Information on Skills in the Labor Market: Experimental Evidence from Uganda*

Vittorio Bassi† **Aisha Nansamba**‡

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Abstract

We study how employers and job seekers respond to verifiable information on skills that are difficult to observe. We schedule 1,200 job interviews between young labor market entrants and small firms in Uganda, and experimentally vary whether certificates on the workers' soft skills are disclosed to both sides of the market during the job interview process. We collect information on the outcome of each job interview, and track the workers for two years. Our results show that the certificates impact beliefs on both sides of the market: a subset of managers revise upwards their assessment of the skills of workers, while workers increase their labor market expectations, such as expected earnings and perceived bargaining power. 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 own skills. The two-sided reaction to the certificates in terms of beliefs does not result in long-lasting impacts on employment probability or hours worked, yet workers with a certificate earn 11% more while employed in the two years after the intervention.

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

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

1 Introduction

The efficient functioning of labor markets crucially depends on the information available to both employers and job seekers. A large body of theoretical and empirical work has emphasized the role of information frictions on *skills* as a potential source of inefficiency: difficulties in observing the true abilities of workers have been shown to reduce employment levels (Terviö, 2009; Pallais, 2014), to distort wages and wage growth (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Kahn and Lange, 2014) and to contribute to mismatch (Chade and Eeckhout, 2017).

Interventions to increase information on skills typically evaluate how information provision to *one side* of the market affects outcomes - such as how *firms* respond to the introduction of job tests in terms of their hiring choices,[1] or how *workers* benefit from opportunities to better signal their skills to potential employers.[2] This type of interventions however generate an informational asymmetry in the labor market, as the side of the market receiving the information can decide strategically whether to disclose this new information or not. Therefore, one-sided interventions make it difficult to learn about the implications of alleviating the information friction for both workers and firms simultaneously. Yet, this is the key exercise of interest to inform policies aimed at increasing efficiency in the labor market.[3]

This paper fills this gap by studying the impacts of disclosing information on skills that are reported by managers as difficult to observe to *both* the worker and the firm at recruitment. To do so, we design and implement a field experiment in the Ugandan labor market that has two components: (i) a *matching* component, whereby firms and workers are matched for job interviews, and (ii) a *signalling* component, by introducing experimental variation in whether information on the *soft skills* of workers, such as work ethic and interpersonal skills, is disclosed to both sides of the market, through the provision of certificates.

Our main contribution is to study how both firms and workers respond to symmetric information revelation during recruitment. 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 and expected probability of employment. We develop a model of in-

[1] Autor and Scarborough (2008) and Hoffman et al. (2017) provide evidence on the importance of job tests in helping firms recruit workers that are a better match with the position.

[2] Abel et al. (2016) show that reference letters raise employment for female job seekers in South Africa. Abebe et al. (2017a) 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.

[3] An important exception is Pallais (2014), who finds that providing performance ratings in an online labor market improves the outcomes of inexperienced workers and overall market welfare. All potential employers (and workers) in the market can observe the ratings, so that the information revelation is symmetric. We further discuss our contribution relative to this study below. Groh et al. (2015), Abebe et al. (2017b) and Alfonsi et al. (2017) evaluate two-sided labor market interventions, although their focus is on reducing search costs rather than information frictions on skills.

formation acquisition at recruitment to link the updating of firms and workers to labor market outcomes, focusing on employment probability and wages, and compare the model predictions to the reduced form impacts of the intervention on these outcomes. We use our estimates of the program impacts in the two years post intervention, along with the model, to perform a cost-benefit analysis of the intervention.

Our setting is an informal labor market, where information frictions are potentially important. The sample includes young workers fresh out of vocational education and looking for jobs, and Small and Medium Enterprises (SMEs) looking for workers. Young workers can be particularly affected by information frictions on skills, given their lack of work experience. Similarly, SMEs in this context do not have access to sophisticated screening technologies, and so might be less able to screen workers, compared to larger firms.

A pre-intervention survey of SMEs reveals that *soft skills* are important for managers, yet difficult to observe. For example, stealing by their own employees is reported as one of the most important problems SMEs face. In addition, managers believe soft skills to be relatively scarce among workers. These key facts justify our focus on soft skills in this context, as opposed to practical skills that are reported as easier to observe. The survey further shows that cognitive ability is a key source of heterogeneity among managers: managers of higher ability have higher profits, and value soft skills relatively more.[4] Based on this, we allow for heterogeneous responses by manager's ability in the empirical analysis.

To identify the specific soft skills to be revealed on the certificates, we use the stated preferences of firm owners over which skills they would find most useful to receive information on during job interviews. We focus on five skills: attendance, communication skills, creativity, trustworthiness and willingness to help others. These are measured through initial assessments at the vocational institutes where the workers are enrolled, before graduation.

We schedule over 1,200 face-to-face real job interviews, and use these to study how both firms and workers react to the symmetric information disclosure. We collect information on: (i) stated beliefs of firm owners on the skills of applicants, (ii) job offers and hires in the intervention, (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 the workers participating in the intervention are *positively* selected on soft skills, relative to the eligible population. This is explained by the recruitment process for the experiment: all eligible workers were informed that information from their skills assessments could be disclosed to firm owners during job interviews. This positive selection is consistent with selection on expected gains, and indicates that workers with higher skills believe they have

[4]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.

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

We show that workers also react to the certificates by revising upwards their expectations of what they can achieve in the labor market: 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 and to shift their search direction towards public sector jobs. 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 model in which firms use job interviews to acquire information on the skills of workers at recruitment. Due to limited liability, workers with *very low* soft skills can cause a loss to the firm – e.g. through stealing – and firms use job interviews to try to avoid hiring these workers. The certificates reduce uncertainty on skills, but increase the cost to the firm of hiring workers with higher skills, through changes in their outside options. In the model, workers with very low skills are hurt by the intervention. On the contrary, high-skilled workers enjoy higher wages, while the effect of certificates on their employment opportunities is ambiguous, and depends on the increase in the expected productivity relative to the increase in the wage.

We find that the certificates cause an increase in employment probability in matches with high ability managers, while there is no effect on employment among low ability managers. So it is precisely those managers that react to the certificates by revising their beliefs who also react in terms of their hiring choices. However, we do not find sustained impacts on workers' probability of employment or hours worked in the two years after the intervention, neither at the initially matched firm, nor in other firms that workers match to later on. While there are no sustained impacts on employment, we find evidence of long-lasting impacts on earnings: workers with a certificate earn 11% more while employed in the two years after the intervention.

These results confirm the model predictions. The impact on earnings is consistent with workers demanding and obtaining higher wages as a result of the certificates. Motivated by

the impacts on managers' updating, we interpret the positive earnings effects of the certificates as coming from a *productivity effect*, rather than just from a *redistributive effect* from firms to workers: the certificates raise the perceived productivity of workers to high ability managers, so that they are willing to pay these workers more. The fact that earnings increase for the average worker is consistent with workers being positively selected, so that workers with very low skills – who the model predicts should be hurt by our intervention – are actually not in the sample. The null impact on employment in the longer term suggests that the two opposing forces of higher expected productivity and higher wages average out. In line with the impacts on employment being short-lived, we do not find evidence of a sustained increase in firm size for the firms targeted in the matching intervention.

We use the documented impacts on both workers and firms to conduct a cost-benefit analysis of the intervention. We find that the earnings benefits from the certificates in the two years post intervention outweigh the program costs. In line with the average worker benefitting from the certificates, we show that workers 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 per worker of the certificates, and so we discuss potential reasons why similar certificates are not already provided by the market in equilibrium.

Finally, we discuss the implications of introducing a mandatory certification policy on soft skills. This is an interesting exercise because we documented 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 that 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 exercise highlights how mandatory certification policies, while raising efficiency through the improved allocation of labor, could also have important redistributive effects among workers.

Our work contributes to an established literature on employer learning and information frictions on skills (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pallais, 2014; Kahn and Lange, 2014).[5] A closely related paper is Pallais (2014), who shows that disclosing more information about worker abilities increases hiring and welfare in an online labor market. Exploiting our two-sided design, we contribute by providing direct evidence on the mechanisms leading to the reaction of both firms and workers to the information. In particular, we highlight how the certificates affect the *beliefs* of both sides of the market, and how this is essential for interpreting the impacts on earnings as coming from a productivity effect of the certificates.

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

The literature on recruitment in firms finds that the introduction of job tests improves the ability of managers to hire workers with higher skills, that are a better match with the position (Autor and Scarborough, 2008; Hoffman et al., 2017).[6] We contribute by showing that a signalling intervention in which *both* the worker and the firm are informed about the results of skills assessments can have very different impacts on employment and wages, through the reaction of *workers* to the information.

Our study also extends a very recent but growing literature on how labor market frictions in developing countries affect young workers and small firms (Abel et al., 2016; Abebe et al., 2017a,b; Alfonsi et al., 2017; Hardy and McCasland, 2017; De Mel et al., 2018). On the worker side, our contribution is to isolate the importance of information frictions on *skills* in reducing the earnings of skilled workers. Our results neatly complement the analysis in Alfonsi et al. (2017). They find that vocational training, which provides workers with a certificate, leads to higher employment and earnings than informal on-the-job training for young workers in Uganda. Our results confirm that part of the higher returns to vocational training in their context are likely to be due to the certification component, which allows vocational trainees to demand and obtain higher wages relative to uncertified on-the-job trainees. On the firm side, our results complement the work by Hardy and McCasland (2017). They find that a government-sponsored apprenticeship program in Ghana increases firm size and profits by helping firms identify high ability workers, thus concluding that screening costs are an important constraint for small firms. We contribute by providing direct evidence on the productivity effects of skills certificates.

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.[7]

The rest of the paper is organized as follows: Section 2 describes the setting and sample. Section 3 presents the experimental design. Section 4 presents the impacts on beliefs. Section 5 develops a model to map the impacts on beliefs to predictions on employment and wages. Section 6 describes the results on labor market outcomes. Section 7 presents the cost-benefit analysis and discusses the implications of introducing a mandatory certification policy. Section 8 concludes.

[6] A related literature studies the role of temporary employment firms (e.g. Autor, 2001), online outsourcing agencies (e.g. Stanton and Thomas, 2016) and referrals (e.g. Beaman and Magruder, 2012; Brown et al., 2016; Burks et al., 2015; Heath, 2017; Pallais and Sands, 2016) in screening workers in the labor market.

[7] Our study also contributes to a growing literature on the labor market returns to soft skills (see, for example, Bowles et al., 2001; Heckman et al., 2006; Deming, 2017; Groh et al., 2016; Adhvaryu et al., 2017).

2 Setting, Sample Selection and Descriptives

The project was implemented in partnership with an NGO, BRAC Uganda.[8] Figure 1 reports the project timeline. We began the study by identifying the sample of firms and workers for the intervention. This section describes the sample selection process, and presents descriptives on both the firm and worker side.

2.1 Firm Census and Selection into the Experimental Sample

Firms were initially identified by means of a census of SMEs conducted in 17 urban areas of Uganda, covering all four regions of the country.[9] To be included in the census, firms had to: (i) be operating in one of the following six business sectors: carpentry, catering, hairdressing, motor-mechanics, tailoring, welding; (ii) employ at least two workers (in addition to a firm owner).[10] 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,[11] and most firms are located in the region of Kampala, the capital city.

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 are common in urban Uganda, and most vocational training institutes (VTIs) offer courses in these sectors. Second, SMEs in this context 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 signalling component of the intervention.[12] Information was recorded on interest to participate in the program, and all interested

[8]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.

[9]The census took place within a 4km radius from the local BRAC branch in each urban area. This is a separate census in different urban areas than the one discussed in Alfonsi et al. (2017), so the samples of firms in the two studies do not overlap.

[10]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 related to whether the current employees were family members of the owner.

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

[12]The number of graduates assigned to each firm was left unspecified. Also, details about the VTIs workers could come from were not disclosed at this stage. BRAC is one of the largest NGOs in Uganda, and is well known in the local communities for its programs targeting youths and firms. The BRAC Job Placement Program

firms were administered a baseline survey: the 422 firms which confirmed their interest in the program and completed the survey form our experimental sample. Table A1 uses data from the census to study selection into the final sample: we notice that 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).[13] 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.

2.2 Worker Census

We defined as eligible for the project 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.[14] 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 reports basic descriptives for the eligible workers: the median 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, while there are no formal training sessions on soft skills. Students receive a certificate at the end of training with the results of assessments on their practical skills, while soft skills are not certified. 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.[15]

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. 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.[16] 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.[17]

described in this paper is a new pilot program, which builds on previous programs targeting youth employment. Given that all firms in the census were operating within 4km of the local BRAC branch, they were most likely already aware of BRAC and its operations. So concerns related to the credibility of BRAC are not first order.

[13]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.

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

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

[16]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.

[17]Our sample is therefore different from the ones in related studies such as Abebe et al. (2017a), Abel et al.

2.3 Selection of Workers into the Experimental Sample

In addition to basic socio-demographics, the questionnaire for the worker census included two measures of skills: (i) a cognitive test, and (ii) a Big 5 questionnaire.[18]After completing the survey, all trainees in the census were informed about *both* the matching and signalling components of the intervention. Specifically, they were told that BRAC would offer to 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 employers during the matching process. All trainees were then asked their interest to participate.[19] All interested trainees were administered a baseline survey and were included in the skills assessment exercises: the 787 trainees that confirmed their interest in the intervention and completed the main skills assessments are included in the experimental sample.

Since workers were informed about the signalling component of the intervention, a natural question is to what extent this affected their selection into the experimental sample: 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 this, by reporting the results of regressions where an indicator for being in the final sample is regressed on the skills and other background characteristics of all trainees in the census. Column 1 shows that while there is no correlation between cognitive skills and interest in the program, the Big 5 traits are significant predictors in this basic specification that does not control for additional worker characteristics: as shown at the foot of the table, the joint *F-test* of significance of the Big 5 variables 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.[20] The Big 5 variables remain jointly significant at the 1% level in column 2, once a number of controls are added. Figure 2 then 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 sample of workers matched to firms 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

(2016) and Alfonsi et al. (2017), which instead specifically target disadvantaged unemployed youth.

[18]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.

[19]On the worker side, we do not believe concerns related to the credibility of BRAC as an implementing agency to be first order: similarly to firm owners, most trainees are likely to have been aware of BRAC from before, given that BRAC operates in over 100 locations in Uganda.

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

than it would do if the sample of graduates was representative of the VTI population. At the same time, 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 of the program. 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.

2.4 Key Facts about SMEs at Baseline

We conducted a baseline survey of the SMEs included in the 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 are used to justify our research design and empirical strategy. More details on each of 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. In particular, soft skills are reported as more important than numeracy, literacy or theoretical skills. This matches evidence that soft skills have high returns in labor markets in both developing countries and the US (Adhvaryu et al., 2017; Heckman et al., 2006; Deming, 2017).

2. Firm owners report stealing by their own employees and difficulties in observing the soft skills of workers 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.[21] 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 firm owners think that workers with good practical skills are relatively more common than workers with good soft 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 workers that apply without any prior connection or referral: we document that over one-third of the workers employed at baseline in our sample of firms were hired in this way.

5. There is substantial heterogeneity in the cognitive ability of firm owners: we find that cognitive skills are a strong predictor of profits, and managers of higher ability value soft

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

skills relatively more. On the other hand, higher cognitive ability is not associated with different reported constraints in screening workers nor with different expectations on the distribution of soft skills among workers.

The first and second key facts justify our focus on soft 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.[22] 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 among workers, the third key fact suggests that the signalling 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 are better managers and value soft skills more, and so we might expect them to respond differently to the signalling intervention.

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 signalling 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.

3.1.1 Worker Screening

Our screening activities targeted seven soft skills identified as relevant during the initial focus groups with firm owners: creativity, communication, discipline, attendance/time-keeping, trustworthiness, willingness to help others/pro-sociality and pro-activity.[23]

[22]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.

[23]We decided not to focus the screening and signalling component of the intervention on the Big 5, as our piloting exercises revealed that certificates reporting the results of such skills would have been difficult to understand for firm owners and workers. So we preferred to focus on skills that could be more easily mapped

From the discussion in the previous section it should not be surprising that trustworthiness was mentioned as important. Also, existing research suggests that firms value discipline and that absenteeism is widespread in developing countries.[24] Creativity is to be intended both as the ability to generate new ideas, but also as the ability to come up with creative solutions to problems. This is something relevant for *all* sectors in our study, as workers are often asked to use in an ingenious way the limited tools available. In addition, it is common for employees to work in teams, and to take care of customers. This explains why skills such as communication, willingness to help others and pro-activity were mentioned as relevant.

To measure those skills that are easier to assess for an external examiner – attendance, discipline, communication, pro-sociality and pro-activity – we used teacher surveys. To measure creativity and trustworthiness, which are harder to assess based on in-class behavior, we created our own assessments: to elicit creativity, we developed a set of questions with the help of a local psychologist; to elicit trustworthiness, we made trainees play trust games with real money.[25]

We limited the information revelation to five soft skills, to address concerns related to attention constraints of firm owners and workers.[26] To select the five soft skills to be revealed, we used the stated preferences of managers in the baseline survey over which skills they would like to receive more information on during interviews. Specifically, firm owners were asked to rate on a 0-10 scale[27] how important it would be for them to receive additional information on the seven soft skills included in our assessments, if they were to interview workers that had just completed training at VTIs. Figure A7 reports the average importance given to each skill. For the information revelation, we choose the top four skills: creativity, trustworthiness, communication and willingness to help others, plus attendance. This design allows us to treat attendance as a "placebo skill", and to check whether it is given a lower weight during recruitment, relative to the other skills.[28] To facilitate the reporting of information on skills, we followed the Ugandan education system, and graded each skill on a A-E scale. Grades were given using an absolute scale, and so were *not* curved within our experimental sample.[29]

It is important to note how the grading procedure links back to the positive selection on the Big 5 traits documented in Section 2: since the grades were not curved within our sample,

into behavior, such as communication skills. We discuss the correlation between the specific soft skills we focus on and the Big 5 later in this section.

[24]For example, Bowles and Gintis (1976) show that dependability – closely related to discipline – 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.

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

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

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

[28]In practice however, as shown in Table A3.1, the five soft skills are correlated, and so this makes it challenging to separately identify the effect of revealing information on each skill. We return to this point in Section 4.

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

if they are positively correlated with the Big 5 traits, then this suggests that our grade distribution is *positively skewed* relative to the entire VTI population. Table A3.2 regresses the five soft skills measured in the assessments, on the Big 5 measures.[30] 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 also positively correlated with, respectively, trustworthiness and creativity. This suggests that our sample of workers is positively selected not just on the Big 5, but also on at least some of the soft skills revealed during the intervention. Figure A8 reports the distribution of grades for the five soft skills: indeed, 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, thus confirming the positive selection into the final sample.

3.1.2 Validating the Soft Skills Measures

We validate our measures of soft skills by showing whether they predict employment and earnings in our sample of workers in the post-intervention period. We use observations from the Control group, where information on skills was never revealed to workers, and report the results of 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 between the five soft skills documented in Table A3.1, 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.[31]

Table A4 shows that even within our positively selected sample of workers, soft skills are a strong predictor of earnings. This holds true conditional on other observable worker characteristics 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 a Fail mark on at least one skill. Benchmarking this magnitude against the returns to education, we notice that a Pass grade on all the five soft skills has the same effect on earnings as 2.5 additional years of education. While these results cannot be interpreted as causal, they show that our measurements are capturing skills that are associated with labor market success, and that cannot simply be inferred from observable worker characteristics such as education or age. Interestingly, columns 1 and 2 show that soft skills do not predict the extensive margin of whether workers are employed, and nor does education, which is instead another 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

[30] The regressions control for dummies for VTI and sector of training, since the skills measurements were conducted by different teachers and different enumerators across the different VTIs and across the different courses within each VTI.

[31] 41% of workers had a Pass grade on all skills.

overall at 75% for this group of workers.[32]

3.1.3 Treatment Assignment and Matching to Firms

The second component of the intervention involved scheduling job interviews between workers and firms. 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; second, workers and firms were randomly matched for job interviews within each submarket and treatment group. Figure 3 shows a summary of our experimental design. The Online Appendix reports more details on the treatment assignment and matching procedure.

The random assignment to Treatment and Control groups produced a balanced sample. Panel A of Table A5.1 reports balance checks on the firm side, and shows that the sample is balanced on 9 of the 10 variables considered.[33] Also, we are not able to reject the null hypothesis that the 10 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* at the bottom of column 2 (*p-value* = .393). Panel A of Table A5.2 shows that the randomization produced a balanced sample also on the worker side. 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.[34] 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. The difference between the two groups lies only in the signalling component, as discussed in the next subsection.

3.1.4 Skills Signalling

For workers in Treatment we created certificates reporting their grades on the five soft skills. Panel A of 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,[35] as well as guidelines to interpret the grades. To stress the credibility of the certifi-

[32]The fact that we document returns to skills in terms of earnings but not in terms of employment is in line with the reality of informal labor markets in developing countries, where the main challenge for young workers is access to remunerative and stable income generating activities (ILO, 2017).

[33]The normalized difference between Treatment and Control for the other variable (i.e. whether the owner has received training from a VTI) is small (.12). This limits concerns that the sample is unbalanced on this specific variable. To further limit concerns related to potential imbalances, we control for this variable and other basic firm characteristics in our main regression specifications.

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

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

cate, the front page reported the signatures of two high BRAC officials.[36]

To control for any potential effects of simply releasing any new document, a placebo 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. It reports no information about the grades on the soft skills or about the skills assessment procedure (the word "skills" is not mentioned anywhere on the document). Otherwise, the certificate is identical to the Treatment one. This ensures that we can interpret any treatment effects as coming from the *content* of the certificate, rather than from the Treatment workers having an additional document in their application files.[37]

The exact timing of the revelation of the certificates was the following (the procedure was the same in Treatment and Control). On the pre-specified interview day, the worker was first met by the BRAC staff at the local BRAC branch, where the BRAC staff showed the certificate and explained its content to the worker. The worker was also 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.[38]

3.2 Data Collection, Compliance and Attrition

Figure 1 reports the study timeline. We collected four post-intervention surveys. First, a "matching survey" was conducted while the matching intervention was taking place. For each scheduled job interview, the matching survey reports information on: (i) whether the job interview actually took place (and the reasons why it did not take place in case); (ii) basic interview descriptives such as information on the additional documents brought by the trainee to the

[36]BRAC is one of the largest NGO in Uganda, operating in more than 100 locations. It is well known in both urban and rural communities for its programs on youth employment and small firm financing. This further limits any concerns about the credibility of the certificates.

[37]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.

[38]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.

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 key firm-level outcomes. Finally, the first and second worker follow-up surveys were conducted 12 and 26 months since the matchings, and contain information on worker-level labor market outcomes, labor market expectations and search behavior.[39]

Figure 4 shows a graphical summary of compliance and attrition in the experiment. 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 actually called for the interviews.[40] Panel A of Table A6 explores the determinants of whether a scheduled job interview took place, and shows very little evidence of selection on observables.[41] 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 actually 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.[42]

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

[39]Information on whether each job interview in the matching intervention turned into a hire (and on the associated job characteristics such as tenure and wages) 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 3 workers, the median worker was matched to only 1 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.

[40]As shown in Figure 1, for logistical reasons there was a lag of about three months from the graduation dates of trainees (which happened around February 2015) to the roll out of the matching intervention (which started in April 2015), 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 to come to the interview (4% of cases); other reasons (4% of cases).

[41]The only significant predictor of whether a match took place is the duration of the vocational course the worker was enrolled in, so that matches involving workers enrolled in longer programs are less likely to happen. This is likely due to misunderstandings at the time when the workers signed up for the intervention 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).

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

both worker follow-ups.[43] Panel B of 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 Table A5.1 and 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.[44] Therefore, we do not correct for attrition in our main regression specifications, but we then show in Appendix B that our main results are robust to correcting for attrition using inverse probability weights (Wooldridge, 2010).

3.3 Estimation

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

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

where y_{ij} is the outcome of the worker i, firm j match, for example whether the worker was hired by the firm. $Treat_{ij}$ 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.[47] 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).[48]

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 Treatment 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

[43] 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).

[44] The firm follow-up data shows that owner's age is slightly higher in Treatment. However, the normalized difference for this variable remains small (.158), thus limiting concerns related to the sample being 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* reported at the bottom of the table (*p-value* = .215). To further limit concerns related to attrition, we control for owner's age in our main specifications.

[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 Appendix B we show that our main results are robust to the exclusion of control variables.

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

Table A6 shows very little evidence that observable worker and firms characteristics drive which job interviews actually take place. However, if unobservables worker and firm characteristics drive the decision to participate in the matching intervention, then the sample of realized matches might not be representative of the sample of 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 for the job interviews.[49]

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

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

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, to maximize power.[50] Standard errors are robust in these regressions as randomization is at the worker level. In this specification β_1 recovers the ITT parameter. 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 Treatment Effects 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 to the certificates by updating their beliefs about what they can achieve in the labor market. As documented in Section 2.4, soft skills are reported as important but difficult to observe by firm owners. So we can expect the certificates to impact their beliefs on the skills of workers. On the other hand, the certificates might affect the labor market expectations of workers if through the certificates workers improve their ability to signal their skills to potential employers, or if they obtain more precise information about their own skills. Therefore, it is possible that *both* sides of the labor market update their beliefs as a result of the information revelation.

[49] We also show robustness to an alternative approach were we estimate equations like (1) on the full sample of 1,230 scheduled matches by assigning a value of 0 to the outcome of those job interviews that never took place. β_1 recovers the Intention To Treat (ITT) parameter in this alternative specification, which is reported in Appendix B.

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

4.1 Do Firm Owners Respond to the Certificates?

At the end of each job interview, firm owners were asked whether they thought the worker they had just met was similar or different to typical job applicants to their firm. In case they thought the worker was different, they were also asked why that was the case. We construct a dummy equal to one if the firm owner reported the matched worker as being *more skilled* than usual applicants. We run OLS regressions analogous to (1) using 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.

The descriptive analysis in Section 2.4 shows that the cognitive skills of managers might be a potential source of heterogeneity, since managers of higher ability are more profitable and 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 test administered at baseline, and zero otherwise. Columns 2 and 3 split the sample of matches by this dummy for the ability of the matched firm. 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 to the certificates by managers of lower ability. The estimates for the two types of managers 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 who had a Pass grade on all skills, but we are unable to reject the null hypothesis 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 in terms of their beliefs, and this is irrespective of the skills of the workers they meet.

One potential concern with the heterogeneous analysis is that the ability of managers 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. Table A7.1 reports the coefficients on such interactions. The results confirm that the cognitive ability of managers is the key source of

heterogeneity: the coefficient on the high ability dummy remains large and significant at the 5% level. The only other significant interaction is the one with years of education of the owner, and this is not surprising, since cognitive ability and years of education are positively correlated.[51] We can 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).

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 are driving 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 are reported in Table A7.2, and show very similar patterns to Table 4, regardless of whether we use creativity or attendance as our measure of skill. This evidence is in line with high ability managers reacting similarly to information on the different skills, so that any heterogeneity across skills is limited.

It is important to link back the positive updating of managers of higher ability to the selection of workers into the experiment and to firm owner priors: earlier in the paper we documented that workers in the experimental sample are positively selected on soft skills. In addition, we discussed that firm owners have relatively low expectations on the distribution of worker soft skills. As they were never informed about the positive selection of workers into the experiment, then this can explain why the certificates are interpreted as mostly *positive news* by this group of firm owners.

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 the ones with higher cognitive ability, who value soft skills more and are more profitable. The results for managers of higher ability 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. Our results are also consistent with the evidence in Hardy and McCasland (2017) that small firms in developing countries are constrained in their ability to screen workers.

4.2 Do Workers 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

[51] A linear regression of the cognitive ability of managers (measured on a 0-10 scale) on other manager characteristics shows that years of education and cognitive ability are positively correlated (coefficient = .104, robust standard error = .044), while cognitive ability is unrelated to gender, previous training at VTIs, age and sector of operation.

the certificates were left to the workers to keep after the job interview. So if the certificates help workers better signal 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.[52] The results are reported in Table 5. Panel A focuses on *beliefs*. Column 1 shows that workers in Treatment report expected monthly earnings that are $7.95 higher than the Control group, a result significant at the 1% level and corresponding to a 7% increase.[53] 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[54] (column 3) and are also 7% more likely to report that their ideal job is in a firm with 10 or more employees (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 further shows whether Treatment workers change their labor market search *behaviors* in a way consistent with the revision of expectations documented in Panel A. Indeed, we find that workers 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 all consistent with Treatment workers trying to transition to better jobs as a result of the certificates.

Table 6 explores the determinants of *why* workers react to the certificates. We use information from the worker follow-up surveys to distinguish between three alternative 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 in the labor market. In 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, when averaged across the five soft skills. This shows that workers are

[52] Appendix B reports the results of specifications that allow for heterogeneous treatment effects by worker skills. We find no evidence of heterogeneous treatment effects along this dimension.

[53] Monthly 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.

[54] Intention to bargain for wages is constructed using two variables from the worker second follow-up survey: a dummy equal one if 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 negotiation, 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.

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 additional information about their skills through the certificates. Column 3 shows that there is no treatment effect on the perceived returns to soft skills, thus confirming that workers are also not changing their beliefs about the importance of soft skills.[55] Finally, column 4 shows that workers in Treatment believe they face fewer challenges in signalling 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).[56] So the evidence is consistent with the impacts on the perceived outside options of workers being driven by the *signalling 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 side and the worker side to the information embedded in screening assessments, once these are made available also to workers during job interviews.

5 A Model of Information Acquisition at Recruitment

In the previous section, we showed the following reactions to the certificates: (i) managers of higher ability respond by revising *upwards* their beliefs on the skills of workers; (ii) workers respond by increasing their beliefs about what they can achieve in the labor market. In this section, we develop a model of information acquisition at recruitment with two-sided updating that links the response of firms and workers in terms of beliefs, to impacts on employment and wages. The model formalizes the firm problem, and makes precise how the documented response of both workers and firms to the certificates in terms of beliefs can help us interpret the results on employment and earnings presented later in the paper. At the same time, the model indicates how the revelation of information would impact those workers at the very low end

[55]The variable on the perceived returns to soft skills 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 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.

[56]The variable capturing perceived constraints in signalling skills 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 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.

of the skill distribution, who have selected out of the intervention. This will help us formalize the discussion later in the paper about the cost-benefit analysis of introducing a *mandatory* certification policy on soft skills.

5.1 Model Setup

We consider a one-period model of the SME sector. The economy is populated by risk-neutral workers and SMEs. There are two types of workers, who differ in their soft skills: H workers produce positive output: $y_H > 0$; L workers produce negative output: $y_L = -d$. The interpretation is that L workers can steal, can upset customers by having poor communication skills, or can create tension in the firm by creating conflict with other co-workers, so that their net output is negative. For ease of exposition, we assume that each worker type represents one half of the worker population. Workers know their skills, firms do not. H workers have outside option $u_H > 0$; we normalize the outside option of L workers to 0: $u_L = 0$. Outside options are exogenously given, and represent the continuation value of workers.[57] The value of the outside options take into account the ability of workers to signal their skills to firms. Following our experimental design, we assume that workers and firms are randomly paired for job interviews. Each firm (worker) only meets one worker (firm). There is no rematching.

Job interviews generate a signal σ on skills. The signal can be: *Good* (G) or *Bad* (B): $\sigma \in \{G, B\}$. The worker and the firm observe the same signal. This is in line with our experimental design, where firms and workers in Treatment are *both* revealed the same information during the interview. Let q be the probability that the signal is correct, that is: $q = P(G|H) = P(B|L)$, with $q \in [1/2, 1]$. If $q = 1/2$, signals are not informative; the closer q is to 1, the more informative signals are. Each worker can send one correct signal, and one wrong signal. The probability of a wrong signal is $(1 - q)$. There are two types of firm owner, which differ in their cognitive ability: *High Ability (HA)* and *Low Ability (LA)*. They do not differ in their ability to screen workers in the control environment, so that $q_{HA} = q_{LA} = q$. This is in line with the evidence reported in Section 2.4 that the cognitive ability of managers does not predict reported constraints in screening workers. However, they differ in whether they value new information on soft skills, so that *HA* managers respond to new information on soft skills while *LA* managers do not. This is in line with the evidence in Section 2.4 that high ability managers think soft skills are relatively more important, so that they might pay more attention to new information on these skills. Assume that each firm type is one half of the firm population. Further assume that the value of q and the share of firm and worker types are common knowledge. Given signal σ, firms compute the posterior probability of the worker type, using Bayes' rule.[58] Since the

[57] For instance, the outside options can be interpreted as employment opportunities in other sectors of the economy, such as the large firm sector.

[58] Bayes' rule relates the prior to the posterior probabilities. For instance, in the case of H workers and G

share of each worker type is 1/2, q is also the posterior probability.

Upon meeting a worker, firms experience a match-specific monetary taste shock θ for the worker, with $\theta \in [\underline{\theta}, \overline{\theta}]$, and $E[\theta] = 0$. After observing signal σ and shock θ, a firm decides whether to make a job offer. Workers must be paid at least their outside option, which is known by firms. Assume that firms can pay workers a share of output s such that H workers are indifferent between accepting the resulting piece-rate wage and taking their outside option.[59] That is, assume that $w_H = sy_H = u_H$. Further assume that workers have *limited liability*, so L workers earn $w_L = 0$ but cannot compensate the firm for the loss of output. So profits from hiring a H worker are: $\pi_H = (1-s)y_H$, while profits from hiring a L worker are: $\pi_L = -d$.[60] Upon observing signal σ, firms compute the expected profits of making a job offer, $E[\Pi|\sigma]$. These take the following values, depending on whether a G or B signal is realized:

$$E[\Pi|G] = q\pi_H + (1-q)\pi_L \tag{3}$$

$$E[\Pi|B] = q\pi_L + (1-q)\pi_H \tag{4}$$

Equation (3) says that with probability q the worker sending the G signal is a H type, and so yields profits π_H; with probability $(1-q)$ she is a L type, and so creates *negative* profits π_L. Equation (4) can be described similarly. Firms make a job offer if $E[\Pi|\sigma] + \theta > 0$. Note that as long as $q > 1/2$, then $E[\Pi|G] > E[\Pi|B]$, because $\pi_H > \pi_L$. Therefore, firms make the highest expected profits from workers who send a G signal.[61]

5.2 Comparative Statics on Employment and Wages

In line with the evidence in the previous section, we interpret the Treatment as having two effects: first, it eliminates the uncertainty on skills for HA owners, so that $q^T_{HA} = 1$, while it has no effect on the screening ability of LA owners, so that $q^T_{LA} = q_{LA}$. Second, it increases the outside option of H workers to $u^T_H > u_H$. We assume that both HA and LA owners can observe

signals, Bayes' rule can be written as follows: $P(H|G) = \frac{P(G|H)\cdot P(H)}{P(G)}$.

[59] 74% of the workers employed in our sample of firms at baseline are paid piece-rate.

[60] An alternative modeling approach would be to assume that L workers also produce positive output $y_L > 0$, but that binding minimum wages are such that L workers still create a net loss to the firm (i.e. $w_L > y_L$ so that $\pi_L < 0$). This alternative modeling approach would generate exactly 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 such as unions.

[61] Because of symmetric information on outside options, the model predicts that all offers should be accepted. To rationalize rejected offers in the data, it suffices to assume that offers are accepted with exogenous probability δ. For instance, workers might get a negative health shock that prevents them from starting work. The introduction of this additional parameter would not alter the firm's problem substantively.

the change in the outside options of H workers.[62] To guide the exposition, Figure A10 reports a graphical representation of the main comparative statics results.

Starting from LA managers, Panel A shows the relationship between the probability of a hire and worker skills in Control, for the case when $q_{HA} = q_{LA} = q > 1/2$, so when managers are able to extract partly informative signals in Control.[63] Since firms make higher expected profits from workers who send a positive signal, and H workers are more likely to send a positive signal, then H workers are more likely to be hired.

If LA managers do not react to the Treatment certificates, then Panel B shows that the relationship between the probability of a hire and worker skills should be unchanged, if H workers also did not react to the information. The wage should also be unchanged. However, if H workers do react to the certificates by increasing their outside options, then we should observe a *reduction* in hiring of H workers by LA managers, as shown in Panel C: H workers have increased their outside option, and so require a higher wage to accept the job offer. If LA managers are not willing to offer higher wages because they do not value the new information, we should see a reduction in job offers and hires of H workers in matches with LA managers.[64]

Moving to HA managers, Panel D shows the comparative statics predictions for the case when HA managers react to the certificates (so that $q^T_{HA} = 1$) but H workers do not react to the certificates, so that their outside options are unchanged: in this case, the probability of hiring L workers should decrease closer to 0 since these workers create a loss to the firm, and can now be precisely identified by the firm.[65] At the same time, the probability of hiring a H worker should get closer to one, since firms make positive profits from H workers, and H workers do not require to be paid a higher wage if they have not increased their outside options.[66] This case represents what we would expect to see if the revelation of information was *one-sided* to firms, so that firms can act upon the new information, while workers cannot.

However, if H workers also respond to the new information, then the predictions on job offers and hires for H workers become ambiguous, and depend on the relative strength of the increase

[62]For example, this could be manifested to managers during job interviews through higher effort of the worker in bargaining for wages.

[63]The analysis remains the same if we assume that $q = 1/2$, so that managers are not able to extract informative signals in Control.

[64]We assume that firms do not make a job offer if they know this will be rejected. This is equivalent to assuming that firms face a cost in making an offer, and we interpret this as the opportunity cost of waiting for the applicant to decide whether to accept the offer. Information from the baseline survey suggests that waiting might have significant costs: as discussed in Section 2.4, it is common for firms to hire workers that walk to the firm and ask for a job. Job offers are typically made on the spot after the job interview, as it is difficult to contact workers later due to the limited communication technologies available. So while waiting for a worker to accept a job or not, firms might incur the cost of having to turn down other applicants that would then be difficult to contact again if the initial worker finally refuses.

[65]This probability is exactly 0 if we assume that $\overline{\theta} < -y_L$, so that taste shocks for the worker cannot compensate for the loss of output.

[66]This probability is exactly 1 if we assume that $\underline{\theta} > -(y_H - w_H)$, so that profits from a H worker (net of taste shocks) are always positive.

in expected productivity relative to the wage. This is depicted in Panel E, which reports the case when the two opposing forces of higher expected productivity and higher wages average out so that the probability of employment of H workers is unchanged. Wages conditional on employment are higher for H workers, because of the impact on their outside options.

Finally, if through the self-selection of workers into the experiment the final sample that is matched to firms does not include L types, then we should not expect to observe a fall in offers and hires at the low end of the skill distribution. This case is reported in Panel F.

5.3 Discussion

The model makes clear that the positive treatment effect on the wages of H workers in matches with HA firms is the result of the certificates increasing the expected productivity of workers to the firm, so that firm owners are willing to pay workers more. That is, the increase in the wage is an indicator of the *productivity effects* of the certificates, and so is not simply a result of the higher bargaining power of workers *per se*. This is consistent with the empirical evidence from the previous section, where we showed that managers of higher ability positively revise their beliefs on the skills of the matched workers, once a certificate is shown.

In addition, the model highlights that while workers with higher skills *always* benefit from the certificates in terms of higher wages, the certificates can hurt workers at the low end of the distribution, by reducing the propensity of HA firms to hire them. So the certificates make the allocation of workers between employment and unemployment more efficient, but can also increase inequality among workers. Whether the gains for H workers outweigh the losses for L workers 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 the 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.

Finally, the model highlights how the effect of the certificates on firm profits is ambiguous: on the one hand, HA managers benefit from an increased ability to screen workers, which allows them to better separate the productive workers from the unproductive ones. On the other hand, the certificates also raise the wages that the productive workers need to be paid. So while the profit effects would necessarily be positive in a *one-sided* information intervention to firms, profit effects become ambiguous in a two-sided intervention, because of the reaction of *workers*

to the information.[67]

6 Treatment Effects on Employment and Earnings

We now discuss the reduced form impact of the certificates on employment and earnings. We first focus on match-level outcomes at the matched firms. We then extend the analysis to consider the worker-level outcomes in the two years post intervention, regardless of whether the worker met any of the matched firms. There is one important difference between the two levels of analysis: when analyzing outcomes at the match level, we note that the allocation of workers to firms was random (conditional on sector and region). So these results capture the impact of the certificates *conditional* on a pre-scheduled meeting taking place, abstracting from any effects of the certificates on search behavior. On the other hand, when analyzing worker outcomes in the two years post intervention, we note that these capture the *total effects* of the certificates on workers, including any changes in search direction or effort. Combining both levels of analysis, we are able to draw conclusions on whether any effects of the certificates on employment and earnings come solely from workers changing their search strategy, or whether instead these are also the direct result of the demand side of the market valuing the new information.

6.1 Impacts at the Matched Firms

Table 7 reports the results of match-level regressions analogous to (1) but 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). As discussed above, the sample includes the 515 matches that took place. Again, we focus on treatment effects on the average worker, since our sample of workers are positively selected on soft skills, so that we do not expect significant heterogeneity by worker skills.[68]

Column 1 shows no impact on job offers for matches with the average manager. The same remains true when considering hires in column 5: the coefficient on the Treatment indicator is very close to 0 and not significant. This result is in line with the empirical evidence in Section 4, where we found that the average manager did not respond to the certificates in terms of beliefs. However, the null impacts for the average managers mask once again 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 higher 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). So it is precisely those managers

[67]The profit effects of the certificates are negative for LA firms since H workers become more expensive but the screening ability of LA firms does not improve, as LA firms do not value the new information.

[68]Heterogeneous effects by worker skills are reported in Appendix B.

who react in terms of beliefs who also react in terms of hiring choices. The model predicts that the impact on hiring is ambiguous in matches with managers of higher ability, and depends on the relative increase in the expected productivity of the worker, relative to the increase in the wage. These results then confirm that in the short run the increase in expected productivity more than offsets any increase in the wage.

Columns 3 and 7 instead show no change in job offers and hires in matches with managers of lower ability. If anything, the estimated treatment effect on hires is negative in column 7. The estimate is noisy, and we cannot rule out even a relatively large decrease in hires of 10-12pp. So this is again in line with the model, which predicts that we should not observe an increase in hires in matches with managers of lower ability, since they did not react to the information. The *p-value* in column 8 confirms that the treatment effects on hires are significantly different between the two groups of managers at the 5% level.[69] To confirm that owner's cognitive ability is the key dimension of heterogeneity, in Table A8.1 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.

While the results on job offers and hires show significant heterogeneous effects by manager's ability, we note that the overall level of hires in the intervention was low: less than 50 workers were hired in total across the two experimental groups.[70] This makes it difficult to study the impact of the certificates on wages conditional on employment at the matched firm. Nevertheless, Table A9 reports the means of total earnings in the first week at the matched firm, by treatment group. We notice that earnings are higher in Treatment, though the estimates are noisy due to the small sample size, and we cannot reject equality of means across treatment groups. While these results obviously need to be interpreted with caution due to the very small sample size, they are in line with the model prediction that the certificates raise worker earnings. We probe this finding in the next subsection, by running earnings regressions which include *all* workers in the two years post intervention. Finally, the small number of hires prevents us from presenting conclusive evidence on the impact of the certificates on productivity and retention at the matched firm. In terms of duration, we notice that by the one year follow-up all but one experimental worker have left the matched firm. This is consistent with the high turnover rates documented in informal labor markets in Uganda and other developing countries (Alfonsi et al., 2017; Blattman and Dercon, 2018).

[69]Not surprisingly, the results on job offers are more noisy, due to the difficulty in defining a job offer in this informal context where offers are not written: the *p-value* on the test of quality of treatment effects between the two groups of owners is .124 for this outcome.

[70]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. (2016), 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.

6.2 Impacts in the Two Years Post Intervention

Tables 8 and 9 report the results of worker-level regressions analogous to (2) where we pool observations across the two follow-up surveys. Again, we focus on treatment effects for the average worker.[71] Table 8 focuses on employment outcomes, while Table 9 on earnings. Table 8 shows very little evidence of any long-lasting 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 and 4). Finally, even though Treatment workers are more likely to have looked for jobs in the public/NGO sector and have revised upwards their ideal firm size (as shown in Table 5), there is no evidence that they are more likely to work in those types of firms (Table 8, columns 5 and 6). This result echoes the findings in Muralidharan et al. (2017) on the role of the NREGA program in India in raising wages: they show that while improved access to NREGA – a guaranteed public works program – did not raise earnings from the program itself, it did raise local wages by creating a *credible outside option* for workers. By a similar reasoning, even though we do not observe Treatment workers transitioning to different jobs, the certificates can raise their wages by making their outside option more credible. The absence of long-lasting effects on employment are consistent with the model, and show that in the longer term the two opposing forces of higher expected productivity and higher wages average out.

Table 9 considers impacts on earnings. Column 1 confirms that there is no impact on the extensive margin of paid employment. That is, the Treatment does not affect the *share* of individuals working for pay. In columns 2-7 the dependent variable is total earnings in the last month. To correct for outliers, earnings are trimmed at the 99th percentile in columns 2 and 4-7. In column 3 we apply an inverse hyperbolic sine transformation to the dependent variable, as an alternative correction for outliers (De Mel et al., 2018).[72] Column 2 shows that when all workers are included in the regression, so that individuals with no earnings are assigned a value of 0 for the dependent variable, we find an 8% increase in total earnings, but this is not significant at conventional levels. Applying the inverse hyperbolic sine transformation in column 3 also shows a positive but not significant impact. Columns 4-6 report the results of quantile regressions using 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.[73] Finally, since we showed that Treatment does not affect selection into paid employment, in column 7 of Table

[71] Additional results reported in Appendix B confirm that we do not find significant heterogeneity by worker skills nor by whether the worker was initially matched to high or low ability managers.

[72] Appendix B shows robustness to alternative corrections for outliers, such as top coding or applying a log transformation.

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

9 we report the impacts for the sample of workers with positive earnings:[74] 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. The evidence on earnings confirms the model predictions that we should observe an increase in wages as a result of the certificates.

Linking the results on earnings to the literature on labor market frictions in developing countries, we note that related evaluations of job search assistance interventions tend to find impacts primarily on the extensive margin of employment probability, rather than on the intensive margin of earnings conditional on employment (Abebe et al., 2017a; Abel et al., 2016). However, these related evaluations target workers that are unemployed at baseline, and so it is natural to expect any impacts to be on the employment margin. By contrast, our results show that in a population of trained workers that face fewer challenges in finding employment, impacts are concentrated on the intensive margin of earnings, as workers increase their labor market expectations and demand higher wages in response to the certificates.

7 Cost-Benefit Analysis

We now present the cost-benefit analysis of the intervention for program participants. We later discuss how this would change with the introduction of a mandatory certification policy on soft skills. This is an interesting comparison because the cost-effectiveness implications could be very different in the mandatory case: 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.

7.1 Cost-Benefit Analysis for Program Participants

Table 10 reports the cost-benefit analysis for program participants, that is, for those eligible workers who accepted to participate in the signalling and matching intervention.[75] We have

[74] 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 who are employed (Table 9, column 1). Also, Table A10 shows the sample of workers employed at follow-up remains well balanced on baseline characteristics, across Treatment and Control. Importantly, the sample remains balanced on soft 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 (the largest difference is .084 for the Age variable), and the observable worker characteristics are all jointly not significant in predicting treatment assignment, as shown by the *p-value* from the *F-test* at the bottom of the table (*p-value* = .384). These results shows that the Treatment is also not changing the *composition* of workers in paid employment, and so limit concerns that estimating treatment effects on earnings for the selected sample of workers in paid employment captures selection effects.

[75] For the purpose of the cost-benefit analysis we assume zero impacts on the firm side. Table A13 confirms that we do no find any long-lasting impacts of the intervention on number of employees by the time of the firm

detailed information on program costs, as all activities were implemented by BRAC.[76] 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.[77] 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.

7.2 Cost-Benefit Analysis of a Mandatory Certification Policy

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. 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 low-type workers could be hurt by a mandatory certification policy, in terms of their employment and earnings. Therefore, we 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 of this thought exercise. 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. These impacts vary from +15% to -100%. We assume that in the absence

follow-up (and this does not vary by the ability of managers). This is not surprising, given the low take-up rate in the matching intervention. Our data does not allow us to study the effects of the intervention on profits: while overall attrition is low in the firm follow-up, information on profits and revenues is missing for about half the sample, who refused to prove this information. Such high prevalence of missing values, coupled with the low levels of hiring in the matching intervention, make it impossible to reliably estimate the profit effects of the certificates. As highlighted in the model, the predictions on the effects of the certificates on firm profits are ambiguous. So while we assume zero impacts of the intervention on firm profits, providing more direct evidence on the firm-side impacts of certification interventions remains an important area for future research.

[76]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.

[77]This is a more conservative approach than using the estimate for the sample of workers in paid employment (column 6 of Table 9), multiplied by the share of workers in employment.

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.[78] 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 Panel B of Table 10). The red dotted line is the break-even point, where benefits equal costs. The figure shows that if those workers that selected out of the intervention 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.

7.3 Why Are Soft Skills Certificates Not Provided in Equilibrium?

The discussion in Section 7.2 shows 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 this information is valued by at least some workers and firms. We can rule out lack of demand by workers as a potential explanation. 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 to the ones disbursed in Treatment.[79] We find that workers in Control would be willing to pay $18 on average for the certificates, corresponding to 44% of their monthly earnings.[80] 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. We also note that

[78] We did not follow-up in the post-intervention period with those workers who selected out of our intervention, so we do not observe their earnings in our data.

[79] 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 the certificate. For more details on measuring willingness to pay in the field, see Berry et al. (2015).

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

providing soft skills certificates might not be profit maximizing for VTIs: as discussed, about 20% of the eligible workers declined the opportunity to participate in the signalling 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 to rate students on soft skills 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.

8 Conclusion

This paper studies how lack of verifiable information on the skills of workers affects both employers and job seekers. We do so by designing and implementing a two-sided labor market experiment in Uganda. We schedule job interviews between young graduates from vocational institutes and small firms that are looking for workers. We then introduce experimental variation in whether soft skills certificates are disclosed to both the worker and the firm during the job interview process.

Our main finding is that both sides of the labor market respond to the certificates in terms of *beliefs*: workers increase their expectations about what they can achieve in the labor market, while firm owners with higher cognitive ability – who manage more profitable firms and value soft skills more – revise upwards their assessments of the skills of workers. We then show how this reaction in terms of beliefs is essential for interpreting the long-lasting impacts of the certificates on the employment and earnings of workers. We find that workers with a certificate earn more while employed in the two years post intervention, and this is so because the certificates increase the ability of workers to signal their skills, which allows them to demand higher wages; on the other side of the market, the certificates improve the perceived productivity of workers to at least some managers, who are willing to pay them a higher wage. That is, the evidence on beliefs allows us to interpret the positive earnings impacts of the certificates as coming from a

productivity effect, rather than just from a *redistributive effect* from firms to workers.

This paper extends an established literature on employer learning and information frictions in the labor market (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pallais, 2014) by providing direct evidence on how new verifiable information at recruitment affects the beliefs of both sides of the market, and how the interplay of these effects is important for interpreting the impacts of the information on employment and earnings. At the same time, this paper extends a growing literature on labor market frictions in developing countries (Abebe et al., 2017a; Abel et al., 2016; Alfonsi et al., 2017; Hardy and McCasland, 2017). Our main contribution here is to highlight that both sides of the labor market value new information on skills, and that skilled workers benefit from this information through higher earnings. We also highlight that information policies in the labor market can carry with them an important trade-off between efficiency and equity: we show that while information interventions are likely to improve the allocation of labor to firms and overall productivity, some workers with lower skills can lose out from such policies, so that inequality among workers can also increase as a result.

Our finding that soft skill certificates are valuable to at least some workers and at least some firms raises the question of why this type of information is 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 to do in settings where corruption is pervasive and information difficult to verify. Also, we discuss that 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 new insights into the functioning of both labor markets and education markets in low income countries, and so is something worth attempting further in future research.

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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[81] how important different skills are in their firms. Figure A1 reports the average importance given to each skill.[82] 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.[83] 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: the cognitive skills of managers are a strong predictor of profits, and also managers of higher ability value soft skills relatively more. 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.[84] Table A2 reports the results of OLS regressions to test whether (i) profits; (ii) perceived returns to soft skills; (iii) perceived constraints in observing soft skills; (iv) priors on the distribution of soft skills in the worker population and

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

[82] 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.

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

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

(v) recruitment channels vary with the cognitive skills of the manager. Specifically, we create a dummy equal to one if the manager scored at the median or above on the cognitive test, and zero otherwise. We then regress these five outcomes on the high ability dummy, and a set of owner and firm characteristics.[85] Column 1 of Table A2 shows that high ability owners earn about 26% higher profits (a result significant at the 10% level). We confirm this result in Figure A6, which plots the residuals from a similar profit regression, splitting by firm owner type. The figure once again confirms that high ability owners have higher residual profits per worker. Column 2 of Table A2 shows that high ability owners value soft skills relative more than practical skills. The rest of Table A2 shows that the two groups of firm owners do not differ in their perceived difficulty in screening workers (column 3), in their priors on the distribution of soft skills among workers (column 4), and in their propensity to recruit unconnected workers (column 5).

B Robustness Checks

This section describes the main robustness checks mentioned in the paper. For a discussion of the sample selection model and related results see the Online Appendix.

B.1 Match-Level Impacts on Manager's Beliefs

B.1.1 Robustness to Using the Full Sample of Scheduled Matches

Table A7.3 reports the results of regressions like (1) but run on the full sample of 1,230 *scheduled* matches, regardless of whether the job interview took place or not. To do so, we assign a value of 0 to the outcome of those job interviews that never took place. The results show that our key finding that high ability owners revise upwards their beliefs on the skills of matched workers is robust to this alternative specification: the coefficient on Treatment in column 2 is significant at the 5% level, and so is the coefficient on the interaction between Treatment and a dummy for whether the worker had a Pass mark on all skills in column 6. In line with the results in Table 4, we do not find any treatment effects for managers of lower ability. The difference in treatment effects between high and low ability managers for workers with a Pass mark on all skills remains significant at the 5% level, as shown by the *t-test* of equality of coefficients across manager types in column 8 (*p-value* = .021). However, column 4 shows that the difference in the treatment effect for the average worker between manager types is now less precisely estimated (*p-value* = .144). This is not surprising, as effectively we are assigning a value of 0 to the outcome of more than half of the sample, which obviously reduces the precision of the estimates. Taken

[85] These include: number of employees, a dummy for gender of the owner, age of the owner and age squared, a dummy for whether the owner attended a VTI in the past, sector dummies, dummies for BRAC branch.

together, these results show that our key findings on firm owner's beliefs are robust to running the analysis on the full sample of scheduled matches. This is consistent with our evidence that Treatment does not affect selection into the final sample that meets presented in Table A6.

B.2 Match-Level Impacts on Job Offers and Hires

B.2.1 Heterogeneous Effects by Worker Skills

Table A8.2 reports the results of regressions equivalent to (1) but where treatment effects are allowed to differ by whether the worker had a Pass grade on all soft skills, or whether had a Fail grade on at least one skill. The table shows that treatment effects on job offers in matches with high ability managers are larger for workers who had a Fail grade on at least one skill (column 2). However, when focusing on our key outcome of whether the job interview turned into a hire, we are unable to reject the null hypothesis that treatment effects vary by worker skills, as shown by the *p-value* on the *F-test* of equality of coefficients reported at the bottom of column 6 (*p-value* = .537). Therefore, taken together the results from this table do not show strong evidence of substantial heterogeneity by worker skills, and so again justify our focus on the average worker in the main specifications.

B.2.2 Robustness to Using the Full Sample of Scheduled Matches

Table A8.3 reports the results of regressions equivalent to (1) but run on the full sample of 1,230 *scheduled* matches, regardless of whether the job interview took place or not. To do so, we assign a value of 0 to the outcome of those job interviews that never took place. The results are broadly consistent with the ones reported in Table 7, and confirm that positive treatment effects of the certificates on hiring are found only for matches with high ability managers. This is shown by the positive treatment effects in columns 2 (for job offers) and column 6 (for hires), which are both significant at the 5% level. However, the difference in treatment effects across manager types is now less precisely estimated, as shown by the *p-values* in columns 4 and 8. This is not surprising though, as effectively we are assigning a value of 0 to the outcome of more than half the sample, which reduces the precision of the estimates. Taken together, these results show that our key findings on employment at the matched firm are robust to using the full sample of matches. This is consistent with our evidence that Treatment does not affect selection into the final sample that meets presented in Table A6.

B.2.3 Robustness to Running the Analysis at the Worker Level

Table A8.4 reports the results of regressions like (1) but at the worker level. To do so, the dependent variable is a dummy equal to one if the worker (i) received at least one job offer

(columns 1-4) and (ii) was hired by at least one firm (columns 5-8). The results confirm that we are still able to pick up a positive treatment effect in matches with high ability managers. This effect is significant at the 1% level for offers in column 2, and remains significant at the 10% level when we consider hires in column 6. However, the comparison of treatment effects across manager types becomes less precise, as shown by the *p-values* in columns 4 and 8.[86]

B.3 Worker-Level Impacts

B.3.1 Heterogeneous Effects by Worker Skills

Table A12.1 reports the results of worker-level regressions like (2) but where treatment effects are allowed to differ by whether the worker had a Pass grade on all soft skills, or whether had a Fail grade on at least one skill. We do so for our main worker-level outcomes from Tables 5, 8 and 9. In short, the *p-values* on the test of equality of treatment effects reported at the bottom of the table show that treatment effects do not vary significantly by the skills of workers.

B.3.2 Heterogeneous Effects by Matches with High and Low Ability Managers

Table A12.2 reports the results of worker-level regressions like (2) but where treatment effects are allowed to differ by whether the worker was matched to high or low ability managers during the matching intervention.[87] We check this dimension of heterogeneity because Table 7 shows that there is a positive treatment effect on hires in matches with high ability owners, and the literature has emphasized that even short term initial employment opportunities can have long-lasting effects on workers by providing a "stepping stone" in the labor market (Pallais, 2014). Again, we consider our main worker-level outcomes from Tables 5, 8 and 9. The *p-values* on the test of equality of treatment effects reported at the bottom of the table show that treatment effects do not vary significantly by whether the worker was matched to a high or low ability manager. In interpreting these findings, we note that even though we do find a positive treatment effect on hiring in matches with high ability managers, this does not mean that the *overall* employment level of Treatment workers matched to high ability managers was higher in the short aftermath of the matching intervention (since we do not know the early employment outcomes of Treatment workers in firms not in our intervention).

[86]The heterogeneous analysis is confined to workers who were matched to either only high ability managers or only low ability managers (those 23% of workers matched to both high and low ability managers are excluded).

[87]The heterogeneous analysis is confined to workers who were matched to either only high ability managers or only low ability managers (those 23% of workers matched to both high and low ability managers are excluded).

B.3.3 Robustness to Outliers in Earnings Measures

In columns 1 and 2 of Table A11 we consider the robustness of our results on labor market earnings to alternative approaches to reduce the influence of outliers. The sample includes workers with positive earnings, and the specifications are equal to the one reported in column 6 of Table 9. In column 1 we top code earnings at the 99th percentile. This corresponds to assuming that values in the top 1% of the earnings distribution were *not* entry mistakes by enumerators, but were indeed the correct amount. We still find a treatment effect equal to a 9% increase in earnings, which is significant at the 10% level (*t-stat* = 1.73). In column 2 we again use the full sample of workers with positive earnings, but apply a log transformation. Our estimated treatment effect is stable at around a 10% increase in earnings, with associated *t-stat* = 1.84. Taken together, these results confirm that our treatment effect estimates on earnings are robust to alternative ways of correcting for outliers.

B.3.4 Robustness to the Exclusion of Control Variables

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 3 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 sample includes workers with positive earnings, and the specification is otherwise equal to the one reported in column 6 of Table 9. The estimated coefficient (6.84) is very similar to the one in the specification with controls (7.19). While the standard error is slightly larger when we remove the controls, the estimated impact remains significant at the 5% level, thus alleviating concerns that the inclusion of controls is affecting our main conclusions in a significant way.

B.3.5 Alternative Corrections for Attrition, and Dynamic Treatment Effects

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 4 of Table A11. The sample includes workers with positive earnings, and the specification is otherwise equal to the one reported in column 6 of Table 9. 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.

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

OLS regression coefficients, robust standard errors in parentheses

	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
	(2.76)	(.114)	(.131)	(.033)	(.024)	(.036)	(.024)
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 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. 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 employment at matched firms

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 post-intervention employment outcomes

OLS regression coefficients, robust standard errors in parentheses

	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	Last job as employee is in a large firm	Last job as employee is in public/ngo sector
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.030	-.003	2.03	-1.16	.001	-.001
	(.027)	(.022)	(1.84)	(1.53)	(.023)	(.015)
Mean of dep. var. in Control	.428	.211	37.0	16.0	.247	.036
Controls for baseline value of outcome	Yes	Yes	No	No	No	No
Uses data from first followup	Yes	Yes	Yes	Yes	Yes	No
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,350	1,350	1,347	1,349	1,344	671

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 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 post-intervention labor market earnings

OLS regression coefficients in columns 1-3 and 7

Quantile regression coefficients in columns 4-6

Robust standard errors 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]					
Transformation:	None	Trimmed at 99th percentile	Inverse hyperbolic sine	Trimmed at 99th percentile			
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**
	(.024)	(2.90)	(.199)	(1.31)	(3.14)	(4.13)	(3.24)
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 adjusted for heteroskedasticity. 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.

Table 10: 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 1: Timeline

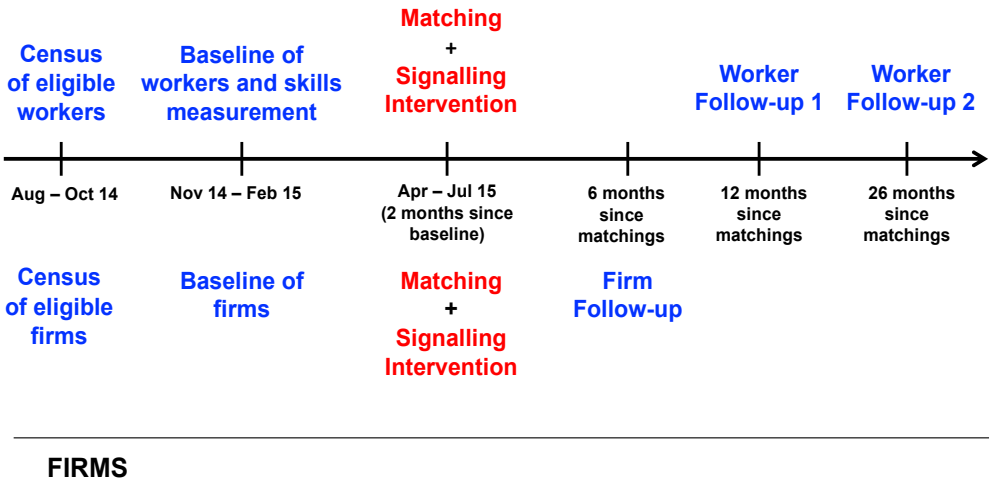
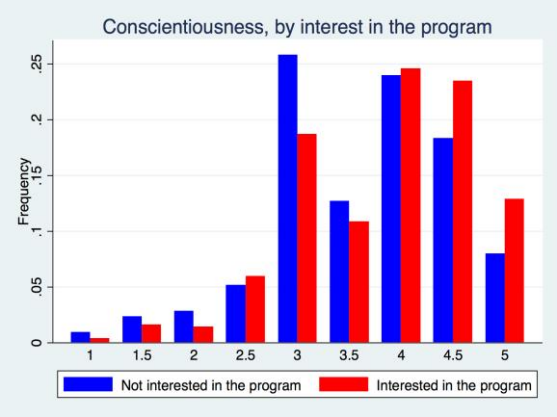
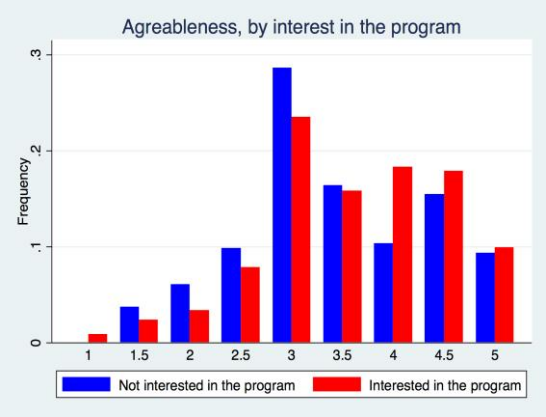


Figure 2: Distribution of worker soft skills, by participation in the program

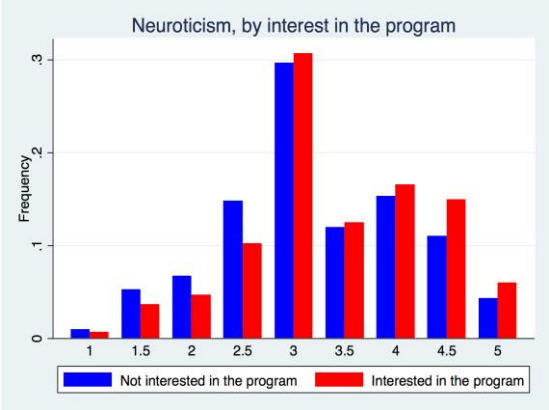
Panel A



Panel B



Panel C



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

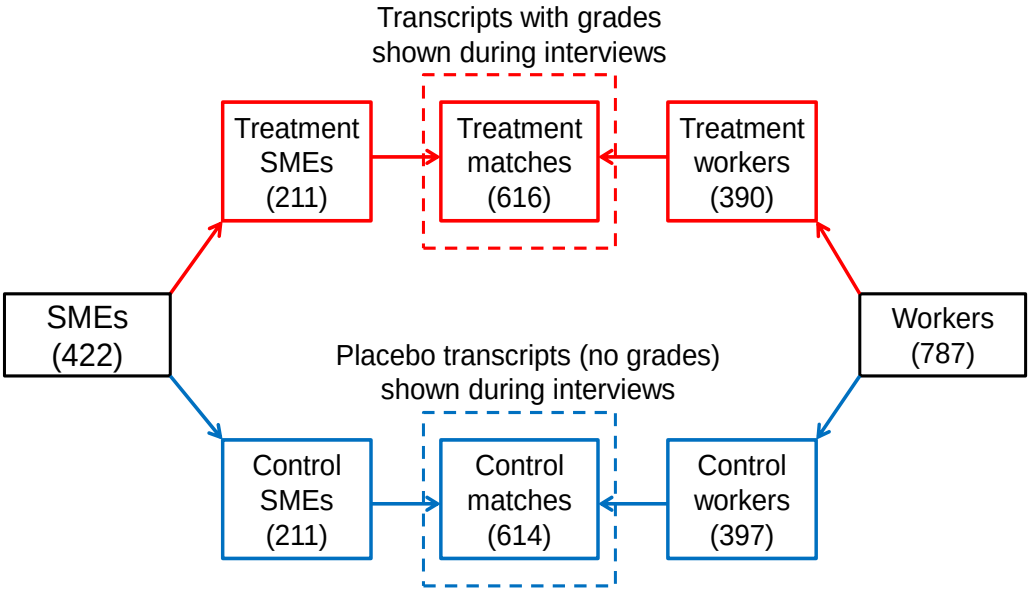


Figure 4: Compliance and attrition

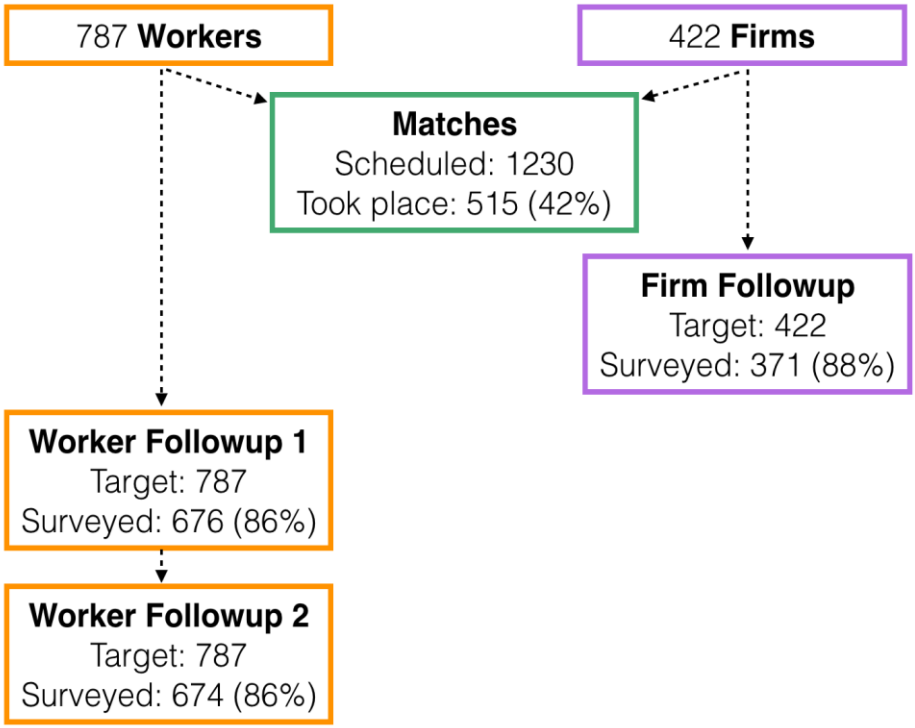
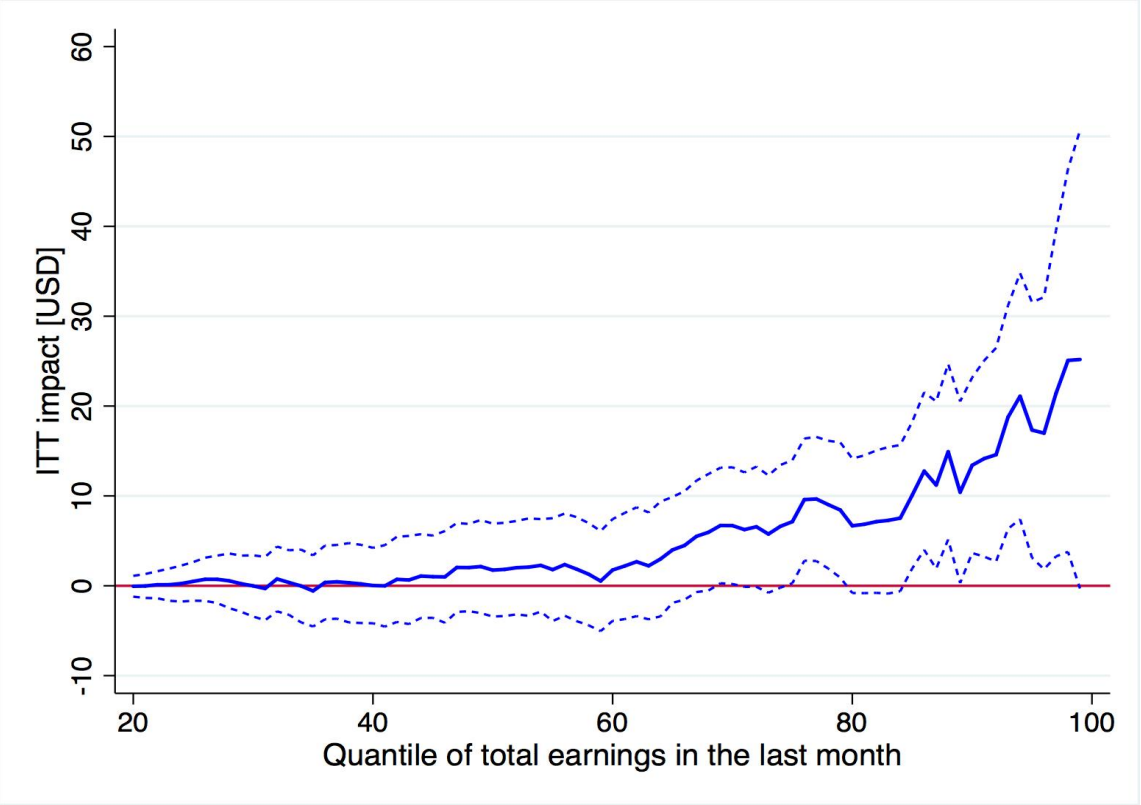
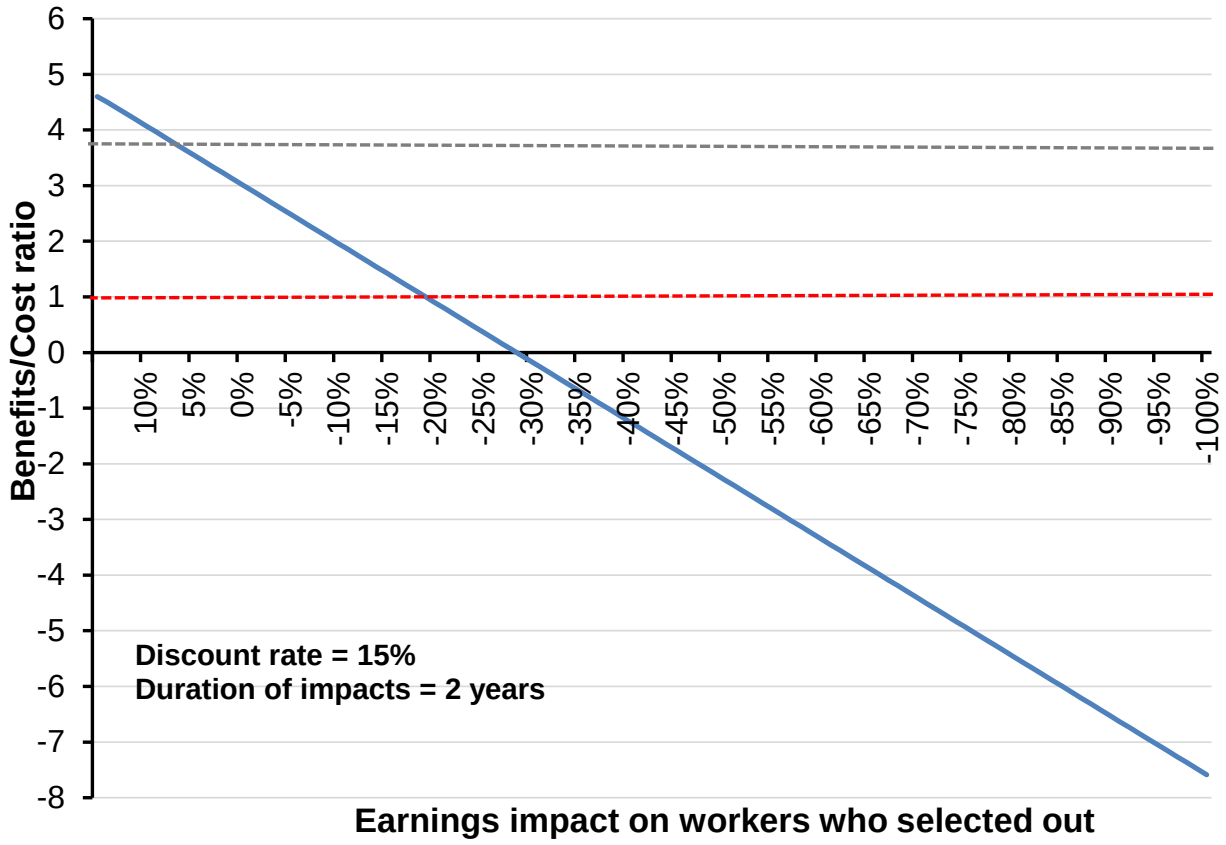


Figure 5: Impacts on post-intervention labor market earnings: Quantile regression evidence



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 10, 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.

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

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Average monthly profits [USD]	High relative perceived returns to soft skills	High relative perceived importance of difficulties in observing soft skills at recruitment	High difference in relative perceived scarcity of practical vs soft skills	Number of employees unconnected at recruitment
	(1)	(2)	(3)	(4)	(5)
High Ability Owner	46.5*	.114**	.022	.011	.112
	(28.1)	(.053)	(.052)	(.054)	(.144)
Number of employees	8.09	-.012	.009	.001	.307***
	(7.27)	(.009)	(.009)	(.010)	(.049)
Owner is female	-59.0*	.103	.027	-.066	.129
	(30.9)	(.077)	(.069)	(.076)	(.224)
Age of owner	-1.59	.016	-.010	.021	.091*
	(9.85)	(.016)	(.018)	(.016)	(.047)
Age of owner squared	.007	-.000	.000	-.000	-.001*
	(.114)	(.000)	(.000)	(.000)	(.001)
Owner attended a VTI	-33.1	.022	.029	.013	-.001
	(29.5)	(.049)	(.052)	(.052)	(.149)
Mean of dep. var. for Low Ability Owners	177.3	.509	.497	.491	.871
Sector and BRAC branch dummies	Yes	Yes	Yes	Yes	Yes
P-value on F-test of joint significance of sector dummies	[.000]	[.471]	[.944]	[.425]	[.204]
P-value on F-test of joint significance of BRAC branch dummies	[.000]	[.000]	[.000]	[.000]	[.001]
Number of observations (firms)	292	422	422	422	422

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the firm baseline survey are reported. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: dummy for female owner; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees; sector and BRAC branch dummies. 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 dependent variable in column 1 is 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 from the regressions in column 1. The dependent variable in column 2 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. The dependent variable in column 2 then takes value one if the relative importance of soft skills is on or above the median. For column 3, 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 dependent variable in column 3 takes value one if the relative importance of difficulties in observing soft skills is on or above the median. For column 4, 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 dependent variable in column 4 takes value one if this measure is on or above the median. In column 5 the dependent variable 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. All regressions further control for dummies for missing values in each of the independent variables.

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, robust standard errors 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**
	(.034)	(.036)	(4.28)	(4.30)	(4.96)	(4.75)
Age		.128		7.85		1.85
		(.084)		(7.97)		(10.9)
Age squared		-.003		-.203		-.058
		(.002)		(.188)		(.259)
Female		-.114*		-15.4*		-13.1
		(.065)		(8.60)		(9.31)
Completed prior education [Years]		.012		3.94***		4.15***
		(.012)		(1.14)		(1.21)
Course duration [Years]		-.038		.490		3.05
		(.023)		(2.66)		(2.92)
Ever employed		.061		10.6*		6.74
		(.044)		(5.76)		(6.36)
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		[.232]		[.150]		[.085]
P-value on F-test of joint significance of BRAC branch dummies		[.032]		[.005]		[.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 A7.1: 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 A7.2: Heterogeneous 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)**
Fail grade on Creativity X Treatment	-.055	-.014	-.060	[.687]				
	(.045)	(.077)	(.076)					
Pass grade on Creativity X Treatment	.018	.149***	-.032	[.019]				
	(.029)	(.055)	(.046)					
Fail grade on Attendance X Treatment					-.038	.059	-.006	[.592]
					(.046)	(.055)	(.100)	
Pass grade on Attendance X Treatment					.016	.124**	-.044	[.015]
					(.028)	(.055)	(.037)	
Mean of dep. var. in Control group	.041	.037	.047		.041	.037	.047	
P-value Fail = Pass	[.163]	[.096]	[.716]		[.308]	[.352]	[.709]	
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]. The dependent variable takes value zero if the match was not carried out (so if the firm and the worker never met). 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 Creativity (columns 1-4) or Attendance (columns 5-8), as 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 A7.3: Impacts on firm owner beliefs about matched workers - Including zeros

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	.002	.040**	-.006	[.144]				
	(.012)	(.020)	(.024)					
Fail grade on at least one skill X Treatment					.003	.024	.005	[.652]
					(.015)	(.022)	(.031)	
Pass grade on all skills X Treatment					.001	.066**	-.026	[.021]
					(.017)	(.028)	(.031)	
Mean of dep. var. in Control group	.041	.037	.047		.041	.037	.047	
P-value Fail = Pass					[.957]	[.162]	[.454]	
Number of observations (matches)	1,229	532	489		1,229	532	489	

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 dependent variable takes value zero if the match was not carried out (so if the firm and the worker never met). 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 column 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 A8.1: Heterogeneous impacts on employment at matched firms

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 A8.2: Heterogeneous impacts on employment at matched firms

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)
Pass grade on all skills	.049	.108	.117	[.944]	-.021	.067	-.041	[.255]
	(.057)	(.086)	(.098)		(.043)	(.075)	(.058)	
Fail grade on at least one skill X Treatment	.088	.230***	.100	[.232]	.010	.158***	-.016	[.028]
	(.060)	(.067)	(.089)		(.042)	(.055)	(.056)	
Pass grade on all skills X Treatment	-.068	.026	-.170	[.236]	-.011	.094	-.038	[.273]
	(.065)	(.102)	(.130)		(.051)	(.088)	(.083)	
Mean of dep. var. in Control	.222	.136	.256		.116	.080	.105	
P-value Pass = Fail	[.148]	[.003]	[.146]		[.732]	[.537]	[.779]	
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 A8.3: Impacts on employment at matched firms - Including zeros

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	.004	.057**	.023	[.382]	-.001	.041**	.005	[.196]
	(.019)	(.023)	(.033)		(.013)	(.019)	(.021)	
Mean of dep. var. in Control	.079	.053	.093		.041	.031	.038	
Number of observations (matches)	1,126	473	449		1,126	473	449	

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker followup surveys are reported. Standard errors are adjusted for heteroskedasticity. The dependent variable takes value zero if the match was not carried out (so if the firm and the worker never met). 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 A8.4: Impacts on employment at matched firms - Worker level

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 received at least one job offer by the matched firms [Yes=1]				Worker was hired by at least one matched firm [Yes=1]			
Sample of workers match to:	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	.006	.112***	.030	[.491]	.007	.067*	-.004	[.113]
	(.023)	(.042)	(.043)		(.017)	(.037)	(.025)	
Mean of dep. var. in Control	.104	.065	.096		.053	.037	.044	
Number of observations (workers)	709	210	248		709	210	248	

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the first worker follow-up survey are reported. Standard errors are adjusted for heteroskedasticity in these regressions. 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. 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 workers. In columns 2 and 6 it is restricted to workers matched only with High Ability firm owners. In columns 3 and 7 it is restricted to workers matched only with Low Ability firm owners. The p-values in columns 4 and 8 are from similar OLS regressions estimated on the full sample of workers and where each independent variable is interacted with a dummy for whether the worker was matched with High Ability owners only. 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)		

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

	Balance conditional on positive earnings at followup			
	Control Workers (1)	Treatment Workers (2)	P-value (3)	Normalized Differences (4)
Number of observations	500	497		
A. Background characteristics at baseline				
Age [Years]	20.4	20.7	[.103]	-.084
	(2.24)	(2.76)		
Female	.459	.464	[.759]	-.007
	(.499)	(.499)		
Completed prior education [Years]	10.4	10.4	[.951]	.000
	(1.77)	(1.70)		
Course duration [Years]	1.54	1.49	[.557]	.041
	(.879)	(.862)		
Ever employed	.217	.212	[.816]	.009
	(.413)	(.409)		
Monthly expected earnings [USD]	126.1	123.4	[.662]	.026
	(76.0)	(68.7)		
B. Skills at baseline				
Attendance [1-5 scale]	3.35	3.33	[.975]	.013
	(1.12)	(1.14)		
Communication skills [1-5 scale]	3.20	3.27	[.186]	-.045
	(1.07)	(1.15)		
Creativity [1-5 scale]	3.43	3.48	[.220]	-.032
	(1.12)	(1.06)		
Trustworthiness [1-5 scale]	3.49	3.50	[.774]	-.007
	(.982)	(.959)		
Willingness to help others [1-5 scale]	3.34	3.30	[.840]	.026
	(1.08)	(1.07)		
Cognitive test score [0-10 scale]	5.37	5.21	[.281]	.047
	(2.40)	(2.37)		
F-test of joint significance		{.384}		

Notes: The sample includes observations with positive earnings from the first and 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 post-intervention labor market earnings - Robustness

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

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Total earnings in the last month [USD]				
Transformation:	**Top coded at 99th percentile**	**Log, no top coding, no trimming**	**Trimmed at 99th percentile, no controls**	**Trimmed at 99th percentile, IPW**	**Trimmed at 99th percentile**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Treatment	6.05*	.101*	6.84**	8.07**	
	(3.49)	(.055)	(3.26)	(3.39)	
Dynamic treatment effects					
Treatment X First followup					5.25
					(4.05)
Treatment X Second followup					8.82*
					(4.83)
Mean of dep. var. in Control group	66.1	68.4	63.1	63.1	63.1
Pvalue First fup = Second fup					[.568]
Controls for baseline value of outcome	Yes	Yes	Yes	Yes	Yes
Controls for stratification variables	Yes	Yes	Yes	Yes	Yes
Controls for other worker characteristics	Yes	Yes	No	Yes	Yes
Uses data from first followup	Yes	Yes	Yes	Yes	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes
Number of observations	997	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 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 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.

Table A12.1: Heterogeneous impacts on post-intervention worker outcomes - Skills

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Monthly expected earnings [USD]	Expected probability of employment in the next six months (0 to 10 scale)	Any work as employee in the last week	Weekly hours worked in last job as employee	Any paid work in the last month	Total earnings in the last month [USD]	
Sample of workers:	**All**	**All**	**All**	**All**	**All**	**All**	**Positive earnings**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Pass grade on all skills	5.28	.179	-.052	-.739	.013	8.39**	9.13**
	(3.90)	(.174)	(.04)	(2.73)	(.035)	(4.15)	(4.64)
Fail grade on at least one skill X Treatment	8.85**	.429**	.000	-.008	.007	7.27**	8.87**
	(3.38)	(.151)	(.035)	(2.36)	(.031)	(3.38)	(3.83)
Pass grade on all skills X Treatment	6.71	.084	.070*	4.80	-.043	-1.05	4.77
	(4.63)	(.178)	(.042)	(2.97)	(.037)	(5.03)	(5.64)
Mean of dep. var. in Control group	114.5	5.53	.428	37.0	.750	47.2	63.1
Pvalue Pass = Fail	[.709]	[.144]	[.206]	[.207]	[.299]	[.167]	[.546]
Controls for baseline value of outcome	Yes	Yes	Yes	No	Yes	Yes	Yes
Uses data from first and second followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,326	1,349	1,350	1,347	1,338	1,329	988

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 region 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. The top 1% values of expected earnings are excluded. 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 columns 3 and 5. The dependent variable in columns 6 and 7 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. The top 1% values of labor market earnings are excluded. 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.

Table A12.2: Heterogeneous impacts on post-intervention worker outcomes - Matched to High or Low Ability Owner

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Monthly expected earnings [USD]	Expected probability of employment in the next six months (0 to 10 scale)	Any work as employee in the last week	Weekly hours worked in last job as employee	Any paid work in the last month	Total earnings in the last month [USD]	
Sample of workers:	**All**	**All**	**All**	**All**	**All**	**All**	**Positive earnings**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Matched to High Ability Owners	3.71	.180	.010	-.191	-.010	-5.91	-7.07
	(5.01)	(.230)	(.051)	(3.59)	(.046)	(5.26)	(6.01)
Matched to Low Ability Owners X Treatment	8.91*	.520**	.039	-1.12	-.040	2.28	6.71
	(5.08)	(.206)	(.049)	(3.39)	(.043)	(5.27)	(6.04)
Matched to High Ability Owner X Treatment	3.23	.109	.026	1.57	.006	-1.48	-.884
	(5.34)	(.230)	(.052)	(3.50)	(.045)	(5.40)	(6.36)
Mean of dep. var. in Control group	114.5	5.53	.428	37.0	.750	47.2	63.1
Pvalue Matched High = Matched Low	[.448]	[.195]	[.864]	[.592]	[.489]	[.629]	[.399]
Controls for baseline value of outcome	Yes	Yes	Yes	No	Yes	Yes	Yes
Uses data from first and second followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	868	879	879	877	870	864	631

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 region 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. The top 1% values of expected earnings are excluded. 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 columns 3 and 5. The dependent variable in columns 6 and 7 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. The top 1% values of labor market earnings are excluded. 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 variable Matched to high Ability Owners takes value one if the worker was matched only to High Ability owners, and zero otherwise. The variable Matched to Low Ability owners takes value one if the worker was matched only to Low Ability owners, and zero otherwise. 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 A13: Results on post-intervention firm outcomes

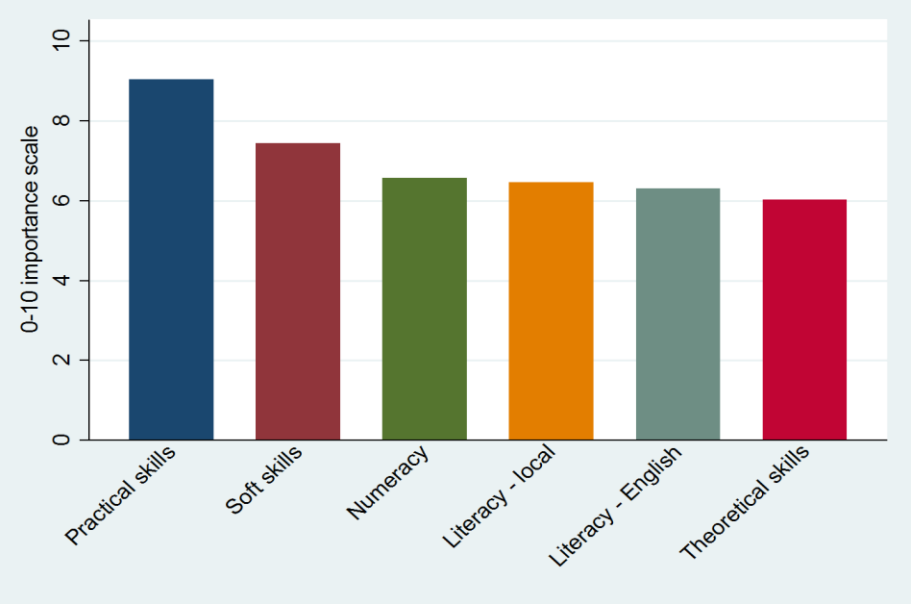
OLS regression coefficients, robust standard errors in parentheses

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	

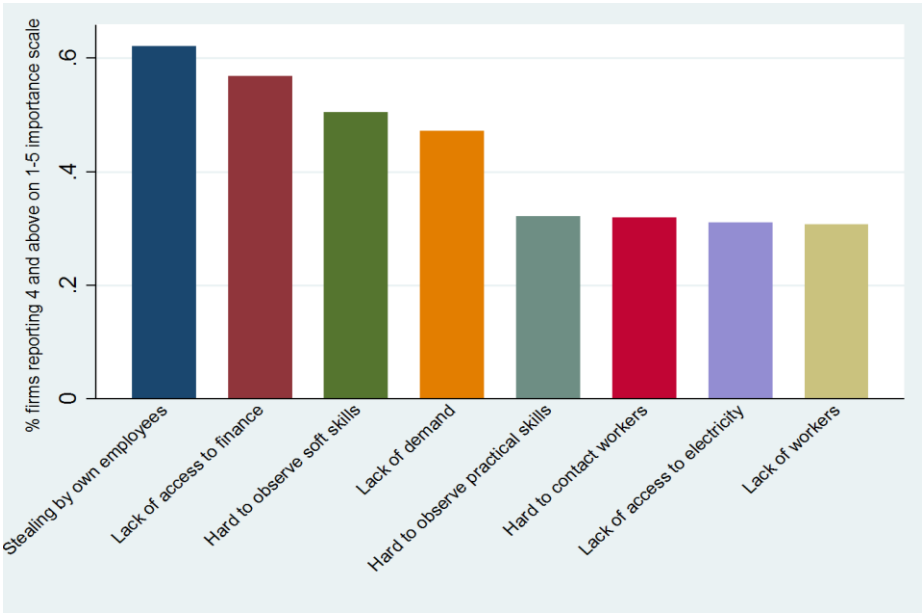
Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the firm follow-up survey are reported. Standard errors are adjusted for heteroskedasticity. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of 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.

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



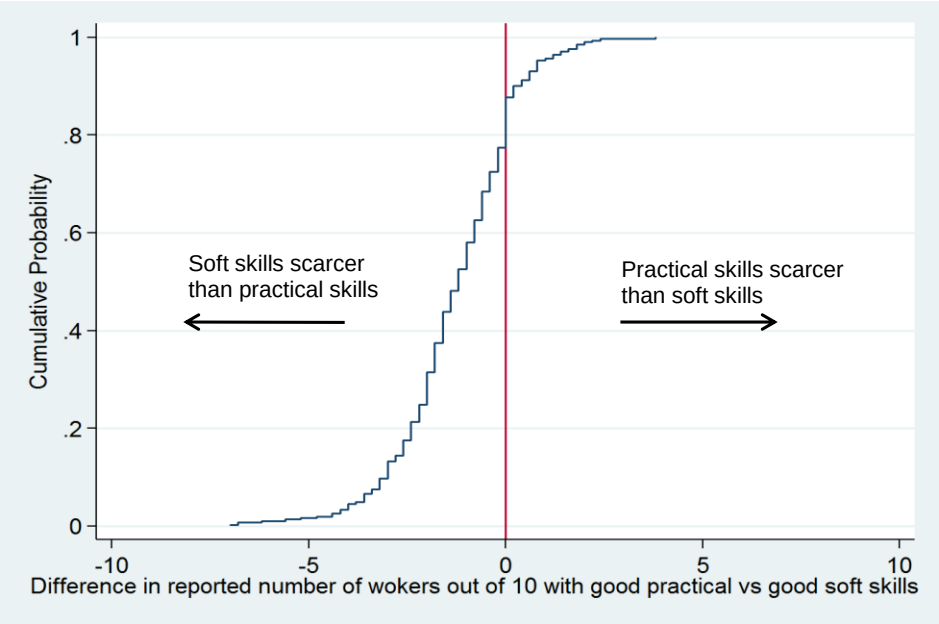
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 0-10 scale, where 0 = "Not important at all", and 10 = "Extremely important", the importance of various skills for their operations. The figure reports the average importance given 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



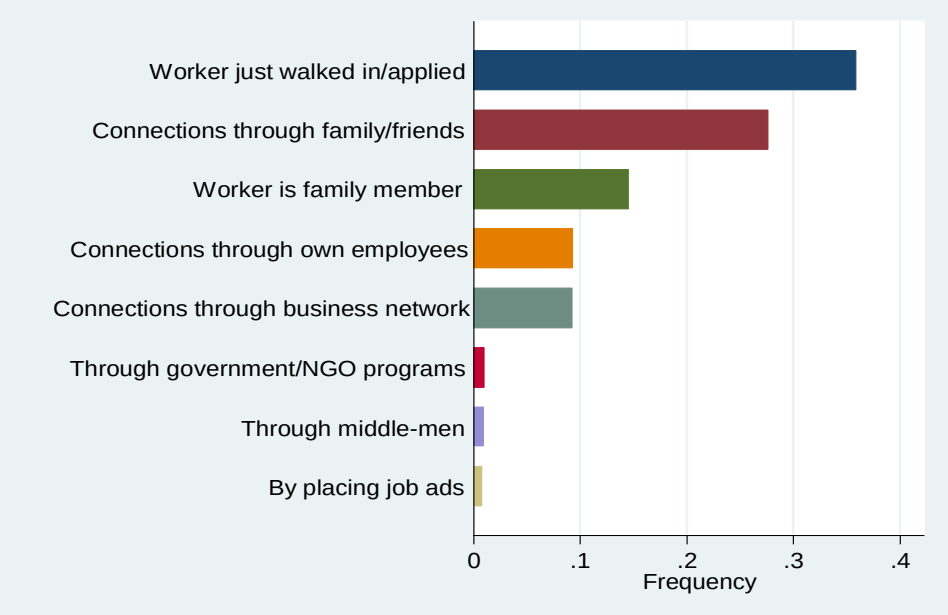
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 1-5 scale, where 1 = "Not important at all", and 5 = "Extremely important", the importance of these potential constraints. The figure reports the percentage of firm owners that answered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



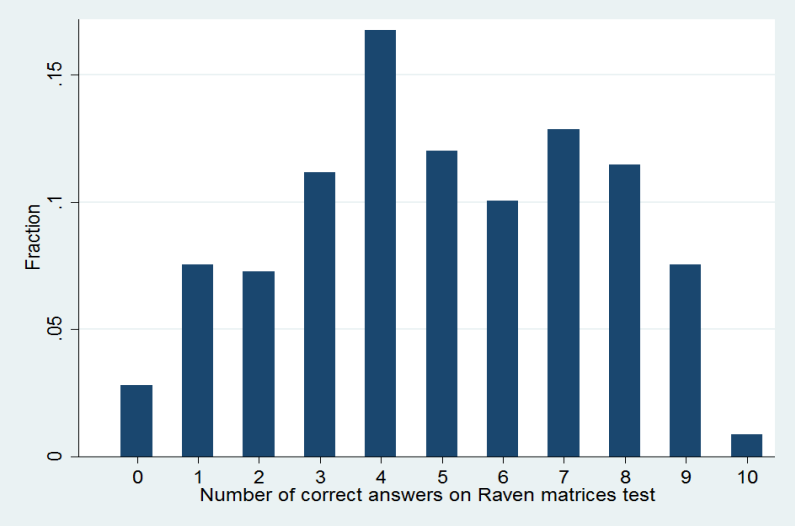
Notes: The figure reports the CDF of within-firm owner differences in the number of potential workers out of 10 reported as having a good level of practical skills vs. a good level of soft skills. To estimate the reported number of workers with a good level of soft skills we compute the average of the 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



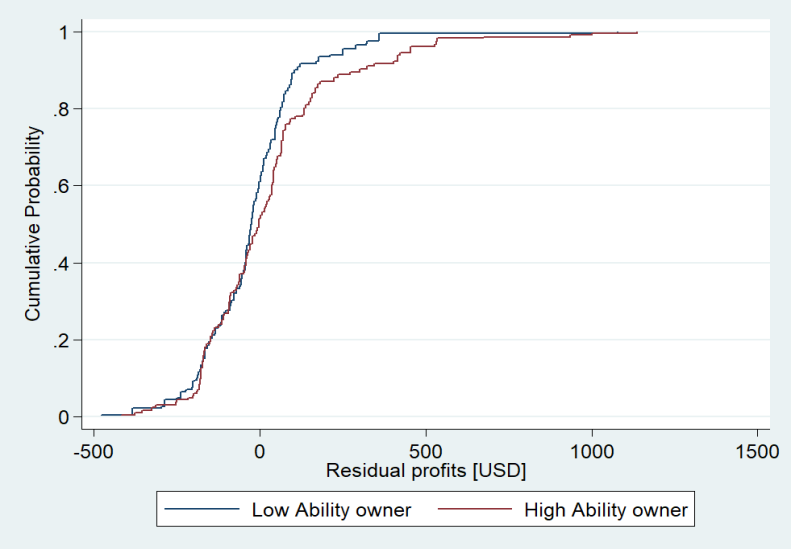
Notes: The figure reports the frequency of recruitment channels for the workers employed at baseline in the sample of firms included in the intervention.

Figure A5: Distribution of cognitive ability among firm owners



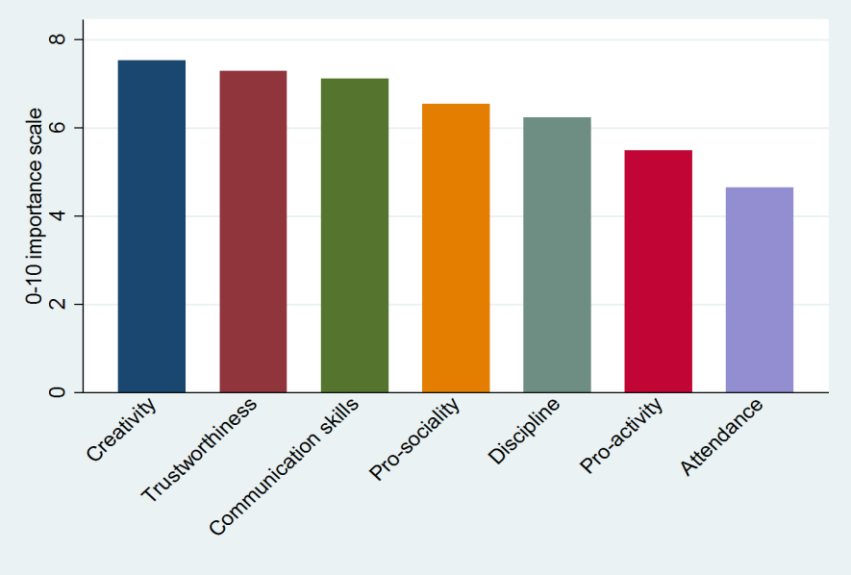
Notes: Data is from the firm baseline survey. The Figure reports the frequency histogram of the scores on the 10-item Raven matrices test administered to firm owners at baseline.

Figure A6: Distribution of residual profits per worker, 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

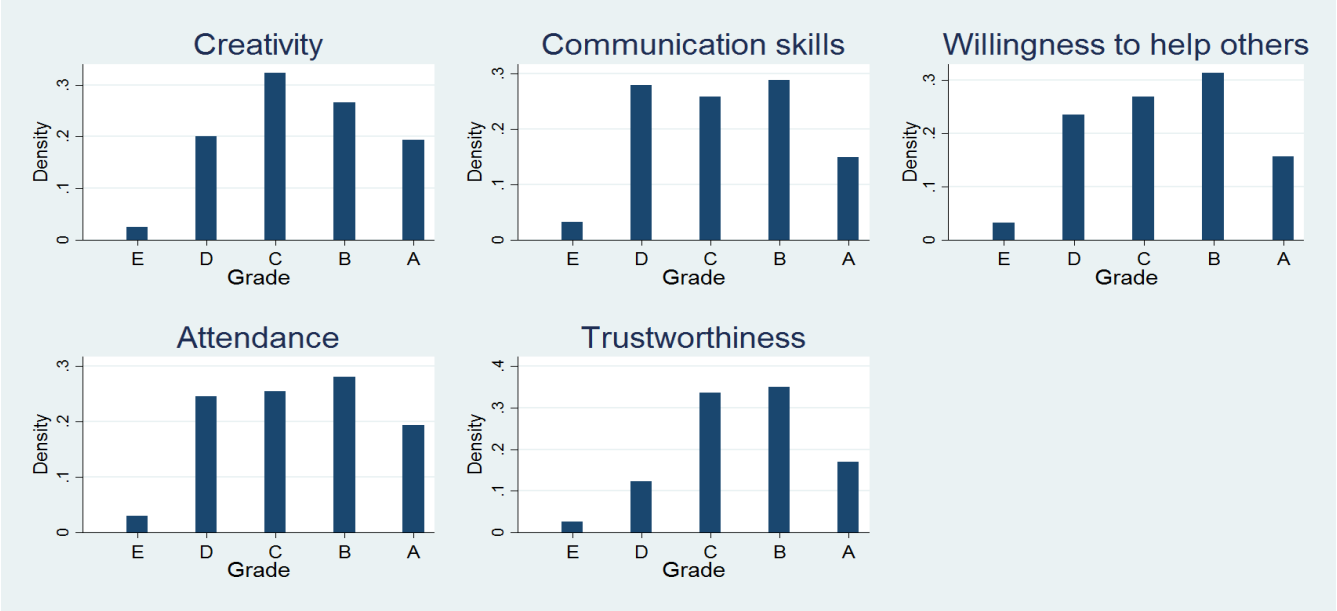


Figure A9: Skills certificates

Panel A: Treatment

ID XXX

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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

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Acknowledgment of Participation

This is to acknowledge that

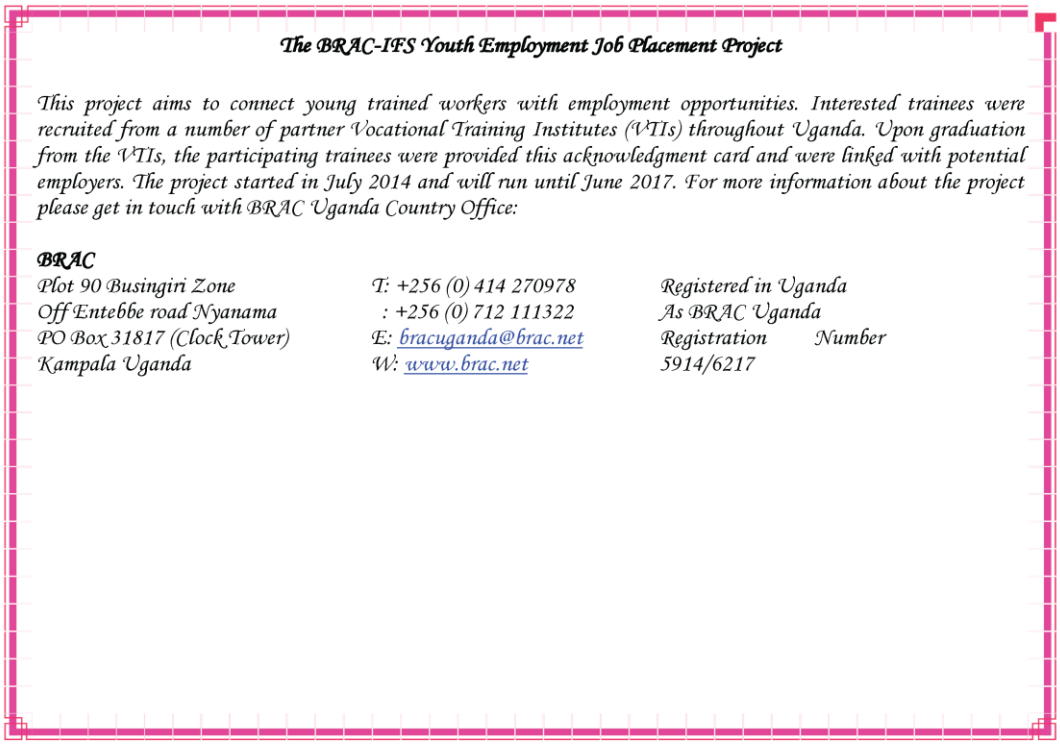
NAME OF TRAINEEE

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 Jeniphier Twebaze Musoke
Coordinator, Research,
BRAC Africa Programme

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



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BRAC

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T: +256 (0) 414 270978
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Figure A10: Model predictions

