducted 6 months after the matching intervention, and contains information on firm-level outcomes. Finally, two worker follow-up surveys were conducted 12 and 26 months after the matchings, and contain information on worker-level labor market outcomes, expectations and search behavior.[38]

[35] In fact, the word "skills" is not mentioned anywhere on the document.

[36] It is possible that managers interpret the placebo certificate as a form of "endorsement" of the worker by BRAC. This would bias our analysis towards finding no positive effect of the Treatment certificates.

[37] Our design allows us to estimate the impact of disclosing the soft skill certificates over and above any additional application documents already presented by the worker to the firm. Data collected during the matching intervention shows that 37% of workers brought at least one additional education/training certificate and 22% brought at least one reference letter to the interview (with 52% of trainees bringing either). This is in contrast with the results in Abel et al. (2016), who show very low rates of usage of reference letters at 2% among their sample of disadvantaged job seekers in South Africa.

[38] Information on whether each job interview in the matching intervention turned into a hire (and on the associated job characteristics) was collected in both the firm and worker follow-ups. We prefer to use information from the worker follow-ups for these match-level outcomes as measurement error is likely to be lower there for at least two reasons: (i) while the median firm was matched to three workers, the median worker was matched

Figure 4 shows a summary of compliance and attrition. Starting from compliance, of the 1,230 *scheduled* job interviews, 515 (or 42%) actually took place. Lack of compliance is mainly due to workers having lost interest in being matched (32% of cases) or to the firm having lost interest (30% of cases) by the time they were called for the interviews.[39] Panel A of Appendix Table A6 explores the determinants of compliance, and shows very little evidence of selection on observables.[40] Importantly, Treatment does not predict the likelihood of the job interview taking place. This is not surprising, as the certificates were shown to firms and workers only *conditional* on the job interview taking place. Consistently with this, the Online Appendix confirms that the sample of job interviews that took place remains balanced on the main observable worker and firm characteristics. All the Treatment workers who showed up to the job interviews were given the certificates (corresponding to 49% of Treatment workers). The remaining Treatment certificates were disbursed to the workers shortly after the first worker follow-up survey. So by the second follow-up survey about 81% of Treatment workers had received the certificate.[41]

Moving on to attrition, the follow-up surveys targeted all firms and workers in the experimental sample, irrespective of whether the scheduled job interviews took place or not. We have very moderate attrition rates: these are about 12% in the firm follow-up, and about 14% in both worker follow-ups.[42] Panel B of Appendix Table A6 shows that attrition is not related to Treatment in either sample, and there is also very little evidence of observable characteristics determining attrition. Panel B of Appendix Table A5.1 and Panels B and C of Table A5.2 confirm that the samples of both workers and firms remain balanced on baseline characteristics at follow-up, so that attrition is not likely to affect the validity of the initial random assignment.[43] Therefore, we do not correct for attrition in our main regression specifications.[44]

to only one firm, so possible recall errors related to the respondent getting confused about the different job interviews are lower on the worker side; (ii) in 13% of the cases, the person that answered the firm follow-up survey is different from the owner that conducted the job interviews. Results using corresponding match-level information from the firm follow-up survey (not reported) are qualitatively similar.

[39]As shown in Figure 1, for logistical reasons there was a lag of about three months from the graduation of trainees to the roll out of the matching intervention, and so this can explain the loss of interest by a sizable fraction of firms and workers. Other common reasons why the scheduled job interviews did not take place were: (i) either the firm or the worker could not be contacted (18% of cases), (ii) neither the worker nor the firm were interested (10% of cases); (iii) the worker lived too far (4% of cases); other reasons (4% of cases).

[40]The only significant predictor is the duration of the vocational course, so that matches involving workers enrolled in longer programs are less likely to happen. This is likely due to misunderstandings regarding when the intervention would take place, so that some workers enrolled in longer programs had in fact not graduated yet from the institutes when the intervention was rolled out (and so were unable to participate).

[41]The remaining 19% did not receive the certificate as they could not be contacted during the disbursement.

[42]These attrition rates are in line with other labor market studies in similar settings. For example, the attrition rate is 6.5% in Abebe et al. (2017a), 13% in Alfonsi et al. (2017) and 17% in Abel et al. (2016).

[43]The firm follow-up data shows that owner's age is slightly higher in Treatment. However, the normalized difference for this variable is small (-.132), thus limiting concerns that the sample is unbalanced. Also, we cannot reject that all observable firm characteristics are jointly not significant in predicting treatment assignment at follow-up, as shown by the *p-value* on the *F-test* at the bottom of the table (*p-value* = .215). To further limit concerns related to attrition, we control for owner's age and other firm characteristics in our main specifications.

[44]In Appendix B we show robustness of our main results to correcting for attrition using inverse probability

3.3 Estimation

For the match-level analysis we estimate the following regression specification, by OLS:

$$y_{ij} = \beta_0 + \beta_1 T_{ij} + \gamma \mathbf{X}_i + \delta \mathbf{X}_j + \theta \mathbf{Strata}_{ij} + \alpha Int_{ij} + \varepsilon_{ij}, \quad (1)$$

where y_{ij} is the outcome of the worker i, firm j match, for example whether the worker was hired by the firm. T_{ij} is a treatment group indicator. $\mathbf{X}_i$ is a vector of baseline worker controls.[45] $\mathbf{X}_j$ are baseline firm controls.[46] $\mathbf{Strata}_{ij}$ are dummies for the stratification variables (sector and BRAC branch). Int_{ij} are dummies for the month of interview. We cluster standard errors both at the level of the firm and the worker, to account for the fact that workers were potentially matched to more than one firm, and firms were potentially matched to more than one worker (Cameron et al., 2011; Abebe et al., 2017b).[47]

The estimation sample for the match-level analysis includes the 515 matches that took place. This is our preferred sample because as discussed above: (i) Treatment does not predict selection into the sample of job interviews that take place, and (ii) the sample remains balanced on observable worker and firm characteristics conditional on meeting for a job interview. As the certificates were disclosed in all but two of the job interviews that took place, we interpret β_1 as the Average Treatment Effect (ATE) on the population that meets. Panel A of Appendix Table A6 shows very little evidence that observable worker and firms characteristics drive which job interviews take place. However, if unobservables characteristics drive the decision to participate, then the sample of realized matches might not be representative of the initially scheduled matches. We address this concern by showing in the Online Appendix that our results are robust to estimating a two-sided econometric model of sample selection, where we use as instruments to predict whether a job interview takes place the characteristics of the enumerators that were (randomly) assigned to contact the workers and firms.[48]

For the worker-level analysis, we estimate the following ANCOVA specification by OLS, on the two follow-up survey waves, $t = 1, 2$:

$$y_{it} = \beta_0 + \beta_1 T_i + \beta_2 y_{i0} + \gamma \mathbf{X}_i + \theta \mathbf{Strata}_i + \alpha Int_{it} + \vartheta_t + \varepsilon_{it}, \quad (2)$$

weights (Wooldridge, 2010).

[45] These include: dummy for female, age and age squared, dummy for any work experience, VTI course duration (in years), completed years of formal education, dummy for whether the worker scored C or above (that is, had a "Pass" grade) on all five soft skills reported on the certificates.

[46] These include: dummy for female owner, owner age and age squared, dummy for whether the owner attended a VTI in the past, number of employees.

[47] In the Online Appendix we show robustness to running the analysis at the worker level, as an alternative way to control for the fact that workers were potentially matched to more than one firm.

[48] In the Online Appendix we also show robustness to estimating equations like (1) on the full sample of 1,230 *scheduled* matches by assigning a value of zero to the outcome of those job interviews that did not take place. β_1 recovers the Intention To Treat (ITT) parameter in this alternative specification.

where y_{it} is the outcome of worker i in follow-up survey wave t, for instance their total earnings in the last month, y_{i0} is the baseline value of the outcome, ϑ_t is an indicator for the second follow-up, and the other variables are the same as previously defined. In our preferred worker-level specification we pool observations from the first and second follow-up surveys and cluster standard errors at the worker level, to maximize power.[49] In this specification β_1 recovers the ITT parameter since the estimation sample includes all experimental workers, regardless of whether they met any firms for the job interviews. For the firm-level analysis, we estimate equations like (2) but at the firm level, and so using only one round of follow-up firm surveys.

4 Impacts on Beliefs

We begin the empirical analysis by studying whether the certificates lead firm owners to revise their beliefs on the skills of the matched workers, and whether workers react by updating their labor market expectations. We can expect managers to react to the certificates because, as documented in Section 2.4, they reported soft skills as being important but difficult to observe at baseline. On the other hand, the labor market expectations of workers might change if through the certificates workers are better able to signal their skills to potential employers, or if they obtain more precise information on their own skills. Therefore, it is possible that *both* sides of the market update their beliefs as a result of the new information.

4.1 Do Managers Respond to the Certificates?

After each job interview, managers were asked whether they thought the worker they had just met was similar or different to typical job applicants to their firm. If they thought the worker was different, they were asked why that was the case. We construct a dummy equal to one if the manager reported the matched worker as *more skilled* than usual applicants, and zero otherwise. We run OLS regressions analogous to (1) with this as dependent variable. As discussed above, the sample includes the 515 job interviews that took place. Table 4 reports the results. Column 1 shows that our estimate of β_1 is very close to zero and not significant. In column 5 we allow the treatment effect to differ by whether the worker had a Pass grade (i.e. C or above) on all soft skills, or whether instead had a Fail grade on at least one skill. Again, the coefficients on the interaction terms are small and not significant. Thus, there is no evidence that the average manager reacts to the certificates in terms of beliefs.

In Section 2.4 we discussed that the cognitive skills of managers might be a potential source of heterogeneity, since managers of higher ability value soft skills more. To explore this, we create a dummy equal to one if the manager scored at the median or above on the cognitive

[49] We show dynamic treatment effects for our main outcomes in Appendix B.

test administered at baseline, and zero otherwise. Columns 2 and 3 split the sample of matches by this dummy.[50] The results show that the null treatment effect in column 1 masks important heterogeneity. Column 2 shows that the certificates lead firm owners of higher ability to revise *upwards* their beliefs on the skills of the matched workers: these managers are 11pp more likely to report the matched worker as more skilled than the usual applicant, relative to when the certificates are not shown, a result significant at the 5% level, and corresponding to a more than twofold increase. On the other hand, column 3 shows that there is no reaction by managers of lower ability. The estimates for the two manager types are statistically different at the 5% level, as shown by the *p-value* in column 4, which is estimated from a fully interacted model. Column 6 shows that the positive revision of managers of higher ability is somewhat stronger for workers with a Pass grade on all skills, but we are unable to reject that the impacts are the same for the two groups of workers, as shown by the *p-value* on the *F-test* of equality of treatment effects at the bottom of column 6 (*p-value* = .194). Column 7 confirms that managers of lower ability do not react to the certificates, and this is irrespective of the skills of the workers they meet.[51]

One potential concern with the heterogeneous analysis is that manager's ability might proxy for other firm characteristics. To rule out this concern, we run a regression on the full sample of matches, and where the Treatment dummy is interacted with a number of firm and manager characteristics, all at the same time. Appendix Table A7 reports the coefficients on such interactions, and confirms that cognitive ability is the key source of heterogeneity, as the coefficient on the interaction with the high ability dummy remains large and significant at the 5% level.[52]

Earlier in the paper we documented that the experimental workers are positively selected on soft skills and that managers have relatively low expectations on the distribution of worker soft skills. As managers were never informed about the positive selection of workers, then this can explain why the certificates are interpreted as mostly *positive news* by higher ability managers. In summary, the evidence is consistent with at least some managers valuing the information on the certificates, as this allows them to better identify productive workers: these managers are

[50]The high ability dummy is missing for about 15% of owners, who refused to take the test. The Online Appendix shows that the results in Table 4 are robust to imputing missing values for the high ability dummy using information from the baseline survey of managers.

[51]As discussed in Section 3, the five soft skills were all shown on the certificates simultaneously and are correlated. This makes it difficult to identify which specific skills drive the results. Nevertheless, we study heterogeneous effects by whether the worker has a Pass/Fail grade: i) on the skill reported as most important by managers at baseline (creativity), and ii) on the skill reported as least important (attendance). The results, reported in the Online Appendix, show very similar patterns to Table 4, regardless of our measure of skill. This is in line with high ability managers reacting similarly to information on the different skills.

[52]The only other significant interaction in Table A7 is the one with owner's years of education. Panel A of Appendix Table A2 compares high and low ability owners on a number of characteristics, and shows that high ability owners have almost one year more education, so that ability an education are positively correlated, which explains the pattern of coefficients in Table A7. Panel A of Table A2 further shows that there are no other differences between the two groups of managers. The results in Appendix Table A7 additionally rule out that manager's ability is capturing sectoral heterogeneity, as shown by the *p-value* on the *F-test* of joint significance of the interactions of the sector dummies with Treatment, reported at the bottom of the table (*p-value* = .510).

the ones with higher cognitive ability, who value soft skills more and are more profitable. The results on manager updating are in line with the findings of the literature on recruitment in firms (Autor and Scarborough, 2008; Hoffman et al., 2017) which shows that the introduction of job tests helps managers identify and select workers with higher skills.

4.2 Do Job Seekers Respond to the Certificates?

We now turn to the impacts of the certificates on the labor market expectations of workers, and in particular on proxies for their outside options. We can expect such an impact because the certificates were left to the workers to keep after the job interview. So if the certificates help workers better signaling their skills, or if workers learn about their soft skills through the certificates, then this could affect their outside options.

We pool data from the two worker follow-up surveys, and estimate regressions analogous to (2). We estimate treatment effects for the average worker, as the results in the previous subsection suggest that heterogeneity by worker skills is limited, since the sample of workers are positively selected.[53] Table 5 reports the results. Panel A focuses on *beliefs*. Column 1 shows that workers in Treatment report expected monthly earnings that are $7.95 higher than Control, a result significant at the 5% level and corresponding to a 7% increase.[54] The rest of Panel A shows that Treatment workers report 5% higher expected probability of employment (column 2); a higher intention to bargain for wages[55] (column 3) and are also 7% more likely to report that their ideal job is in a firm with 10 or more employees (i.e. a "large" firm) (column 4). These results suggest that the certificates raise the perceptions of workers about what they can achieve in the labor market. Panel B of Table 5 shows that Treatment workers change their labor market search *behaviors* in a way consistent with their revision of expectations: they are 15% less likely to be engaged in casual work (column 5), which is a poorly paid and insecure form of employment; they are also 37% more likely to have looked for a job in the public or NGO sector in the last year (column 6); however, Treatment workers do not increase their overall search intensity (column 7). These changes in behavior are consistent with Treatment workers trying to transition to better jobs where their skills might have higher returns.

Table 6 explores *why* workers react to the certificates. We are able to distinguish between three explanations: (i) workers learn about their own soft skills; (ii) workers learn about the returns to soft skills; (iii) workers are better able to signal their skills to the market. In

[53]The Online Appendix shows that we find no evidence of heterogeneous treatment effects by worker skills.

[54]To construct monthly expected earnings, respondents were asked: (i) their minimum expected earnings; (ii) their maximum expected earnings; (iii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings for each respondent.

[55]Intention to bargain for wages is measured using a standardized index of two variables from the second follow-up survey: a dummy for whether the worker would accept a job without negotiating on the wage; a variable reporting how much the worker would expect wages to be influenced by negotiation, on a 0-10 scale.

the second follow-up survey, we asked workers in both Treatment and Control to self-assess themselves on the five soft skills included on the certificates, using the same A-E scale we used for grading. Column 1 shows that there is a positive and significant correlation between the worker self-evaluations and our measurements. This shows that workers are already aware of their soft skills, and is in line with the positive selection into the experiment documented above. Columns 1 and 2 further show that being assigned to Treatment does not alter the correlation between the self-reports and our skills measurements. So workers are *not* learning about their skills through the certificates. Column 3 shows that there is no impact on the perceived returns to soft skills, thus confirming that workers are also not changing their beliefs about the importance of soft skills.[56] Finally, column 4 shows that workers in Treatment believe they face fewer challenges in signaling their skills to potential employers. The dependent variable is measured on a 5-point scale, and so the effect corresponds to a 7% reduction over the Control mean (significant at the 5% level).[57] So the evidence is consistent with the impacts on the perceived outside options of workers being driven by the *signaling value* of the certificates.

Taken together, these results highlight one important difference with the literature studying the introduction of job tests in firms (Autor and Scarborough, 2008; Hoffman et al., 2017). Since the outcomes of job tests are typically not disclosed to applicants, workers cannot respond to the information generated in the job tests. By contrast, our analysis shows a significant response on *both* the firm and the worker side to the information embedded in screening assessments, once these are made available also to workers during job interviews.

5 Theoretical Framework

In the previous section, we showed that, upon receiving the certificates: (i) managers of higher ability revise *upwards* their beliefs on the skills of workers, and (ii) workers increase their labor market expectations. We now develop a partial equilibrium search model that links the response of firms and workers in terms of beliefs to impacts on employment and wages. The model formalizes the worker problem, and helps us interpret the reduced form impacts on employment and earnings presented later in the paper. In addition, the model shows how the signal would impact those workers at the low end of the skill distribution, who have selected

[56] To construct the variable on the perceived returns to soft skills, at second follow-up workers were asked how important each of the five soft skills disclosed on Treatment certificates was for making someone a productive worker, using a 0-10 scale, where 0 = "Not important at all" and 10 = "Extremely important". We compute the average perceived return of the five skills.

[57] To construct the variable on perceived constraints in signaling skills we use two questions from the second follow-up survey. Workers were asked their perceived importance of constraints related to signaling skills to the market, using a 1 to 5 scale where 1 = "Not a problem at all" and 5 = "A very important problem", separately for practical and soft skills. This question was asked about soft skills in general, and so not specifically about the five soft skills disclosed on the Treatment certificates. We compute the average importance of constraints related to signaling the two types of skills (practical and soft) and use this as dependent variable in column 4.

out of the intervention. This will help us formalize the cost-benefit analysis of a *mandatory* certification policy on soft skills later in the paper.

5.1 Model Setup

Production There are two types of workers and two types of firms, which we will refer to as h and l, and H and L, respectively. Workers differ in their soft skills: workers of type h have higher soft skills than type-l workers. Firms differ in their production function. Each firm only hires one worker, who inelastically supplies one unit of labor. This implies that firms only differ in terms of productivity, which we allow to be match specific. A firm of type H produces surplus equal to $y_H^h = a$ when matched to a worker of type h, and *negative* surplus equal to $y_H^l = -d < 0$ when matched to a worker of type l. On the contrary, workers are equally productive at firms of type L, and they generate surplus equal to $y_L^h = y_L^l = b$, such that $a > b > 0 > -d$. This modeling choice is motivated by the key facts from Section 2.4, which suggest that soft skills have higher returns when matched to high ability managers, and therefore that workers of type h are more productive at these firms. This set up is also consistent with the fact that high ability managers are better able to delegate: because high ability managers are less likely to supervise workers, workers with low soft skills can generate a *loss* to their firms, for instance through stealing.[58] Because low ability managers are less able to delegate, they engage in more supervision, and are not hurt by workers with low soft skills. For ease of exposition, we assume that each worker type represents one half of the worker population. Note that given the production functions specified above, productivity and surplus in the economy are maximized when workers of type h match with firms of type H, and when workers of type l match with firms of type L. In this sense, the efficient allocation exhibits *positive sorting* on skills/ability.

Search Process and Wages We build on the simplest search model with random search and chance of an offer (McCall, 1970). Each period unemployed workers engage in random search, and face a chance of receiving a job offer from either type of firm. In line with the empirical results so far, we assume asymmetric information on the skills of workers, such that the probability of an offer depends on the *expected* productivity of the worker at the matched firm. We assume Nash bargaining with equal weights, so that, if an offer is made, the offered wage corresponds to half of the expected surplus generated by the match (Eeckhout and Kircher, 2011). For simplicity, there are no separations, and workers discount the future at rate $\beta < 1$.

[58] In line with the informal nature of labor markets in Uganda, we assume that managers cannot sue employees for losses, so that employees have *de facto* limited liability. An alternative modeling approach would be to assume that workers of type l also produce positive surplus at firms of type H (i.e. $y_H^l > 0$), but that binding minimum wages are such that type-l workers still create a net loss to the firm. This alternative modeling approach would generate the sample predictions. Kaur (2017) shows that fairness norms can create significant downward wage rigidities in developing countries, even in the absence of formal labor market institutions.

The surplus from a match with a type-L firm is unrelated to the skills of the worker, and the probability of meeting and receiving a wage offer from these firms, defined as p_L, is the same for all workers.[59] The per-period wage offered by firms of type L is simply half the surplus, i.e.:

$$w_L = \frac{b}{2}. \tag{3}$$

On the other hand, because production at firms of type H depends on the worker type, these firms try to screen the high-skilled types through job interviews.[60] Job interviews generate a signal σ on skills, which can be *Good* (G) or *Bad* (B). We let q denote the probability of a truthful signal. The closer q is to 1, the more informative signals are. We assume that in the baseline environment the value of q is such that $q = P(G|h) = P(B|l) \in (\frac{1}{2}, 1)$, namely that signals are somewhat informative of the worker's type, but firms are not able to extract perfectly informative signals, due to asymmetric information on skills. This assumption is motivated by our descriptive evidence that firms report problems in screening workers' soft skills as being important. Also, this is in line with our reduced form evidence that managers of higher ability respond to the certificates by revising their beliefs on the skills of workers, which would not have been the case if these managers were already able to perfectly screen workers.

Given a signal σ, firms of type H compute the posterior probability of the worker type, using Bayes' rule.[61] Since we assumed that the share of each worker type is $\frac{1}{2}$, q is also the posterior probability, that is, $q = P(h|G) = P(l|B)$. With this in mind, firms compute the *expected productivity* of the worker as

$$E[y_H|\sigma] = \begin{cases} qa + (1-q)(-d) & \text{if } \sigma = G \\ (1-q)a + q(-d) & \text{if } \sigma = B \end{cases}. \tag{4}$$

We assume that the expected output from the match is *negative* in the case of a B signal, namely that $E[y_H|B] < 0$, so that firms of type H do not make a job offer if they see a B signal.[62] This implies that the only wage offer these firms make is equal to:[63]

$$w_H = \frac{qa + (1-q)(-d)}{2}. \tag{5}$$

[59] p_L depends on other search frictions unrelated to worker skills and on taste shocks of the firm for the worker.

[60] Note that both types of firms might also use job interviews to screen workers on other characteristics that are important in production (e.g. previous work experience of the applicant). Our analysis should then be interpreted as being *conditional* on these other characteristics.

[61] Bayes' rule relates the prior to the posterior probabilities. For instance, in the case type-h workers and G signals, Bayes' rule can be written as follows: $P(h|G) = \frac{P(G|h) \cdot P(h)}{P(G)}$.

[62] Assuming that $E[y_H|B] > 0$ but that binding minimum wages result in an expected loss for the firm would yield equivalent predictions.

[63] Assuming that firms of type H pay two wages, one in the case of a G signal, and one in the case of a B signal, would complicate the analysis but not alter the main predictions.

We define the probability of meeting and receiving an offer from a type-H firm as $p_H(k)$, with $k \in \{h, l\}$. This probability is positive only for workers who send a G signal, and is a function of the worker type.[64] Finally, we let parameters be such that $w_H > w_L$ in the baseline environment, which is consistent with type-H firms paying higher wages.[65]

5.2 Worker Problem

The value function of an unemployed worker of type k, with $k \in \{h, l\}$, is then given by :

$$V^N(k) = \beta \left[p_L V^L(k) + p_H(k) V^H(k) + (1 - p_L - p_H(k)) V^N(k) \right]. \tag{6}$$

The unemployed worker earns zero in the present period. Next period, with probability p_L she meets and gets an offer from a type-L firm, which she values at $V^L(k)$; with probability $p_H(k)$ she meets and gets an offer from a type-H firm which is valued at $V^H(k)$; with residual probability she does not meet any firm and so remains unemployed. $V^L(k)$ is defined as:

$$V^L(k) = \max \left(\frac{w_L + \theta}{1 - \beta}, V^N(k) \right), \tag{7}$$

where θ is a match-specific taste-shock for the firm. Workers compare the utility from being forever employed at that firm, to their outside option of remaining unemployed and continuing to search. The existence of a taste-shock θ ensures that acceptances are random events. Similarly, the value of an offer from a type-H firm $V^H(k)$ is defined as:

$$V^H(k) = \max \left(\frac{w_H + \theta}{1 - \beta}, V^N(k) \right). \tag{8}$$

Worker behavior in this model crucially depends on: i) the relative probabilities of job offer at the two types of firm (i.e. p_L vs $p_H(k)$), and ii) the relative wage (i.e. w_H vs w_L). Note that while p_L and w_L are deterministic, both $p_H(k)$ and w_H depend on q. In particular, the higher is q, the higher is $p_H(h)$, the lower is $p_H(l)$ and the higher is w_H. Therefore, asymmetric information on skills, which is modeled as a value of q lower than one, can create mismatch and wage compression: if workers of type h are not perfectly able to signal their skills, this reduces the probability that they receive an offer at firms of type H, and the wage they can earn there. At the same time, this increases the probability that they accept an offer from a firm of type L, since their outside option from rejecting such offer and continuing to search is reduced. Also, workers of type l have a chance of being employed at type-H firms, since these firms are not perfectly able to screen them out, which further increases mismatch and reduces output.

[64] Specifically, $p_H(h)$ is an increasing function of q. On the other hand, $p_H(l)$ is a decreasing function of q.

[65] In Section 2.4 we showed that higher ability managers are more profitable at baseline, and there is an extensive body of literature linking firm productivity and wages. See, for instance, Card et al. (2018).

5.3 Predictions

We interpret the Treatment as a *credible* increase in the precision of the signal, so that $q(T) = P(h|G,T) = P(l|B,T) = 1$. This generates the following comparative statics predictions with respect to Treatment:

Prediction 1. *There is an increase in sorting: workers of type h are more likely to be employed at firms of type H, and workers of type l are more likely to be employed at firms of type L.* As the signal becomes precise, workers always send a truthful signal to firms of type H. This implies that type-H firms only make offers to high-skilled workers. The Treatment also leads to an increase in the wage offered by firms of type H, through an increase in q.[66] Therefore, workers of type h become more likely to accept an offer from type-H firms and to reject an offer from type-L firms. At the same time, because workers of type l do not receive offers from high-ability firms, they become more likely to accept an offer from a firm of type L, since their outside option is only unemployment. Both firms and workers behavior leads to an increase in sorting and therefore to a reduction in mismatch.

Prediction 2. *The change in the overall employment probability is ambiguous for both worker types.* While workers of type h are more likely to accept offers from firms of type H, they are also more likely to *reject* offers from type-L firms. Similarly, workers of type l are less likely to be employed at firms of type H, but more likely to accept offers at firms of type L. Therefore, the treatment effect on the overall employment probability of both worker types is ambiguous.[67]

Prediction 3. *Average wages conditional on employment increase for workers of type h, and decrease for workers of type l.* As q increases, workers of type h are more likely to be employed at firms of type H, who now pay a higher wage, due to a reduction in the probability of hiring a low-skilled worker. Therefore, average wages conditional on employment are higher for type-h workers in Treatment. On the other hand, workers of type l are now more likely to be employed at firms of type L, and so their average wage conditional on employment is lower, since the share of workers of type l employed at type-H firms, which pay higher wages, has decreased. This also implies that in Treatment, wage dispersion between h and l types increases.

5.4 Discussion and Mapping to Data

The model makes clear that the positive treatment effect on wages at firms of type H is the result of the certificates increasing the expected productivity of type-h workers to the firm, so that

[66] Specifically, when $q(T) = 1$, the wage at firms of type H increases to $w_H(T) = \frac{a}{2}$.

[67] Since in this model everyone is employed in the long run, the comparative static result on employment is to be interpreted in the short run. In the data we observe workers for two years after the intervention, and so this justifies why we focus on short-run predictions and do not model separations.

such firms are willing to pay these workers more. This is consistent with the empirical evidence from the previous section, where we showed that managers of higher ability revise upwards their beliefs on the skills of workers, once a certificate is shown. The fact that the increase in q is *credible* and observed by both sides of the market is important for the prediction on wages: in Treatment, workers of type h can better signal their skills to *all* potential employers. This increases their outside option, and so any given type-H employer has to increase the wage for workers of type h to be willing to accept the offer. Note that this would have been different in a one-sided information revelation to firms, where workers are not provided with a credible signal on their skills. In such case, firms of type H could increase the hiring of type-h workers without increasing the wage, since the outside option of the worker does not change.

In addition, the model highlights that while workers of type h *always* benefit from the certificates in terms of higher wages, the certificates can hurt workers at the low end of the distribution, through a reduction in their probability of employment at type-H firms and the resulting loss of earnings. So the certificates make the allocation of workers to jobs more efficient, but can also increase wage dispersion and inequality among workers. Whether the gains for workers of type h outweigh the possible losses for workers of type l depends on the value of the parameters and the size of the treatment effects, and is therefore an empirical question. As discussed, the workers participating in our intervention are positively selected on skills, so that type-l workers are likely not to be in our experimental sample. Therefore, we will not be able to use the experiment to study the impact of a certification intervention on these workers. However, in Section 7 we use the insights from this model together with the reduced form evidence from the experiment to discuss the implications of introducing a mandatory certification policy on soft skills, which would include workers of *all* skill levels.

6 Impacts on Sorting, Employment and Earnings

We now discuss the reduced form impacts of the certificates on sorting, overall employment and earnings, and test the model Predictions 1-3, respectively. Because workers are positively selected into the experiment, we interpret our results as applying to h-type workers.

Altogether, the empirical evidence presented in this Section confirms the model predictions. We document an increase in earnings consistent with a reduction in mismatch and with an increase in productivity. Since workers have a credible signal of their skills that they can show to other potential employers, their outside options increase and they can capture part of the increase in surplus. In related work that studies labor market frictions in developing countries, evaluations of job search assistance interventions tend to find impacts primarily on the extensive margin of overall employment probability (Abebe et al., 2017a; Abel et al., 2016).[68] By contrast,

[68]These studies specifically target unemployed workers, and so it is natural to expect any impacts to be on

in our context we show that impacts are concentrated on the allocation of labor and on the intensive margin of earnings.

6.1 Impacts on Sorting

To study treatment effects on sorting, we use data on the outcome of the intervention for the sample of 515 matches that took place. This allows us to verify whether the certificates change the allocation of workers to firms in our sample. Table 7 reports the results of match-level regressions analogous to equation (1), where the outcomes are: (i) whether the worker was made a job offer by the matched firm (columns 1-4); (ii) whether the worker was hired by the matched firm (columns 5-8). Again, we focus on treatment effects for the average worker, since our workers are positively selected on soft skills, so that we do not expect significant heterogeneity by skills and we interpret the experimental results are referring to type-h workers.[69]

Columns 1 and 5 show no significant impact on job offers and hires for matches with the average manager. This result is in line with the evidence in Section 4, where we found that the average manager did not respond to the certificates in terms of beliefs. However, this result masks substantial heterogeneity: columns 2 and 6 show a positive and large treatment effect on both offers and hires in matches with high ability managers. Focusing on hires, column 6 shows that the probability of a match with high ability managers turning into a hire increases by 13.4pp in Treatment, relative to a Control mean of 8% (a result significant at the 1% level). This is in line with Prediction 1, which states that certificates should lead to an increase in the probability of a hire between type-h workers and type-H firms.

According to Prediction 1, we should further see a *decrease* in the probability that type-h workers are employed at firms of type L, as type-h workers should become more likely to reject offers at these firms. Column 3 shows no change in job offers from low ability managers, and column 7 shows a *reduction* in the probability that a worker is employed by a low ability manager. The estimate is not significant at conventional levels, and we cannot rule out even a relatively large decrease in hires of 11-13pp. The estimates in column 3 and 7 provide some evidence that, if anything, the Treatment *reduces* the probability that a worker accepts a job offer by a low ability manager, in line with Prediction 1. As shown by the *p-value* in column 8, the treatment effects on hires are significantly different between the two groups of managers, at the 5% level.[70] Taken together, the results from Table 7 show that the certificates lead to

the employment margin.

[69]Heterogeneous effects by worker skills are reported in the Online Appendix, and again confirm that there is no strong evidence of heterogeneity along this dimension.

[70]To confirm that owner's cognitive ability is the key dimension of heterogeneity, in Appendix Table A8 we again run regressions like (1) on the full sample of matches that took place, but where we allow for interactions between Treatment and a number of firm and manager characteristics all at the same time. The results confirm that owner's ability is the only significant source of heterogeneity.

a change in the allocation of jobs, corresponding to an increase in sorting between high ability managers and the experimental workers, who are positively selected on soft skills.

6.2 Impacts on Employment

Prediction 2 states that the impact of the certificates on overall employment probability of workers of type h is ambiguous, as these workers are more likely to get employed by type-H firms, but are also less likely to accept a job offer from a type-L firm. Column 5 of Table 7 shows that indeed the introduction of the certificates does not lead to a change in the overall probability of employment in the matching intervention: the estimate of the Treatment effect in column 5 is close to zero and not significant.

We validate these null impacts on employment by studying treatment effects on employment outcomes in the two years post intervention. We can expect an impact over this time period because workers were left with a copy of the certificate after the initial matching intervention, which they can use as a credible signal in their job search. Table 8 reports the results of worker-level regressions analogous to (2) where we pool observations from the two follow-up surveys. Taken together, the table shows very little evidence of any impact of the certificates on employment outcomes: workers in Treatment are no more likely to be in wage or self-employment at follow-up (columns 1 and 2). Also, there is no effect on hours worked in the last job (columns 3-6).[71] The results in Table 8 confirm the evidence from Table 7 that the Treatment is changing the allocation of workers to firms, but is not increasing overall employment.[72]

6.3 Impacts on Earnings

The overall number of hires in the matching intervention was low: less than 50 workers were hired across the two experimental groups.[73] Therefore, while the matching intervention is useful for studying the impact of the certificates on sorting, the low take-up makes it difficult to study wages at the matched firm. Nevertheless, in Appendix Table A9 we show suggestive evidence that earnings are higher in Treatment, though the estimates are imprecise due to the small sample size. While noisy, these results are in line with Prediction 3, which states that the certificates should increase wages for type-h workers, since high ability managers are willing to pay these workers more, and the outside option of these workers has increased.

[71] Columns 4 and 6 of Table 8 reveal that conditional on employment, workers in Control tend to work 50-60 hours a week. So there is not much scope for the Treatment to increase the intensive margin of hours worked, as the average employed worker in Control is working full-time.

[72] Results reported in the Online Appendix show that we do not find heterogeneous effects on employment or earnings by whether the worker was initially matched to a high or low ability manager.

[73] This result is in line with recent evaluations of matching interventions such as Abebe et al. (2017b), Alfonsi et al. (2017) and Groh et al. (2015), which also find very limited take-up. Taken together, these results confirm that small firms in developing countries do not face particular challenges in *meeting* workers.

We further probe this finding on earnings by running earnings regressions which include *all* workers in the two years post intervention, regardless of the outcomes of the matching intervention. The results are reported in Table 9. Column 1 confirms that there is no impact of Treatment on the *share* of individuals working for pay. In columns 2-7 the dependent variable is total earnings in the last month prior to the survey. Column 2 shows that when all workers are included in the regression, so that individuals with no earnings are assigned a value of zero for the dependent variable, we find an 8% increase in total earnings, but this is not significant at conventional levels.[74] Columns 4-6 report the results of quantile regressions on the full sample, and show that there is a positive and significant (at the 10%) treatment effect on earnings at the 75th quantile. Figure 5 reports the results of quantile regressions along the entire distribution of earnings, and confirms a positive and significant treatment effect in the upper quartile of the earnings distribution.[75] Finally, since we showed that Treatment does not affect selection into paid employment, in column 7 of Table 9 we report impacts for the sample of workers with positive earnings:[76] we find that the Treatment leads to an increase of about $7 per month, a result significant at the 5% level, and corresponding to an 11% increase in monthly earnings, relative to the Control mean.[77]

7 Policy Implications

In this section we discuss the main policy implications of this study. First, we discuss the cost-benefit considerations of introducing a mandatory certification policy on soft skills. This is an interesting exercise because we documented that those workers that decided not to participate in the intervention are *negatively* selected on soft skills, and so, as made clear by the model, they could be negatively affected by the certificates. Therefore, even though we showed that the experimental workers benefit in terms of higher earnings, it is possible that some workers at the low end of the skill distribution might be made worse off by a mandatory certification

[74] Applying the inverse hyperbolic sine transformation in column 3 to reduce the incidence of outliers (De Mel et al., 2018) also shows a positive but not significant impact.

[75] The *x-axis* in the figure starts at the 20th quantile since about 25% of the observations have zero earnings.

[76] Running regressions on earnings conditional on employment is problematic if the Treatment affects *selection* into employment (Lee, 2009). However, in this case we know that the Treatment does not change the *share* of workers employed (Table 9, column 1). Also, Appendix Table A10 shows that the sample of workers employed at follow-up remains well balanced on baseline characteristics, across Treatment and Control. Importantly, the sample remains balanced on skills, and on expected earnings at baseline, a very strong predictor of earnings at follow-up. Normalized differences between Treatment and Control are also remarkably small, and the observable worker characteristics are all jointly not significant in predicting treatment assignment at follow-up. These results shows that the Treatment is also not changing the *composition* of workers in employment, and so limit concerns that estimating treatment effects on earnings conditional on employment captures selection effects in this case.

[77] Appendix B shows that the earnings impacts are robust to: i) applying a log transformation to reduce the influence of outliers; ii) excluding control variables; iii) reweighing observations using inverse probability weights (IPW) to correct for attrition (Wooldridge, 2010). Appendix B also shows that we do not find significant evidence of the impacts on earnings varying between first and second follow-up.

policy. Second, we discuss why soft skills certificates are not already provided by the market, and consider the potential for government intervention in this area.

7.1 Introducing a Mandatory Certification Policy

Appendix C presents the cost-benefit analysis for those eligible workers who accepted to participate in the intervention.[78] As this was a relatively inexpensive intervention, with a cost per-worker of $19, Appendix C shows that the certificates were cost-effective at increasing earnings. We now consider how the cost-benefit analysis would change if the intervention was extended to include also those 22% of the eligible workers who decided *not* to participate when given the opportunity to do so. The analysis in Section 2 shows that these workers are *negatively selected* on soft skills, and the model highlights that it is possible that these type-l workers could be hurt by a mandatory certification policy. Therefore, we perform a bounding exercise, and show how the cost-benefit calculations for the average *eligible* worker change as those 22% of workers are allowed to experience a (conjectured) reduction in their earnings.

Figure 6 reports the results. On the *x-axis* we report a possible range of conjectured treatment impacts on earnings for those workers that initially selected out of the experiment. We assume that in the absence of Treatment, those eligible workers who decided not to participate would have experienced the same outcomes as the participating workers in Control, so that their mean monthly earnings would have been $47.2.[79] The *y-axis* shows the benefits/cost ratio for the average *eligible* worker, under the various scenarios. The gray dotted line corresponds to our preferred benefits/cost ratio estimate for the average *participating* worker (taken from Appendix C). The red dotted line is the break-even point, where benefits equal costs. The figure shows that if those workers that selected out would experience a loss of less than 20% of their earnings as a result of the certificates, the intervention would still be cost-effective for the average *eligible* worker. However, if the earnings losses for this group of workers were larger than 20% of their earnings, then the benefits/cost ratio would fall below one. In the limit case in which all these workers became unemployed as a result of the intervention, the benefits/cost ratio would show very negative results. In summary, this exercise shows that while workers with higher skills are made better off by the intervention, it is possible that workers with lower skills could be hurt by policies which increase information on skills in the labor market.

[78] For the cost-benefit analysis we assume zero impacts on firms. Appendix Table A12 confirms that we do no find any impacts of the intervention on firm size at follow-up (and this does not vary by the ability of managers). This is not surprising, given the low take-up in the matching intervention. Our data does not allow us to study treatment effects on profits: while overall attrition in our firm sample is low, information on profits is missing for about half the sample, who refused to prove this information. If the intervention produces productivity gains through the improved allocation of labor, then setting these to zero creates a lower bound to the benefits of the intervention on the firm-side. Providing more direct evidence on the firm-side impacts of certification interventions remains an important area for future research.

[79] We did not follow-up with workers who selected out of our intervention, so we do not observe their earnings.

7.2 Why Are Soft Skills Certificates Not Provided by the Market?

The results in Appendix C show that the certificates are cost-effective at increasing the earnings of participating workers. A natural question is then why soft skill certificates are not already provided by the market, given that these are valued by at least some workers and firms. We can rule out lack of demand by workers. At first follow-up we asked workers in Control – who never saw the results of their skills assessments – for their willingness to pay for certificates similar Treatment ones.[80] We find that workers in Control would be willing to pay $18 on average for the certificates, corresponding to 44% of their monthly earnings.[81] So their willingness to pay is substantial, and interestingly, very similar to the cost of the certificates ($19).

Since at least some workers would be willing to pay for the certificates, why are the certificates not provided by an entrepreneur? One important barrier is risk, as the profitability of this activity relies on building a reputation for providing truthful information. We were able to overcome credibility concerns because BRAC is the largest NGO in Uganda and has a strong reputation, but a new market entrant might take years to establish credibility. Providing soft skills certificates might also not be profit maximizing for VTIs: as discussed, about 20% of the eligible workers declined the opportunity to participate in the signaling program, and this is consistent with them realizing they would not have benefited from it. Therefore, if vocational institutes started advertising new certificates on soft skills, this might affect the enrollment decisions of students in the first place, potentially reducing VTI profits.

In summary, it is unclear that any private agent has enough incentives to create and sell certificates on soft skills, even though these are valuable to at least some firms and workers. On the potential of the government to produce this kind of information, we note that concerns related to scalability are not first order: while it is true that psychometric tests or behavioral games such as trust games would be difficult to scale up (since workers could learn how to "game" such tests), relying on teacher surveys is a scalable alternative. When thinking instead about the incentives of the government to produce this kind of information, our results show that while more information on skills can improve the allocation of workers to firms and overall productivity, it is possible that some workers with low skills might lose out from this. The willingness of the government to step in and provide this information would then depend on the welfare function the government is trying to maximize and on political economy considerations on the trade-off between equity and efficiency in the labor market.

[80] Willingness to pay was elicited using a "take-it-or-leave-it" (TIOLI) approach: workers were presented with options to purchase the soft skill certificate at a series of prices. We use the highest price indicated by workers as a measure of the lower bound of their willingness to pay. For more details on measuring willingness to pay in the field, see, for instance, Berry et al. (2015).

[81] This result is in contrast with Abel et al. (2016), who show that lack of demand by workers is an important factor limiting usage of reference letters by disadvantaged job seekers in South Africa.

8 Conclusion

This paper studies how lack of credible information on the skills of workers affects matching in the labor market. We do so using a randomized experiment that aims to understand how employers and job-seekers react to the disclosure of certificates on the non-cognitive skills of workers during recruitment. Our main finding is that both sides of the market respond to the certificates in terms of beliefs: workers increase their labor market expectations, while managers who value soft skills revise upwards their assessments of the skills of workers. We show that this two-sided reaction leads to a more efficient allocation of labor by allowing firms to better screen productive workers, and by allowing workers to better signal their skills to potential employers. As a result of better matching, earnings increase in our experimental sample of employed workers in the two years after the intervention.

Our study extends a growing literature on labor market frictions in developing countries. On the worker side, we complement the analysis in Alfonsi et al. (2017), who also study the Ugandan labor market. Our results confirm that one important reason why formal vocational training has higher returns than informal on-the-job training in their context is that the certification component of vocational training allows workers to match to better firms and obtain higher wages, relative to uncertified on-the-job trainees. On the firm side, we complement the work by Hardy and McCasland (2017), who find that a government-run apprenticeship program in Ghana increases profits by helping firms identify high skilled workers. We contribute by showing that skills certificates lead to an improvement in the allocation of labor in a similar context.

Our results speak to the importance of credible information in reducing mismatch and increasing productivity, and so contribute to a classic literature on matching and learning in the labor market (Jovanovic, 1979; Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pallais, 2014). Our finding that soft skill certificates are valuable to at least some workers and firms raises the question of why these signals are not already provided by the market in Uganda and other developing countries: we argue that risk is an important factor, as any private provider would have to build a reputation for providing truthful information, which might be difficult where corruption is pervasive and information difficult to verify. Also, vocational institutes might not be willing to introduce soft skills assessments if this affects the enrollment of workers with lower skills in these programs. These considerations open up a rich set of research questions on the incentives faced by private and public providers to generate labor market information in developing countries. Answering these questions would provide important insights into the functioning of both labor and education markets in low income countries, and so is something worth attempting in future research.

References

Abebe, G., S. Caria, M. Fafchamps, P. Falco, S. Franklin, and S. Quinn (2017a): "Anonymity or Distance? Job Search and Labor Market Exclusion in a Growing African City," mimeo.

——— (2017b): "Job Fairs: Matching Firms and Workers in a Field Experiment in Ethiopia," mimeo.

Abel, M., R. Burger, and P. Piraino (2016): "The Value of Reference Letters - Experimental Evidence from South Africa," mimeo.

Adhvaryu, A., N. Kala, and A. Nyshadham (2017): "The Skills to Pay the Bills: Returns to On-the-job Soft Skills Training," mimeo.

Alfonsi, L., O. Bandiera, V. Bassi, R. Burgess, I. Rasul, M. Sulaiman, and A. Vitali (2017): "Tackling Youth Unemployment: Evidence from a Labor Market Experiment in Uganda," mimeo.

Altonji, J. G. and C. R. Pierret (2001): "Employer Learning and Statistical Discrimination," *The Quarterly Journal of Economics*, 116, 313–350.

Autor, D. H. and D. Scarborough (2008): "Does Job Testing Harm Minority Workers? Evidence from Retail Establishments," *The Quarterly Journal of Economics*, 123, 219–277.

Bandiera, O., R. Burgess, N. Das, S. Gulesci, I. Rasul, and M. Sulaiman (2017): "Labor Markets and Poverty in Village Economies," *The Quarterly Journal of Economics*, 132, 811–870.

Banerjee, A. V. and E. Duflo (2014): "Do Firms Want to Borrow More? Testing Credit Constraints Using a Directed Lending Program," *The Review of Economic Studies*, 81, 572–607.

Beaman, L., J. Magruder, and J. Robinson (2014): "Minding Small Change Among Small Firms in Kenya," *Journal of Development Economics*, 108, 69–86.

Berry, J., G. Fisher, and R. P. Guiteras (2015): "Eliciting and Utilizing Willingness-to-Pay: Evidence from Field Trials in Northern Ghana," *mimeo*.

Bowles, S. and H. Gintis (1976): *Schooling in Capitalist America*, vol. 57, New York: Basic Books.

Bowles, S., H. Gintis, and M. Osborne (2001): "Incentive-Enhancing Preferences: Personality, Behavior, and Earnings," *The American Economic Review*, 91, 155–158.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2011): "Robust Inference with Multiway Clustering," *Journal of Business and Economic Statistics*, 29, 238–249.

Card, D., A. R. Cardoso, J. Heining, and P. Kline (2018): "Firms and Labor Market Inequality: Evidence and Some Theory," *Journal of Labor Economics*, 36, 13–70.

Caria, S. and P. Falco (2016): "Do Employers Trust Workers Too Little? An Experimental Study of Trust in the Labour Market," mimeo.

Chade, H. (2006): "Matching with Noise and the Acceptance Curse," *Journal of Economic Theory*,

129, 81–113.

Chade, H. and J. Eeckhout (2017): "Stochastic Sorting," *Society for Economic Dynamics*, mimeo.

Chaudhury, N., J. Hammer, M. Kremer, K. Muralidharan, and F. H. Rogers (2006): "Missing in Action: Teacher and Health Worker Absence in Developing Countries," *The Journal of Economic Perspectives*, 20, 91–116.

De Mel, S., D. McKenzie, and C. Woodruff (2008): "Returns to Capital in Microenterprises: Evidence from a Field Experiment," *The Quarterly Journal of Economics*, 123, 1329–1372.

——— (2018): "Labor Drops: Experimental Evidence on the Return to Additional Labor in Microenterprises," *American Economic Journal: Applied Economics*, forthcoming.

Deming, D. J. (2017): "The Growing Importance of Social Skills in the Labor Market," *The Quarterly Journal of Economics*, 132, 1593-1640.

Deming, D. J., N. Yuchtman, A. Abulafi, C. Goldin, and L. F. Katz (2016): "The Value of Postsecondary Credentials in the Labor Market: An Experimental Study," *The American Economic Review*, 106, 778–806.

Duflo, E., R. Glennerster, and M. Kremer (2007): "Using Randomization in Development Economics Research: A Toolkit." *Handbook of development economics*, 4, 3895–3962.

Eeckhout, J. and P. Kircher (2011): "Identifying Sorting—In Theory," *The Review of Economic Studies*, 78, 872–906.

Farber, H. S. and R. Gibbons (1996): "Learning and Wage Dynamics," *The Quarterly Journal of Economics*, 111, 1007–1047.

Foster, A. D. and M. R. Rosenzweig (2008): "Economic Development and the Decline of Agricultural Employment," *Handbook of development economics*, 4, 3051–3083.

Groh, M., D. McKenzie, N. Shammout, and T. Vishwanath (2015): "Testing the Importance of Search Frictions and Matching Through a Randomized Experiment in Jordan," *IZA Journal of Labor Economics*, 4, 1.

Hardy, M. and J. McCasland (2017): "Are Small Firms Labor Constrained? Experimental Evidence from Ghana," mimeo.

Heckman, J. J., J. Stixrud, and S. Urzua (2006): "The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior," *Journal of Labor Economics*, 411–482.

Hoffman, M., L. B. Kahn, and D. Li (2017): "Discretion in Hiring," *The Quarterly Journal of Economics*, forthcoming.

Hsieh, C.-T. and B. A. Olken (2014): "The Missing "Missing Middle"," *The Journal of Economic Perspectives*, 28, 89–108.

ILO (2017): *World Employment and Social Outlook: Trends 2017*, International Labour Office.

Jayachandran, S. (2006): "Selling Labor Low: Wage Responses to Productivity Shocks in Develop-

ing Countries," *The Journal of Political Economy*, 114, 538–575.

John, O. P. and S. Srivastava (1999): "The Big Five Trait Taxonomy: History, Measurement, and Theoretical Perspectives," *Handbook of personality: Theory and research*, 2, 102–138.

Jovanovic, B. (1979): "Job Matching and the Theory of Turnover," *The Journal of Political Economy*, 85, 972–990.

Kahn, L. B. and F. Lange (2014): "Employer Learning, Productivity, and the Earnings Distribution: Evidence from Performance Measures," *The Review of Economic Studies*, 81, 1575–1613.

Kaur, S. (2017): "Nominal Wage Rigidity in Village Labor Markets," *The American Economic Review*, forthcoming.

Lee, D. S. (2009): "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *The Review of Economic Studies*, 76, 1071–1102.

MacLeod, W. B., E. Riehl, J. E. Saavedra, and M. Urquiola (2017): "The Big Sort: College Reputation and Labor Market Outcomes," *American Economic Journal: Applied Economics*, 9, 223-261.

McCall, J. J. (1970): "Economics of Information and Job Search," *The Quarterly Journal of Economics*, 113–126.

McKenzie, D. J. (2017): "How Effective Are Active Labor Market Policies in Developing Countries? A Critical Review of Recent Evidence," World Bank Policy Research Working Paper 8011.

Muralidharan, K., P. Niehaus, and S. Sukhtankar (2017): "General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India," mimeo.

Pallais, A. (2014): "Inefficient Hiring in Entry-Level Labor Markets," *The American Economic Review*, 104, 3565–3599.

Schönberg, U. (2007): "Testing for Asymmetric Employer Learning," *Journal of Labor Economics*, 25, 651–691.

Spence, M. (1973): "Job Market Signaling," *The Quarterly Journal of Economics*, 87, 355–374.

Thomas, D., F. Witoelar, E. Frankenberg, B. Sikoki, J. Strauss, C. Sumantri, and W. Suriastini (2012): "Cutting the Costs of Attrition: Results from the Indonesia Family Life Survey," *Journal of Development Economics*, 98, 108–123.

UN (2017): *UN Desa Population Division*, World Population Prospects: The 2017 Revision Population Database.

Wooldridge, J. M. (2010): *Econometric Analysis of Cross Section and Panel Data*, MIT press.

Table 1: Firm descriptives from initial census

	Mean (1)	SD (2)	Median (3)
A. Owner and firm characteristics			
Owner is female	.397	.490	0
Number of employees	5.88	7.14	4
Business is registered	.938	.242	1
Age of business [Years]	7.09	5.90	5
B. Sector			
Carpentry	.138	.345	0
Catering	.157	.364	0
Hairdressing	.302	.459	0
Motormechanics	.123	.328	0
Tailoring	.082	.275	0
Welding	.198	.399	0
C. Region			
Kampala	.425	.495	0
North	.124	.329	0
East	.270	.444	0
West	.182	.386	0

Notes: The table uses data from the initial census of 1,086 firms conducted for the job placement intervention. The census was conducted in 17 urban areas of Uganda, and targeted all firms employing at least 2 employees and operating in six sectors: carpentry, catering, hairdressing, motor-mechanics, tailoring and welding.

Table 2: Worker descriptives from initial census

	Mean (1)	SD (2)	Median (3)
A. Worker characteristics			
Age [Years]	20.2	2.50	20
Female	.449	.498	0
Completed prior education [Years]	10.3	2.05	11
Course duration [Years]	1.41	.934	2
Ever employed	.260	.439	0
Has a job waiting at the end of training	.085	.280	0
Plans to look for wage employment	.629	.483	1
Ideal firm size <= 20 employees	.605	.489	1
B. Sector of training			
Carpentry	.072	.259	0
Catering	.129	.335	0
Hairdressing	.266	.442	0
Motormechanics	.292	.455	0
Tailoring	.179	.384	0
Welding	.062	.242	0

Notes: The table uses data from the census of the 1,011 workers eligible to participate in the job placement intervention. The census took place at 15 partner Vocational Training Institutes throughout Uganda, and included all workers currently receiving training in one of the following six sectors: carpentry, catering, hairdressing, motor-mechanics, tailoring, welding, and expected to graduate by February 2015.

Table 3: Selection into the program - Workers

OLS regression coefficients
Robust standard errors in parentheses

Dependent variable:	Worker included in the experimental sample [Yes=1]	
	(1)	(2)
A. Skills		
Cognitive test score	.009	-.001
	(.006)	(.006)
Extraversion	.026*	.019
	(.013)	(.013)
Agreeableness	.029*	.026*
	(.015)	(.015)
Conscientiousness	.031*	.032**
	(.016)	(.016)
Neuroticism (reversed scale)	.030**	.031**
	(.015)	(.015)
Openness to Experience	-.021	-.005
	(.017)	(.017)
B. Other worker characteristics		
Age		.010
		(.036)
Age squared		-.000
		(.001)
Female		.160***
		(.050)
Completed prior education		.008
		(.008)
Course duration		.009
		(.017)
Ever employed		-.055*
		(.031)
Mean of dependent variable	.775	.775
Sector of training dummies	No	Yes
P-value on F-test of joint significance of Big 5 variables	[.000]	[.002]
P-value on F-test of joint significance of sector dummies		[.004]
R-squared	.045	.101
Number of observations (workers)	1,011	1,011

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. The Table uses data from the initial census of trainees for the job placement intervention. Standard errors are adjusted for heteroskedasticity. The regression in column 2 additionally controls for 5 sector dummies. The cognitive test score is defined as the number of right answers on a 10-item Raven matrices test, and so the corresponding variable goes from 0 to 10. Extraversion, Agreeableness, Conscientiousness, Neuroticism and Openness to Experience are measured through a 10-item Big-5 scale. Each of these variables takes values 1 to 5, where 5 indicates a higher level of the skill. The Neuroticism variable is recoded so that a higher level of the variable corresponds to a lower level of Neuroticism (i.e. to more self-control). All regressions further control for dummies for missing values in each of the independent variables.

Table 4: Impacts on firm owner beliefs about matched workers

OLS regression coefficients, standard errors adjusted for two-way clustering in parentheses

P-values on t-test of equality of coefficients for High and Low Ability Owners in brackets

Dependent variable:	**Matched worker reported as MORE SKILLED than usual applicant [Yes=1]**							
Sample of firm owners:	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (2) = (3)**	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (6) = (7)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treatment	.001	.105**	-.040	[.036]				
	(.025)	(.045)	(.045)					
Fail grade on at least one skill X Treatment					-.015	.072	-.026	[.267]
					(.031)	(.050)	(.065)	
Pass grade on all skills X Treatment					.025	.174**	-.064	[.015]
					(.038)	(.072)	(.065)	
Mean of dep. var. in Control group	.097	.079	.115		.097	.079	.115	
P-value Fail = Pass					[.408]	[.194]	[.699]	
Number of observations (matches)	515	232	222		515	232	222	

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the matching surveys are reported. Standard errors are adjusted for two-way clustering (at the level of both the firm and the worker), following the procedure in Cameron et al. [2011]. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. All regressions also control for the following firm characteristics measured at baseline: dummy for female owner; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees. Firm owners who scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners who scored below the median are assigned to the Low Ability group. In columns 1 and 5 the sample includes all matches that took place. In columns 2 and 6 it is restricted to matches with High Ability firm owners that took place. In columns 3 and 7 it is restricted to matches with Low Ability firm owners that took place. The p-values in columns 4 and 8 are from similar OLS regressions estimated on the full sample of matches that took place and where each independent variable is interacted with the High Ability owner dummy. All regressions further control for dummies for missing values in each of the independent variables.

Table 5: Impacts on worker outside options

Standard errors in parentheses are robust in columns 3, 4 and 6, and clustered at the worker level in columns 1, 2, 5 and 7

	Panel A: Beliefs				Panel B: Behaviors		
	Monthly expected earnings [USD]	Expected probability of employment in the next six months (0 to 10 scale)	Expected bargaining over wages (standardized index)	Ideal job is in large firm [Yes=1]	Any casual work in the last week	Looked for a job in the public/ngo sector in the last year	Looked for a job in the last year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	7.95**	.283**	.231*	.067**	-.048*	.104***	-.018
	(3.12)	(.117)	(.131)	(.033)	(.026)	(.036)	(.025)
Mean of dep. var. in Control	114.5	5.53	0	.624	.323	.268	.749
Controls for baseline value of outcome	Yes	Yes	Yes	No	Yes	No	Yes
Uses data from first followup	Yes	Yes	No	No	Yes	No	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,326	1,349	663	668	1,350	674	1,350

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. Standard errors (in parentheses) are robust in columns 3, 4 and 6, and clustered at the worker level in columns 1, 2, 5 and 7. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. In column 1 the dependent variable is constructed as follows: respondents were asked to report: (i) their minimum expected earnings; (ii) their maximum expected earnings; (iii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings for each respondent. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted in January 2015 USD. The top 1% values of expected earnings are excluded. The dependent variable in column 3 is constructed using two variables from the worker second follow-up survey: a dummy for whether the worker would not accept a job without negotiating on the wage; a variable reporting how much the worker would expect wages to be influenced by negotation, on a 0-10 scale. The index is constructed by converting each of the two components into a z-score, averaging these and taking the z-score of the average. z-scores are computed using means and standard deviations from the control group. In column 4 the dependent variable is a dummy equal to one if the worker reported an ideal firm size on or above the median (that is, a firm size of at least 10 workers). Since at baseline all workers were enrolled at vocational training institutes and only 1% of them were currently doing in any paid work, for the employment outcomes we consider as baseline value of the outcome the expected probability of employment in the six months after graduation, as reported in the baseline survey. So we control for such baseline expected probability of employment in column 5. All regressions further control for dummies for missing values in each of the independent variables.

Table 6: Why do workers update their outside options?

OLS regression coefficients, robust standard errors in parentheses

	Average self-assessed soft skills grade (1 to 5 scale)		Average perceived returns to soft skills (0 to 10 scale)	Average perceived constraints in signalling skills (1 to 5 scale)
	(1)	(2)	(3)	(4)
Treatment	.017	.219	-.003	-.179**
	(.033)	(.168)	(.106)	(.087)
Average soft skills grade	.302**	.442**		
	(.137)	(.179)		
Average soft skills grade X Treatment		-.297		
		(.239)		
Mean of dep. var. in Control group	4.35	4.35	8.06	2.73
Controls for baseline value of outcome	No	No	No	Yes
Uses data from first followup	No	No	No	No
Uses data from second followup	Yes	Yes	Yes	Yes
Number of observations	673	673	673	673

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. Standard errors are adjusted for heteroskedasticity. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. The dependent variable in columns 1 and 2 is constructed as follows: at second followup workers were asked to self-assess themselves on the five soft skills measured in the baseline assessments and disclosed on the Treatment group certificates. They were asked to do so using the same A-E scale used in the baseline assessments. We compute the average self-assessed grade on the five skills and use this as dependent variable. The dependent variable in column 3 is constructed as follows: at second followup workers were asked how important each of the five soft skills measured in the baseline assessments and disclosed on the Treatment group certificates were for making someone a productive worker, using a 0-10 scale, where 0="Not important at all" and 10="Extremely important". We compute the average perceived return of the five skills. The dependent variable in column 4 is constructed as follows: workers were asked to report their perceived importance of constraints related to signaling skills in the labor market, using a 1 to 5 scale, where 1="Not a problem at all" and 5="A very important problem". Workers were asked this question separately for practical and soft skills. Note that this question was asked about soft skills in general, and so not specifically about the five soft skills measured in the baseline assessments and disclosed on the Treatment group certificates We compute the average importance of constraints related to signaling the two types of skills (practical and soft) and use this as dependent variable in column 4. All regressions further control for dummies for missing values in each of the independent variables.

Table 7: Impacts on sorting

OLS regression coefficients, standard errors adjusted for two-way clustering in parentheses
P-values on t-test of equality of coefficients for High and Low Ability Owners in brackets

Dependent variable:	Worker was made a job offer by the matched firm [Yes=1]				Worker was hired by the matched firm [Yes=1]			
Sample of firm owners:	All	High Ability Owners	Low Ability Owners	P-value (2) = (3)	All	High Ability Owners	Low Ability Owners	P-value (6) = (7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	.025	.154***	.005	[.124]	.001	.134***	-.023	[.032]
	(.047)	(.059)	(.081)		(.034)	(.048)	(.055)	
Mean of dep. var. in Control	.222	.136	.256		.116	.080	.105	
Number of observations (matches)	412	173	182		412	173	182	

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker followup surveys are reported. Standard errors are adjusted for two-way clustering (at the level of both the firm and the worker), following the procedure in Cameron et al. [2011]. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. All regressions also control for the following firm characteristics measured at baseline: dummy for female owner; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees. Firm owners who scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners who scored below the median are assigned to the Low Ability group. In columns 1 and 5 the sample includes all scheduled matches. In columns 2 and 6 it is restricted to matches with High Ability firm owners. In columns 3 and 7 it is restricted to matches with Low Ability firm owners. The p-values in columns 4 and 8 are from similar OLS regressions estimated on the full sample of matches and where each independent variable is interacted with the High Ability owner dummy. All regressions further control for dummies for missing values in each of the independent variables.

Table 8: Impacts on employment

OLS regression coefficients

Standard errors in parentheses are clustered at the worker level

Dependent variable:	Any work as employee in the last week	Any work as self-employed in the last week	Weekly hours worked in last job as employee		Weekly hours worked in last job as self-employed	
Sample of workers:	All	All	All	Positive hours	All	Positive hours
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.030	-.003	2.03	2.26	-1.16	-.489
	(.029)	(.024)	(1.96)	(1.53)	(1.61)	(2.95)
Mean of dep. var. in Control	.428	.211	37.0	61.1	16.0	51.9
Controls for baseline value of outcome	Yes	Yes	No	No	No	No
Uses data from first followup	Yes	Yes	Yes	Yes	Yes	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,350	1,350	1,347	816	1,349	411

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. Standard errors (in parentheses) are clustered at the worker level. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments and disclosed on the Treatment group certificates; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. Since at baseline all workers were enrolled at vocational training institutes and only 1% of them were currently doing in any paid work, for the employment outcomes we consider as baseline value of the outcome the expected probability of employment in the six months after graduation, as reported in the baseline survey. So we control for such baseline expected probability of employment in column 1 and 2. All regressions further control for dummies for missing values in each of the independent variables.

Table 9: Impacts on labor market earnings

OLS regression coefficients in columns 1-3 and 7

Quantile regression coefficients in columns 4-6

Standard errors clustered at the worker level in parentheses in columns 1-3 and 7

Bootstrapped standard errors in parentheses in columns 4-6

Dependent variable:	Any paid work in the last month	Total earnings in the last month [USD]	Inverse hyperbolic sine of total earnings in the last month [USD]	Total earnings in the last month [USD]			
Sample of workers:	**All**	**All**	**All**	**All**	**All**	**All**	**Positive earnings**
Estimation method:	**OLS**	**OLS**	**OLS**	**Q(25)**	**Q(50)**	**Q(75)**	**OLS**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Treatment	-.014	3.79	.034	.489	1.74	7.13*	7.19**
	(.025)	(3.22)	(.213)	(1.31)	(3.14)	(4.13)	(3.56)
Mean of dep. var. in Control group	.750	47.2	51.3	47.2	47.2	47.2	63.1
Controls for baseline value of outcome	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Uses data from first followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,338	1,329	1,338	1,329	1,329	1,329	988

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. OLS regressions are estimated in columns 1-3 and 7, where standard errors are clustered at the worker level. Quantile regressions are estimated in columns 4-6 (estimated at the 25th, 50th and 75th quantile of the distribution of total earnings, respectively), where standard errors are bootstrapped (with 600 replications). All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments and disclosed on the Treatment group certificates; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. In column 1 the dependent variable is a dummy equal to one if the worker conducted any paid work in the month prior to survey, and zero otherwise. In columns 2-7 the dependent variable is total labor earnings from all activities in the month prior to the survey. This variable takes value zero for workers with no labor earnings. Since at baseline all workers were enrolled at vocational training institutes and only 1% of them were currently doing in any paid work, for the employment outcomes we consider as baseline value of the outcome the expected probability of employment in the six months after graduation, as reported in the baseline survey. So we control for such baseline expected probability of employment in column 1. Similary, in columns 2-7 we consider as baseline value of the outcome expected earnings at baseline. Expected earnings are constructed as follows: respondents were asked to report: (i) their minimum expected earnings; (ii) their maximum expected earnings; (iii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings for each respondent. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted in January 2015 USD. The top 1% values of total earnings in the last month are excluded in columns 2 and 4-7. In column 7 the sample is restricted to observations with positive earnings at followup. All regressions further control for dummies for missing values in each of the independent variables.

Figure 1: Timeline

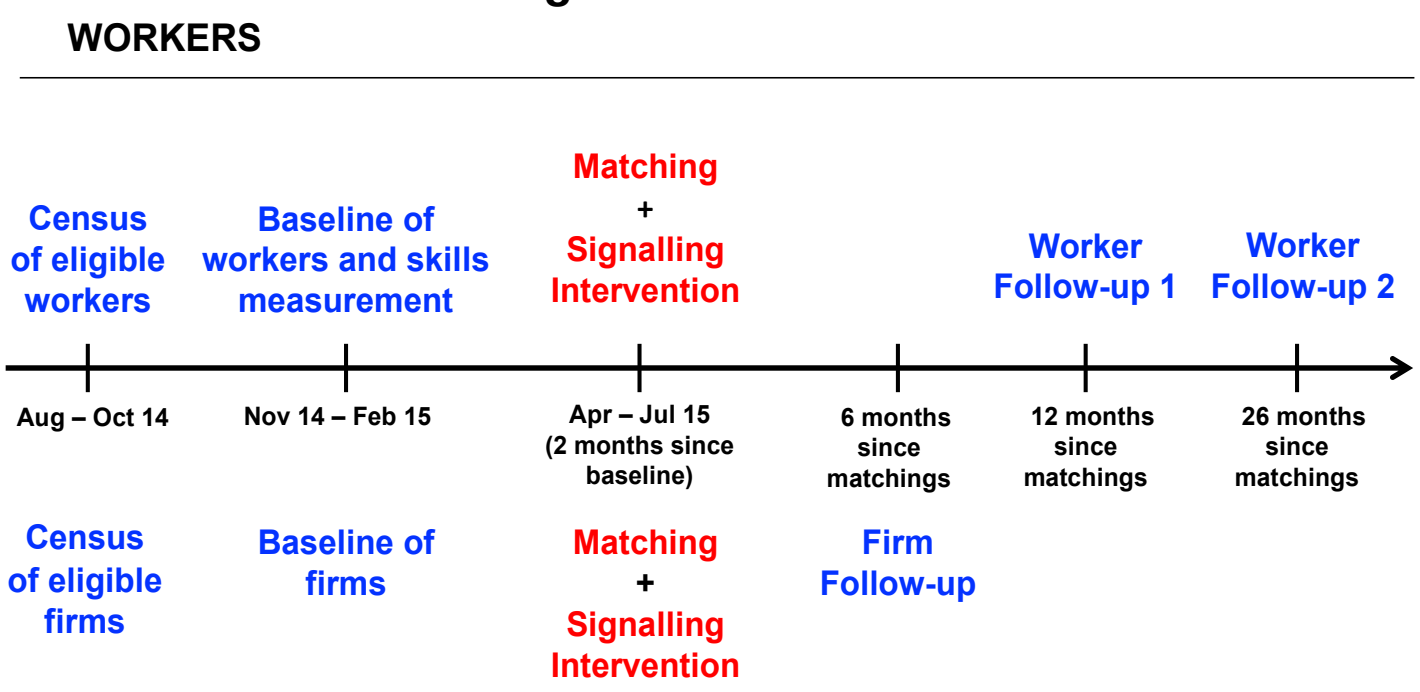
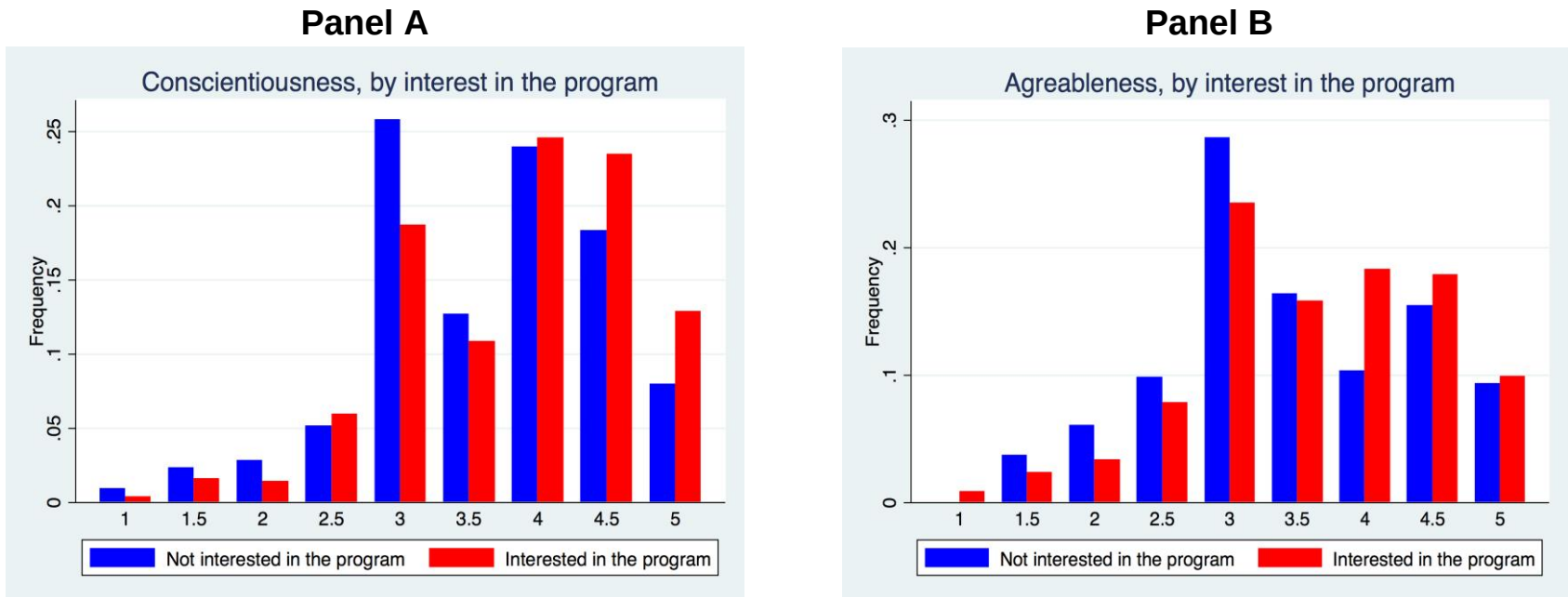
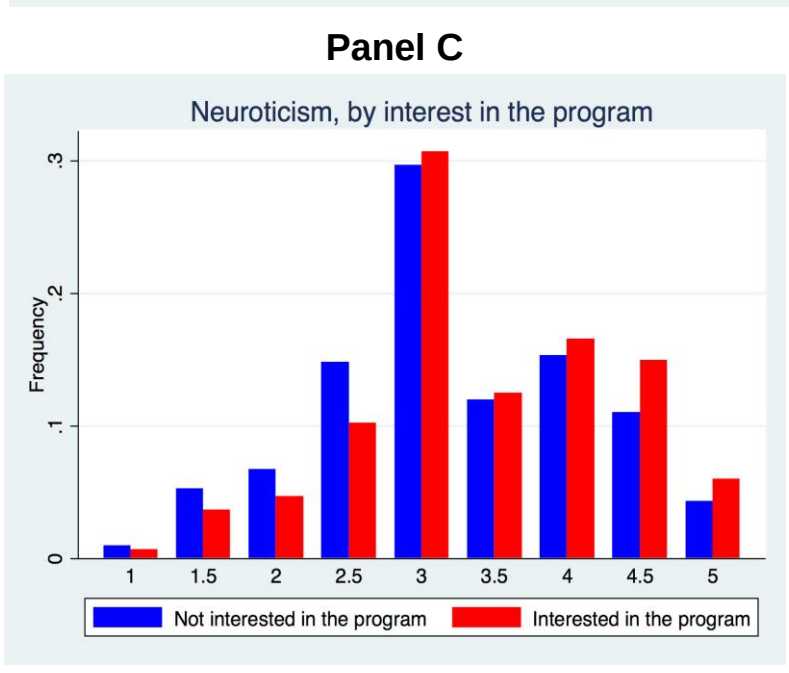


Figure 2: Distribution of worker soft skills, by participation in the program





Notes: Agreeableness, Conscientiousness and Neuroticism are measured using a 10-item Big-5 scale. The Neuroticism variable is recoded so that a higher level of the variable corresponds to a lower level of Neuroticism (i.e. to more self-control). The sample includes the 1011 trainees eligible for the intervention.

Figure 3: Summary of experimental design

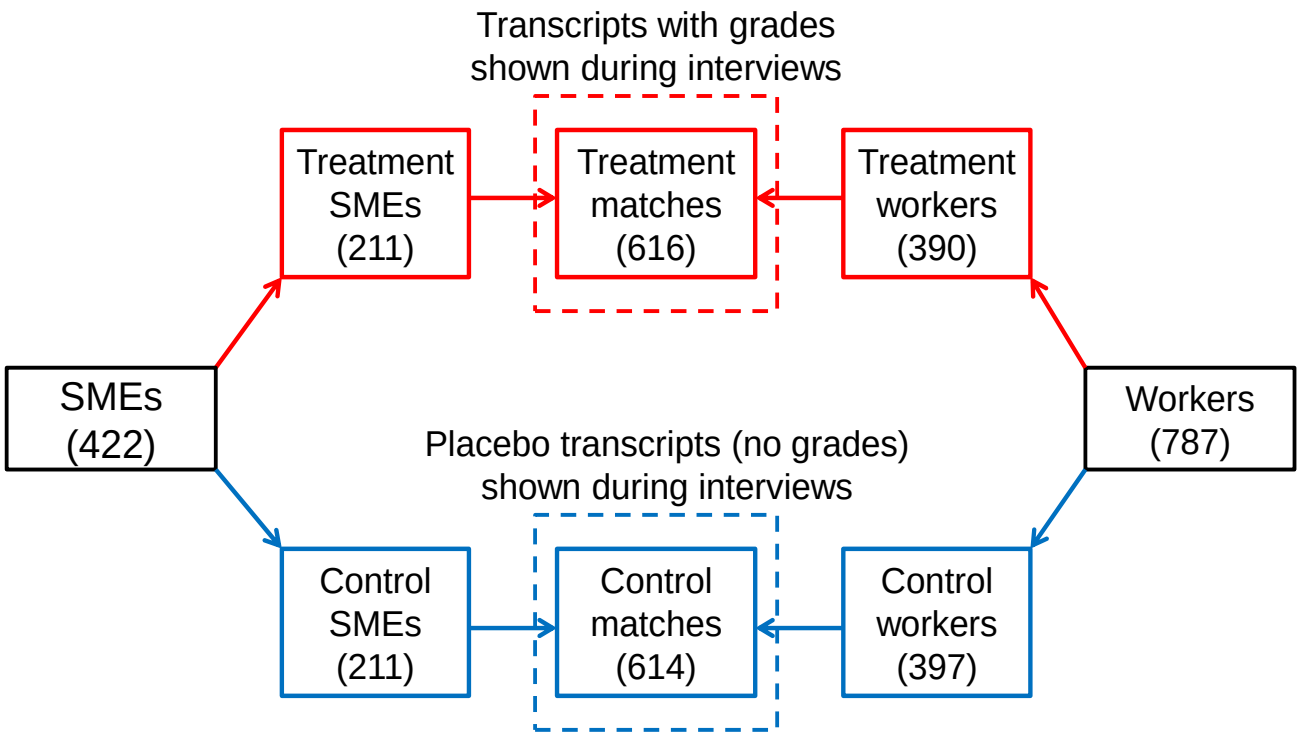


Figure 4: Compliance and attrition

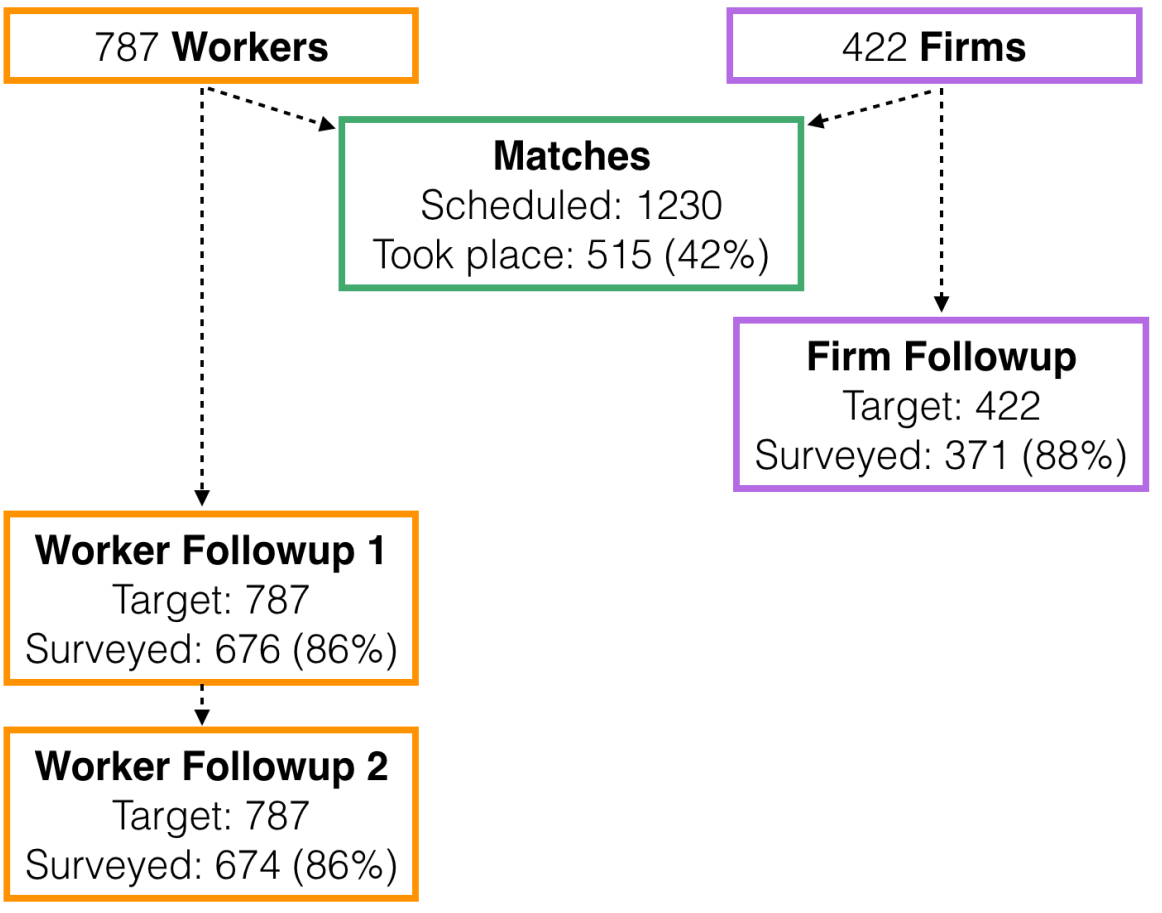
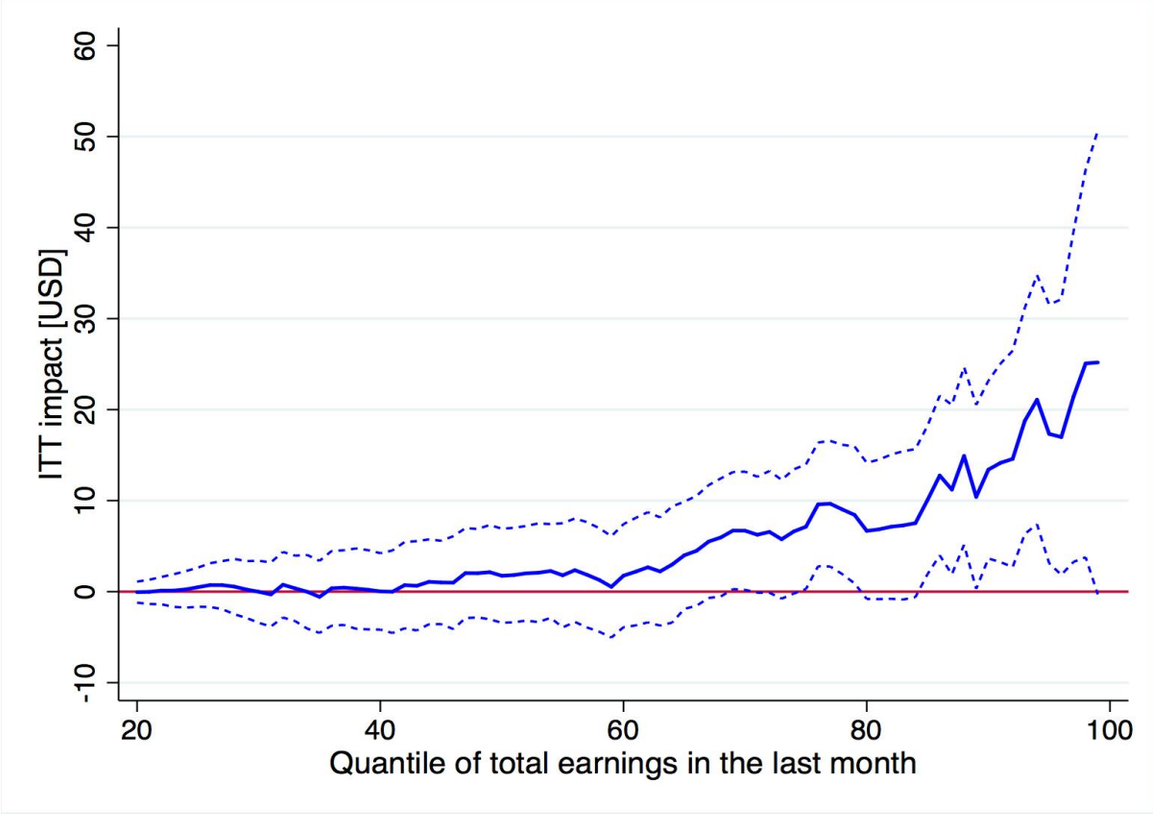
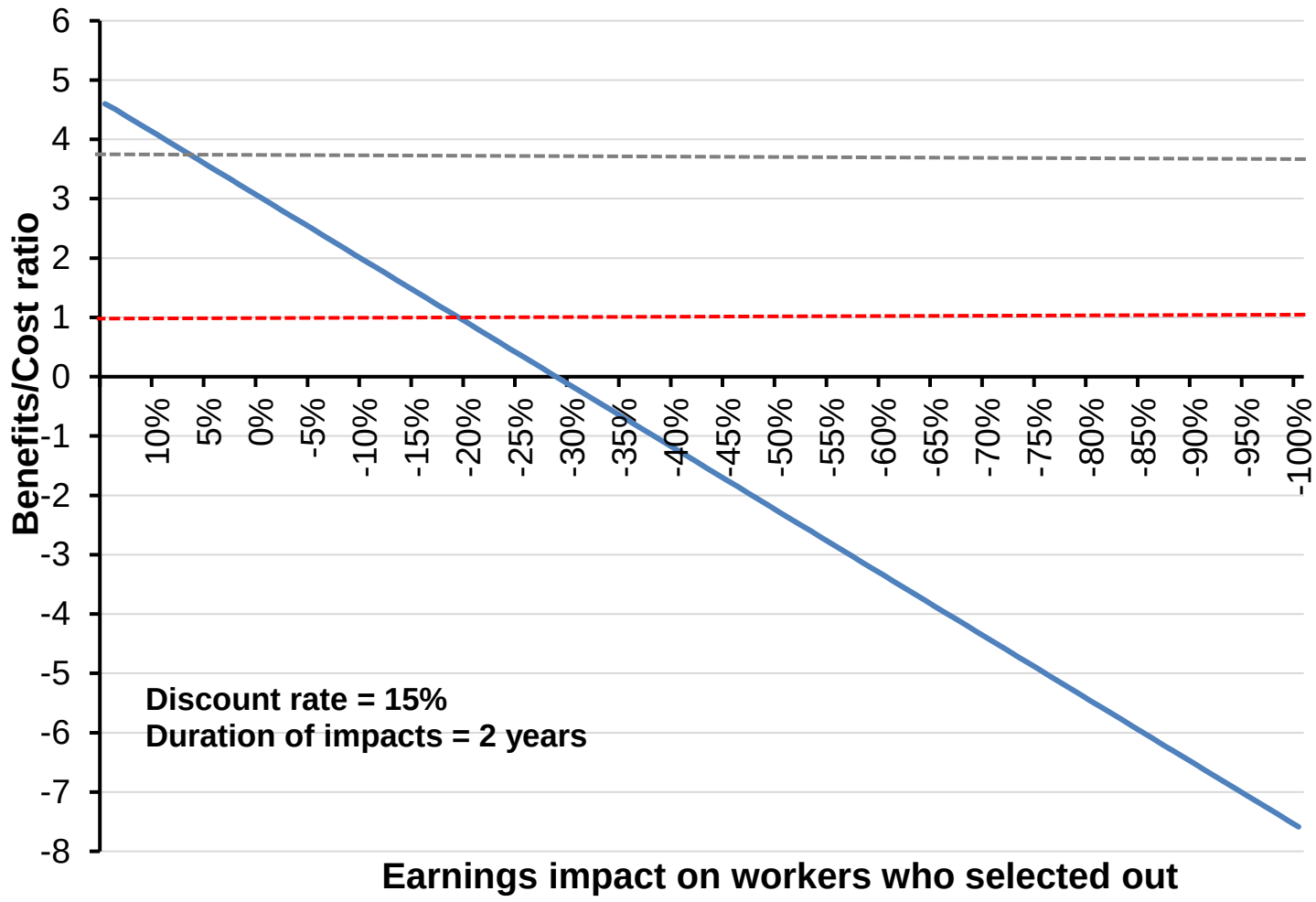


Figure 5: Impacts on post-intervention labor market earnings: Quantile regression evidence



Notes: The figure reports quantile regression estimates of treatment effects on total labor earnings from all activities in the month prior to the survey, with 90% confidence intervals. Standard errors are bootstrapped (with 600 replications) in these quantile regressions. The sample includes all workers from first and second followup. The regressions control for stratification variables (dummies for region and sector), a dummy for second followup and dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments and disclosed on the Treatment group certificates; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience; expected earnings at baseline. Expected earnings are constructed as follows: respondents were asked to report: (i) their minimum expected earnings; (ii) their maximum expected earnings; (iii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings for each respondent. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted in January 2015 USD. The top 1% values of total earnings are excluded.

Figure 6: Cost-benefit analysis of a mandatory certification policy



Notes: The figure considers how the cost-benefit analysis of a mandatory certification policy depends on the (conjectured) impacts on those workers who were invited to participate in the intervention, but decided not to participate (these are 22%of the trainees identified in the initial census). In particular, the blue line in the Figure shows the Benefits/ Cost ratio for the average worker in the initial census of trainees, as a function of the (conjectured) earnings impact on those workers who selected out. The Benefits/Cost ratio is defined as the NPV of the intervention over the total costs of the intervention at year zero, assuming that the benefits last 2 years, and that the social discount rate is 15%. For those workers who participated in the intervention, the Benefits/Cost ratio is taken from Table A13, and is equal to 3.87. This is the gray line in the Figure. For those workers who did not participate to the intervention, we consider a range of potential treatment effects on earnings representing what the effect of the program would have been for them, had they been forced to participate in the intervention. The conjectured range of treatment effects goes from +15% to -100%, and so also considers the extreme case in which the intervention would have crowded out entirely from the labor market those workers who decided not to participate to the intervention. The red line represents the break-even point at which benefits equal costs.

Appendix

A Key Facts about SMEs: Additional Details

The first key fact from the firm baseline survey is that soft skills are perceived as having relatively high returns. Firm owners were asked to rate on a 0 to 10 scale[82] how important different skills are in their firms. Figure A1 reports the average importance given to each skill.[83] While practical skills are reported as having the highest returns, soft skills are reported as the second most important skill, and more important than numeracy, literacy or theoretical skills.

The second key fact is that firm owners report stealing by their own employees and difficulties in observing the soft skills of workers among their main perceived constraints. Owners were asked to rate the importance of a range of potential constraints on a 1 to 5 scale.[84] We create an indicator variable for whether the firm owner reported a value of 4 or 5 on the importance scale of each constraint, and use these to compare their relative importance. Figure A2 shows that stealing by employees is reported as the most important constraint. At the same time, difficulties in assessing the soft skills of workers are reported as more important than lack of demand, access to electricity, difficulties in finding workers, or screening on practical skills.

The third key fact is that firm owners have relatively low priors on the distribution of soft skills among workers. Firm owners were asked to report how many potential workers out of 10 they thought had (i) a good level of practical skills, and (ii) a good level of soft skills. We compute the difference between these two variables for each owner, and plot the resulting CDF in Figure A3. The figure shows that as many as 80% of firm owners think that practical skills are relatively more common among potential workers. The fourth key fact is that it is common for firms to recruit workers that just show up and ask for a job/apply, without any prior connection or referral. Figure A4 shows that over one-third of the workers employed at baseline were hired in this way.

Finally, the fifth key fact is that there is substantial heterogeneity in the cognitive ability of firm owners, and that this is related to their profitability, their perceived returns to skills and their management style. Owners were asked to answer the same cognitive test administered to the trainees. Figure A5 reports the distribution of scores and shows that there is substantial variation.[85] In Table A2 we then create a dummy equal to one if the manager scored at the median or above on the cognitive test, and zero otherwise, and use this to compare how high and low ability managers differ along a number of dimensions. Panel B of Table A2 confirms that managers of higher ability: i) have higher profits; ii) value soft skills relatively more; iii) are better able to delegate tasks and iv) are less likely to be present at the firm premises every day and so engage in less supervision of their workers. However, Panel C shows that high ability managers recruit through similar channels and do not report fewer problems in screening workers nor different expectations on the skill distribution of potential employees. Finally, Panel C shows that firms owners of higher ability recruit workers through similar

[82]The scale goes from 0 = "Not important at all" to 10 = "Extremely important".

[83]Firm owners were asked to rate the importance of each of the Big 5 skills separately, and so in Figure A1 and Figure A2 we label as "soft skills" the average importance given to the Big 5 skills.

[84]The scale goes from 1 = "Not important at all" to 5 = "A very serious problem".

[85]The results of the cognitive test are missing for about 15% of owners, who refused to take the test.

channels as lower ability managers, so that the proportion of workers hired without any prior connection or referral does not vary between the two groups of managers.

Panel B of Table A2 shows that high ability owners earn about 46% higher profits (a result significant at the 10% level). To provide further support to the claim that high ability managers are more profitable, in Figure A6 we plot the residuals from a profit regression which controls for other firm and manager characteristics, splitting by firm owner type. The figure once again confirms that high ability owners have higher residual profits, even after the inclusion of control variables.

B Robustness of Earnings Impacts

In column 1 of Table A11 we consider the robustness of our results on labor market earnings to applying a log transformation to reduce the influence of outliers. Our estimated treatment effect is stable at around a 10% increase in earnings, and remains significant at the 10% level.

In our main regression specifications we control for the parsimonious set of worker and firm-level controls described in Section 3. We do so because these are correlated with our main outcome variables of interest, and so help improve the precision of our treatment effect estimates (Duflo et al., 2007). In column 2 of Table A11 we show robustness of the results on earnings to not controlling for any worker characteristics other than the baseline value of the outcome. The estimated coefficient (6.84) is very similar to the one in the specification with controls (7.19). The standard error is slightly larger when we remove the controls, but the estimated impact remains significant at the 10% level, thus alleviating concerns that the inclusion of controls is affecting our main conclusions in a significant way.

In our main regression specifications we do not correct for attrition, since we find no evidence that Treatment predicts attrition rates and that attrition introduces unbalancedness at follow-up. To further rule out concerns about attrition, we estimate a specification that reweighs the sample using inverse probability weights (IPW) (Wooldridge, 2010). This procedure is implemented in two steps. In the first step, we run a probit regression predicting whether an observation is included in the follow-up surveys, which controls for the same observables as our main specifications. In the second step, we reweigh the sample of individuals observed in the follow-up surveys by the inverse of the predicted probabilities from the first step, and then run treatment effect regressions using the reweighed sample. In the first step we add as instruments to predict attrition specific characteristics of the enumerators assigned to contact the respondents (Thomas et al., 2012), and in particular their gender, education level and age (enumerators were randomly assigned to respondents). The estimates for worker earnings are reported in column 3 of Table A11. If anything, the estimated treatment effect on earnings is *larger* once we reweigh the sample using this procedure, and remains significant at the 5% level.

Finally, in column 5 of Table A11 we allow treatment effects on earnings to differ by first and second follow-up. To do so, we interact the Treatment indicator with dummies for first and second follow-up. We find no evidence of dynamic treatment effects, as shown by the *p-value* on the test of equality of treatment effects reported at the bottom of column 5. If anything, the treatment effect is slightly larger at second follow-up.

C Cost-Benefit Analysis for Program Participants

Table A13 reports the cost-benefit analysis for program participants, that is, for those eligible workers who accepted to participate in the signaling and matching intervention. We have detailed information on program costs, as all activities were implemented by BRAC.[86] Panel A shows that this was a relatively inexpensive intervention: the cost per worker of producing and disbursing the skill certificate was \$19. This is in line with other similar information and certification interventions in developing countries, which are also found to be relatively inexpensive (McKenzie, 2017). The costs include: (i) \$9.2 for developing and administering the skills tests; (ii) \$6.4 for producing and disbursing the certificates; (iii) \$3.5 for program management and overheads.

For the benefits we consider the estimated earnings gains in the two years post intervention. Specifically, we use the estimate from column 2 of Table 9, which corresponds to the earnings impact for the average worker, estimated on the full sample of workers.[87] The results show that even if we assume that benefits last only for the two years post intervention, still the intervention is cost effective at raising earnings: Panel B shows that under a 15% discount rate the Net Present Value (NPV) of the intervention is \$73.94, and the benefits/cost ratio is well above 3, so that the benefits are more than three times higher than the costs. Panel B also shows that these estimates are not very sensitive to the discount rate. For instance, the benefits/cost ratio is still higher than 3 under a 30% discount rate.

[86] For the purpose of the cost-benefit analysis, we only consider the costs of the certification component of the intervention. So we do not include here any of the costs incurred for matching workers to firms.

[87] This is a more conservative approach than using the estimate for the sample of workers in paid employment (column 7 of Table 9), multiplied by the share of workers in employment.

Table A1: Selection into the program - Firms

OLS regression coefficients
Robust standard errors in parentheses

Dependent variable:	Firm included in the experimental sample [Yes=1]
	(1)
A. Owner and firm characteristics	
Owner is female	.044
	(.046)
Number of employees	-.008*
	(.004)
Number of employees squared	.000***
	(.000)
Business has trading licence	.004
	(.067)
Age of business	.001
	(.005)
Age of business squared	-.000
	(.000)
B. Sector	
Carpentry	.253***
	(.063)
Catering	-.059
	(.050)
Motormechanics	.091
	(.066)
Tailoring	.090
	(.062)
Welding	.130**
	(.060)
C. Region	
North	.011
	(.053)
East	-.086**
	(.037)
West	-.010
	(.042)
Mean of dependent variable	.383
Number of observations (firms)	1,086

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. The table uses data from the initial census of firms conducted for the job placement interventions. The excluded sector is hairdressing. The excluded region is Kampala. Number of employees are trimmed at the 99th percentile. All regressions further control for dummies for missing values in each of the independent variables.

Table A2: Comparison of High and Low ability owners

Means, standard deviations in parentheses

P-value on t-test of equality of means in brackets

	High Ability Owner	Low Ability Owner	P-value	Normalized Differences
	(1)	(2)	(3)	(4)
Number of owners	196	163		
A. Owner and firm characteristics				
Owner is female	.332	.411	[.548]	-.116
	(.472)	(.494)		
Owner age [Years]	35.8	35.6	[.772]	.013
	(8.33)	(8.96)		
Owner completed years of education	10.7	9.94	[.034]**	.168
	(3.48)	(3.15)		
Owner has received training from a VTI	.403	.429	[.824]	-.038
	(.492)	(.497)		
Number of employees	2.96	2.56	[.189]	.108
	(2.82)	(2.33)		
Business has trading licence	.929	.926	[.709]	.006
	(.258)	(.262)		
Age of business [Years]	6.75	7.41	[.761]	-.075
	(5.24)	(6.93)		
B. Profitability, returns to skills and management				
Monthly profits [USD]	259	177	[.072]*	.228
	(296)	(199)		
High relative perceived returns to soft skills	.617	.509	[.038]**	.155
	(.487)	(.501)		
Owner is able to delegate tasks/manage more workers	.923	.871	[.088]*	.122
	(.267)	(.336)		
Owner is present at the business premises every day	.801	.859	[.099]*	-.109
	(.400)	(.349)		
Panel C: Recruitment				
High relative perceived importance of difficulties in observing soft skills at recruitment	.561	.497	[.633]	.091
	(.498)	(.502)		
High difference in relative perceived scarcity of practical vs soft skills	.531	.491	[.583]	.056
	(.500)	(.501)		
Number of employees unconnected at recruitment	1.14	.871	[.166]	.121
	(1.85)	(1.29)		

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Data is from the baseline survey of firms. The t-tests are from OLS regressions of the variable of interest on a dummy variable that takes value one if the firm has a High Ability Owner, and stratification variables (dummies for BRAC branch and sector), with robust standard errors. Firm owners that scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners that scored below the median are assigned to the Low Ability group. The variable "Monthly profits" is the average reported monthly profits in the three months prior to the survey, expressed in January 2015 USD. The top 1% values are excluded. The variable "High relative perceived returns to soft skills" is constructed as follows: firm owners were asked to rate on a 0-10 scale, where 0 = “Not important at all”, and 10 = “Extremely important”, the importance of various skills for their operations, including each of the Big 5 personality traits (firm owners were asked about the importance of each of the Big 5 traits separately). For each firm owner, we compute the average importance given to the Big 5 traits, and then divide this by the average importance across all skills to create a measure of the relative importance of soft skills. This variable then takes value one if the relative importance of soft skills is on or above the median. The variable "Owner is able to delegate tasks/manage more workers" is constructed using the answer to a question where firm owners were asked whether they thought they had enough ability to manage/train more workers/delegate tasks. This variable is a dummy equal to one if the owner answered Yes, and zero otherwise. For the construction of the variable "High relative perceived importance of difficulties in observing soft skills at recruitment", firm owners were asked to rate on a 1-5 scale, where 1 = “Not important at all”, and 5 = “Extremely important”, the importance of a number of potential constraints. For each firm owner, we divide the importance given to difficulties in observing the soft skills of job candidates by the average importance across all contraints, to create a mesure of the relative importance of difficulties in observing soft skills. The resulting variable takes value one if the relative importance of difficulties in observing soft skills is on or above the median. To create the variable "High difference in relative perceived scarcity of practical vs soft skills", we compute within-firm owner differences in the number of potential workers out of 10 reported as having a good level of practical skills vs. a good level of soft skills. To estimate the reported number of workers with a good level of soft skills we compute the average of the reported number of workers with a good level of each of the Big 5 traits (the question was asked for each of the Big 5 traits separately). This creates a measure of the relative perceived scarcity of practical vs soft skills. The resulting variable then takes value one if this measure is on or above the median. The variable "Number of employees unconnected at recruitment" is the number of workers who are currently employed at the firm and who had no previous connection with anyone in the firm at the time of recruitment.

Table A3.1: Correlation among soft skills

Pairwise correlation coefficients

	Creativity	Communication skills	Willingness to help others	Attendance	Trustworthiness
Creativity	1				
Communication skills	.0824	1			
Willingness to help others	.0989	.6329	1		
Attendance	.0727	.6608	.6583	1	
Trustworthiness	.0472	-.0346	-.0615	.0357	1

Notes: Data is from the skills assessments of the 787 trainees participating in the matching intervention. The soft skills were measured while the trainees were still enrolled at the VTIs. Each variable is measured on a 1-5 scale.

Table A3.2: Correlation between Big 5 traits and soft skills

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Attendance	Communication skills	Trustworthiness	Willingness to help others	Creativity
	(1)	(2)	(3)	(4)	(5)
Extraversion	.002	.064**	-.042	.008	.005
	(.032)	(.033)	(.038)	(.033)	(.040)
Agreeableness	.004	.033	-.046	.016	.089*
	(.038)	(.036)	(.043)	(.039)	(.049)
Conscientiousness	.040	.027	.092**	.014	.031
	(.039)	(.036)	(.042)	(.041)	(.048)
Neuroticism (reversed)	.006	-.007	-.032	.043	-.035
	(.034)	(.033)	(.041)	(.039)	(.043)
Openness to Experience	.008	-.000	-.004	-.001	.057
	(.043)	(.042)	(.049)	(.046)	(.055)
VTI and sector dummies	Yes	Yes	Yes	Yes	Yes
Number of observations	724	724	724	724	724

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Standard errors are adjusted for heteroskedasticity. The five soft skills (dependent variables) were measured while the trainees were still enrolled at the VTIs. Extraversion, Agreeableness, Conscientiousness, Neuroticism and Openness to Experience are measured using a 10-item Big-5 scale. All variables shown in the Table are measured on a 1-5 scale. The Neuroticism variable is recoded so that a higher level of the variable corresponds to a lower level of Neuroticism (i.e. to more self-control).

Table A4: Mincerian returns to soft skills in Control

OLS regression coefficients

Standard errors clustered at the worker level in parentheses

Dependent variable:	Any paid work in the last month		Total earnings in the last month [USD]			
Sample of workers:	All		All		Positive earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Pass grade on all soft skills	-.013	.022	11.7**	9.58**	17.2***	10.3*
	(.035)	(.037)	(5.04)	(4.86)	(5.83)	(5.36)
Age		.128		7.85		1.85
		(.085)		(9.26)		(12.0)
Age squared		-.003		-.203		-.058
		(.002)		(.221)		(.286)
Female		-.114		-15.4		-13.1
		(.069)		(10.6)		(10.5)
Completed prior education [Years]		.012		3.94***		4.15***
		(.012)		(1.28)		(1.32)
Course duration [Years]		-.038		.490		3.05
		(.024)		(3.01)		(3.15)
Ever employed		.061		10.6*		6.74
		(.046)		(6.40)		(6.91)
Mean of dependent variable in Control	.750	.750	47.2	47.2	63.1	63.1
P-value on F-test of joint significance of sector dummies		[.304]		[.292]		[.128]
P-value on F-test of joint significance of BRAC branch dummies		[.071]		[.032]		[.000]
Uses data from first followup	Yes	Yes	Yes	Yes	Yes	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.037	.092	.047	.195	.040	.263
Number of observations	663	663	657	657	491	491

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Data comes from the two worker follow-up surveys. The "Pass grade on all soft skills" variable is a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments. In columns 1-4 the the sample includes all workers. In columns 5 and 6 it is restricted to workers with any positive labor market earnings in the month prior to the survey. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted in January 2015 USD. The top 1% values of total earnings in the last month are excluded. All regressions further control for dummies for missing values in each of the independent variables.

Table A5.1: Firm balance at baseline and followup

Means, standard deviations in parentheses
P-value on t-test of equality of means with control group in brackets
P-value on F-tests in braces

	Panel A: Balance at baseline				Panel B: Balance at followup			
	Control Firms	Treatment Firms	P-value	Normalized Differences	Control Firms	Treatment Firms	P-value	Normalized Differences
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of firms	211	211			190	180		
A. Owner characteristics at baseline								
Owner is female	.360	.374	[.384]	-.021	.353	.328	[.572]	.037
	(.481)	(.485)			(.479)	(.471)		
Owner age [Years]	35.7	36.1	[.585]	-.039	35.2	36.8	[.054]	-.132
	(8.71)	(8.54)			(8.58)	(8.69)		
Owner completed years of education	10.6	10.2	[.253]	.088	10.7	10.2	[.220]	.099
	(3.23)	(3.37)			(3.18)	(3.22)		
Owner has received training from a VTI	.422	.341	[.077]	.117	.459	.350	[.010]	.158
	(.495)	(.475)			(.500)	(.478)		
Owner scored at median or above on cognitive test	.538	.554	[.767]	-.023	.539	.540	[.948]	-.002
	(.500)	(.498)			(.500)	(.500)		
B. Firm characteristics at baseline								
Business is registered	.915	.919	[.734]	-.012	.926	.933	[.697]	-.019
	(.280)	(.273)			(.262)	(.250)		
Number of employees	2.98	2.79	[.461]	.051	3.03	2.87	[.435]	.041
	(2.91)	(2.47)			(2.99)	(2.48)		
Age of business [Years]	6.73	6.97	[.667]	-.028	6.86	7.50	[.484]	-.074
	(5.28)	(6.52)			(5.41)	(6.76)		
Average monthly revenues [USD]	548	565	[.909]	-.018	539	592	[.880]	-.057
	(642)	(677)			(597)	(709)		
Average monthly profits [USD]	214	213	[.822]	.001	198	216	[.969]	-.061
	(270)	(228)			(223)	(211)		
F-test of joint significance from column regression		{.393}				{.215}		

Notes: Data is from the 422 firms included in the final research sample. The t-tests are from OLS regressions of the variable of interest on a constant, treatment dummy and stratification variables (dummies for BRAC branch and sector). Standard errors are robust in such regressions. The F-test is from a regression where the dependent variable is the treatment dummy, and the independent variables are all the variables considered for the balance checks in the table as well as stratification variables (dummies for BRAC branch and sector). Standard errors are robust in such regression. Panel A considers balance at baseline, and so the sample includes all the 422 firms included in the final research sample. Panel B considers balance at first followup, and so the sample includes those 370 firms successfully interviewed at first followup. Profits and revenues are trimmed at the 99th percentile.

Table A5.2: Worker balance at baseline and followup

Means, standard deviations in parentheses

P-value on t-test of equality of means with control group in brackets

P-value on F-test that worker characteristics do not jointly predict treatment assignment in braces

	Panel A: Balance at baseline				Panel B: Balance at first followup				Panel C: Balance at second followup			
	Control Workers	Treatment Workers	P-value	Normalized Differences	Control Workers	Treatment Workers	P-value	Normalized Differences	Control Workers	Treatment Workers	P-value	Normalized Differences
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Number of workers	397	390			336	340			332	342		
A. Background characteristics at baseline												
Age [Years]	20.3	20.6	[.119]	-.081	20.4	20.7	[.099]	-.083	20.4	20.7	[.092]	-.084
	(2.43)	(2.81)			(2.29)	(2.82)			(2.31)	(2.74)		
Female	.504	.492	[.706]	.017	.470	.471	[.601]	-.001	.464	.477	[.463]	-.018
	(.501)	(.501)			(.500)	(.500)			(.499)	(.500)		
Completed prior education [Years]	10.2	10.3	[.383]	-.038	10.3	10.4	[.932]	-.039	10.3	10.4	[.827]	-.038
	(1.88)	(1.85)			(1.83)	(1.79)			(1.84)	(1.86)		
Course duration [Years]	1.50	1.48	[.753]	.016	1.57	1.51	[.390]	.050	1.58	1.51	[.579]	.057
	(.898)	(.857)			(.868)	(.840)			(.878)	(.852)		
Ever employed	.194	.218	[.470]	-.042	.202	.221	[.580]	-.033	.202	.213	[.771]	-.019
	(.396)	(.413)			(.402)	(.415)			(.402)	(.410)		
Monthly expected earnings [USD]	118.3	124.5	[.305]	-.061	122.8	126.1	[.699]	-.032	122.4	124.8	[.578]	-.024
	(72.0)	(71.8)			(72.6)	(72.6)			(72.4)	(70.8)		
B. Skills at baseline												
Attendance [1-5 scale]	3.39	3.34	[.700]	.031	3.38	3.35	[.913]	.019	3.40	3.34	[.741]	.038
	(1.13)	(1.14)			(1.12)	(1.12)			(1.12)	(1.13)		
Communication skills [1-5 scale]	3.23	3.25	[.733]	-.013	3.24	3.26	[.640]	-.013	3.24	3.28	[.455]	-.026
	(1.08)	(1.13)			(1.05)	(1.13)			(1.07)	(1.14)		
Creativity [1-5 scale]	3.38	3.43	[.442]	-.033	3.42	3.44	[.588]	-.013	3.44	3.45	[.499]	-.007
	(1.11)	(1.05)			(1.08)	(1.07)			(1.10)	(1.06)		
Trustworthiness [1-5 scale]	3.49	3.53	[.369]	-.029	3.53	3.54	[.620]	-.007	3.52	3.52	[.753]	.000
	(1.01)	(.974)			(.992)	(.972)			(.997)	(.974)		
Willingness to help others [1-5 scale]	3.34	3.32	[.925]	.013	3.37	3.34	[.940]	.020	3.36	3.30	[.778]	.039
	(1.10)	(1.07)			(1.09)	(1.06)			(1.11)	(1.07)		
Cognitive test score [0-10 scale]	5.23	5.21	[.771]	.006	5.36	5.32	[.798]	.012	5.31	5.29	[.774]	.006
	(2.49)	(2.43)			(2.42)	(2.44)			(2.47)	(2.40)		
F-test of joint significance		{.919}				{.933}				{.805}		

Notes: Data is from the 787 workers included in the final research sample. The t-stats are from OLS regressions of the variable of interest on a constant, treatment dummy and stratification variables (dummies for BRAC branch and sector). Standard errors are robust in such regressions. The F-stats are from OLS regressions where the dependent variable is the treatment dummy, and the independent variables are all the variables considered for the balance checks in the table as well as stratification variables (dummies for BRAC branch and sector). Standard errors are robust in such regressions. Panel A considers balance at baseline, and so the sample includes all the 787 workers included in the final research sample. Panel B considers balance at first followup, and so the sample includes those 676 workers successfully interviewed at first followup. Finally, Panel C considers balance at second followup, and so the sample includes those 674 workers successfully interviewed at second followup. Expected earnings are constructed as follows: respondents were asked to report: (i) their minimum expected earnings; (ii) their maximum expected earnings; (iii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings for each respondent. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted in January 2015 USD. The top 1% values of expected earnings are excluded.

Table A6: Compliance and attrition

OLS regression coefficients are reported throughout

Standard errors adjusted for two-way clustering in parentheses in column 1
Robust standard errors in parentheses in columns 2-4

	Panel A: Compliance	Panel B: Attrition		
	Job interview took place	Worker in sample at first followup	Worker in sample at second followup	Firm in sample at followup
	(1)	(2)	(3)	(4)
Treatment	-.013	.011	.026	-.033
	(.039)	(.023)	(.021)	(.032)
A. Worker characteristics				
Pass grade on all soft skills	.024	.008	.021	
	(.032)	(.024)	(.023)	
Age	-.002	.060	.083**	
	(.035)	(.038)	(.041)	
Age squared	.000	-.001	-.002*	
	(.001)	(.001)	(.001)	
Female	-.092	-.027	.032	
	(.063)	(.056)	(.047)	
Completed prior education [Years]	-.013	.008	-.001	
	(.010)	(.007)	(.007)	
Course duration [Years]	-.078***	.001	.004	
	(.023)	(.017)	(.016)	
Ever employed	-.013	-.034	-.023	
	(.040)	(.029)	(.030)	
B. Firm characteristics				
Number of employees	-.002			.004
	(.005)			(.006)
Owner is female	-.005			-.075
	(.051)			(.062)
Owner attended a VTI	-.008			.004
	(.039)			(.011)
Age of owner	.007			-.000
	(.015)			(.000)
Age of owner squared	-.000			.057
	(.000)			(.037)
High Ability Owner	-.007			-.028
	(.041)			(.038)
Mean of dep. var. in Control group	.419	.846	.836	.917
P-value from F-test of joint significance of worker covariates	[.002]	[.328]	[.281]	NA
P-value from F-test of joint significance of firm covariates	[.937]	NA	NA	[.254]
Number of observations	1,229	787	787	422

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Column 1 uses data at the match level from the matching surveys, and reports the results of a regression where the dependent variable is a dummy equal to one if the scheduled match was carried out, and zero otherwise. Standard errors are clustered both at the level of the worker and the firm in this regression, following the procedure in Cameron et al. [2011]. Columns 2-3 use data at the worker level, and report the results of regressions where the dependent variable is a dummy equal to one if the worker was successfully interviewed in the corresponding followup survey round, and zero otherwise. Standard errors are robust in this regression. Column 4 uses data at the firm level, and reports the results of a regression where the dependent variable is a dummy equal to one if the firm was successfully interviewed in the followup survey, and zero otherwise. Standard errors are robust in this regression. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview or match. In addition, all regressions in column 1-3 control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. The regressions in columns 1 and 4 control for the following firm characteristics measured at baseline: dummy for female owner; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees; a dummy for High Ability owner. Firm owners who scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners who scored below the median are assigned to the Low Ability group.

Table A7: Heterogeneous impacts on firm owner beliefs about matched workers

OLS regression coefficients, standard errors adjusted for two-way clustering in parentheses

Dependent variable:	Matched worker reported as MORE SKILLED than usual applicant [Yes=1]
Sample of firm owners:	**All**
	(1)
Interaction of Treatment with:	
High Ability Owner	.124**
	(.052)
Owner is female	-.000
	(.078)
Owner attended a VTI	-.002
	(.061)
Owner years of education above median	.098*
	(.057)
Number of employees above median	.010
	(.051)
Capital stock above median	-.008
	(.050)
Mean of dep. var. in Control group	.097
P-value on F-test of joint significance of interactions of sector dummies with Treatment	[.510]
Number of observations (matches)	515

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the matching surveys are reported. Standard errors are adjusted for two-way clustering (at the level of both the firm and the worker), following the procedure in Cameron et al. [2011]. The regression controls for a treatment indicator, stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. All regressions also control for the following firm characteristics measured at baseline: dummy for female owner; age and age squared of the owner; dummy for whether owner attended a VTI in the past; dummy for whether firm size is above median; dummy for whether the capital stock of the firm is above median; dummy for whether the owner has above median years of education. Firm owners who scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners who scored below the median are assigned to the Low Ability group. The regression further controls for dummies for missing values in each of the independent variables.

Table A8: Impacts on sorting - Robustness

OLS regression coefficients, standard errors adjusted for two-way clustering in parentheses

Dependent variable:	Worker received a job offer by the matched firm [Yes=1]	Worker was hired by the matched firm [Yes=1]
Sample of matches:	**All**	**All**
	(1)	**(2)**
Interaction of Treatment with:		
High Ability Owner	.244***	.163**
	(.088)	(.069)
Owner is female	-.131	-.167
	(.137)	(.111)
Owner attended a VTI	.056	-.008
	(.092)	(.067)
Owner years of education above median	-.016	.007
	(.088)	(.068)
Number of employees above median	.016	.048
	(.089)	(.067)
Capital stock above median	-.080	-.052
	(.096)	(.079)
Mean of dep. var. in Control group	.222	.097
P-value from F-test of joint significance of interactions of sector dummies with Treatment	[.405]	[.174]
Number of observations (matches)	412	412

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the matching surveys are reported. Standard errors are adjusted for two-way clustering (at the level of both the firm and the worker), following the procedure in Cameron et al. [2011]. All regressions control for a treatment indicator, stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. All regressions also control for the following firm characteristics measured at baseline: dummy for female owner; age and age squared of the owner; dummy for whether owner attended a VTI in the past; dummy for whether firm size is above median; dummy for whether the capital stock of the firm is above median; dummy for whether the owner has above median years of education. Firm owners who scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners who scored below the median are assigned to the Low Ability group. All regressions further control for dummies for missing values in each of the independent variables.

Table A9: Impacts on earnings at matched firms

Means, standard deviations in parentheses

	Treatment	Control	P-value from unconditional t-test of equality of means	P-value from conditional t-test of equality of means
	(1)	(2)	(3)	(4)
Total earnings in the first week employed at the matched firm [USD]	4.05	2.46	[.385]	[.278]
	(7.03)	(5.10)		
Number of workers	23	23		

Notes: Data is from the worker follow-up survey. All monetary amounts are in USD. The sample includes the 46 workers who started a job at the matched firm in the intervention and for whom earnings data are available. The observation with the highest earnings is excluded from the analysis (this corresponds to the top 1% earner in the sample). The p-value in column 3 is from an unconditional t-test of equality of means. The p-value in column 4 is from a conditional test of equality of means, controlling for the following interview, worker and firm characteristics: dummies for BRAC branch and sector; dummies for month of interview; a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments; worker age and age squared; dummy for whether the worker is female; years of formal education of the worker; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience of the worker; dummy for female owner; age and age squared of the owner; dummy for whether the owner attended a VTI in the past; number of employees.

Table A10: Worker balance at followup, conditional on positive earnings

Means, standard deviations in parentheses

P-value on t-test of equality of means with control group in brackets

P-value on F-tests in braces

	Panel A: Balance conditional on positive earnings at first followup				Panel B: Balance conditional on positive earnings at second followup			
	Control Workers (1)	Treatment Workers (2)	P-value (3)	Normalized Differences (4)	Control Workers (5)	Treatment Workers (6)	P-value (7)	Normalized Differences (8)
Number of observations	222	229			275	271		
A. Background characteristics at baseline								
Age [Years]	20.5	20.7	[.616]	-0.06	20.3	20.7	[.092]	-0.11
	(2.19)	(2.71)			(2.28)	(2.81)		
Female	.468	.472	[.892]	-0.01	.451	.458	[.760]	-0.01
	(.500)	(.500)			(.498)	(.499)		
Completed prior education [Years]	10.5	10.4	[.429]	0.04	10.3	10.4	[.430]	-0.04
	(1.63)	(1.75)			(1.88)	(1.65)		
Course duration [Years]	1.53	1.49	[.634]	0.03	1.55	1.50	[.756]	0.04
	(.875)	(.856)			(.884)	(.869)		
Ever employed	.221	.214	[.824]	0.01	.215	.210	[.934]	0.01
	(.416)	(.411)			(.411)	(.408)		
Monthly expected earnings [USD]	127.6	124.7	[.668]	0.03	124.9	122.2	[.748]	0.03
	(77.6)	(71.2)			(74.7)	(66.7)		
B. Skills at baseline								
Attendance [1-5 scale]	3.29	3.34	[.548]	-0.03	3.40	3.32	[.660]	0.05
	(1.13)	(1.14)			(1.11)	(1.14)		
Communication skills [1-5 scale]	3.19	3.28	[.301]	-0.06	3.21	3.27	[.372]	-0.04
	(1.08)	(1.15)			(1.06)	(1.15)		
Creativity [1-5 scale]	3.45	3.47	[.640]	-0.01	3.42	3.48	[.222]	-0.04
	(1.12)	(1.06)			(1.12)	(1.07)		
Trustworthiness [1-5 scale]	3.54	3.57	[.708]	-0.02	3.46	3.43	[.997]	0.02
	(1.00)	(.937)			(.963)	(.975)		
Willingness to help others [1-5 scale]	3.32	3.32	[.876]	0.00	3.36	3.29	[.756]	0.05
	(1.09)	(1.07)			(1.08)	(1.07)		
Cognitive test score [0-10 scale]	5.39	5.20	[.495]	0.06	5.35	5.23	[.431]	0.04
	(2.39)	(2.43)			(2.40)	(2.33)		
F-test of joint significance		{.975}				{.553}		

Notes: The sample in Panel A includes observations with positive earnings from the first worker followup survey. The sample in Panel B includes observations with positive earnings from the second worker followup survey. The t-stats are from OLS regressions of the variable of interest on a constant, treatment dummy and stratification variables (dummies for BRAC branch and sector). Standard errors are robust in such regressions. The F-stats are from OLS regressions where the dependent variable is the Treatment dummy, and the independent variables are all the variables considered for the balance checks in the table as well as stratification variables (dummies for BRAC branch and sector). Standard errors are robust in such regressions. Expected earnings are constructed as follows: respondents were asked to report: (i) their minimum expected earnings; (ii) their maximum expected earnings; (iii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings for each respondent. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted in January 2015 USD. The top 1% values of expected earnings are excluded.

Table A11: Impacts on labor market earnings - Robustness

The sample includes all workers with positive earnings at follow-up

OLS regression coefficients, standard errors clusteted at the worker level in parentheses

Dependent variable:	**Total earnings in the last month [USD]**			
Specification:	**Log**	**No controls**	**IPW**	**Dynamics**
	(1)	**(2)**	**(3)**	**(4)**
Treatment	.108*	6.84*	8.07**	
	(.064)	(3.59)	(3.63)	
Dynamic treatment effects				
Treatment X First followup				5.25
				(4.05)
Treatment X Second followup				8.82*
				(4.85)
Mean of dep. var. in Control group	68.4	63.1	63.1	63.1
Pvalue First fup = Second fup				[.519]
Controls for baseline value of outcome	Yes	Yes	Yes	Yes
Controls for stratification variables	Yes	Yes	Yes	Yes
Controls for other worker characteristics	Yes	No	Yes	Yes
Uses data from first followup	Yes	Yes	Yes	Yes
Uses data from second followup	Yes	Yes	Yes	Yes
Number of observations	997	988	988	988

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. Standard errors are clustered at the worker level. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions with the exception of column 3 control for the following worker characteristics measured at baseline: a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments and disclosed on the Treatment group certificates; age and age squared; dummy for female; years of formal education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. The dependent variable is total labor earnings from all activities in the month prior to the survey. Since at baseline all workers were enrolled at vocational training institutes and only 1% of them were currently doing in any paid work, we consider as baseline value of the outcome expected earnings at baseline. Expected earnings are constructed as follows: respondents were asked to report: (i) their minimum expected earnings; (ii) their maximum expected earnings; (iii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings for each respondent. To check robustness to attrition, the regressions reported in column 4 are weighted using the Inverse Probability Weighting prodecure described in Wooldridge [2010], and where we use as instruments to predict attrition the gender, education level and age of the enumerators assigned to contact the workers. Enumerators were assigned to workers randomly. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted in January 2015 USD. All regressions further control for dummies for missing values in each of the independent variables. The top 1% values of total earnings in the last month are excluded in columns 2-4.

Table A12: Results on post-intervention firm outcomes

OLS regression coefficients, robust standard errors in parentheses

P-values on t-test of equality of coefficients for High and Low ability owners in brackets

Dependent variable:	**Number of employees**			
Sample of firms:	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (2) = (3)**
	(1)	**(2)**	**(3)**	**(4)**
Treatment	-.137	-.143	.006	[.627]
	(.133)	(.207)	(.199)	
Mean of Dep. Var. in Control	2.67	2.70	2.58	
Controls for baseline value of outcome	Yes	Yes	Yes	
Number of observations (firms)	371	172	146	

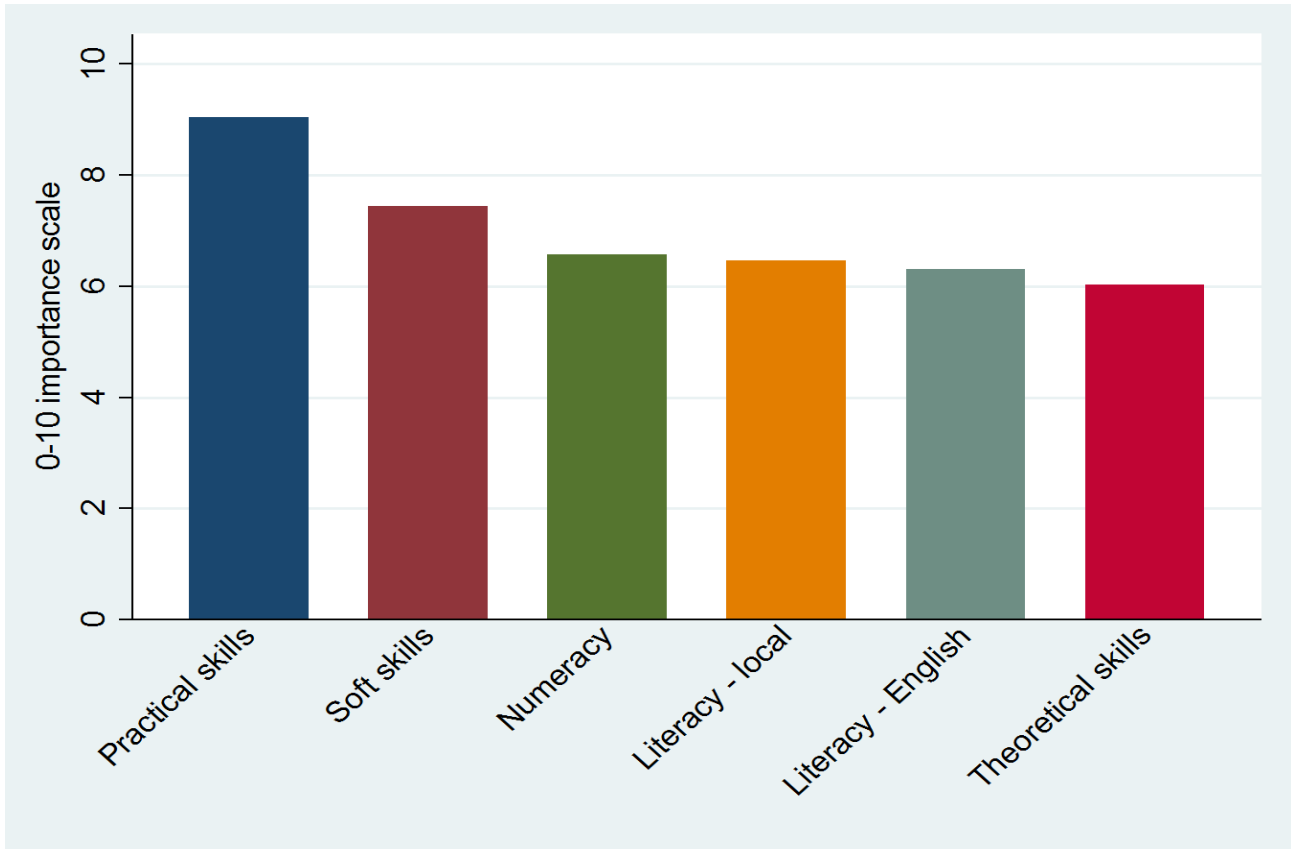
Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the firm follow-up survey are reported. Standard errors are adjusted for heteroskedasticity. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. In addition, all regressions control for the following firm characteristics measured at baseline: number of employees, dummy for female owner, business age and age squared. Firm owners who scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners who scored below the median are assigned to the Low Ability group. In column 1 the sample includes all firms. In column 2 it is restricted to High Ability firm owners. In column 3 it is restricted to Low Ability firm owners. The dependent variable is the number of employees at followup. This variable takes value zero for firms with no employees. The p-values in column 4 are from similar OLS regressions estimated on the full sample of firms and where each independent variable is interacted with the High Ability owner dummy. All regressions further control for dummies for missing values in each of the independent variables.

Table A13: Cost-benefit analysis

	Program participants (1)
Panel A. External parameters	
Total cost per individual at year 0 [USD]	**19.10**
Developing and administering skill tests	9.19
Producing and distributing certificates	6.40
Program management and overheads	3.50
Duration of impacts	2 years
Discount rate	15%
Panel B. Estimated expected annual earnings benefits [USD]	
1 Change in annual earnings per individual in year 1	45.48
2 Change in annual earnings per individual in year 2	45.48
3 NPV change in annual earnings per individual	**73.94**
Sensitivity to different assumptions about discount rate	
3.1 *Discount rate = 20%*	69.48
3.2 *Discount rate = 30%*	61.90
4 Benefits/Cost ratio	**3.87**
Sensitivity to different assumptions about discount rate	
4.1 *Discount rate = 20%*	3.64
4.2 *Discount rate = 30%*	3.24

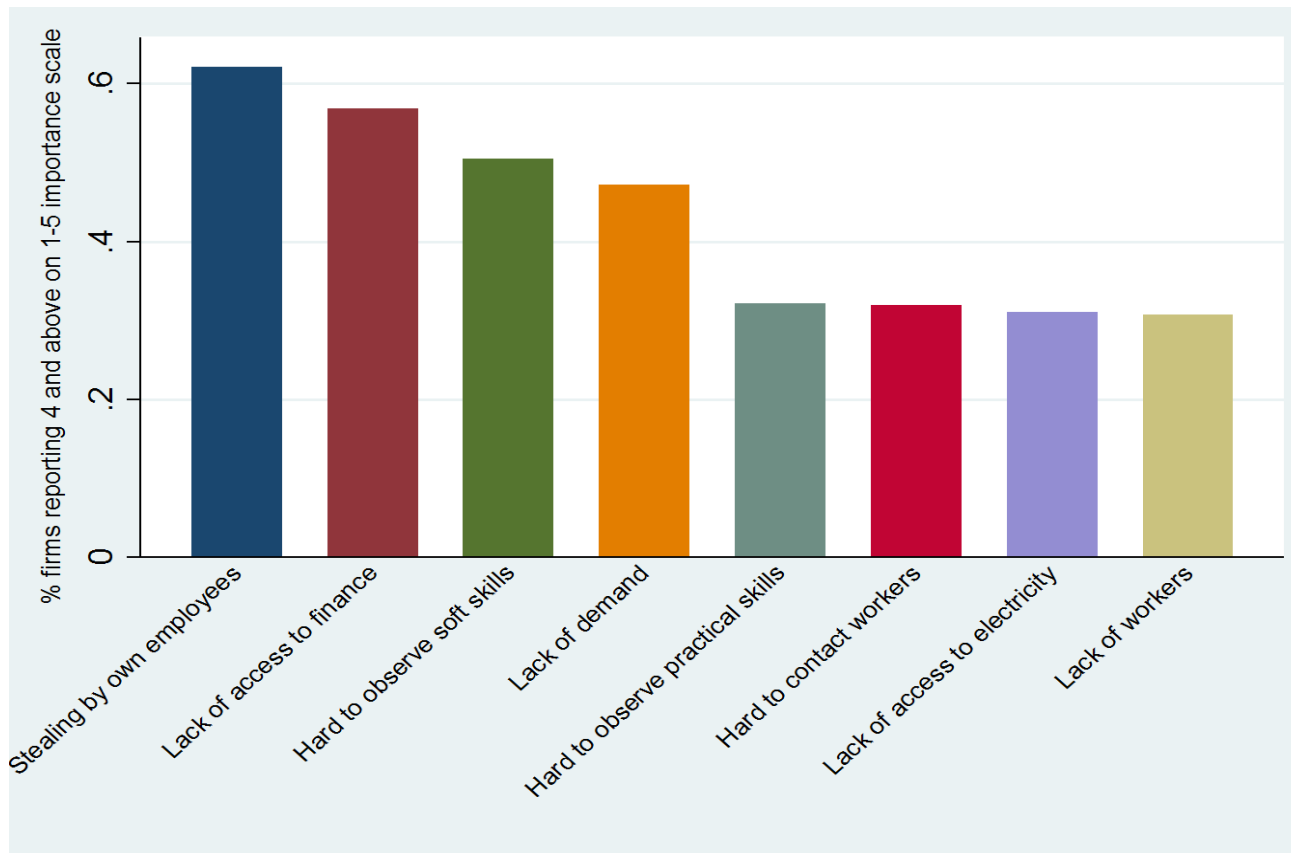
Notes: The total cost per individual at year zero includes: (i) cost of developing and administering the skill tests (9.19 USD); (ii) cost of producing and distributing certificates (6.40 USD); (iii) overheads (3.50 USD). Monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary earnings are then converted in January 2015 USD. The change in annual earnings per individual is estimated using the treatment effect from column 2 of Table 9 ($3.79), and multiplying that by 12.

Figure A1: Perceived returns to various skills, as reported by firm owners



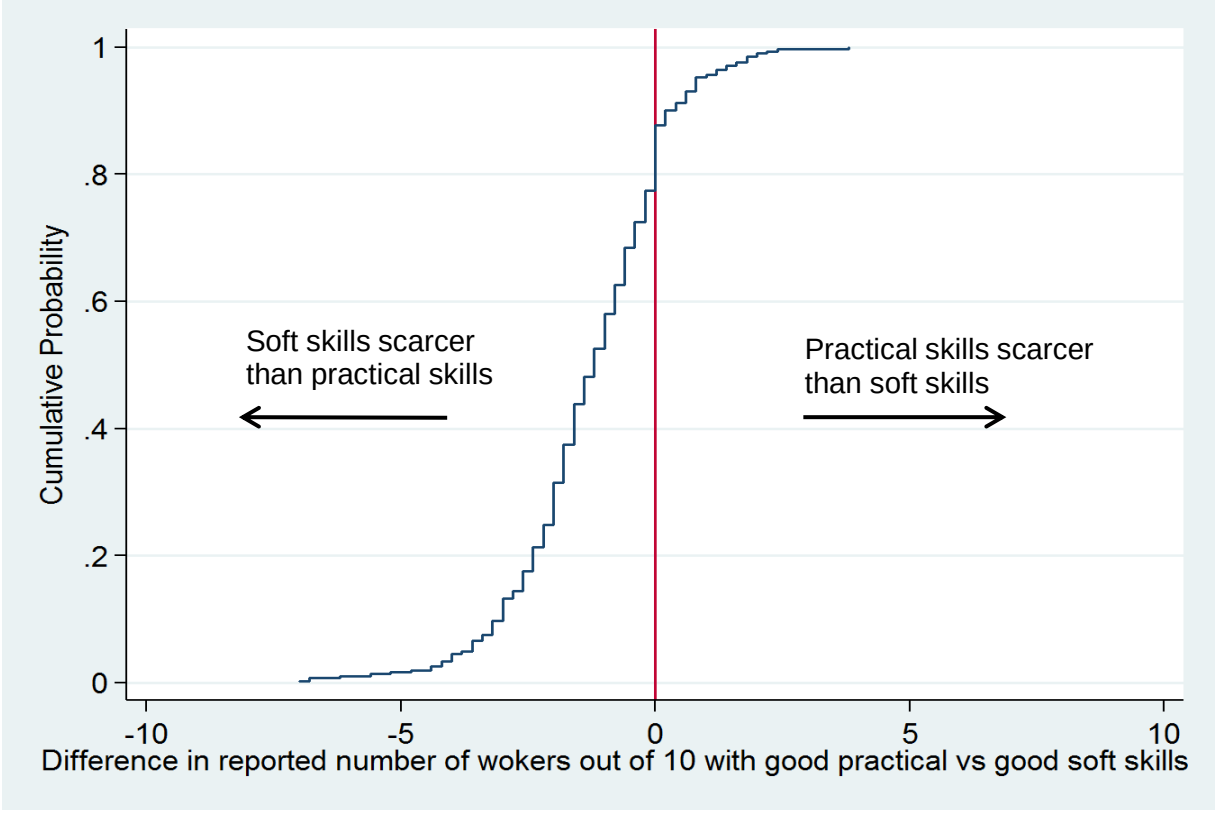
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 0-10 scale, where 0 = “Not important at all”, and 10 = “Extremely important”, the importance of various skills for their operations. The figure reports the average importance given to each skill in the sample. The column “Soft skills” reports the average given to each of the Big 5 traits (firm owners were asked about the importance of each of the Big 5 traits separately).

Figure A2: Perceived importance of various constraints, as reported by firm owners



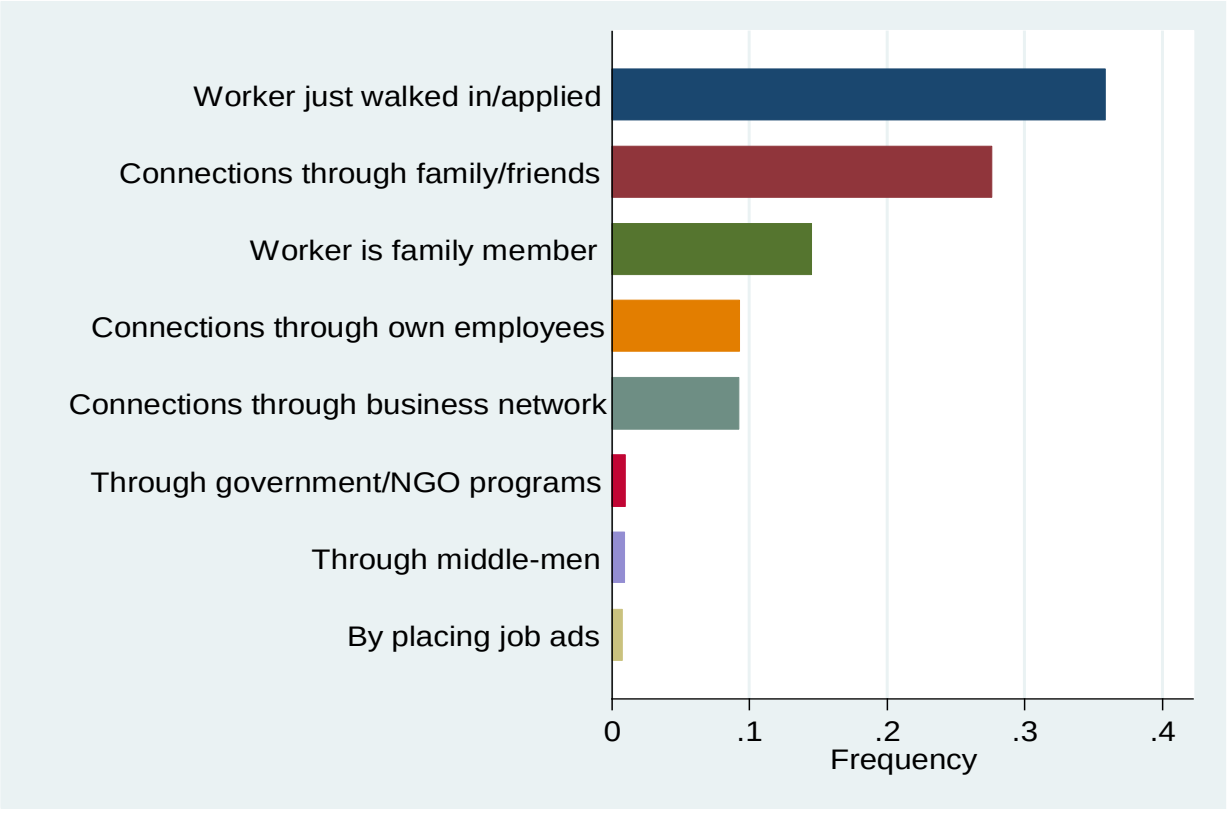
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 1-5 scale, where 1 = “Not important at all”, and 5 = “Extremely important”, the importance of these potential constraints. The figure reports the percentage of firm owners that answered 4 or above on the scale for each constraint.

Figure A3: Relative perceived scarcity of practical vs soft skills in the worker population, as reported by firm owners



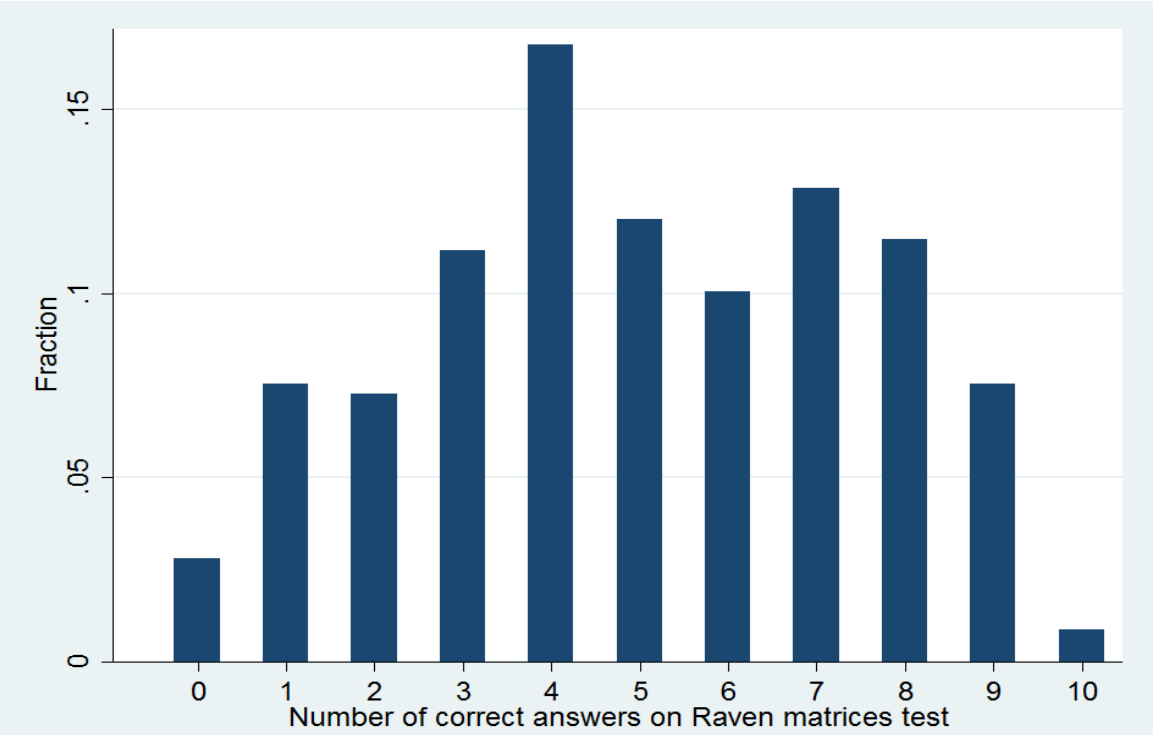
Notes: The figure reports the CDF of within-firm owner differences in the number of potential workers out of 10 reported as having a good level of practical skills vs. a good level of soft skills. To estimate the reported number of workers with a good level of soft skills we compute the average of the reported number of workers with a good level of each of the Big 5 traits (the question was asked for each of the Big 5 traits separately).

Figure A4: Recruitment channel of current employees



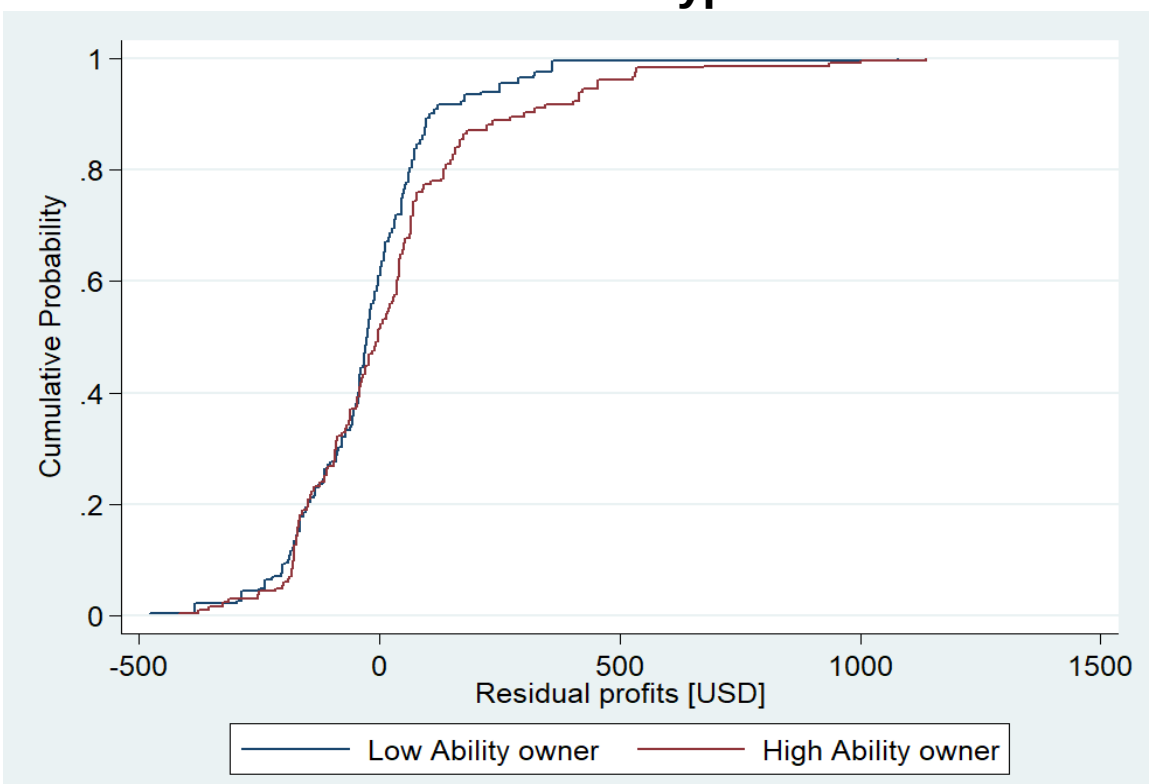
Notes: The figure reports the frequency of recruitment channels for the workers employed at baseline in the sample of firms included in the intervention.

Figure A5: Distribution of cognitive ability among firm owners



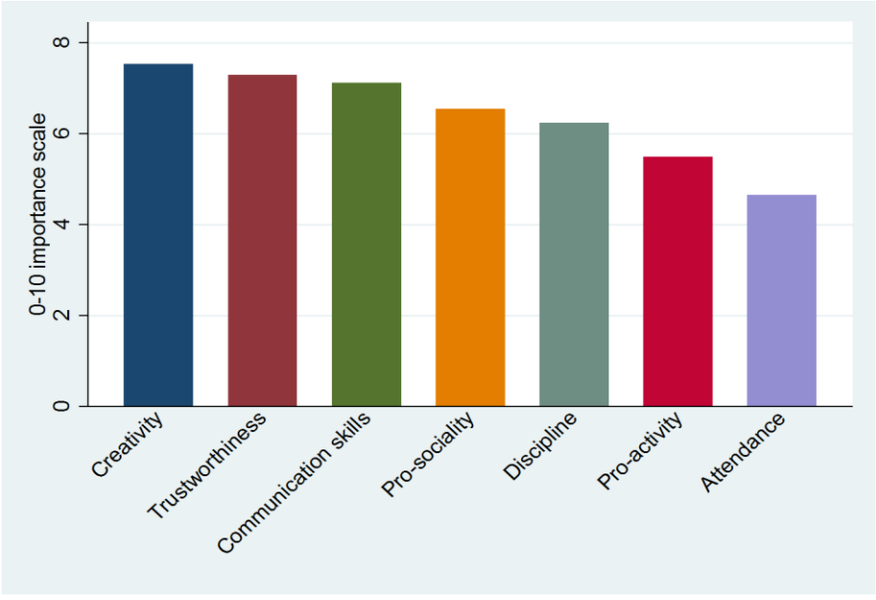
Notes: Data is from the firm baseline survey. The Figure reports the frequency histogram of the scores on the 10-item Raven matrices test administered to firm owners at baseline.

Figure A6: Cumulative distribution of residual profits, by firm owner type



Notes: The sample includes all firms interviewed at baseline, for which the score of the firm owner on the cognitive test is available. The figure plots the residuals from an OLS regression of baseline profits on the following covariates: dummy for female owner; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees; BRAC branch and sector dummies. Standard errors are robust in this regression. The figure plots the CDF of residual profits, by whether the owner is classified as a Low ability owner, or a High ability owner. Firm owners that scored on or above the median on a cognitive test administered at baseline are assigned to the High Ability group; owners that scored below the median are assigned to the Low Ability group. Profits are computed as the average reported monthly profits in the three months prior to the survey. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary values are then converted in January 2015 USD. The top 1% monetary values are excluded.

Figure A7: Information that firm owners would like to see about trainees



Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 0-10 scale, where 0 = “Not important at all”, and 10 = “Extremely important”, how important it would be for them to be provided additional information on different skills of job candidates during recruitment. The figure shows the mean importance given to each skill in the sample.

Figure A8: Distribution of soft skills among trainees

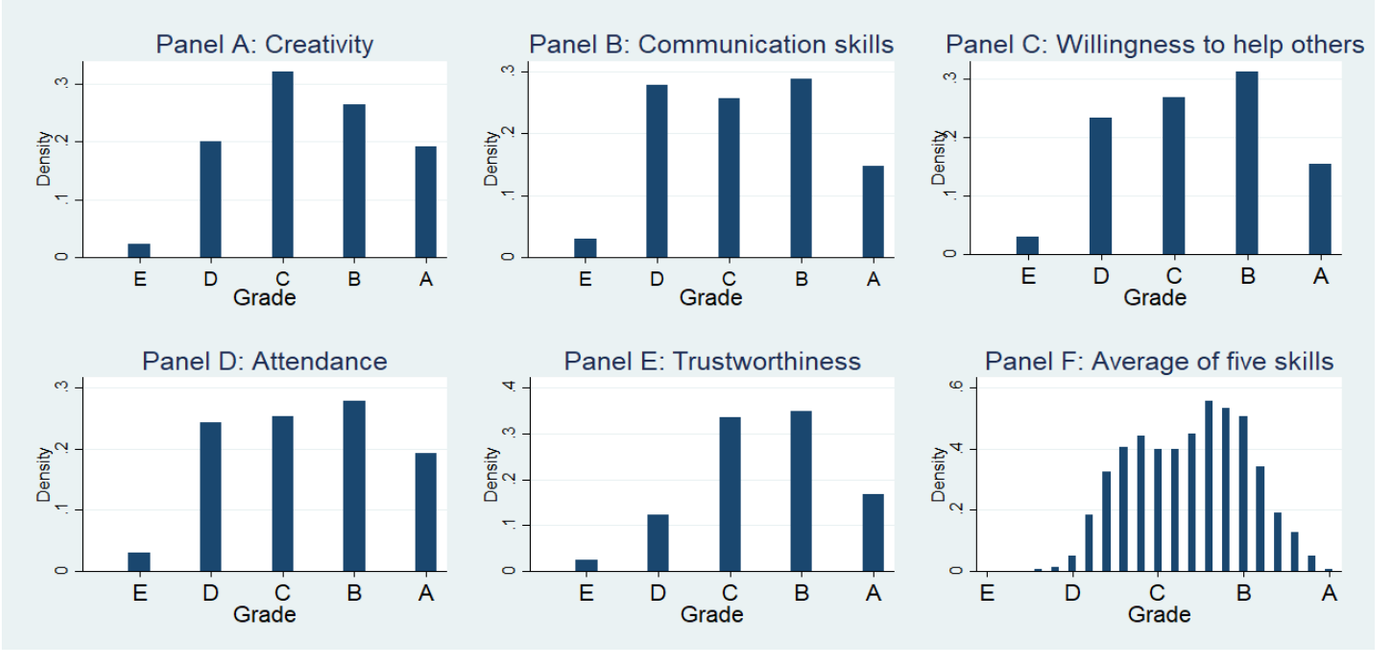


Figure A9: Skills certificates

Panel A: Treatment

ID XXX

brac

Acknowledgment

This is to acknowledge that

NAME OF TRAINEE

Has participated in the Youth Employment Job Placement Project in collaboration with BRAC Uganda and the Institute for Fiscal Studies, obtaining the following grades in our soft skills assessments:

Creativity	**B**
Trustworthiness	**A**
Willingness to help others	**C**
Attendance	**B**
Communication skills	**A**

Date: 02/04/2015

.....

Dr Jenipher Twebaze Musoke
Coordinator, Research,
BRAC Africa Programme

.....

Bhuiyan Muhammad Imran
Country Representative
BRAC Uganda

The BRAC-IFS Youth Employment Job Placement Project

This project aims to connect young trained workers with employment opportunities. Interested trainees were recruited from a number of partner Vocational Training Institutes (VTIs) throughout Uganda. As part of the project, participating trainees were assessed on the specific soft skills reported on the front on this document. The assessments took place while the trainees were still enrolled at the VTIs. Note that the participating trainees did not receive any soft-skills specific training as part of this project. Upon graduation from the VTIs, the participating trainees were provided this acknowledgment card and were linked with potential employers. The project started in July 2014 and will run until June 2017. For more information about the project please get in touch with BRAC Uganda Country Office:

BRAC

Plot 90 Busingiri Zone
Off Entebbe road Nyanama
PO Box 31817 (Clock Tower)
Kampala Uganda

T: +256 (0) 414 270978
: +256 (0) 712 111322
E: bracuganda@brac.net
W: www.brac.net

Registered in Uganda As BRAC Uganda Registration Number 5914/6217

Note on soft skills assessment procedure

The grades reported on the front of this document are the results of soft skills assessments conducted by the project team with the trainees participating in the project. Soft skills were measured using both standard self-administered psychometric scales as well as by means of a teacher survey whereby class teachers were asked to evaluate each individual trainee on these specific soft skills.

Note on grades from soft skills assessments

A = 85-100

B = 65-84

C = 50-64

D = 30-49

E = 0-29

Figure A9: Skills certificates (continued)

Panel B: Control

ID XXX

brac

Acknowledgment of Participation

This is to acknowledge that

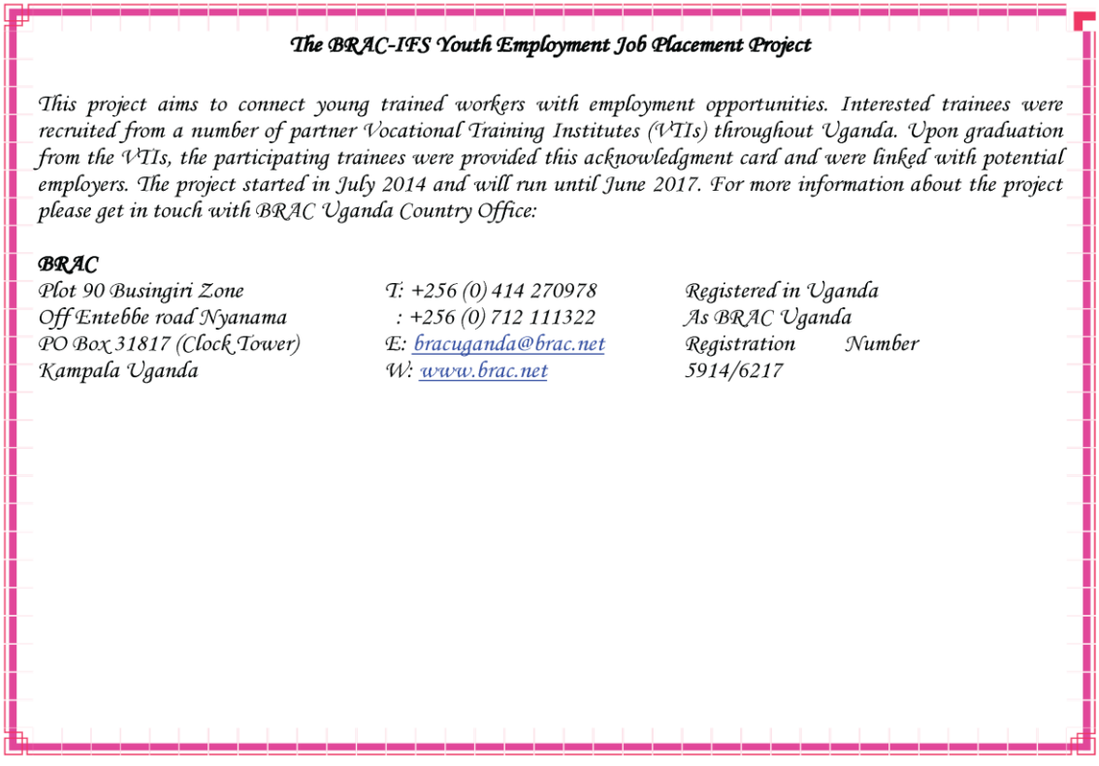
NAME OF TRAINEEE

Has participated in the Youth Employment Job Placement Project in collaboration with BRAC Uganda and the Institute for Fiscal Studies.

Date: 02/04/2015

.....
Dr Jenipher Twebaze Musoke
Coordinator, Research,
BRAC Africa Programme

.....
Mr Bhuiyan Muhammad Imran
Country Representative
BRAC Uganda



The BRAC-IFS Youth Employment Job Placement Project

This project aims to connect young trained workers with employment opportunities. Interested trainees were recruited from a number of partner Vocational Training Institutes (VTIs) throughout Uganda. Upon graduation from the VTIs, the participating trainees were provided this acknowledgment card and were linked with potential employers. The project started in July 2014 and will run until June 2017. For more information about the project please get in touch with BRAC Uganda Country Office:

BRAC

Plot 90 Busingiri Zone
Off Entebbe road Nyanama
PO Box 31817 (Clock Tower)
Kampala Uganda

T: +256 (0) 414 270978
: +256 (0) 712 111322
E: bracuganda@brac.net
W: www.brac.net

Registered in Uganda
As BRAC Uganda
Registration Number
5914/6217