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

Vittorio Bassi† **Aisha Nansamba**‡

5th August 2021

Abstract

We study how employers and job-seekers respond to credible information on skills that are difficult to observe, and how this affects matching in the labor market. We experimentally vary whether certificates on workers' non-cognitive skills are disclosed to both sides of the market during job interviews between young workers and small firms in Uganda. The certificates cause workers to increase their labor market expectations, while high-ability managers revise their assessments of the workers' skills upwards. The reaction in terms of beliefs leads to an increase in positive assortative matching and to higher earnings for workers, conditional on employment.

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1 Introduction

Labor productivity and wages remain far lower in developing countries (Caselli, 2005; Hall and Jones, 1999; Bloom et al., 2010). This is particularly true in Africa, where the number of workers earning less than $3.10 per day is increasing by almost 4 million a year (ILO, 2017). Understanding which factors contribute to keeping productivity and wages low in such contexts is thus of primary importance not only to foster business growth, but also to raise incomes and improve standards of living.

Theoretical models of the labor market highlight how the information available to both workers and firms plays a key role in determining the efficiency of the job matching process, and hence labor productivity and wages (Jovanovic, 1979; Chade and Eeckhout, 2017): difficulties in *screening* workers can prevent firms from selecting the right employees; at the same time, difficulties in *signaling* skills to employers can impact the ability of workers to match with the right jobs, or even their ex ante decision to acquire human capital (Spence, 1973).

Little is known on how information frictions on skills affect matching in urban labor markets in low-income countries, despite the potential importance of this question (McKenzie, 2017). On the one hand, information frictions may have less severe consequences in developing countries, where the production process is likely simpler and so the scope for inefficiencies related to mismatch may be lower. On the other hand, information problems could be exacerbated in more informal labor markets, due to weak institutions and limited rule of law (Bloom et al., 2010, 2012, 2013): for instance, if managers cannot prosecute workers in case of misconduct, this makes screening on reliability or trustworthiness at recruitment even more important.

This paper studies how lack of information on the skills of workers at recruitment affects matching in a developing country. To do so, we design a field experiment in the Ugandan labor market that has two key components: (i) a *matching* component, whereby firms and job-seekers are matched together for real job interviews, and (ii) a *signaling* component, by introducing experimental variation in whether a credible signal on skills that are difficult to observe is shown to both sides of the market during the recruitment process, through the provision of certificates.

Our main contribution is to show how the credible signal affects the allocation of labor. On the firm side, we identify whether managers revise their beliefs on the skills of workers; on the worker side, we study whether the certificates impact the workers' perceptions about what they can achieve in the labor market, such as their expected earnings. We then show how the updating of beliefs of workers and managers translates into reduced form impacts on the allocation of labor, overall employment, and worker earnings in the two years post intervention.

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

We focus the information revelation on non-cognitive or "soft" skills, which have been shown to have high labor market returns in both high- and low-income countries, but which are hard to observe by nature.[1] In our context, managers report difficulties in observing the soft skills of job applicants as among their most important concerns. We focus on five specific soft skills that our manager survey reveals to be important but are difficult to observe: communication skills, willingness to help others, trustworthiness, creativity and attendance. We assess our sample of workers on these soft skills, using a combination of teacher surveys, incentivised trust games and psychometric scales. We then schedule and observe over 1,200 real job interviews between our sample of SMEs and job-seekers. In a randomly selected half of these interviews, certificates on the workers' soft skills are revealed to both the worker and the firm, while in the other half, neither the worker nor the firm get to observe the certificate. We collect information on the result of each job interview, and track the sample of firms and workers for two years.

Our first set of results relate to whether managers and workers respond to the certificates by updating their beliefs. We find that the information strengthens the correlation between workers' skills and managers' assessment of their skills. This effect is driven by workers with relatively higher skills: while managers revise upwards their beliefs for workers in the upper part of the skill distribution, we find little evidence of negative updating for workers further down the distribution. We explain this by documenting that: (i) our sample of workers are positively selected on soft skills, relative both to the eligible population of vocational training graduates and to typical young employees in SMEs, which is possible since participation in the experiment was voluntary and we worked with particularly reputable training institutions; and (ii) managers have low expectations about workers' soft skills at baseline. These two facts together explain why the certificates create mostly positive news for managers. Further, exploiting our two-sided design, we study heterogeneity by manager characteristics, and document that the revision of beliefs is driven by managers of higher ability, who manage more profitable firms and value soft skills more in production.

We show that workers also react to the certificates by revising upwards their labor market expectations: in the two years post intervention workers with a certificate report 7% higher expected earnings, 5% higher expected employment probability, a higher intention to bargain for wages and a larger size of their ideal employer. The certificates also lead workers to pull away from poorly paid casual work and to increase their investment in training, which points to the presence of complementarities between the certification and human capital accumulation. We interpret these findings as evidence that the certificates raise the perceived outside option of workers, who try to transition to better and higher paying jobs. We further show that the

[1] On the importance of non-cognitive skills in the US see Bowles et al. (2001), Heckman et al. (2006), and Deming (2017). Adhvaryu et al. (2019) show that non-cognitive skills have high returns in India.

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. Again, these overall positive treatment effects for workers are in line with the positive selection into the experiment and with the low priors of managers about their skills.

Informed by these reduced form findings on beliefs, we then develop a partial equilibrium search model with heterogeneous workers and firms, in order to: (i) guide the interpretation of the impacts on labor market outcomes; and (ii) study how workers at the low end of the skill distribution, who selected out of the experiment, would be affected by the information revelation, which is important for cost-benefit analysis considerations. In the model, soft skills have higher returns when matched to higher ability managers, and so the efficient allocation exhibits positive assortative matching between workers and firms. However, search frictions and lack of information on workers' skills result in mismatch and loss of output. We model the intervention as an increase in the precision of the signal on workers' skills during job interviews, and we show that the certificates improve the allocation of labor. Workers with higher skills earn higher wages after the intervention, as the certificates allow them to send a precise signal about their type to those firms who are willing to pay them more. Conversely, the model highlights that workers with lower skills can be negatively affected by the certificates, as these reduce their chance of employment at higher ability firms.

In line with the model, we find that the certificates cause an increase in sorting, by raising the probability of employment between our experimental sample of workers – who are positively selected on soft skills – and higher ability managers. In addition, the certificates increase sorting between workers with a high value of specific soft skills and employers where those skills might be particularly needed. For instance, workers with high communication skills are more likely to be hired by firms with more employees and where interactions with customers are more frequent. While the certificates lead to a change in the allocation of labor, they do not result in an overall increase in the probability of employment. This is consistent with the model, which shows that total employment does not necessarily increase, as workers reallocate across different types of firms. However, in line with the allocation of labor being more efficient, workers with a certificate earn 11% more in the two years post intervention, conditional on being employed, and this effect is larger at the top of the skill distribution.

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

Finally, we address the potential for government intervention in this area, and note that in a large-scale version of this program in which workers are not given the option to select out, some workers at the low end of the skill distribution might be negatively affected by the certificates, as indicated by the evidence on positive selection on soft skills into the experiment and by our model. This highlights how certification policies, while raising efficiency through the improved allocation of labor, can also have important distributional effects among workers. However, we discuss how a large-scale certification intervention might also provide incentives to invest in skills, which could weaken this type of distributional concerns.

Related literature Our paper extends a growing experimental literature on how labor market frictions in developing countries affect workers and firms. On the worker side, recent studies evaluate how helping job-seekers signal their skills impacts job search, employment and earnings (Abebe et al., 2021; Abel et al., 2020; Carranza et al., 2020). On the firm side, this literature studies whether screening and training costs at recruitment impede hiring and constrain firm growth (De Mel et al., 2018; Hardy and McCasland, 2020).

Two papers closely related to ours are Abebe et al. (2021) and Carranza et al. (2020). Abebe et al. (2021) evaluate a job application workshop targeting a representative sample of unemployed youth in Addis Ababa. The workshop includes an orientation on conducting effective job applications, as well as the provision of a certificate on cognitive skills, mathematical ability, language skills and performance on a generic work sample. As the certificates are provided only to job-seekers, the study aims to identify the gains for workers from a having a more precise signal that they can use during job search. They document that the intervention leads to short-term improvements in formal employment, and to a significant increase in earnings after four years. Carranza et al. (2020) conduct a series of field experiments with economically disadvantaged young work-seekers and firms in Johannesburg. Their main intervention provides workers with a certificate on cognitive ability, numeracy, English proficiency, grit, focus and planning. They also conduct a separate audit study with firms, where a skills certificate is attached to CVs in formal job applications. By focusing on workers and firms, the study aims to identify the strength of information frictions on both sides of the market. They find that the certificates improve workers' job search direction, employment and earnings 3-4 months after treatment, and that they increase callbacks by firms.

We make the following contributions relative to these papers. Since our unique design includes both a matching *and* a signaling component, we are the first to identify the role of information frictions on skills, conditional on a real job interview between a worker and a firm taking place. That is, we can separately identify the impacts of certification arising from reduced uncertainty on the skills of workers at recruitment, from those resulting from changes in search behaviour of workers. This distinction is important to fully characterise how information frictions in the labor market operate.[2] Second, our two-sided experimental design

[2]While the firm-side audit study in Carranza et al. (2020) holds fixed job search behaviour, it does not speak

and data collection allows us to study heterogeneity in responses on both sides of the market, and to directly estimate impacts on sorting between workers and firms. This is crucial to identify the channels through which information on skills can improve productivity, and to determine the efficiency of intervention. Third, while both Abebe et al. (2021) and Carranza et al. (2020) disclose information on a mix of hard and soft skills, we focus on soft skills specifically. We show that firms in our context are already able to screen on hard/practical skills, which then allows us to highlight soft skills as the key source of information frictions. Finally, while both Abebe et al. (2021) and Carranza et al. (2020) focus on unemployed job-seekers, our target sample are semi-skilled labor market entrants that have not experienced long unemployment spells and are not disadvantaged. Such vocational training graduates constitute an important source of employment for SMEs, and so our results speak directly to the role of information frictions in impeding matching between SMEs and the pool of applicants they typically face.

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

Finally, we add to an established literature on the role of job referrals in hiring (Beaman and Magruder, 2012; Burks et al., 2015; Brown et al., 2016; Dustmann et al., 2015; Heath, 2018; Pallais and Sands, 2016). While referrals can potentially mitigate information frictions and improve match quality, their effectiveness can be lower in developing countries, where social and economic networks are often overlapping, so that employees might face social incentives to refer network members who are not be the best match for the job (Beaman and Magruder, 2012). In fact, while referrals are common in our context, we show that they do not fully solve the hiring problem, which then justifies the focus on certificates as an alternative policy tool to alleviate information frictions at recruitment.[3]

Structure of the paper Section 2 presents the sample. Section 3 describes the experiment. Section 4 shows the impacts on beliefs. Section 5 introduces the model and then shows treatment effects on labor market outcomes. Section 6 discusses policy implications and concludes. Additional details are in the Online Appendix, and other results and analysis not intended for

directly to the impacts of revealing information during job interviews, where workers and firms interact and potentially generate information. Informal walk-ins leading to job interviews are a primary recruitment channel in SMEs in our context, while formal applications through mailing of CVs are rare.

[3]Our study is also related to a growing literature about how lack of information on job opportunities and on the consequences of unemployment impedes effective job search (Altmann et al., 2018; Belot et al., 2019). More broadly, our study contributes to the empirical literature on the micro-foundations of the aggregate matching function (Petrongolo and Pissarides, 2001).

publication can be found on the authors' websites in a document called Supplemental Material.

2 Setting, Sample Selection and Descriptives

The project was implemented in partnership with a large and reputable NGO, BRAC Uganda. This section describes the sample of both workers and firms, and presents descriptive evidence.

2.1 Firm Census and Selection into the Experimental Sample

As shown in Figure 1, we began the study by identifying the sample of firms and workers for the intervention. Firms were identified through a census of SMEs conducted in urban areas of Uganda, covering all four regions of the country. To be in the census, firms had to: (i) operate in either carpentry, catering, hairdressing, motor-mechanics, tailoring, welding; and (ii) employ at least two workers in addition to a firm owner. We identified 1,086 eligible SMEs through the census.[4] Table 1 reports summary statistics from the census. The median firm employs four workers and has been operating for five years, so the typical owner has experience recruiting and managing workers.[5]

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

At the end of each interview in the census, firm owners were asked their interest in participating 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.[6] All firms interested in participating in the program were administered a baseline survey, and the 422 firms which confirmed their interest and completed the survey form our experimental sample. Importantly, firm owners were only informed about the matching component of the intervention and were not told that certificates on soft skills would be disclosed during the job

[4] This is a separate and non-overlapping census than the one discussed in Alfonsi et al. (2020). Our census was conducted in 17 urban areas in 11 of the 121 districts of Uganda. In each urban area, the census took place within a 4km radius from the local BRAC branch. The census covered about 1% of the total area of the 11 districts we worked in. The 2010 Census of Business Establishments (UBOS, 2011) further reveals that in 2010 there were 23,366 firms operating in the same sectors and districts targeted by our census, so that we covered less than 5% of the firms. This limits concerns that our intervention could generate general equilibrium effects.

[5] Given the small average firm size, in the great majority of cases the firm owner is also the manager. Therefore, we use the terms "firm owner" and "manager" interchangeably.

[6] BRAC is one of the largest NGOs in Uganda, and is well known across the country for its programs targeting youths and firms. Therefore, concerns related to its credibility are not first order.

interviews. This limits the possibility that expected gains from the signaling intervention are a driver of selection into the sample.[7]

2.2 Worker Census and Selection into the Experimental Sample

Worker census We defined as eligible for the intervention all trainees currently enrolled at 15 large and reputable partner VTIs in one of the six business sectors covered by the project, and expected to graduate in time for the placement intervention.[8] We conducted an initial survey of all the 1,011 eligible trainees. The survey was administered before any information was given to trainees about the BRAC Job Placement Program. Table 2 shows that the median eligible trainee is 20 years old, has completed 11 years of education before enrolling at the VTI, and is undertaking a 2-year course. Training focuses on practical skills, which are also certified. On the other hand, VTIs do not provide formal training nor certificates on soft skills. Over 60% of workers plan to look for wage employment – as opposed to self-employment – as their first job, and the ideal firm size is less than 20 employees for about the same fraction of trainees. Thus the typical trainee will look for jobs in SMEs after training.[9]

We focus on young trainees for two reasons. First, the share of young workers is higher in developing countries, and this is particularly true for Uganda, which has one of the youngest populations in world (UN, 2017). Second, young workers can be particularly affected by information frictions at recruitment given their lack of work experience (Pallais, 2014). It is important to note that our sample of workers are representative of the population of students who were able to self-finance training at our partner VTIs, and so are substantially more educated and wealthier than the average Ugandan youth.[10] In addition, since the census was conducted before graduation, we did not restrict the sample to individuals who were unemployed or were

[7]The Supplemental Material reports an analysis of selection into the experiment on the firm side and shows that: (i) of the observable firm and manager characteristics collected in the census, which included sector and region of operation, owner's gender, number of employees, whether the business is registered and business age, only sector and region are significant predictors of selection; (ii) we do not find strong evidence that the sample of firms that self-selected into the experiment differs in terms of manager's skills from the representative sample of Ugandan SMEs in Bassi et al. (2021). As managers were informed about the matching component, but not about the certification component, self-selection into the experiment can indicate unmet demand for labor, and so these results are consistent with the tightness of the labor market varying across sectors and regions, but not across other firm or manager characteristics.

[8]More details on the selection and summary statistics of the 15 VTIs are discussed in the Supplemental Material. In 2014-17 there were 204 VTIs accredited with the Directorate of Industrial Training and operating in the same districts as our partner VTIs. This further limits concerns that our intervention could generate general equilibrium effects.

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

[10]The median youth aged 18-25 in the 2012/13 Ugandan National Household Survey (UNHS) has 6 years of formal education, while the median level of formal education is 11 years in our sample (UBOS, 2014). In Appendix A we compare our experimental sample with youth in the UNHS, and show that our sample is also less likely to be married and comes from households that are significantly better off on multiple measures of asset ownership.

facing particular challenges in finding employment. Our sample includes entry-level workers in semi-skilled occupations, and so is different from the ones in related studies such as Abebe et al. (2021), Abel et al. (2020), Alfonsi et al. (2020) and Carranza et al. (2020), who instead specifically target disadvantaged youth. We can therefore expect a larger share of workers to find employment in the follow-up period relative to these other studies, which makes this an appropriate context to study the impact of certificates on the allocation of labor and wages, over and above the extensive margin of overall employment probability.

Selection into the experimental sample After completing the survey, all eligible trainees were informed about *both* the matching and signaling components of the intervention. Specifically, they were told that BRAC would schedule job interviews with potential employers among SMEs, but also that BRAC would conduct additional skills measurements – on both cognitive and soft skills – and that the information from the assessments might be disclosed to potential employers during the matching.[11] All trainees were then asked their interest to participate. The interested trainees were administered a baseline survey and were included in the skills assessments: the 787 trainees that confirmed their interest in the intervention and completed the main skills assessments form our final experimental sample.

There are two potential margins of selection into the experiment that might be relevant. First, the experimental sample might be selected relative to the eligible population of graduates from our 15 partner VTIs, which is possible since participation was voluntary. Second, our sample might be selected relative to the broader population of typical entry-level employees in SMEs in our sectors, which is possible since we worked with particularly reputable training institutions. Exploring both margins is important to correctly interpret the skill distribution of the sample of workers that are presented to employers in the matching intervention, and how this compares with managers' priors.

On the first point, since workers were informed about the signaling component, 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. In addition to socio-demographics, the worker census included two measures of skills: (i) a cognitive test, and (ii) a Big Five questionnaire.[12] Appendix Table A1.1 uses this information to study selection into the experiment, and shows that indeed the five Big Five variables are jointly significant in predicting selection. As expected, the selection is *positive*, so that workers with higher soft skills are more likely to be in

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

[12]The Big Five 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 Five traits. More details on the measurement and distribution of these skills in our sample are given in the Supplemental Material.

the final sample.[13] The magnitude of this selection is sizeable. The score on each of the Big Five goes from 1 to 5, and so we summarise the extent of the selection by considering the share of workers who scored 3 or above on all Big Five: this proportion is 24% in the excluded workers, and increases to 33% in the group included in the experiment, a difference significant at the 5% level and corresponding to a 38% increase. As shown in column 3 of Table A1.1, a dummy for whether the worker scored 3 or above on all Big Five remains a sizeable predictor of selection when controlling for worker characteristics: workers with a high value on all the Big Five are 7pp more likely to be in the final sample, a result significant at the 5% level. The Big Five are as important in driving selection as lack of past work experience, which the literature has highlighted as a key source of heterogeneity in explaining the impacts of signaling interventions (Pallais, 2014). Figure 2 plots the distributions of the three Big Five traits that are individually significant in column 2 of Table A1.1: agreeableness, conscientiousness and neuroticism, and confirms that the positive selection is all along the skill distribution.

On the second point, we compare our experimental sample to the representative sample of employees in small scale Ugandan manufacturing in Bassi et al. (2021). The Bassi et al. (2021) survey covered SMEs in similar sectors to ours and collected the soft skills of every worker, using exactly the same Big Five scale that we used in our paper. In short, we find that our sample is positively selected on soft skills relative to comparable young employees in the Bassi et al. (2021) sample: for instance, while 21% of the young VTI graduates in Bassi et al. (2021) had a score of 3 or above on all Big Five, the same share is 33% in our sample, which is more than a 50% increase. This analysis, described in detail in Appendix A, indicates that our sample is positively selected not only within the workers eligible for the experiment, but also with respect to typical young VTI graduates employed in similar sectors.

This positive selection is important for interpreting the results of our intervention in two ways. First, if managers expect the matched workers to be a random sample of VTI graduates with similar characteristics to the workers that they typically hire (i.e., if they miss the positive selection), then the revelation of information might provide mostly positive news on the skills of workers. Second, this selection implies that we will not be able to estimate the impacts of the intervention for workers at the lower end of the distribution, who are selecting out. However, we will use evidence on the importance of soft skills in our sample of firms, along with a model, to discuss what the impacts of the information revelation would be for these workers in a counterfactual exercise.[14]

[13]Interestingly, we find no selection on cognitive ability. This suggests that cognitive skills may be harder to hide from employers, something that indeed we verify in Section 3.1.

[14]In the Supplemental Material we compare our sample to the US and Canadian samples in Srivastava et al. (2003), and show that our sample fares better on four of the five Big Five traits. With the caveat that the sampling strategies differ and that there might be cultural differences in how the questions are interpreted, these results are again in line with our sample being positively selected relative to the broader population.

2.3 Key Facts about SMEs at Baseline

We now present four key facts from the baseline survey of SMEs, which inform our research design and empirical strategy. More details and the supporting evidence are in Appendix B.

1. Soft skills are perceived by firm owners as having relatively high returns. This matches evidence that non-cognitive skills have high returns in labor markets in both developing countries and the US (Adhvaryu et al., 2019; Heckman et al., 2006; Deming, 2017).

2. Firm owners report difficulties in observing workers' soft skills and theft by their own employees among their main perceived constraints.[15]

3. Firm owners have relatively low expectations on the distribution of soft skills among workers, and think that workers with good soft skills are hard to find. In addition, firm owners are not familiar with the VTIs in our sample and have low expectations on the soft skills of their graduates.

4. It is common for firms to recruit applicants who walk up to the firm and ask for a job, without any prior connection with the firm.

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. Since soft skills are difficult to observe, as made precise later in the model, this can limit the propensity of managers to hire and can reduce wages. Given the relatively low expectations of firm owners on the distribution of soft skills, the third key fact suggests that the signaling intervention might be especially beneficial for those workers who can now credibly signal they are not among those low types that firm owners are afraid of hiring. The third key fact also suggests that firms might miss the positive selection of workers into the experiment, since they are not familiar with our partner VTIs and have low expectations on their graduates. Finally, the fourth key fact indicates that referrals do not fully solve the hiring problem, and so an intervention introducing workers to firms they have no prior contact with is likely to be informative of the regular hiring process in the labor market.

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;

[15] These results are in line with Bassi et al. (2021), who find that screening difficulties are the primary labor market constraint reported by managers in a representative survey of manufacturing firms in Uganda.

(ii) a matching component, whereby job interviews were scheduled between workers and firms; (iii) a signaling component, by introducing experimental variation in whether information from the screening assessments was disclosed to *both* workers and firms during the job interview process, through the provision of skills certificates.

Worker screening Our screening activities targeted seven soft skills identified as relevant in initial focus groups with firm owners: creativity, communication skills, willingness to help others/pro-sociality, pro-activity, trustworthiness, discipline, and attendance/time-keeping.[16] Creativity, to be intended as the ability to come up with creative solutions to problems, is relevant for all sectors in our study, as workers are often asked to use in a creative way the limited tools available. Because employees are often asked to work in teams, and to take care of customers, skills such as communication, willingness to help others and pro-activity were mentioned as relevant in the focus groups. We discussed in the previous section how trustworthiness is important to firms. Existing research further suggests that firms value discipline, and that absenteeism is widespread in developing countries.[17]

We used teacher surveys to measure those skills that are easier to assess for an external examiner, namely attendance, discipline, communication, pro-sociality and pro-activity. To measure creativity and trustworthiness we developed our own assessments: for creativity, we used a battery of questions; for trustworthiness, we made trainees play incentivised trust games.[18] We limited the revelation of information to five skills, to address concerns related to attention constraints. We selected the five soft skills based on the stated preferences of managers in the baseline survey. Specifically, we asked firm owners how much they would value additional information on the seven soft skills in our assessments, if they were to interview workers fresh out of VTIs. We then selected the top four skills: creativity, trustworthiness, communication and willingness to help others, plus attendance.[19] To facilitate the reporting, we followed the Ugandan education system, and graded each skill on a A-E scale. Both trainees and firm owners are used to this grading scale in this context, and grades of C and above are considered Pass

[16] For the information revelation we chose to focus on soft skills such as creativity, rather than on the Big Five, because the former were easier to explain to firm owners and workers, as revealed by our piloting exercises. We discuss the correlation between our chosen soft skills and the Big Five later in this section.

[17] Bowles and Gintis (1976) and Sackett and Walmsley (2014) show that dependability and conscientiousness are among the skills most valued by US employers. Adhvaryu et al. (2020) and Krishnaswamy (2019) find absence rates of 8-14% in Indian manufacturing, and Chaudhury et al. (2006) document absence rates of 19% and 35% in the health and education sector respectively, across many developing countries including Uganda.

[18] Specifically, as our measure of trustworthiness we use the amount sent back by the "recipient" in the trust game, which is standard in the literature (see, for example, Glaeser et al., 2000). The Supplemental Material reports more details on the skills assessments, including extracts from the actual scripts/questionnaires used.

[19] This design allows us to check whether attendance is then given a lower weight during recruitment, relative to the other skills. In practice however, as shown in Appendix Table A2.1, the five soft skills are correlated, which limits our ability to separately identify the effect of revealing information on each skill. In Figure A5 in the Appendix we report the average importance given to each skill by managers.

grades in Uganda. Grades were given using an absolute scale, and so were not curved within our experimental sample.[20]

To provide further evidence on selection into the experimental sample, in Appendix Table A2.2 we verify how our five soft skills are correlated with the Big Five. We document a positive correlation for some of the skills: in particular, conscientiousness and agreeableness, which predict selection in the final sample, are positively correlated with trustworthiness and creativity, respectively. While these correlations are not particularly strong, they do suggest that our sample of workers is positively selected not just on the Big Five, as documented in Section 2, but also on at least some of the specific soft skills revealed during the intervention. Appendix Figure A6 reports the distribution of grades for the five soft skills: indeed, only 2% of workers have a grade of D or below (which would be considered Fail grades in Uganda) on all soft skills, and 88% of workers have a grade or A or B on at least one skill, so that the skill distribution is left skewed.

Validating the soft skills measures To validate our skills measures, we check whether they predict employment and earnings in our sample. We focus on the control group, where information on skills was never revealed to workers, and report employment and earnings regressions estimated by pooling the two post-intervention worker follow-up surveys. Even if there are sizeable information frictions, we would expect soft skills to matter for earnings as employers learn them over time (Altonji and Pierret, 2001). On the other hand, soft skills might not predict the extensive margin of employment if they are hard to observe at recruitment. To account for the correlation among soft skills, we aggregate them by creating a dummy equal to one if the worker scored C or above (that is, had a Pass grade) on all five skills.[21]

Appendix Table A3 shows that soft skills are a significant predictor of earnings in the two-year study period, even within our positively selected sample. This holds true conditional on observables such as gender, age and education, and also conditional on cognitive skills. For instance, column 6 shows that workers with a Pass grade on all skills earn $9 more per month, corresponding to an increase of 19% over the group with at least one Fail mark. Such increase in earnings is equivalent to having 2.5 additional years of education. While not causal, these results show that our measurements capture skills that are associated with labor market success.[22]

Columns 1 and 2 show that soft skills do not predict whether the worker is engaged in any paid work (including from wage employment, self employment and casual work), and nor does education, another strong predictor of earnings. This can in part be explained by the fact that

[20] More details on the grading procedure are given in the Supplemental Material.

[21] 41% of workers had a Pass grade on all skills.

[22] While the focus of this paper is on screening and signaling soft skills, these results also point to the potential value of *teaching* soft skills. In line with this, Adhvaryu et al. (2019) document high returns to soft skills training for garment workers in India.

engagement in paid work in the post-intervention period is already relatively high at 75%, which is expected given that by design we focus on youth that are more educated than the Ugandan average and are not disadvantaged, so that they do not face particular challenges in finding paid work. In columns 3 and 4 we focus on wage employment specifically, and find that soft skills are again not a significant predictor. On the other hand, column 4 shows that cognitive skills, years of education, vocational training duration and past employment are jointly significant at the 5% level in predicting wage employment. These four variables are all positively correlated and can be interpreted as proxies for hard/practical skills. This result is consistent with managers already being able to screen on hard/practical skills but not on soft skills. In line with this, Figure A2 shows that managers perceive practical skills as easier to screen than soft skills.

Treatment assignment and matching The second component of the intervention involved scheduling job interviews. This was done in two steps: first, workers and firms were randomly assigned to a treatment and a control group. The randomisation was done at the individual worker and firm level, and stratified by submarkets, where a submarket is a sector-BRAC branch combination. Workers and firms were then randomly matched within each strata and treatment group. Figure 3 shows a summary of our experimental design. The random assignment to treatment and control groups produced a balanced sample. Panel A of Appendix Table A4.1 reports balance checks on the firm side, showing a balanced sample on nine of the ten variables considered.[23] Panel A of Table A4.2 shows that the randomisation produced a balanced sample also for workers. Importantly, the sample is well balanced on all soft skills.

A total of 1,230 job interviews were scheduled: 616 in the treatment group, and 614 in control. The median firm (worker) was matched with three workers (one firm). There were no cross-treatment matches: treatment firms (workers) only met treatment workers (firms); control firms (workers) only met control workers (firms). Both groups got the matching component, but only the treatment group got the signaling component, as discussed below.[24]

Skills signaling We created certificates reporting the grades of treatment workers on the five soft skills. Panel A of Appendix Figure A7 shows an example. The order of the skills on the certificates was randomised. On the back page, the certificates reported a brief description of the skills assessment procedure, as well as guidelines to interpret the grades.[25] To stress the credibility of the certificate, the front page reported the signatures of two high BRAC officials.

[23]To limit concerns related to potential lack of balance, we note that the normalised difference for the other variable (i.e., whether the owner has received training from a VTI) is small (.12), and we further control for this variable and other firm characteristics in our main specifications. Also, we are not able to reject the null hypothesis that the ten variables considered for the balance checks are all jointly insignificant in predicting treatment assignment (p-value = .393).

[24]The Supplemental Material reports more details on treatment assignment and matching procedures. It also reports balance checks at the match level, and shows that the sample remains well balanced.

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

To control for any potential effects of simply releasing any new document, a placebo certificate was produced for workers in the control group. An example is shown in Panel B of Figure A7: the document simply states that the trainee was willing to be put in contact with potential employers, which is something that both the worker and the matched firms already knew, while it does not report information on skills. The certificate is otherwise identical to the treatment one. Any treatment effects will thus be due to the *content* of the certificate, rather than to just having an additional document in their application files.[26]

The timing of the revelation of the certificates was the same in the treatment and control groups. On the interview day, the worker was first met by the BRAC staff, who showed the certificate and explained its content to the worker. The worker was informed that the firm owner would be shown the same certificate at the start of the interview. The worker then had the option to pull out, in case she had changed her mind about wanting to meet the firm owner. In practice, only two workers met the BRAC staff but decided not to proceed to the job interviews.[27] The worker was then introduced to the matched firm by the BRAC staff, and the firm owner was also shown the transcript by the BRAC staff, who made sure the firm owner understood how to interpret the grades. 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.[28]

3.2 Data Collection, Compliance and Attrition

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

Figure 4 shows a summary of compliance and attrition. Starting from compliance, of the

[26]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.

[27]This is consistent with the sample of workers being positively selected on soft skills, as discussed above.

[28]Our design allows us to estimate the impact of the soft skill certificates over and above any 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% bringing either). This is in contrast to the results in Abel et al. (2020), who show very low rates of usage of reference letters at 2% among their sample of disadvantaged job-seekers in South Africa. This again confirms that our sample of youth are not disadvantaged.

1,230 scheduled job interviews, 515 (or 42%) took place. Lack of compliance is mainly due to workers having lost interest in being matched (32% of cases) or to the firm having lost interest (30% of cases) by the time they were called for the interviews.[29] Panel A of Appendix Table A5 explores the determinants of compliance, and shows little evidence of selection on observables.[30] Importantly, treatment assignment does not predict whether job interviews took 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 sample of job interviews that took place remains balanced on the main observable worker and firm characteristics.[31] All the treatment workers who showed up to the job interviews were given the certificates (corresponding to 49% of treatment workers). The remaining certificates were disbursed to the workers shortly after the first follow-up survey, so that 81% of treatment workers received the certificate in total.[32]

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. We have very moderate attrition rates: these are 12% in the firm follow-up, and 14% in both worker follow-ups. Panel B of Appendix Table A5 shows that attrition is not related to treatment assignment in either sample, and there is also very little evidence of observable characteristics determining attrition. Therefore, we do not correct for attrition in our main regression specifications.[33]

3.3 Empirical Strategy and Regression Specifications

Empirical strategy Going into this experiment, we had two main priors about the labor market impacts of the certificates intervention: (i) the overall benefits of the intervention in terms of employment and earnings should be larger for workers with higher soft skills, and could be negative for workers at the bottom of the skill distribution, although the voluntary nature of the intervention should limit the possibility of any such negative impacts; (ii) more productive firms with higher returns to soft skills should respond more to the certificates, leading to an

[29] As shown in Figure 1, for logistical reasons there was a lag of about three months from the graduation of trainees to the roll out of the matching intervention. This can explain the loss of interest. Another common reason why job interviews did not take place was failure to contact the worker/firm (18% of cases).

[30] The only significant predictor is the duration of the vocational course, so that workers in longer programs are less likely to participate. This is likely due to misunderstandings about when the intervention would take place, so that some workers had in fact not graduated yet by the time of roll out (and so were unable to participate).

[31] See the Supplemental Material for more details (Tables S3.1 and S3.2).

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

[33] Our attrition rates are in line with other studies in similar settings. For example, the attrition rate is 17% in Abel et al. (2020) and 15% in Abebe et al. (2021). Panel B of Appendix Table A4.1 and Panels B and C of Table A4.2 confirm that the samples of workers and firms overall remain balanced on baseline characteristics at follow-up. The firm follow-up shows some lack of balance on owner's age and past VTI training. However, we cannot reject that all observable firm characteristics are jointly insignificant in predicting treatment assignment at follow-up (p-value = .259). To further limit concerns related to attrition, we control for owner's age and VTI training in our main specifications. In the Supplemental Material (Table S7) we further show robustness of our main results to correcting for attrition using inverse probability weights (Wooldridge, 2010).

increase in sorting and efficiency. We planned our matched worker-firm design and our two-sided data collection to explore both dimensions of heterogeneity on the worker and firm side.

Ultimately, however, the impacts on employment and earnings depend on the priors of managers and workers, and how these are revised with the certificates. The extent to which managers will increase or decrease hiring depends on whether they adjust their beliefs upwards or downwards for different workers. Similarly, the wage adjustment accepted by workers will depend on the extent to which workers revise their labor market expectations. As we have shown, the sample of workers is positively selected. Thus, it is crucial to document impacts on beliefs, as we cannot merely assume that the revision will be positive for workers with above-average soft skills and negative for workers with below-average soft skills.

Therefore, we start by presenting treatment effects on the beliefs of both workers and firms, allowing for heterogeneity on both sides of the market. Then, we discuss the implications of the documented impacts on beliefs for the treatment effects that we should expect on employment and earnings. We do so by writing down a simple job search model with two-sided heterogeneity and beliefs updating, and use the model to guide the interpretation of the treatment effects on employment and earnings along the skill distribution. We return to how our results match our initial priors at the end of Section 5, after presenting our main results.

Regression specifications Our data and experimental design allow us to run regressions both at the match level and the worker/firm level. For the match-level analysis, we start by estimating the following OLS regression equation:

$$y_{ij} = \beta_0 + \beta_1 T_i + \beta_2 S_i + \beta_3 T_i \times S_i + \gamma \mathbf{X}_i + \delta \mathbf{X}_j + \theta \mathbf{Strata}_{ij} + \alpha Int_{ij} + \varepsilon_{ij}, \tag{1}$$

where y_{ij} is the outcome of the worker i-firm j match, such as the beliefs of firm j about worker i. T_i is a treatment group indicator. S_i captures the soft skills of worker i: in our baseline specification, we use the average grade on the five soft skills disclosed on the certificates, standardised to have mean 0 and standard deviation 1. As the soft skill measure is standardised, β_1 recovers the treatment effect for the average worker, and β_3 is then the heterogeneous effect by soft skills. We also estimate alternative specifications where S_i is a dummy equal to one if the worker had a Pass grade (i.e., C or above) on all soft skills. In this case, β_1 is the treatment effect for workers who had a Fail grade on at least one skill, and β_3 is the differential effect for workers who passed on all skills. $\mathbf{X}_i$ are baseline worker controls.[34] $\mathbf{X}_j$ are baseline firm controls.[35] $\mathbf{Strata}_{ij}$ are dummies for the stratification variables (sector and BRAC branch). Int_{ij} are dummies for the month of interview. We cluster standard errors both at the level

[34]These include: dummy for female, age and age squared, dummy for any work experience, VTI course duration (in years) and completed years of formal education.

[35]These include number of employees and the following owner characteristics: dummy for female, age and age squared, dummy for having attended a VTI.

of the firm and the worker, to reflect our research design whereby 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., 2020).

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 assignment does not predict which job interviews take place, and (ii) the sample of workers and firms remain balanced conditional on meeting. As the certificates were disclosed in all but two of the job interviews that took place, we interpret β_1 as the Average Treatment Effect (ATE) on the population that meets. Panel A of Appendix Table A5 shows very little evidence that observable worker and firm characteristics drive which job interviews take place. However, if unobservable characteristics drive the decision to participate, then the sample of realised matches might not be representative of the initially scheduled matches. We address this concern in the Supplemental Material, where we show that our results are robust to estimating a two-sided econometric model of sample selection.[36]

The main advantage of our two-sided experimental design is that it allows to study impacts on the *allocation* of labor. To do so, we need a measure of heterogeneity also on the firm side. What we are after is a measure of manager productivity and returns to soft skills, as this would allow us to test whether the intervention increased positive assortative matching between workers and jobs. In the baseline survey we collected three measures of manager skills: (i) cognitive skills (through a Raven matrices test); (ii) years of education; and (iii) the Big Five. Panels B and C of Appendix Table A6.1 show that firm owners with higher cognitive ability: (i) manage more skilled employees; (ii) have higher profits; (iii) value soft skills relatively more; and (iv) are better able to delegate task. This suggests that indeed they are more productive managers.[37] In particular, since they employ more skilled workers and value soft skills more, higher ability managers might be particularly responsive to the certificates, as hiring skilled workers is especially important for this group of managers. Indeed, we note that our trainees have similar education to the employees of high ability owners, and are instead substantially more educated than those of low ability firms, having over one more year of education than them on average. This justifies using a measure of manager ability in the heterogeneous analysis. As shown in Panel B of Table A6.1, cognitive ability is positively correlated with both the Big Five and years of schooling. Therefore, we start by aggregating all these skill measures (i.e., cognitive skills, the Big Five and years of education), and create a dummy equal to one if the manager

[36] In the Supplemental Material we also show robustness to estimating equations like 1 on the full sample of 1,230 *scheduled* matches by assigning a value of zero to the outcome of those job interviews that did not take place (see Table S5.2). β_1 recovers the Intention To Treat (ITT) parameter in this alternative specification.

[37] The fact that we find significant dispersion in skills and productivity within our firm sample is in line with Bassi et al. (2021), who find large differences in labor productivity and managerial quality across firms within three manufacturing sectors in Uganda. More generally, the literature on productivity dispersion tends to find substantial heterogeneity in productivity even within narrowly defined industries (Syverson, 2011).

has a value of the first principal component of these skills above the median.[38] This allows us to run heterogeneous effects by manager ability, and also to estimate a sorting regression similar to equation 1 but where a full set of interactions is included between worker skills, manager ability and a treatment indicator (so a triple difference specification).

As another approach to study sorting, we also estimate heterogeneous specifications that exploit the score on the individual soft skills of workers and their match with the needs of different employers. For example, we study whether workers with high communication skills are more likely to be hired by firms where interactions with customers are more frequent.

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

$$y_{it} = \beta_0 + \beta_1 T_i + \beta_2 S_i + \beta_3 T_i \times S_i + \beta_4 y_{i0} + \gamma \mathbf{X}_i + \theta \mathbf{Strata}_i + \alpha Int_{it} + \vartheta_t + \varepsilon_{it}, \qquad (2)$$

where y_{it} is the outcome of worker i in follow-up t, for instance their total earnings, y_{i0} is the baseline value of the outcome (when available), ϑ_t is an indicator for the second follow-up, and the other variables are as previously defined. In our preferred specification we pool observations from the two follow-up surveys and cluster standard errors at the worker level, to maximise power.[39] Some of the outcomes are only available at second follow-up, and we indicate when that is the case in the notes to the relevant tables. In this specification, β_1 recovers the ITT parameter since the sample includes all experimental workers, regardless of whether they met any firms for the job interviews. We also show heterogeneous effects by whether the worker met any firms as part of the matching intervention. For the firm-level analysis, we estimate equations like 2 but at the firm level, and so using only one round of follow-up firm surveys.

4 Impacts on Beliefs

We begin the empirical analysis by studying whether the certificates led firm owners to revise their beliefs on the skills of the matched workers, and whether workers reacted by updating their labor market expectations. We can expect managers to react to the certificates because, as documented in Section 2.3, they report soft skills as important but difficult to observe. At the same time, the labor market expectations of workers might change if through the certificates workers are better able to signal their skills to potential employers, or if they obtain more precise information on their own skills. Therefore, it is possible that *both* sides of the market updated their beliefs as a result of the new information.

[38] We also check robustness to using a continuous version of the first principal component and to using only cognitive skills as a measure of manager ability.

[39] We show dynamic treatment effects for our main outcomes in the Supplemental Material (Table S7).

4.1 Impacts on Managers' Beliefs

After each job interview, we elicited the beliefs of managers on the skills of the worker. Managers were asked whether: (i) there was anything they particularly liked about the worker; (ii) there was anything they particularly did not like about the worker; (iii) they thought the worker was more skilled than usual applicants; (iv) they thought the worker was less skilled than usual applicants. We create four dummies using the answers to these questions, and combine these into a standardised index, with a more positive value indicating a more positive assessment. Importantly, this index allows to document both positive and negative updating. We run OLS regressions analogous to equation 1 with this as dependent variable. As discussed above, the sample includes the 515 job interviews that took place.

Table 3 reports the results, both with and without controls. Column 1 shows that: (i) the estimated treatment effect for the average worker ($\hat{\beta_1}$) is positive and relatively large at around .1 standard deviations, although it is not significant at conventional levels (p-value = .165), and (ii) there is significant heterogeneity by soft skills, with the revision being more positive for workers with higher skills: the treatment effect is .26 standard deviations larger for those with soft skills one standard deviation above the average, a result significant at the 1% level. This conclusion is unaffected by whether we omit control variables, as shown in column 2.

In columns 3 and 4 we consider our alternative measure of skill heterogeneity, and find that the revision of beliefs is positive and significant for workers with a Pass grade on all soft skills: as shown by the interactions in row (v), the treatment effect for these workers is .36-.41 standard deviations larger than for workers with at least one Fail grade, a result significant at the 5% level. However, we find no strong evidence of negative updating for workers with lower skills, as indicated by the small and insignificant coefficients in row (i). That is, the certificates strengthen the correlation between workers' skills and managers' assessments, but this is driven by workers with higher soft skills. We confirm this in Panel A of Appendix Figure A8, which reports a non-parametric regression of the managers' assessments on the average soft skill grade of the worker, by treatment group. The Figure shows a relatively flat relationship in the control group, which is in line with managers being unable to screen on soft skills without the certificates. The correlation instead becomes positive in the treatment group, and the Figure confirms that the revision of beliefs is positive along most of the skill distribution. There is some evidence of negative revision at the very low end of the distribution, but this is at most limited to the 10% of workers who have an average soft skill grade of 2.4 or below (the median soft skill grade is 3.4 and the distribution is left skewed).[40] The positive revision for workers with higher skills, and the limited revision for workers with lower skills are consistent with the sample being

[40] In Appendix Table A7.1 we confirm that these results are not sensitive to alternative ways of aggregating the five soft skills. In the Supplemental Material, we also estimate more flexible specifications that allow for heterogeneous effects by terciles or quartiles of the average soft skills grade (see Table S4). These reinforce the main conclusions from Appendix Figure A8.

positively selected on soft skills, and with the low priors of managers on the skills of workers documented in Section 2.3, so that the revelation of information provides mostly *positive news* to managers. The fact that, as discussed in Section 2.3, managers are not familiar with the VTIs in our sample can explain why they miss the positive selection of workers into the experiment and why the reaction to the certificates is mostly positive.

In columns 5 and 6 we consider heterogeneity by manager ability, and find that the positive response is concentrated among higher ability managers, who revise upwards their beliefs significantly even for the average worker. This is in line with the evidence documented earlier that higher ability managers are more productive and value soft skills more, so that they are more responsive to the information.[41] Note that, as shown in Table A6.2, there are no differences by manager ability in how familiar managers are with our partner VTIs, and in their expectations about the quality of workers graduating from them. This can explain why also higher ability managers miss the positive selection of workers into the experiment.[42] Finally, in columns 7 and 8 we include a full set of interactions between worker skills, manager ability and the treatment dummy. The coefficients in rows (i) and (v) show that while low ability managers do also revise their beliefs, this revision is weaker and imprecisely estimated. The coefficients in rows (vii) and (viii) of columns 7 and 8 decompose the difference between high and low ability managers documented in row (vii) of columns 5 and 6 into that part coming from matches with lower skilled workers (row (vii) of columns 7 and 8), and the *additional* effect from meeting a worker with higher skills (row (viii) of columns 7 and 8). The results confirm that the positive revision of higher ability managers is stronger for all types of workers. The positive triple-difference estimates in row (viii) suggest that the difference in treatment effects between high and low ability managers is stronger for workers with higher skills, but not significantly so.[43]

One potential concern with this heterogeneous analysis is that manager ability might proxy for other firm characteristics. To address this, we run a regression where the treatment dummy is interacted with a number of firm and manager characteristics, all at the same time. Appendix Table A7.2 reports the coefficients on such interactions, and confirms that manager ability is the key source of heterogeneity, as the coefficient on the interaction with the high ability dummy remains large, stable and significant at the 1% level as other interactions are progressively added.[44] As discussed later in Section 5, we will show that these heterogeneous impacts on

[41]Appendix Table A7.1 shows that these results are robust to alternative ways of defining manager ability.

[42]Row (vi) of Table 3 shows that in the control group, higher ability managers have a lower assessment of workers' skills. On this point, we note that soft skills grades do not predict managers' assessments in the control group (for either type of manager), so this result is not driven by higher ability managers being more or less able to screen the soft skills of control workers. As higher ability managers are more skilled themselves and employ workers with higher skills, this more negative assessment in the control group might simply reflect the fact that high ability managers compare the matched workers against a higher benchmark.

[43]Overall, Table 3 shows that the inclusion of control variables does not alter the results significantly. Therefore, we relegate remaining results without controls to the Supplemental Material.

[44]In particular, Table A7.2 controls for interactions between the treatment dummy and: (i) manager's risk

beliefs then map into heterogeneous impacts on hiring. As hiring is a high-stakes outcome, this alleviates the additional potential concern that the impacts on beliefs might be driven by experimenter demand effects related to high ability owners being better able to understand what the experimenter is after and responding to this.[45]

In summary, the overall positive revision of beliefs is consistent with workers being positively selected and with managers having low priors on the distribution of soft skills. The fact that the revision is stronger for managers of higher ability is in line with these managers being more productive and valuing soft skills more, and so paying more attention to the certificates. The results on manager updating are in line with the findings of the literature on recruitment in firms, which shows that the introduction of job tests helps managers select workers with higher skills (Autor and Scarborough, 2008), and that better managers are more likely to follow the recommendations of job tests (Hoffman et al., 2018).

4.2 Impacts on Job-Seekers' Beliefs and Outside Options

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

We pool data from the two worker follow-up surveys, and estimate regressions analogous to equation 2. Table 4 reports the results. Panel A focuses on *beliefs*. Column 1 shows that the average treated worker reports expected monthly earnings that are \$7.9 higher than the control group, a result significant at the 5% level and corresponding to a 7% increase. 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, a result at the margin of significance (column 3), and are also 7pp more likely to report that their ideal job is in a firm with 10 or more employees (i.e., a "large" firm) (column 4). These results suggest that the certificates raise the perceptions of workers about what they can achieve in the labor market.

While impacts for the average worker are positive, there is no significant evidence of heterogeneity by worker skills, as shown by the estimates of β_3. This is consistent with the positive

aversion, (ii) manager's English proficiency, and (iii) baseline firm performance indicators, namely number of employees and profits. This helps rule out that the stronger reaction of higher ability managers to the certificates is capturing differences in risk preferences, in whether the content of the certificates was understood properly, or in labor demand due to differential firm growth. The results in Table A7.2 additionally rule out that manager ability is capturing sectoral heterogeneity, as shown by the p-value on the test of joint significance of the interactions of the sector dummies with the treatment dummy, reported at the bottom of the table.

[45] The consistency of the impacts on beliefs with those on hiring, and the stability of the coefficients on the interactions between the high ability dummy and treatment in Table A7.2 as other interactions are added reassure us that these heterogeneous results on beliefs are not simply driven by noise in small samples.

selection of workers into the experiment, which limits the extent of skill heterogeneity in our sample, and with the fact that higher ability managers revise their beliefs upwards even for the average worker. Another factor that reduces the scope for heterogeneity in Table 4 is that information comes from the follow-up surveys, when workers are searching in the wider labor market: as at that point workers can strategically decide whether to show the certificates during job search, we do not expect a negative effect on labor market expectations even at the bottom of the skill distribution, which then further limits the potential for heterogeneous effects. In line with this, Panel C of Appendix Figure A8 reports non-parametric treatment effects on expected earnings along the skill distribution, and shows that the impact is positive along most of the distribution, and there is no evidence of significant negative revision at the bottom of the distribution. This is in contrast to Panel A, which shows some evidence that in the matching intervention treated managers revise downwards their beliefs for workers with very low skills.

Panel B of Table 4 shows that treated workers change their labor market *behaviours* in a way consistent with their revision of expectations: the average treatment workers are 15% less likely to engage in casual work (column 5), which is a poorly paid and insecure form of employment; in addition, they are 3.8pp more likely to have attended further education or training in the post-intervention period, from a control mean of 12% (column 6). This effect on human capital accumulation is particularly revealing, as it points to the presence of complementarities between certification and investment in human capital (Spence, 1973). We investigate this further in Appendix Table A9, where we show that the treatment effects on education/training are entirely driven by individuals that we predict to have high ability for schooling, based on their baseline characteristics.[46] These results are then consistent with talented workers limiting their human capital investments because of information frictions, and with the certificates improving sorting also in terms of the allocation of labor between work and education. Moving to job search, treated workers are also 39% more likely to have looked for a job in the public or NGO sector in the last year (column 7); however, they do not increase their overall search intensity (column 8), so that the impact is primarily on search direction. These changes in behaviour are all consistent with treated workers trying to transition to better and more productive jobs. Again, we do not find any significant heterogeneity by worker soft skills.

Table 5 explores *why* workers reacted to the certificates. We distinguish between three potential explanations: (i) workers might have learned about their own soft skills; (ii) the certificates might have affected the perceptions of workers about the labor market returns to soft skills; (iii) the certificates might have improved the ability of workers to signal their skills. In the second follow-up, we asked workers in both treatment and control groups to assess

[46]In particular, we proxy schooling ability by a standardised index of four variables measured at baseline: (i) completed years of formal education prior to enrolling at the VTI; (ii) cognitive skills (measured through Raven matrices); (iii) a dummy for whether the worker was planning to gain further formal training or education in the future; (iv) the highest level of formal education that the worker would like to achieve in the future.

themselves on the five soft skills reported on the certificates, using the same A-E scale used for grading. Column 1 shows that there is a positive and significant correlation between the worker self-evaluations and our measurements. This shows that workers are already aware of their soft skills, and is in line with the positive selection into the experiment documented above. Column 1 further shows that the certificates do not alter the correlation between the self-reports and our measurements. So workers are *not* learning about their skills. Column 2 shows that there is no impact on perceived returns to soft skills, thus confirming that workers are also not changing their beliefs about the importance of soft skills. Finally, column 3 shows that treated workers believe they face fewer challenges in signaling their skills to employers. The effect corresponds to a 7% reduction over the control mean (significant at the 5% level). This evidence indicates that the impacts on the perceived outside options of workers are driven by the signaling value of the certificates. Consistently, we further note that among treatment workers: (i) 92% still had the certificate after two years, and (ii) 74% reported using it in job search.[47]

In Appendix Table A8.1 we report heterogeneous effects on the outcomes in Tables 4 and 5 by whether the worker met any firms in the matching intervention. This is a valid comparison because, as discussed in Section 3.2, treatment assignment does not predict whether the job interviews took place. The Table shows that the treatment effects are driven by the matched sample. In particular, treatment effects for the matched sample are largely significant and in almost all cases stronger than in the whole sample, as indicated by the coefficient on the Treatment dummy. On the other hand, impacts for the unmatched sample are largely not significant, as shown by the p-values from the test of significance of the sum of the coefficients on the Treatment dummy and the interaction with the dummy for whether the worker did not meet any firms, reported at the bottom of the table. The larger impacts for the matched sample suggest that workers learned about the importance/value of the certificates from the reaction they got from the matched employers. As managers mostly responded positively, this can then explain why the matched workers revise their labor expectations upwards on average. This result again reinforces our interpretation that both sides of the market face information frictions and react to the certificates, and that job interviews are a crucial stage when information is revealed.

5 Impacts on Sorting, Employment and Earnings

In the previous section, we showed that, upon receiving the certificates: (i) managers revise upwards their beliefs on the skills of workers, with a stronger effect among higher ability managers, and (ii) workers increase their labor market expectations. We now present the reduced form impacts on sorting, overall employment and earnings. To guide the discussion, we inter-

[47]This is consistent with employers outside our experiment also valuing the BRAC certificate, which is not surprising given that BRAC is very well known throughout Uganda for its youth and firm programs.

pret these results in the context of a search model with two-sided heterogeneity, which makes precise the impacts on labor market outcomes that we should expect given the revision of beliefs documented above. Altogether, the empirical evidence in this section shows that the treatment certificates lead to an increase in sorting, as well as to an increase in earnings conditional on employment, which is particularly pronounced at the top of the skill distribution.

5.1 Conceptual Framework and Mapping to Data

We develop a partial equilibrium search model with asymmetric information on workers' skills and two-sided updating, building on our evidence on two-sided belief updating documented above. We provide full details of the model in Appendix C. Here, we sketch the main argument and provide the economic intuition behind the main results.

There are two types of workers, who differ in their soft skills, and two types of firms, who differ in their production technology. High-skill workers generate higher output only when matched to high-type firms. Therefore, the output-maximising allocation exhibits positive assortative matching. Search frictions and asymmetric information on workers' skills result in worker-firm mismatches and output losses. We model the intervention as an increase in the precision of the signal on workers' soft skills during job interviews.[48] The model generates the following comparative statics implications about the introduction of the certificates:

Implication 1. *There is an increase in positive assortative matching.*

Implication 2. *The change in employment probability is ambiguous for both types of workers.*

Implication 3. *Average wages conditional on employment increase for high-skill workers and decrease for low-skill workers.*

The effect on assortative matching follows from the reduction in information asymmetries caused by the certificates, which improve the precision of workers' signals. On the one hand, high-skill workers are more likely to receive and accept an offer from high-type firms, who now offer a higher wage due to higher expected productivity. On the other hand, low-skill workers are more likely to accept offers from low-type firms, due to lower employment opportunities at high-type firms. As the treatment leads workers to *reallocate* across firms, effects on overall employment probability are ambiguous. Earnings conditional on employment increase for high-skill workers, since they are more likely to get employed at high-type firms, who offer a higher wage. On the other hand, low-skill workers earn less, as their probability of employment at high-type firms is reduced.

[48] For simplicity, the model abstracts from the positive workers' selection and low managers' priors documented above.

The fact that the certificate is observed by both sides of the market is crucial for the intervention to have an impact on wages: in the treatment group, high-skill workers can truthfully reveal their skills to all high-type employers. This increases their outside option, and so each high-type employer has to increase the offered wage for high-skill workers to accept their offer.

The model also highlights that while high-skill workers always benefit from the certificates, the latter can worsen the outcomes of workers at the low end of the distribution by reducing their employment probability at high-type firms. So while the certificates improve the efficiency of the allocation of workers to jobs, they can increase wage dispersion and inequality among workers. Since low-skill workers represent a small share of the sample due to the positive selection on soft skills that we documented, we are unable to use the experiment to study impacts on these workers. In Section 6 we use the insights from this model together with the reduced form evidence from the experiment to discuss the implications of scaling up the certification intervention to workers of all skill levels.

5.2 Impacts on Sorting

We present two sets of results on sorting impacts from the matching intervention: first, we study whether the certificates increase the sorting of workers with high soft skills to high ability managers, who we have shown are more productive and value soft skills more in general; second, we leverage the scores on specific soft skills and their match with the needs of different employers, to examine whether firms in need of certain skills are more likely to hire a worker with a higher grade on those skills.

Sorting between soft skills and manager ability Table 6 reports the results of match-level regressions analogous to equation 1, but where the outcome is a dummy for whether the worker was hired by the matched firm. The data is from the first worker follow-up, and so the observations are 412 (instead of 515), due to the attrition discussed in Section 3.2.[49] Columns 1 and 2 show that: (i) there is no significant treatment effect for the average worker, and (ii) there is no evidence of significant treatment heterogeneity by worker skills.[50] This is consistent with model Implication 2, which states that the impact on overall employment is ambiguous if workers reallocate between different types of firms. That is, given that managers revise upwards

[49]Information on hiring outcomes in the matching intervention was collected in both the firm and worker follow-ups. We prefer to use information from the worker follow-ups as measurement error is likely lower there for at least two reasons: (i) while the median firm was matched to three workers, the median worker was matched to one firm, so possible recall errors related to the respondent getting confused about the different job interviews are lower on the worker side; (ii) in 13% of the cases, the person answering the firm follow-up survey is different from the owner that conducted the job interviews. However, we note that the correlation between hiring outcomes in the two surveys is .82, and results using firm reports (not shown) are qualitatively similar.

[50]Appendix Table A7.1 shows that this result does not depend on the way skills are aggregated. In line with this, Panel B of Appendix Figure A8 also shows a lack of significant heterogeneity in non-parametric regressions of hiring probability on worker skills.

their beliefs on the skills of workers (columns 1-4, Table 3), we would expect this to increase the employment probability of treated workers at firms that value those skills, and to reduce it at firms that do not value them, so that the net effect on overall employment is ambiguous.

In column 3 we consider heterogeneity by manager ability: the coefficient in row (i) is the treatment effect for low ability managers. This is negative, even though not significant. On the other hand, higher ability managers in the treatment group are 13pp more likely than low ability managers to hire a worker, as shown by the positive and significant estimate in row (vii). The sum of the coefficients in rows (i) and (vii) is the treatment effect for high ability managers, and the p-value at the bottom of column 3 shows that this is positive and at the margin of significance. As these effects are imprecisely estimated, we check robustness in Appendix Table A7.1, where we use only cognitive ability (rather than the first principal component) as a measure of ability. We find that the treatment effect for low ability managers is negative and significant at the 10% level, and the one for high ability managers is positive and significant at the 5% level (Table A7.1, column 8). As discussed, we interpret our positively selected sample as mostly comprising high-skill workers. These results are then in line with Implication 1, which states that the treatment should result in an increase in the probability of employment between high-skill workers and high-type firms, and to a *decrease* in employment between high-skill workers and low-type firms. Finally, in column 4 we include the full set of interactions between worker skills, manager ability and the treatment dummy. This does not add further insights compared to column 3, again confirming that heterogeneity by worker skills is less relevant, while manager ability is the key source of heterogeneity.[51]

Taken together, these results show that the certificates lead to an increase in positive assortative matching between higher ability managers and our positively selected sample of experimental workers. As discussed in Section 3.3, our experimental workers have similar education level to the employees of higher ability managers, but are more educated than those working for low ability ones. The results in Table 6 therefore show that the certificates also lead to an increase in positive assortative matching on education.

Sorting between soft skills components and specific managers' needs While the workers in our sample are positively selected on soft skills on average, we can exploit variation in the grades on specific soft skills to study whether the treatment leads to an increase in positive assortative matching between workers with high values of specific skills and firms particularly

[51]To further rule out that manager ability is proxying for other characteristics, Appendix Table A7.2 progressively adds interactions between the treatment dummy and other firm and manager characteristics. The stability of the interaction between the high ability dummy and treatment reassures us that manager ability is the key source of heterogeneity. In particular, these results rule out that risk aversion plays a significant role in explaining the results. This indicates that the stronger reaction of high ability managers to the certificates is more in line with the sample of workers being positively selected and high ability managers valuing soft skills more, rather than high ability managers valuing a reduction in uncertainty per se due to risk aversion.

in need of those skills. We proxy firms' needs with various firm characteristics and examine the following dimensions of sorting between worker skills and firm characteristics: (i) communication skills and number of employees; (ii) willingness to help others and number of employees; (iii) communication skills and number of customers per worker. Larger firms may be particularly in need of workers who can communicate effectively and are pro-social, as employees in this context typically work in the same small space on similar tasks, and often in teams. In addition, firms with more customers may particularly value communication skills because in this context there is no clear separation between production and retail space. Therefore, interactions with customers happen directly at the firm premises and can involve employees, as the firm owner might be away, for instance.[52]

The results are in Table 7. We regress a dummy for whether the worker was hired by the matched firm on our triple-interaction specification between workers' skill, our proxy for managers' need for that skill, and treatment. Workers are divided into having a high or low value of the specific skill considered (e.g., communication skills), by whether they scored above or below the median. For both communication skills and willingness to help others, scoring above the median corresponds to having a Pass grade on that skill (i.e., C or above). Similarly, managers are divided into low and high need for a given skill by whether the specific firm characteristics related to that skill (e.g., firm size) is above or below the median.

Columns 1-3 show that we find significant evidence that the treatment leads workers with higher communication skills and willingness to help others to match with larger firms, and to reallocate away from smaller firms where those skills may be less needed. This is indicated by the positive triple interaction in row (vi) and the negative interactions between the high skill dummy and treatment in row (iii). As shown by the coefficients in rows (i) and (v), there is also some evidence that treated workers with low communication/willingness to help others are more likely to be hired in smaller firms, and less likely to be hired in larger firms. However, the effect on lower ability workers is weaker. This is again consistent with our sample being positively selected, so that treatment effects for workers at the bottom of the distribution are more muted. Finally, column 4 shows increased sorting also when looking at the match between the number of customers per worker and communication skills, as the coefficient on the triple interaction is positive and significant.[53]

[52] Bassi et al. (2021) provide direct evidence that in this context workers' specialisation across tasks is limited, teamwork is frequent, and interactions with customers happen directly at the firm premises. In particular, their analysis for the carpentry sector in Uganda shows that: (i) 80% of orders are placed at the firm premises through walk-ins by customers; (ii) the typical employee works on more than half of the production steps; and (iii) production steps feature direct teamwork between employees in about 20% of cases.

[53] We also examined sorting on trustworthiness and the following proxies for firms' need of trustworthiness: (i) presence of expensive machines; (ii) (reported) importance of stealing as a constraint; and (iii) how often the owner is away from the firm premises, as a proxy for the amount of supervision. We do not find any significant evidence of sorting on these dimensions (results not reported). We note that the interpretation of these results is less straightforward however, as lack of trust related to stealing is something that firms may be able to hedge

The results in Table 7 indicate that heterogeneity within our sample, while limited, is still relevant, and leads to sorting on specific dimensions such as communication skills and firm size. To reconcile these results with the positive treatment effects for the average worker documented in most of the other tables, note again that most workers have a high value of at least one skill: only 2 workers out of the 787 in our sample have a Fail grade on every skill, and 88% of workers have a grade or A or B on at least one skill. Therefore, we expect the average worker to have high scores on at least some dimensions valued by at least some firms. This can then explain why we see positive effects on outside options for the average worker. This is also consistent with high ability managers reacting positively for the average worker – because high ability managers value soft skills more on average, and the average worker has high grades on at least some dimensions of soft skills. So, overall, our results are consistent with the information improving sorting – of our positively selected sample of workers in general to higher ability managers (who value soft skills more in general), and of workers with a high value of specific skills to firms with a need for those skills.

5.3 Impacts on Employment

Implication 2 states that the impact of the certificates on overall employment of high-skill workers is ambiguous: the certificates increase their probability of employment at high-type firms; however, since these workers also revise upwards their reservation wages, this reduces their employment at low-type firms, who value soft skills less and so are not willing to pay them more. As discussed above, column 1 of Table 6 shows that indeed the certificates do not lead to a change in the overall probability of employment in the matching intervention. We further study treatment effects on employment in the two years post intervention. Table 8 reports the results of worker-level regressions analogous to equation 2 where we pool observations from the two follow-up surveys. The Table shows that treated workers are not significantly more likely to be in wage or self-employment at follow-up (columns 1 and 2), although the effect on wage employment is positive. Also, there is no significant effect on hours worked in the last job (columns 4-7).[54] The interactions between soft skills and treatment throughout the table show that there is no significant heterogeneity. These results confirm the evidence from Table 6 and Table 7 that the certificates change the allocation of workers to firms, but do not significantly increase overall employment, in line with model Implication 2.

against through protective investments: for instance, if firms with expensive machines have taken actions to secure them, such as locking them, then this could weaken their response to information on trustworthiness, making any predictions on this margin of sorting ambiguous. Indeed, we find a *negative* correlation between the presence of expensive machines and reported episodes of stealing, which is consistent with such protective investments taking place. These results suggest that providing information on skills associated with behaviours that firms can respond to through protective investments might not change hiring decisions.

[54] Conditional on employment, workers in the control group work 50-60 hours a week, so there is not much scope for the treatment to increase the intensive margin of hours worked.

Finally, in column 3 of Table 8 we study impacts on the probability of being involved in non-casual employment or education/training, that is, in "productive" activities. We find a positive and significant treatment effect on this margin for the average worker, corresponding to an 8% increase. This result is driven by a reallocation away from casual work (Table 4, column 5) and onto education/training (Table 4, column 6) as well as to a lesser extent wage employment (Table 8, column 1). In a broad sense, this highlights how the certificates improve not only the sorting of workers to jobs, but also the allocation of labor across sectors.[55]

5.4 Impacts on Earnings

The overall number of hires in the matching intervention was low: fewer than 50 workers were hired across the two experimental groups.[56] Such low take-up makes it difficult to study wages at the matched firm. Nevertheless, when using information on weekly earnings at the matched firm, we find that treatment workers earned $4.05 on average in the first week of employment, compared to $2.46 in the control group. While this difference in means is not statistically significant given the small sample, these results are in line with Implication 3, which states that the certificates should increase wages for high-skill workers, since high-type firms are willing to pay these workers more, and their outside option has increased.

We further probe this finding by running earnings regressions on all workers in the two years post intervention, regardless of the outcomes of the matching intervention. Since very few workers are still employed at the originally matched firms after one year, we interpret any results on earnings as arising mostly from other firms that workers match to later on. Table 9 shows the results. Column 1 confirms that there is no impact on paid employment. In columns 2-7 the dependent variable is total earnings in the month before the survey. Column 2 shows that when all workers are included in the regression, so that those with no earnings are assigned a value of zero, we find an 8% increase in earnings for the average worker, but this is not significant. Columns 3-6 report quantile regressions on the full sample, and show that there is a positive and significant impact on earnings at the 75th quantile and above. Figure 5 reports quantile regressions along the entire distribution of earnings, and confirms a positive and significant treatment effect in the upper quartile of the earnings distribution.[57] Finally, since we showed

[55] We have very limited information on the characteristics of the firms where workers found employment outside the matching intervention. Specifically, the information on manager ability and number of customers used to study sorting in Tables 6 and 7 is not available for these firms. This limits our ability to study sorting in the end-line surveys. We do however have data on the size of the worker's last employer at follow-up (including the current job). We do not find significant impacts on this outcome, but note that we cannot reject a positive impact on firm size similar to the effect documented for ideal firm size in Table 4 (results not reported).

[56] This result is in line with recent evaluations of matching interventions, such as Abebe et al. (2020), Alfonsi et al. (2020), Bandiera et al. (2020) and Groh et al. (2015), which all find very limited take-up. Taken together, these results suggest that small firms in developing countries do not face particular challenges in *meeting* workers.

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

that treatment assignment does not affect selection into paid employment, in column 7 of Table 9 we report impacts conditional on employment at follow-up: we find that the certificates lead to an increase of about \$7 per month for the average worker, a result significant at the 5% level, and corresponding to an 11% increase in monthly earnings, relative to the control mean.[58]

On heterogeneity by soft skills, we note that all interactions between skills and treatment in Table 9 are positive, with the one in column 6 being significant. In Panel D of Appendix Figure A8 we probe this further by running a non-parametric regression of earnings (conditional on employment) on the average soft skills grade, by treatment group. The Figure shows that the earnings gains are significantly larger for workers at the top of the distribution. These results are consistent with Figure 5, which shows that the positive effects are concentrated at the upper quantiles of earnings. Taken together, these results suggest that highly skilled workers gained more from the intervention, which is again in line with the information strengthening positive assortative matching between workers and jobs. The fact that the earnings gains are larger at the top of the distribution also explains why we find significant impacts on earnings conditional on employment, but not on unconditional earnings: since there is no impact on involvement in paid employment and no effect at the lower quantiles of earnings, including unemployed workers in the earnings regression just dilutes the positive effect found further up the distribution.[59]

Taken together, the results from Tables 4, 6, 7 and 9 give support to the claim that the increase in earnings is due to workers transitioning to more productive employment: Table 4 shows that workers reallocate away from poorly paid casual work, they increase their investment in skills, and they search more ambitiously in the two years post intervention, which suggests that they are transitioning to more productive jobs. Tables 6 and 7 further substantiate this claim by showing that in the matching intervention, more productive managers and those in particular need of specific soft skills are the ones that respond to the certificates and increase their hiring rates. Since we have shown that our sample of workers are positively selected on skills, these results are then consistent with the provision of information resulting in an increase in sorting between workers and jobs, and with higher wages as a result.[60]

[58]Column 1 of Table 9 shows that the treatment does not change the share of workers employed. In the Supplemental Material, we show that the sample of workers employed at follow-up remains well balanced on baseline characteristics, across treatment and control groups (Table S6). This limits potential concerns about the interpretation of results conditional on employment (Lee, 2009; Attanasio et al., 2011), since there is little evidence that the certificates affect selection into employment in our case.

[59]The Supplemental Material shows that the earnings impacts are robust to: (i) applying a log transformation; (ii) excluding control variables; (iii) using inverse probability weights to correct for attrition (Wooldridge, 2010), and that we do not find significant evidence of the impacts on earnings varying across follow-ups (Table S7).

[60]The earnings results cannot be explained solely by the impact on human capital accumulation documented in Table 4, and so must reflect also a change in the allocation of labor: assuming a return to education of 7.5% per year (taken from the Mincerian regression in Appendix Table A3) and assuming that the 3.8pp increase in the probability of further enrolment in education/training documented in column 6 of Table 4 corresponds to two full extra years of education, the treatment effect on skills accumulation can then explain an earnings impact of around 1% (i.e., $.038 \times (7.5\% \times 2) \approx 1\%$), while the impact in column 7 of Table 9 is 11%.

5.5 Discussion

While the results on sorting, employment and earnings can be explained by our model, we highlight three points on how they fit with our initial priors. First, the positive effects on earnings for the average worker were not necessarily expected: sensible priors would have been that only workers with relatively high skills gain from the certificates. This result can be explained by the positive selection of workers into the experiment and the low managers' priors that we have documented. Thinking about external validity, the fact that we document impacts even for the average worker likely generalises to other settings where recruiters would plausibly find it difficult to understand the selection of candidates in the applicant pool, such as in developing-country labor markets dominated by small firms with limited experience hiring workers. On the other hand, we would not necessarily expect the certificates to have an effect for the average applicant in settings with larger and more sophisticated recruiters, who may have less biased beliefs about the underlying ability distribution of applicants (Autor and Scarborough, 2008).

Second, the increase in sorting is in line with sensible priors based on standard sorting models. Third, the lack of positive effects on overall employment was also not expected necessarily. This result highlights that the primary impact of the certificates is on worker *reallocation* across jobs, rather than on the extensive margin of overall employment. This can be explained by the relatively high employment rates in the control group, so that any scope for impacts on overall employment is limited in this context. A larger impact could be expected in settings with more disadvantaged job-seekers and lower employment rates, such as those in the related studies by Abebe et al. (2021) and Carranza et al. (2020).

On this last point, we further note that column 1 of Appendix Table A8.2 shows that we do find a positive and significant treatment effect on wage employment for the sample of workers who met at least one firm in the matching intervention, corresponding to a 17% increase over the control mean. This result confirms that the impacts are stronger for the matched sample, which is in line with Table A8.1, and further helps to reconcile the findings on employment with our initial priors.

6 Policy Implications and Conclusion

6.1 Policy Implications

As this was a relatively inexpensive intervention, with a cost per worker of $19, we find that the certificates were cost-effective at increasing earnings for those workers who found employment, even if we assume that the gains only lasted for the 2-year study period.[61] This then raises

[61] Details of the cost-benefit analysis are in Appendix D. We only consider the costs of the certification intervention. So we do not include here the costs of matching workers to firms. We assume zero impacts on

the question of why soft skill certificates are not already provided by the market. We can rule out lack of demand by workers. At first follow-up we asked workers in control – who never saw the results of their skills assessments – for their willingness to pay for certificates similar to the treatment ones. We find that workers would be willing to pay $18 on average for the certificates, corresponding to 44% of their monthly earnings. So their willingness to pay is substantial, and interestingly, very similar to the cost of the certificates ($19).[62]

It is possible however that credibility concerns explain why there is no firm assessing workers on soft skills and selling this information to the market, as the profitability of this activity clearly 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, increasing the entrepreneurial risk of this activity. Finally, we note that providing soft skills certificates might also not be profit maximising for VTIs: as discussed, about 20% of workers decided not to participate in the intervention, and this is consistent with them realising they would not have benefited from it. Therefore, if vocational institutes started advertising new certificates on soft skills, this might affect enrolment decisions 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. When thinking about the incentives of the government to produce this kind of information, our reduced form results and our model show that while more information on skills can improve the allocation of labor and productivity, it is possible that some workers with low skills might lose out from this, thus also increasing wage inequality among workers.[63] The willingness of the government to step in and provide this information would then depend on political economy considerations regarding the trade-off between equity and efficiency in the labor market.

Finally, we highlight four important considerations about scaling up this type of intervention.

profits, as we do not have reliable profit data. If the intervention produced productivity gains through the improved allocation of labor, then setting these to zero creates a lower bound to the benefits on the firm side. While we find no impact on firm size at follow-up, Appendix Table A10 shows some evidence that higher ability owners in the treatment group report lower challenges in screening workers (column 2), and also revise upwards their ideal size (column 8), suggesting possible long-lasting benefits to firms. Providing more direct evidence on the firm-side impacts of certification interventions remains an important area for future research.

[62]Willingness to pay was elicited using a "take-it-or-leave-it" (TIOLI) approach (Berry et al., 2020): 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. These results are in contrast to Abel et al. (2020), who show that lack of demand by workers limits usage of reference letters by disadvantaged job-seekers in South Africa.

[63]We formalise this in Appendix Figure A9, where we perform a bounding exercise, and show how the cost-benefit calculations for the average *eligible* worker change as those 22% of workers that initially selected out are allowed to experience a (conjectured) reduction in earnings from participation in the program. We find that if those workers that selected out experienced a loss of less than 20% of their earnings, the intervention would still be cost-effective for the average *eligible* worker. However, if their earnings losses were larger than 20%, 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.

First, in terms of feasibility, while there might be concerns related to the scalability of trust games or psychometric assessments (as workers might learn to "game" these tests over time), teacher surveys seem a scalable alternative. Second, making participation voluntary in a large-scale certification program might produce equivalent results to introducing a mandatory policy due to unravelling effects (Jin and Leslie, 2003), as not presenting a soft skills certificate at a job interview would then be perceived by firms as a negative signal. Third, the total effects of this type of interventions depend also on any additional changes in the hiring behaviour of firms. We provide tentative evidence on this in Appendix Table A10. There is some evidence that treatment assignment reduces the perceived constraints in screening soft skills for higher ability managers at follow-up (column 2), and this is associated with an increase in the duration of the typical interview (column 4). While these results are imprecise due to the small sample of firms, they suggest that the total impacts of signaling interventions could extend beyond their direct effect through additional improvements in the recruitment behaviour of firms coming for example from longer interviews as firm learn about the importance of screening on soft skills.[64] Finally, perhaps the most important consideration about scaling up is that such policies would provide an incentive for new generations to increase their investment in skills, thus potentially weakening the trade-off between efficiency and equity in the long-run. The positive treatment effect on human capital accumulation documented in Table 4 is particularly revealing in this respect, and so this is an area that deserves more attention in future research.[65]

6.2 Conclusion

This paper studies how lack of credible information on the skills of workers affects job matching in a developing country. We do so using a field experiment that aims to understand how employers and job-seekers react to certificates on the non-cognitive skills of workers at recruitment. Our main finding is that both sides of the market respond to the certificates in terms of beliefs, and that this improves the allocation of labor, as firms are better able to screen productive workers, and workers are better able to signal their skills in the market. Consistent with better matching, earnings increase in our sample of employed workers. Taken together, our findings highlight that: (i) lack of information on the skills of workers at recruitment creates a significant friction that contributes to keeping wages and productivity low; (ii) a credible signal from a trusted institution can significantly reduce the information friction. We believe these results have important implications for the design of labor market policies in the developing world.

In this paper we have taken a first step towards understanding how both sides of the labor

[64] Abebe et al. (2020) show that firms in Ethiopia invested more in recruitment after participating in a job fair that produced very few hires.

[65] If improved information on skills boosts aggregate demand for labor, then this would provide an additional channel weakening the efficiency-equity trade-off of providing information at scale.

market are affected by information frictions at recruitment. Looking ahead, a promising extension would be to understand the general equilibrium effects of scaling up this type of information interventions. This would require a randomisation at the regional level, introducing certificates in some local labor markets but not others. While the challenges of implementing such a design would be substantial due to the high spatial mobility of workers in developing countries, this type of study would generate important directives for labor market policy, and so is something worth attempting in future research.

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Table 1: Firm descriptives from initial census

	Mean (1)	SD (2)	Median (3)
A. Owner and firm characteristics			
Owner is female	.397		
Number of employees	5.88	7.14	4
Business is registered	.938		
Age of business (Years)	7.09	5.90	5
B. Sector			
Carpentry	.138		
Catering	.157		
Hairdressing	.302		
Motormechanics	.122		
Tailoring	.082		
Welding	.199		
C. Region			
Kampala	.425		
North	.123		
East	.270		
West	.181		

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 two employees and operating in six sectors: carpentry, catering, hairdressing, motor-mechanics, tailoring and welding.

Table 2: Worker descriptives from initial census

	Mean	SD	Median
	(1)	(2)	(3)
A. Worker characteristics			
Age (Years)	20.2	2.50	20
Female	.551		
Completed prior education (Years)	10.3	2.05	11
Course duration (Years)	1.41	.934	2
Ever employed	.260		
Has a job waiting at the end of training	.085		
Plans to look for wage employment	.629		
Ideal firm size is 20 employees or less	.605		
B. Sector of training			
Carpentry	.072		
Catering	.129		
Hairdressing	.266		
Motormechanics	.292		
Tailoring	.179		
Welding	.062		

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 in time for the matching intervention.

Table 3: Impacts on manager beliefs about matched workers

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

Dependent variable:	Assessment of worker skills by owner (standardized index)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(i) Treatment	.117	.130	-.041	-.007	-.047	-.062	-.197	-.193
	(.084)	(.089)	(.121)	(.120)	(.104)	(.105)	(.152)	(.148)
(ii) Average worker soft skills grade (standardized)	-.091	-.084						
	(.063)	(.063)						
(iii) Average worker soft skills grade (standardized) X Treatment	.264***	.250***						
	(.097)	(.095)						
(iv) Worker has Pass grades on all soft skills			-.086	-.081	.133	.118	.040	.057
			(.134)	(.134)	(.098)	(.098)	(.168)	(.164)
(v) Worker has Pass grades on all soft skills X Treatment			.414**	.360**			.390*	.340
			(.189)	(.183)			(.233)	(.224)
(vi) Owner is high ability					-.298**	-.308**	-.156	-.148
					(.146)	(.138)	(.174)	(.171)
(vii) Owner is high ability X Treatment					.570***	.621***	.522**	.567***
					(.174)	(.167)	(.217)	(.216)
(viii) Worker has Pass grades on all soft skills X Owner is high ability X Treatment							.083	.092
							(.339)	(.344)
Mean of dep. var. in Control group	.000	.000	.000	.000	.000	.000	.000	.000
Worker and Firm Controls	Yes	No	Yes	No	Yes	No	Yes	No
p-value from test: (i) + (v) = 0			[.005]	[.009]				
p-value from test: (i) + (vii) = 0					[.000]	[.000]		
Number of observations (matches)	515	515	515	515	515	515	515	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 Cameron et al. (2011). Dependent variable: standardized index of the following variables: dummy equal to one if the worker was reported by the owner as more skilled than usual applicants, and equal to zero otherwise; dummy equal to zero if the worker was reported as less skilled than usual applicants, and equal to one otherwise; dummy equal to one if there was anything that the owner particularly liked about the worker, and equal to zero otherwise; dummy equal to zero if there was anything that the owner particularly did not like about the worker, and equal to one otherwise. The index is constructed by converting each component 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. All regressions control for stratification variables (dummies for BRAC branch and sector) and for dummies for month of interview. The regressions in the odd columns also control for the following baseline worker characteristics: age and age squared; female dummy; years of education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. The odd columns also control for the following baseline firm characteristics: female owner dummy; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees. We use two measures of worker skills. The first, used in columns 1-2, is the average grade (standardized) on the five soft skills measured in the baseline assessments. The second, used in the rest of the table, is a dummy for whether the worker had a pass grade (C or above) on all five soft skills. To proxy for owner ability we use the first principal component of the following skills measured at baseline: cognitive skills, years of education and the Big 5. Firm owners with a value of the first principal component on or above the median are assigned to the high ability group. Each regression also controls for the variable by which heterogeneous effects are considered, uninteracted with Treatment. All regressions further control for dummies for missing values in each of the independent variables.

Table 4: Impacts on worker labor market expectations and outside options

OLS coefficients; standard errors in parentheses are robust in columns 3, 4 and 7, and clustered at the worker level in the other columns

	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	Any casual work in the last week	Attended further education or training in the last year	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)	(8)
Treatment	7.90**	.280**	.104	.068**	-.048*	.038*	.105***	-.018
	(3.13)	(.116)	(.063)	(.033)	(.026)	(.020)	(.036)	(.025)
Average soft skills grade (standardized)	-.442	.126	-.028	.024	.018	.019	.018	.008
	(1.98)	(.083)	(.045)	(.023)	(.019)	(.012)	(.025)	(.019)
Average soft skills grade (standardized)	2.85	-.039	.064	-.010	-.027	-.016	-.038	-.012
X Treatment	(3.11)	(.117)	(.063)	(.033)	(.026)	(.019)	(.035)	(.026)
Mean of dep. var. in Control	114.7	5.53	-.001	.624	.323	.118	.268	.749
Controls for baseline value of outcome	Yes	Yes	Yes	No	Yes	No	No	Yes
Uses data from first followup	Yes	Yes	No	No	Yes	Yes	No	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,330	1,349	666	668	1,350	1,348	674	1,350

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-ups are reported. The outcome variables in columns 3, 4, and 7 are available only at second follow-up, which explains why the number of observations is lower in those columns. All regressions control for stratification variables (dummies for BRAC branch and sector) and dummies for month of interview. The regressions in columns 1, 2, 5, 6 and 8 further control for a dummy for second follow-up. In addition, all regressions control for the following baseline worker characteristics: age and age squared; female dummy; years of 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 and maximum expected earnings; (ii) the probability that they could earn at least the midpoint. We use this information to fit a triangular probability distribution of expected earnings. 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 second follow-up: 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 negotiation (0-10 scale). The index is constructed by converting each component 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 (i.e., at least 10 workers). Since at baseline all workers were enrolled in vocational training and only 1% of them were currently doing 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 at baseline. So we control for this variable in column 5. In column 6 the dependent variable is a dummy equal to one if the worker attended further formal education or vocational training in the year prior to the survey. All regressions further control for dummies for missing values in each of the independent variables.

Table 5: Impacts on worker beliefs about own skills, returns to skills and signaling

OLS coefficients; robust standard errors in parentheses

	Average self-assessed soft skills grade (1 to 5 scale) (1)	Average perceived returns to soft skills (0 to 10 scale) (2)	Average perceived constraints in signaling skills (1 to 5 scale) (3)
Treatment	.019	-.004	-.185**
	(.033)	(.106)	(.087)
Average soft skills grade (standardized)	.059**	.036	.061
	(.024)	(.075)	(.065)
Average soft skills grade (standardized) X Treatment	-.040	-.006	.098
	(.032)	(.106)	(.088)
Mean of dep. var. in Control group	4.35	8.06	2.73
Controls for baseline value of outcome	No	No	Yes
Uses data from first followup	No	No	No
Uses data from second followup	Yes	Yes	Yes
Number of observations	673	673	673

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-ups are reported. The outcome variables in this table are available only at second follow-up. All regressions control for stratification variables (dummies for BRAC branch and sector) and dummies for month of interview. In addition, all regressions control for the following baseline worker characteristics: age and age squared; female dummy; years of 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 column 1 is constructed as follows: workers were asked to self-assess themselves on the five soft skills measured in the baseline assessments and disclosed on the certificates, using the same A-E scale. We compute the average self-assessed grade on the five skills. The dependent variable in column 2 is constructed as follows: workers were asked how important each of the five soft skills disclosed on the certificates were for making someone a productive worker, using a 0-10 scale. We compute the average perceived return to the five skills. The dependent variable in column 3 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. Workers were asked this question separately for practical and soft skills. Note that this question was asked about soft skills in general. 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 3. All regressions further control for dummies for missing values in each of the independent variables.

Table 6: Impacts on sorting between worker soft skills and manager ability

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

Dependent variable:	Worker was hired by the matched firm			
	(1)	(2)	(3)	(4)
(i) Treatment	-.001	.009	-.047	-.047
	(.034)	(.041)	(.041)	(.054)
(ii) Average worker soft skills grade (standardized)	-.004			
	(.025)			
(iii) Average worker soft skills grade (standardized) X Treatment	-.025			
	(.035)			
(iv) Worker has Pass grades on all soft skills		-.018	-.036	-.050
		(.042)	(.032)	(.048)
(v) Worker has Pass grades on all soft skills X Treatment		-.025		-.001
		(.064)		(.073)
(vi) Owner is high ability			-.066	-.102*
			(.048)	(.059)
(vii) Owner is high ability X Treatment			.133**	.166**
			(.065)	(.081)
(viii) Worker has Pass grades on all soft skills X Owner is high ability X Treatment				-.076
				(.135)
Mean of dep. var. in Control group	.116	.116	.116	.116
Worker and Firm Controls	Yes	Yes	Yes	Yes
p-value from test: (i) + (v) = 0		[.766]		
p-value from test: (i) + (vii) = 0			[.102]	
Number of observations (matches)	412	412	412	412

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker first follow-up are reported. The number of observations is lower than in Table 3 due to attrition. Standard errors are adjusted for two-way clustering (at the level of both the firm and the worker), following Cameron et al. (2011). All regressions control for stratification variables (dummies for BRAC branch and sector) and dummies for month of interview. In addition, all regressions control for the following baseline worker characteristics: age and age squared; female dummy; years of 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 baseline firm characteristics: female owner dummy; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees. We use two measures of worker skills. The first, used in column 1, is the average grade (standardized) on the five soft skills measured in the baseline assessments. The second, used in the rest of the table, is a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments. To proxy for owner ability we use the first principal component of the following skills measured at baseline: cognitive skills, years of education and the Big Five. Firm owners with a value of the first principal component on or above the median are assigned to the high ability group. Each regression also controls for the variable by which heterogeneous effects are considered, uninteracted with Treatment. All regressions further control for dummies for missing values in each of the independent variables.

Table 7: Impacts on sorting between specific soft skills and manager needs

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

Dependent variable:	Worker was hired by the matched firm			
Worker skill:	Communication skills	Willingness to help others	Communication skills and willingness to help others	Communication skills
Firm characteristic:	Number of employees	Number of employees	Number of employees	Number of customers per worker
	(1)	(2)	(3)	(4)
(i) Treatment	.109*	.105	.106*	-.040
	(.066)	(.068)	(.062)	(.079)
(ii) Worker has Pass grade on skill	.019	.027	.055	.020
	(.056)	(.050)	(.050)	(.067)
(iii) Worker has Pass grade on skill X Treatment	-.152**	-.151*	-.162**	-.067
	(.076)	(.079)	(.075)	(.093)
(iv) Firm characteristic above median	.124	.075	.113*	.056
	(.084)	(.066)	(.068)	(.100)
(v) Firm characteristic above median X Treatment	-.214**	-.064	-.145	-.005
	(.102)	(.092)	(.090)	(.123)
(vi) Worker has Pass grade on skill X	.332***	.107	.248**	.273**
Firm characteristic above median X Treatment	(.121)	(.115)	(.116)	(.136)
Mean of dep. var. in Control group	.116	.116	.116	.116
Worker and Firm Controls	Yes	Yes	Yes	Yes
Number of observations (matches)	412	412	412	412

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker first follow-up are reported. The number of observations is lower than in Table 3 due to attrition. Standard errors are adjusted for two-way clustering (at the level of both the firm and the worker), following Cameron et al. (2011). A Pass grade is C or above. The median grade is C for all skills considered in this table. The Firm characteristics used in these regressions are defined as follows: Number of workers: number of employees at baseline; Number of customers per worker: number of customers that placed orders at the business in the seven days prior to the baseline, divided by number of employees. All regressions control for stratification variables (dummies for BRAC branch and sector) and dummies for month of interview. In addition, all regressions control for the following baseline worker characteristics: age and age squared; female dummy; years of education; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. In addition, all regressions include dummies for whether the worker scored on the median or above on each of the five soft skills measured in the baseline assessments. All regressions also control for the following baseline firm characteristics: female owner dummy; age and age squared of the owner; dummy for whether owner attended a VTI in the past; number of employees. Each regression also controls for the variable by which heterogeneous effects are considered, uninteracted with Treatment. All regressions further control for dummies for missing values in each of the independent variables.

Table 8: Impacts on employment and engagement in productive activities

OLS coefficients; standard errors in parentheses are clustered at the worker level

Dependent variable:	Any work as wage employee in the last week	Any work as self-employed in the last week	Main activity in last week is non-casual employment or education/training	Weekly hours worked in last job as employee		Weekly hours worked in last job as self-employed	
Sample of workers	All	All	All	All	Employed	All	Self-employed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	.029	-.002	.048*	1.91	2.18	-1.08	-1.17
	(.029)	(.024)	(.028)	(1.95)	(1.54)	(1.61)	(2.99)
Average soft skills grade (standardized)	-.020	-.012	-.015	-.183	-.155	-1.23	1.03
	(.021)	(.016)	(.020)	(1.49)	(1.16)	(1.11)	(2.36)
Average soft skills grade (standardized)	.036	-.010	.032	2.72	1.35	-1.29	-3.89
X Treatment	(.030)	(.023)	(.029)	(2.04)	(1.59)	(1.57)	(3.12)
Mean of dep. var. in Control	.428	.211	.600	37.0	61.1	16.0	51.9
Controls for baseline value of outcome	Yes	Yes	Yes	No	No	No	No
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	1,350	1,350	1,350	1,347	816	1,349	411

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-ups are reported. All regressions control for stratification variables (dummies for BRAC branch and sector), a dummy for second follow-up and dummies for month of interview. In addition, all regressions control for the following baseline worker characteristics: age and age squared; female dummy; years of 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 in vocational training 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 at baseline. So we control for this variable in column 1 and 2. All regressions further control for dummies for missing values in each of the independent variables.

Table 9: Impacts on labor market earnings

OLS coefficients in columns 1-2 and 7; quantile regression coefficients in columns 3-6

Standard errors clustered at the worker level in parentheses in columns 1-2 and 7

Bootstrapped standard errors in parentheses in columns 3-6

Dependent variable:	Any paid work in the last month	Total earnings in the last month (USD)					
Specification:	OLS	OLS	Q(50)	Q(25)	Q(75)	Q(90)	OLS, conditional on any paid work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-.014	3.72	1.27	.398	7.71*	12.9**	7.10**
	(.025)	(3.20)	(3.26)	(1.27)	(4.10)	(6.25)	(3.56)
Average soft skills grade (standardized)	-.013	-.866	-1.48	-.085	-1.55	-5.40	-.654
	(.018)	(1.95)	(2.06)	(1.06)	(2.67)	(4.01)	(2.08)
Average soft skills grade (standardized) X Treatment	.010	3.03	1.48	.097	3.46	11.0*	3.78
	(.025)	(3.10)	(3.06)	(1.34)	(3.71)	(6.62)	(3.45)
Mean of dep. var. in Control group	.750	47.2	47.2	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	1,329	1,329	1,329	1,329	988

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-ups are reported. In the quantile regressions standard errors are bootstrapped (with 600 replications). All regressions control for stratification variables (dummies for BRAC branch and sector), a dummy for second follow-up and dummies for month of interview. In addition, all regressions control for the following baseline worker characteristics: age and age squared; female dummy; years of 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 for whether the worker conducted any paid work in the month prior to survey. In columns 2-7 the dependent variable is total labor earnings in the month prior to the survey. This variable is set to zero for workers with no labor earnings. Since at baseline all workers were enrolled in vocational training 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 at baseline. So we control for this variable in column 1. Similarly, in columns 2-7 we consider as baseline value of the outcome expected earnings at baseline. 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 2-7. All regressions further control for dummies for missing values in each of the independent variables.

Figure 1: Timeline

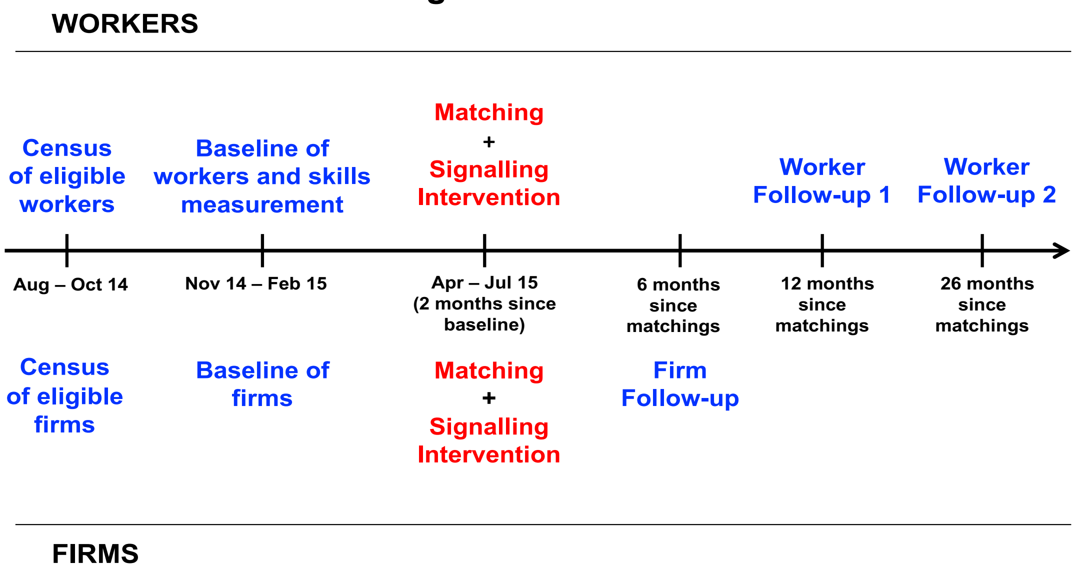
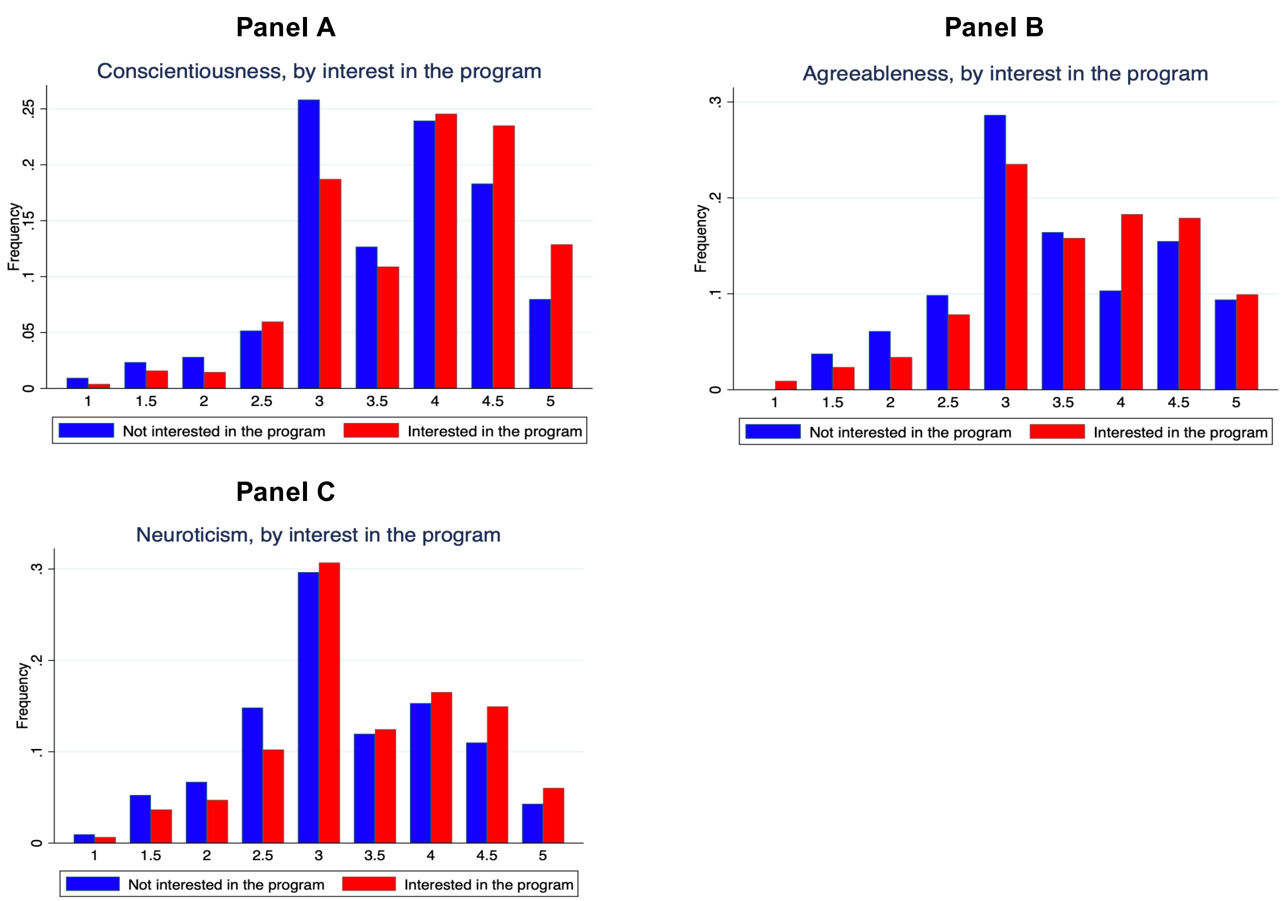


Figure 2: Distribution of worker soft skills, by participation in the experiment



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 1,011 trainees eligible for the intervention.

Figure 3: Summary of experimental design

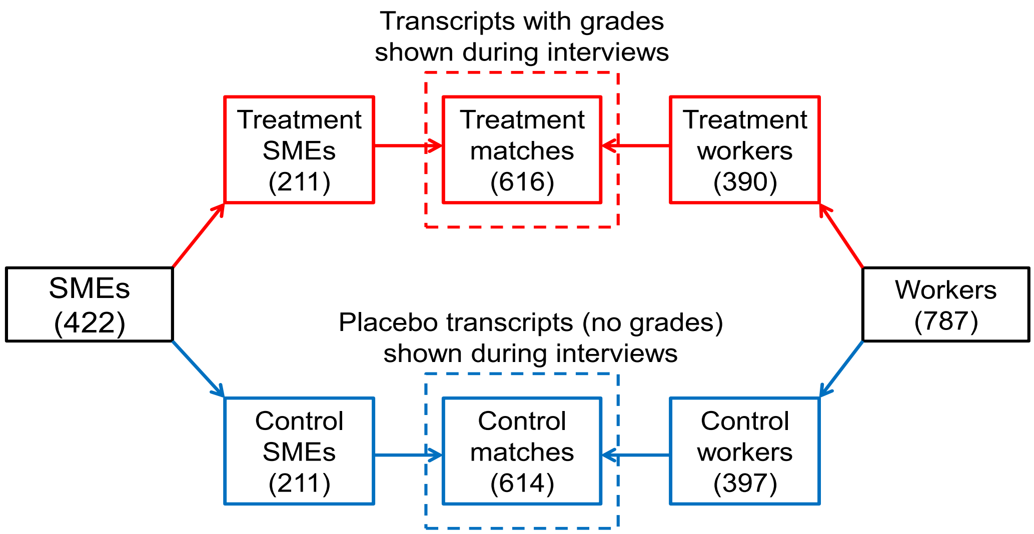


Figure 4: Compliance and attrition

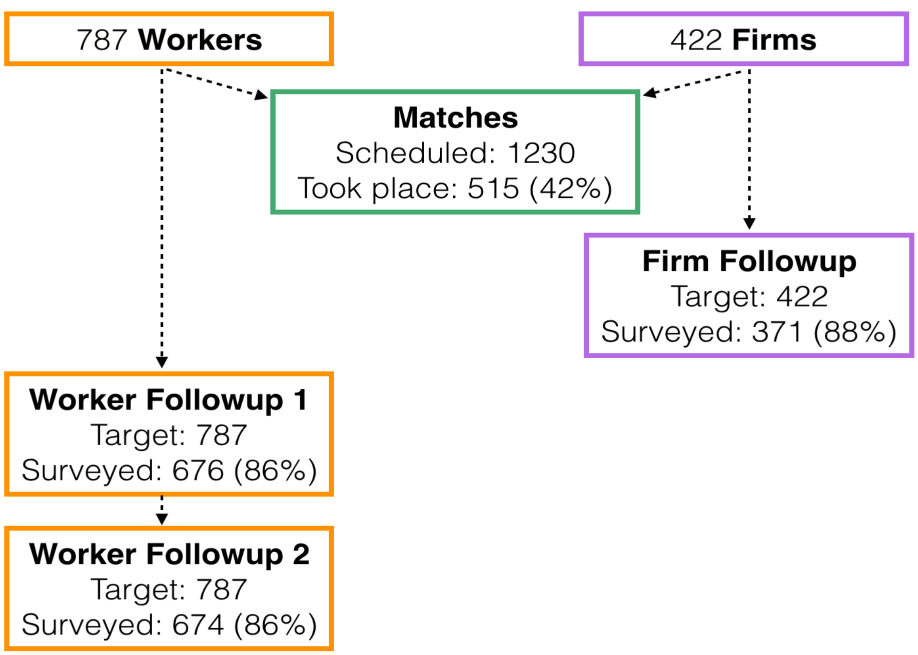
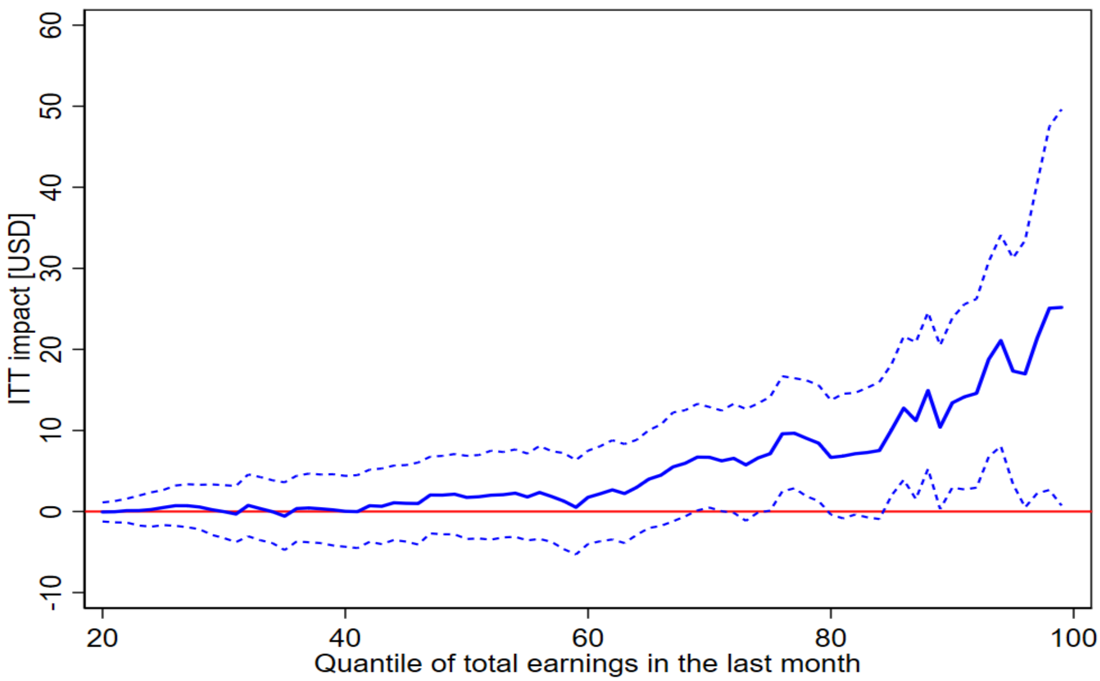


Figure 5: Quantile treatment effects on labor market earnings



Notes: The figure reports quantile regression estimates of treatment effects on total labor earnings in the month prior to the survey, with 90% confidence intervals. Standard errors are bootstrapped (with 600 replications). 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 baseline worker characteristics: 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 certificates; age and age squared; female dummy; years of 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. 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.

Online Appendix

A Worker Selection into the Experiment: Additional Details

This section compares our experimental sample of workers to two other representative worker surveys in Uganda, to provide further support to the claim that our sample is positively selected also relative to the underlying worker population.

Comparison to Bassi et al. (2021) sample In Table A1.2 we compare our experimental sample to the sample of workers in small scale manufacturing described in Bassi et al. (2021). The Bassi et al. (2021) survey covered a representative sample of about 1,000 firms and their employees operating in carpentry, welding and grain milling (note that two of these three sectors overlap with sectors in our study). Importantly, the survey in Bassi et al. (2021) collected the soft skills of every worker, using exactly the same Big Five scale that we used in our paper. We compare our worker sample to four sub-samples of workers in Bassi et al. (2021): (i) those in a similar age range as our sample; (ii) those in a similar age range and operating in carpentry and welding, the two overlapping sectors; (iii) those in a similar age range and who received vocational training; (iv) those in a similar age range who received vocational training and who work in carpentry and welding.[66]

We find that our sample is positively selected on soft skills with respect to all these four groups, and in particular with respect to those workers who also received vocational training: as all the workers in our experiment received vocational training, this might be the more natural comparison group. For instance, while 21% of the workers in Bassi et al. (2021) who previously received vocational training had a score of 3 or above on all Big Five, the same share is 33% in our sample. This is more than a 50% increase and is significant at the 1% level. We reach similar conclusions if we compare these samples in terms of years of education rather than soft skills. We acknowledge that this comparison is not perfect as the two surveys were conducted at different times and by different survey teams. Still, the fact that the tool used to measure the Big Five traits was exactly the same across the two studies gives us confidence that this comparison is meaningful. Finally, note that our baseline was conducted in 2014, while the survey in Bassi et al. (2021) was conducted in 2018-19, so to the extent that there are cohort effects in education/skills, our comparison likely underestimates the true extent of the positive selection.

Taken together, the analysis in Table A1.2 indicates that our sample of experimental workers

[66]Note that restricting the sample of employees to those in the same age range as our study is important as Srivastava et al. (2003) show evidence consistent with soft skills increasing over the life cycle, even past age 30.

are positively selected not only within the workers eligible for the experiment, but also with respect to typical young employees in similar sectors. The fact that the selection on soft skills appears to be stronger with respect to employees with previous vocational training is particularly revealing: if managers were expecting our sample of VTI graduates to have a skill distribution similar to that of typical employees with prior vocational training, this might then result in the certificates providing mostly positive news to managers about the skills of the matched workers.

Comparison to Uganda National Household Survey In Table A1.3, we then compare our experimental sample to workers in the 2012/13 Ugandan National Household Survey (UBOS, 2014). This is a representative household survey conducted by the Ugandan Bureau of Statistics. The 2012/13 survey covered over 36,000 individuals in almost 7,000 households. We restrict the UNHS sample to youth that are labor market active, defined as either working or actively seeking employment. Comparing the first and second rows of the table shows that our experimental workers: (i) have substantially higher education (10.4 vs. 6.8 years); and (ii) come from much wealthier households, as indicated by the various measures of asset ownership. This reinforces the view that our workers are positively selected relative to the underlying population of job-seekers more broadly. In the third row of the table, we further restrict the UNHS sample to those youth who received vocational training. Comparing the first and third rows shows that our experimental sample is positively selected also relative to the representative vocational training graduate, both in terms of education and household assets. Note that this is the case even though the UNHS sample of vocational trainees are themselves more educated and wealthier than the representative youth in Uganda, which can be seen by comparing the second and third rows in the table. This provides additional evidence that our sample is positively selected also relative to the underlying population of vocational trainees, and so is consistent with the results in Table A1.2.

B Key Facts about SMEs: Additional Details

In this Section we describe in more detail the four key facts presented in Section 2.3. 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 how important different skills are in their firms. Figure A1 reports the average importance of each skill.[67] 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.

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

The second key fact is that firm owners report difficulties in observing the soft skills of workers and theft by their own employees among their main perceived constraints. Owners were asked to rate the importance of a range of potential constraints on a 1 to 5 scale. 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 weak institutional environment in Uganda severely limits the ability of firm owners to prosecute workers who misbehave on the job, creating *de facto* limited liability for workers. In turn, this can explain the importance given by managers to constraints related to screening on soft 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. This result is in line with the findings in Caria and Falco (2020), who document that firm owners in Ethiopia tend to underestimate the trustworthiness of their employees. Managers were also asked a number of questions about each of the 15 partner VTIs in our study. We use these to show that firm owners are not familiar with the VTIs in our sample and have low expectations on the soft skills of their graduates. Summary statistics from these questions are reported in Table A6.2. Overall we find that managers are not familiar with these VTIs: only in 15% of cases the owner has heard before about the VTIs in our sample, and, conditional on knowing our partner VTIs, only in 11% of cases they have hired someone who previously attended these VTIs. This points to owners having limited information about these VTIs and being uncertain about the skills of their graduates. Conditional on knowing a VTI, firm owners were then asked if they thought workers from that VTI were particularly good in some specific skills. Table A6.2 shows that soft skills were indicated as answer to this question only in 6% of cases. This together with Figure A3 further supports the claim that managers have low priors on the soft skills of workers.

Finally, the fourth key fact is that it is common for firms to recruit workers that just walk up to the firm 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. So while referrals are common – which might in part attenuate screening problems – there is still substantial scope for information frictions at recruitment to play a role, as managers frequently hire workers with no prior connection to the firm.

C A Search Model with Asymmetric Information on the Skills of Workers and Two-Sided Updating

C.1 Setup

Production There are two types of workers and two types of firms, which we will refer to as h and l, and H and L, respectively. Workers differ in their soft skills: type-h workers have higher soft skills than type-l workers. Firms differ in their production function. Each firm only hires one worker, who inelastically supplies one unit of labor. This implies that firms only differ in terms of productivity, which we allow to be match specific. A firm of type H produces surplus equal to $y_H^h = a$ when matched to a type-h worker, and *negative* surplus equal to $y_H^l = -d < 0$ when matched to a worker of type l. On the contrary, workers are equally productive at firms of type L, and they generate surplus equal to $y_L^h = y_L^l = b$, such that $a > b > 0 > -d$. This modelling choice is motivated by the analysis in Table A6.1, which suggests that soft skills have higher returns when matched to high ability managers, and therefore that type-h workers are more productive at these firms. This set up is also consistent with the fact that high ability managers employ more skilled workers at baseline and are better able to delegate, as shown again in Table A6.1: because high ability managers are less likely to supervise workers, workers with low soft skills can generate a *loss* to their firms, for instance through stealing or upsetting customers.[68] Because low ability managers are less able to delegate, they engage in more supervision, and are not hurt by workers with low soft skills. This is consistent with the fact that they employ less skilled workers at baseline. For ease of exposition, we assume that each worker type represents half of the worker population. Note that given the production functions specified above, productivity and surplus in the economy are maximised when type-h workers match with type-H firms, and when type-l workers match with type-L firms. In this sense, the efficient allocation exhibits positive assortative matching on skills/ability.

Search process and wages We build on the simplest search model with random search and chance of an offer (McCall, 1970). Each period unemployed workers face a chance of receiving a job offer from either type of firm. In line with the empirical results so far, we assume asymmetric information on the skills of workers, so that the probability of an offer depends on the *expected* productivity of the worker at the matched firm. For simplicity, we assume that, if an offer is made, the offered wage corresponds to half of the expected surplus generated by the match.

[68] In line with the informal nature of labor markets in Uganda, we assume that managers cannot sue employees for losses, so that employees have *de facto* limited liability. An alternative modelling approach would be to assume that type-l workers also produce positive surplus at firms of type H (i.e., $y_H^l > 0$), but that binding minimum wages are such that type-l workers still create a net loss to the firm. This alternative approach would generate the same predictions. Kaur (2019) shows that fairness norms can create significant downward wage rigidities in developing countries, even in the absence of formal labor market institutions.

There are no separations, and workers discount the future at rate $\beta < 1$.[69]

The surplus from a match with a type-L firm is unrelated to the skills of the worker, and the probability of meeting and receiving a wage offer from these firms, defined as p_L, is the same for all workers.[70] The per-period wage offered by firms of type L is simply half the surplus, i.e.:

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

On the other hand, because production at type-H firms depends on the worker type, these firms try to identify the high-skilled types through job interviews. Job interviews generate a signal σ on skills, which can be *Good* (G) or *Bad* (B). We let q denote the probability that the signal is correct (e.g., the probability that a type-h worker sends a G signal). The closer q is to 1, the more informative signals are. We assume that in the baseline environment the value of q is such that $q = P(G|h) = P(B|l) \in (\frac{1}{2}, 1)$, namely that signals are somewhat informative of the worker type, but there is asymmetric information on skills. This assumption is motivated by our descriptive evidence that firms report problems in screening workers' soft skills as important. Also, this is in line with our evidence from Panel A of Appendix Figure A8, which shows a relatively flat relationship between worker skills and managers' assessments in the control group.

Given a signal σ, firms compute the posterior probability of the worker type, using Bayes' rule.[71] Since we assumed that the share of each worker type is $\frac{1}{2}$, q is also the posterior probability, that is, $q = P(h|G) = P(l|B)$. With this in mind, type-H firms compute the expected productivity of the worker as

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

We assume that the expected output from the match is negative in the case of a B signal, namely that $E[y_H|B] < 0$, so that firms of type H do not make a job offer if they see a B signal. This implies that the only wage offer these firms make is equal to:[72]

[69] The assumption of no separations implies that in the long-run the unemployment rate is zero. This is consistent with evidence that the unemployment rate is very low in Sub-Saharan Africa. For instance, Rud and Trapeznikova (2020) calculate a 1% unemployment rate for Uganda. Since everyone is employed in the long-run, the comparative static result on overall employment described later in this section is to be interpreted in the short run. In the data we observe workers for two years after the intervention, and so this justifies why we focus on short-run predictions and do not model separations.

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

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

[72] Assuming that $E[y_H|B] > 0$ but that binding minimum wages result in an expected loss for type-H firms would yield equivalent predictions. Also, assuming that firms of type H pay two wages, one in the case of a G signal, and one in the case of a B signal, would complicate the analysis but not alter the main predictions.

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

We define the probability of meeting and receiving an offer from a type-H firm as $p_H(k)$, with $k \in \{h, l\}$. This probability is positive only for workers who send a G signal, and is a function of the worker type.[73] Finally, we let the parameters be such that $w_H > w_L$ in the baseline environment, which is consistent with type-H firms paying higher wages.[74]

C.2 Worker Problem

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

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

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

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

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

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

Worker behaviour in this model crucially depends on: (i) the relative probabilities of job offer at the two types of firm (i.e., p_L vs $p_H(k)$), and (ii) the relative wage (i.e., w_H vs w_L). Note that while p_L and w_L are deterministic, both $p_H(k)$ and w_H depend on q. In particular, the higher is q, the higher is $p_H(h)$, the lower is $p_H(l)$ and the higher is w_H. Therefore, asymmetric information on skills, which is modelled as a value of q lower than one, creates mismatch and wage compression: if workers of type h are not perfectly able to show their skills, this reduces the probability that they receive an offer at firms of type H, and the wage they can earn there.

[73] Specifically, $p_H(h)$ is an increasing function of q, while $p_H(l)$ is a decreasing function of q.

[74] In Appendix Table A6.1 we showed that higher ability managers are more profitable, and there is an extensive literature linking firm productivity and wages (see, for instance, Card et al. (2018)). Bassi et al. (2021) show that higher ability managers pay higher wages in a representative sample of manufacturing firms in Uganda.

At the same time, this increases the probability that they accept an offer from a type-L firm, since their outside option from rejecting such offer and continuing to search is reduced. Also, type-l workers have a chance of being employed at type-H firms, since these firms are not perfectly able to screen them out, which further increases mismatch and reduces output.

C.3 Implications

Given the treatment effects on beliefs documented in Section 4, we interpret the treatment as a credible increase in the precision of the signal, so that $q(T) = P(h|G,T) = P(l|B,T) = 1$. This generates the following comparative statics results with respect to the introduction of the certificates:

Implication 1. *There is an increase in positive assortative matching: type-h workers are more likely to be employed at type-H firms, and type-l workers are more likely to be employed at type-L firms.* As the signal becomes precise, type-H firms only make offers to high-skilled workers. The certificates also lead to an increase in the wage offered by firms of type H, through an increase in q.[75] Therefore, workers of type h become more likely to accept an offer from type-H firms, and to reject an offer from type-L firms.[76] At the same time, because workers of type l do not receive offers from high-ability firms, they become more likely to accept an offer from type-L firms, since their outside option is only unemployment. In short, both firm and worker behaviour leads to an increase in sorting and to a reduction in mismatch.

Implication 2. *The change in the overall employment probability is ambiguous for both worker types.* While workers of type h are more likely to accept offers from firms of type H, they are also more likely to *rejec*t offers (or to not receive an offer) from type-L firms. Similarly, workers of type l are less likely to be employed at firms of type H, but more likely to accept offers at firms of type L. Therefore, the treatment effect on the overall employment probability of both worker types is ambiguous as workers reallocate across firms.

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

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

[76] If the cost of making an offer is significant, the signal might also result in a decrease in the probability that type-L firms make an offer to type-h workers, since they know these workers are now less likely to accept.

D Cost-Benefit Analysis for Program Participants

Table A11 reports the cost-benefit analysis for program participants, that is, for those eligible workers who accepted to participate in the signaling and matching intervention. We have detailed information on program costs, as all activities were implemented by BRAC. 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. 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.[77]

For the benefits we consider the estimated earnings gains in the two years post intervention. Specifically, we use the estimate of β_1 from column 2 of Table 9, which corresponds to the earnings impact for the average worker, estimated on the full sample of workers.[78] 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 \$72.57, 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.

[77] These calculations do not account for the value of teachers' time in filling in the teacher surveys. These time costs are likely low as the median teacher rated 13 students, and did so only once.

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

Table A1.1: Worker selection into the experiment
OLS coefficients; robust standard errors in parentheses

Dependent variable:	Worker included in the experimental sample		
	(1)	(2)	(3)
A. Skills (standardized)			
Cognitive test score	.022	-.002	.004
	(.014)	(.014)	(.014)
Extraversion	.030*	.023	
	(.016)	(.016)	
Agreeableness	.033*	.029*	
	(.017)	(.017)	
Conscientiousness	.036*	.037**	
	(.018)	(.018)	
Neuroticism (reversed scale)	.033**	.034**	
	(.017)	(.016)	
Openness to Experience	-.019	-.005	
	(.016)	(.016)	
Score of 3 or higher on all Big Five			.069**
			(.027)
B. Other worker characteristics			
Age		.010	.024
		(.036)	(.037)
Age squared		-.000	-.001
		(.001)	(.001)
Female		-.160***	-.173***
		(.050)	(.046)
Completed prior education (Years)		.008	.007
		(.008)	(.008)
Course duration		.009	.012
		(.017)	(.017)
Ever employed		-.055*	-.063**
		(.031)	(.030)
Mean of dependent variable	.775	.775	.775
Sector of training dummies	No	Yes	Yes
p-value from F-test of joint significance of Big 5 variables	[.000]	[.002]	
p-value from F-test of joint significance of sector dummies		[.004]	[.005]
R-squared	.045	.101	.085
Number of observations (workers)	1,011	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. The regressions in column 2 and 3 additionally control for five 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-Five 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). Both the cognitive score and all the Big Five variables are standardized. All regressions further control for dummies for missing values in each of the independent variables.

Table A1.2: Comparison of experimental worker sample to typical employees

Means; *p*-values from test of equality of means with experimental sample in brackets

	% who scored 3 or above on all Big Five	Years of education
	(1)	(2)
(i) Workers in the study sample who selected into the experiment	**33.3%**	**10.4**
(ii) Workers in the study sample who selected out of the experiment	24.1%	9.96
***p*-value (ii) = (i)**	[.010]	[.005]
(iii) All workers in the study sample	31.4%	10.3
(iv) Workers in similar age range in Bassi et al. (2021) sample	29.5%	9.36
***p*-value (iv) = (i)**	[.090]	[.000]
(v) Workers in similar age range in Bassi et al. (2021) sample, restricted to carpentry and welding sectors	27.0%	9.54
***p*-value (v) = (i)**	[.007]	[.000]
(vi) Workers in similar age range and with previous VT in Bassi et al. (2021) sample	21.3%	10.1
***p*-value (vi) = (i)**	[.007]	[.235]
(vii) Workers in similar age range and with previous VT in Bassi et al. (2021) sample, restricted to carpentry and welding sectors	17.2%	10.1
***p*-value (vii) = (i)**	[.000]	[.221]

Notes: The first three rows use data from the initial census of trainees for the job placement intervention. In the first row the sample includes those trainees who self-selected into the final sample for the experiment. In the second row it is similited to those trainees self-selected out of the intervention. In the third row it includes the entire sample of trainees eligible for the intervention. The sample in the fourth to seventh rows is taken from Bassi et. al (2021), who surveyed a representative sample of firms (and their employees) in carpentry, welding and grain milling in urban areas of Uganda. We limit the sample of employees in Bassi et al. (2021) to those aged 28 and below, as 28 years is the 99th percentile of the age distribution in our sample of workers. In the fourth row the sample includes all such 1,881 workers in the Bassi et al (2021) sample. In the fifth row the sample is limited to the 1,567 workers in carpentry and welding, that are the two overlapping sectors with our study. In the sixth row we limit the sample of employees to the 154 ones with previous vocational training. Finally, in the seventh row we limit the sample of employees to those 144 ones with previous vocational training and employed in carpentry or welding firms. In brackets we report the p-value from the test of equality of means with the sample in the first row. Extraversion, Agreeableness, Conscientiousness, Neuroticism and Openness to Experience are measured through a 10-item Big-Five 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).

Table A1.3: Comparison of experimental worker sample to average youth in the Ugandan labour force

Means; standard deviations in parentheses

	Age	Female	Married	Years of education	Ever attended vocational training	Main construction material of roof is thatch	Anyone in the household owns: Land	TV	Refrigerator	Mobile phone	Computer	Motor-cycle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(i) Workers 18-25 in the experimental sample	20.5	.486	.021	10.5	1.00	.128	.830	.456	.210	.990	.140	.237
	(1.84)			(1.71)								
(ii) Youth 18-25 and labor market active in 2012/13 UNHS	21.4	.525	.448	6.83	.064	.345	.767	.099	.026	.653	.018	.074
	(2.33)			(3.36)								
(iii) Youth 18-25, labor market active and who received vocational training in 2012/13 UNHS	22.4	.528	.376	9.84	1.00	.177	.655	.247	.089	.891	.057	.101
	(2.03)			(3.00)								

Notes: The first row uses data from the worker baseline survey, and includes only workers 18-25 who decided to participate in the intervention. This sample includes 701 workers. In the second row, we compare the sample of trainees in the experiment to the typical youth in the Uganda labor force. To do so, we use the 2012/13 Ugandan National Household Survey (UNHS) published by the Ugandan Bureau of Statistics. We restrict the sample to individuals 18-25 who are labor market active, that is, who are either working or currently actively seeking employment. This sample includes 3,456 workers. In the third row, we restrict the UNHS sample to those between 18-25 who are labor market active and who received vocational training. This sample includes 207 workers. Observations in the UNHS data are weighted using household weights.

Table A2.1: 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 A2.2: Correlation between Big Five traits and other soft skills
OLS 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	.006	-.007	-.032	.043	-.035
	(.034)	(.033)	(.041)	(.039)	(.043)
Openness to Experience	.008	-.000	-.004	-.001	.057
	(.043)	(.042)	(.049)	(.046)	(.055)
Number of observations	724	724	724	724	724

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. 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. All regressions further control for dummies for VTI attended and sector of training (as the skills measurements were conducted by different raters across sectors of training and across VTI).

Table A3: Minncerian returns to soft skills in the control group

OLS coefficients; standard errors clustered at the worker level in parentheses

Dependent variable:	Any paid work in the last month		Any work as wage employee in the last week		Total earnings in the last month (USD)	
Sample of workers:	**Control group**		**Control group**		**Control group**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Pass grade on all soft skills	-.018	.020	-.046	-.062	9.47*	9.00*
	(.036)	(.037)	(.040)	(.041)	(4.93)	(4.81)
Cognitive test score (standardized)	.011	.003	.036*	.015	8.78***	3.89*
	(.018)	(.020)	(.019)	(.022)	(2.45)	(2.34)
Completed prior education (Years)		.012		.034**		3.55***
		(.012)		(.017)		(1.24)
Course duration (Years)		-.039		.048*		-.032
		(.024)		(.029)		(3.02)
Ever employed		.059		.066		10.7*
		(.046)		(.054)		(6.45)
Age		.128		.002		6.60
		(.085)		(.085)		(9.00)
Age squared		-.003		-.000		-.174
		(.002)		(.002)		(.215)
Female		-.114		-.063		-16.1
		(.070)		(.093)		(10.5)
Mean of dependent variable in Control	.750	.750	.428	.428	47.2	47.2
***p*-value from F-test of joint significance of sector dummies**	-	[.309]	-	[.086]	-	[.329]
***p*-value from F-test of joint significance of BRAC branch dummies**	-	[.072]	-	[.670]	-	[.026]
Uses data from first followup	Yes	Yes	Yes	Yes	Yes	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.038	.092	.013	.071	.075	.199
Number of observations	663	663	668	668	657	657

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. 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. The regressions in columns 2, 4 and 6 further control for stratification variables (dummies for BRAC branch and sector) and dummies for month of interview. All regressions further control for a dummy for second follow-up and dummies for missing values in each of the independent variables. The cognitive test score is defined as the number of right answers on a 10-item Raven matrices test.

Table A4.1: Firm balance at baseline and followup

Means, standard deviations in parentheses

***p*-value from t-test of equality of means with control group in brackets; *p*-value from 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	181		
A. Owner characteristics at BASELINE								
Owner is female	.360	.374	[.384]	.021	.353	.326	[.591]	-.040
	(.481)	(.485)			(.479)	(.470)		
Owner age (Years)	35.7	36.1	[.585]	.039	35.2	36.8	[.058]	.129
	(8.71)	(8.54)			(8.58)	(8.67)		
Owner completed years of education	10.6	10.2	[.253]	-.088	10.7	10.2	[.230]	-.100
	(3.23)	(3.37)			(3.18)	(3.22)		
Owner has received training from a VTI	.432	.348	[.055]	-.122	.459	.354	[.012]	-.152
	(.497)	(.477)			(.500)	(.480)		
Owner scored at median or above on cognitive test	.538	.554	[.767]	.023	.539	.543	[.874]	.006
	(.500)	(.498)			(.500)	(.500)		
B. Firm characteristics at BASELINE								
Business is registered	.915	.919	[.734]	.012	.926	.934	[.691]	.020
	(.280)	(.273)			(.262)	(.249)		
Number of employees	2.98	2.79	[.461]	-.051	3.03	2.88	[.435]	-.039
	(2.91)	(2.47)			(2.99)	(2.47)		
Age of business (Years)	6.73	6.97	[.667]	.028	6.86	7.49	[.505]	.073
	(5.28)	(6.52)			(5.41)	(6.74)		
Average monthly revenues (USD)	548	565	[.909]	.018	539	597	[.859]	.063
	(642)	(677)			(597)	(708)		
Average monthly profits (USD)	214	213	[.822]	-.001	208	228	[.881]	.056
	(270)	(228)			(255)	(236)		
***p*-value from F-test of joint significance from column regression**		{.393}				{.259}		

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 371 firms successfully interviewed at first followup. 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. Profits and revenues are trimmed at the 99th percentile.

Table A4.2: Worker balance at baseline and followup

Means, standard deviations in parentheses

***p*-value from t-test of equality of means with control group in brackets**

***p*-value from 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]	.079	20.4	20.7	[.099]	.092	20.4	20.7	[.092]	.088
	(2.43)	(2.81)			(2.30)	(2.82)			(2.31)	(2.74)		
Female	.504	.492	[.706]	-.016	.470	.471	[.601]	.000	.464	.477	[.463]	.018
	(.501)	(.501)			(.500)	(.500)			(.499)	(.500)		
Completed prior education (Years)	10.2	10.3	[.383]	.047	10.4	10.4	[.932]	.017	10.3	10.4	[.827]	.015
	(1.88)	(1.85)			(1.83)	(1.79)			(1.84)	(1.86)		
Course duration (Years)	1.50	1.48	[.753]	-.013	1.57	1.51	[.390]	-.049	1.58	1.52	[.579]	-.050
	(.898)	(.857)			(.868)	(.840)			(.878)	(.852)		
Ever employed	.194	.218	[.470]	.042	.202	.221	[.580]	.031	.202	.213	[.771]	.020
	(.396)	(.413)			(.402)	(.415)			(.402)	(.410)		
Monthly expected earnings (USD)	120.2	123.1	[.756]	.030	124.3	124.6	[.777]	.003	124.3	124.2	[.988]	-.001
	(71.0)	(68.0)			(71.7)	(68.2)			(71.3)	(68.5)		
B. Skills at BASELINE												
Attendance (1-5 scale)	3.39	3.34	[.700]	-.031	3.38	3.35	[.913]	-.023	3.40	3.34	[.741]	-.041
	(1.13)	(1.14)			(1.12)	(1.12)			(1.12)	(1.13)		
Communication skills (1-5 scale)	3.23	3.25	[.733]	.014	3.24	3.26	[.640]	.013	3.24	3.28	[.455]	.025
	(1.08)	(1.13)			(1.05)	(1.13)			(1.07)	(1.14)		
Creativity (1-5 scale)	3.38	3.43	[.442]	.030	3.42	3.44	[.588]	.010	3.44	3.45	[.499]	.011
	(1.11)	(1.05)			(1.08)	(1.07)			(1.10)	(1.07)		
Trustworthiness (1-5 scale)	3.49	3.53	[.369]	.030	3.53	3.54	[.620]	.012	3.52	3.52	[.753]	-.001
	(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]	-.004	5.36	5.32	[.798]	-.010	5.31	5.29	[.774]	-.006
	(2.49)	(2.43)			(2.42)	(2.44)			(2.47)	(2.40)		
***p*-value from F-test of joint significance**		{.953}				{.944}				{.878}		

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 A5: Compliance and attrition

OLS coefficients are reported throughout

Standard errors in parentheses: adjusted for two-way clustering in column 1, robust 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	-.017	.011	.026	-.029
	(.039)	(.023)	(.021)	(.032)
A. Worker characteristics				
Pass grade on all soft skills	.022	.008	.021	
	(.032)	(.024)	(.023)	
Age	-.000	.060	.083**	
	(.035)	(.038)	(.041)	
Age squared	.000	-.001	-.002*	
	(.001)	(.001)	(.001)	
Female	-.094	-.027	.032	
	(.062)	(.056)	(.047)	
Completed prior education (Years)	-.012	.008	-.001	
	(.010)	(.007)	(.007)	
Course duration (Years)	-.078***	.001	.004	
	(.023)	(.017)	(.016)	
Ever employed	-.009	-.034	-.023	
	(.040)	(.029)	(.030)	
B. Firm characteristics				
Number of employees	-.002			.004
	(.005)			(.006)
Owner is female	-.005			-.081
	(.050)			(.061)
Owner attended a VTI	-.012			.063*
	(.039)			(.037)
Age of owner	.007			.004
	(.014)			(.011)
Age of owner squared	-.000			-.000
	(.000)			(.000)
Owner is high ability	.044			.004
	(.047)			(.042)
Mean of dep. var. in Control group	.419	.846	.836	.900
***p*-value from F-test of joint significance of worker covariates**	[.004]	[.328]	[.280]	-
***p*-value from F-test of joint significance of firm covariates**	[.886]	-	-	[.280]
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 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, following the procedure in Cameron et al. (2011). Columns 2-3 use data at the worker level, and 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. Column 4 uses data at the firm level, and 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. 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 whether the owner is high ability. To proxy for owner ability we use the first principal component of the following skills measured at baseline: cognitive skills, years of education and the Big Five. Firm owners who have a value of the first principal component on or above the median are assigned to the high ability group. All regressions further control for dummies for missing values in each of the independent variables.

Table A6.1: Comparison of high and low cognitive ability owners

Means, standard deviations in parentheses; *p*-value from t-test of equality of means in brackets

	High cognitive ability (1)	Low cognitive ability (2)	*p*-value (3)	Normalized Differences (4)
A. Owner and firm characteristics				
Owner is female	.332	.411	[.548]	-.116
	(.472)	(.494)		
Owner age (Years)	35.8	35.6	[.772]	.013
	(8.33)	(8.96)		
Number of employees	2.96	2.56	[.189]	.108
	(2.82)	(2.33)		
Age of business (Years)	6.75	7.41	[.761]	-.075
	(5.24)	(6.93)		
B. Skills and traits				
Owner completed years of education	10.7	9.94	[.034]**	.168
	(3.48)	(3.15)		
Conscientiousness (1 to 5)	4.10	3.92	[.018]**	.185
	(.662)	(.751)		
Agreeableness (1 to 5)	3.79	3.66	[.150]	.110
	(.794)	(.810)		
Neuroticism (1 to 5)	2.15	2.33	[.010]**	-.166
	(.683)	(.846)		
Openness to Experience (1 to 5)	3.15	2.90	[.009]***	.244
	(.742)	(.710)		
Extraversion (1 to 5)	3.33	3.42	[.982]	-.075
	(.766)	(.922)		
Risk aversion (1 to 6)	3.08	3.25	[.527]	-.076
	(1.48)	(1.68)		
Average completed years of education of employees	9.76	8.99	[.008]***	.233
	(2.49)	(2.17)		
C. Profitability, returns to skills and management				
Monthly profits (USD)	259	177	[.072]*	.228
	(296)	(199)		
High relative perceived productivity returns to soft skills	.617	.509	[.038]**	.155
	(.487)	(.501)		
Soft skills have higher perceived wage returns than practical skills	.179	.123	[.153]	.110
	(.384)	(.329)		
Owner is able to delegate tasks/manage more workers	.923	.871	[.088]*	.122
	(.267)	(.336)		
D. Recruitment				
High relative perceived importance of difficulties in observing soft skills at recruitment	.561	.497	[.633]	.091
	(.498)	(.502)		
High difference in relative perceived scarcity of practical vs soft skills	.531	.491	[.583]	.056
	(.500)	(.501)		
Number of employees unconnected at recruitment	1.14	.871	[.166]	.121
	(1.85)	(1.29)		

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Data is from the firm baseline survey. The t-tests are from OLS regressions of the variable of interest on a dummy variable that takes value one if the firm has a high cognitive ability owner, and stratification variables (dummies for BRAC branch and sector), with robust standard errors. Firm owners who scored on or above the median on a cognitive test administered at baseline are assigned to the high cognitive ability group; those who scored below the median are assigned to the low cognitive ability group. The variable "Monthly profits" are average monthly profits in the three months prior to the survey, expressed in January 2015 USD. The top 1% values are excluded. The variable "High relative perceived productivity returns to soft skills" is constructed as follows: firm owners were asked to rate on a 0-10 scale, where 0 = “Not important at all”, and 10 = “Extremely important”, the importance of various skills for their operations, including each of the Big Five (firm owners were asked about the importance of each of the Big Five traits separately). For each firm owner, we compute the average importance given to the Big Five, and then divide this by the average importance across all skills to create a measure of the relative importance of soft skills. This variable then takes value one if the relative importance of soft skills is on or above the median. The variable "Soft skills have higher perceived wage returns than practical skills" is constructed as follows: managers were asked how much more they thought a worker with a high level of practical skills could earn in a firm like theirs relative to someone with a low level of practical skills. The same question was then asked also for soft skills (this was asked in general about soft skills and not about the Big Five specifically). The resulting variable takes value one if managers reported a higher wage return to soft skills relative to practical skills. To construct the variable "Owner is able to delegate tasks/manage more workers", firm owners were asked if they thought they had enough ability to manage/train more workers/delegate tasks. This variable is a dummy equal to one if the owner answered Yes. For the construction of the variable "High relative perceived importance of difficulties in observing soft skills at recruitment", firm owners were asked to rate on a 1-5 scale, where 1 = “Not important at all”, and 5 = “Extremely important”, the importance of a number of potential constraints. For each firm owner, we divide the importance given to difficulties in observing the soft skills of job candidates by the average importance across all constraints, to create a measure of the relative importance of difficulties in observing soft skills. The resulting variable takes value one if the relative importance of difficulties in observing soft skills is on or above the median. To create the variable "High difference in relative perceived scarcity of practical vs soft skills", we compute within-firm owner differences in the number of potential workers out of 10 reported as having a good level of practical skills vs. a good level of soft skills. To estimate the reported number of workers with a good level of soft skills we compute the average of the reported number of workers with a good level of each of the Big Five (the question was asked for each of the Big Five traits separately). This creates a measure of the relative perceived scarcity of practical vs soft skills. The resulting variable then takes value one if this measure is on or above the median. The Big Five of the owners were measured at baseline using a 10-item scale. Risk aversion was measured through a gamble choice game with real money, based on Attanasio et al. (2012). More details on the elicitation of risk aversion are in the Supplemental Material.

Table A6.2: Descriptives on VTI reputation among managers

	% (1)	*p*-value High ability = Low ability (2)
Owner has heard of VTI	15.5%	.778
Owner has hired someone who attended VTI, conditional on knowing VTI	11.5%	.151
Owner thinks that workers from VTI are paricularly good in soft skills, conditional on knowing VTI	6.22%	.324
Owner willing to pay a worker from VTI a higher salary than workers from other VTIs, conditional on knowing VTI	26.6%	.805

Notes: The table uses data from the firm baseline survey. Firm owners were asked questions about each of the 15 VTIs included in our study sample. The unit of observation is an owner-VTI combination, as owners were asked questions for each of the 15 VTIs separately. Column 2 reports the results of OLS regressions of the variables considered in the table on a dummy equal to one if the owner is assigned to the high ability group, and stratification variables (dummies for BRAC branch and sector). The p-value from the t-test that the coefficient on the high ability dummy is equal to zero is reported. Standard errors are clustered by firm owner in these regressions. To proxy for owner ability, we use the first principal component of the following skills measured at baseline: cognitive skills, years of education and the Big Five. Firm owners who have a value of the first principal component on or above the median are assigned to the high ability group.

Table A7.1: Impacts on match-level outcomes - Robustness to skills aggregation and definition of owner ability

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

Dependent variable:	Assessment of worker skills by owner (standardized index)				Worker was hired by the matched firm			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(i) Treatment	.115	.157*	.132	.010	-.001	-.004	.008	-.078*
	(.085)	(.084)	(.084)	(.112)	(.034)	(.034)	(.032)	(.047)
(ii) Worker principal component of soft skills (standardized) X Treatment	.195**				-.007			
	(.096)				(.033)			
(iii) Owner principal component of skills (standardized) X Treatment		.245***				.021		
		(.093)				(.025)		
(iv) Owner score on cognitive test (standardized) X Treatment			.155				087**	
			(.094)				(.040)	
(v) Owner score on cognitive test above median X Treatment				.264				.184***
				(.175)				(.069)
Mean of dep. var. in Control group	.000	.000	.000	.000	.116	.116	.116	.116
***p*-value from test: (i) + (v) = 0**				[.037]				[.027]
Number of observations (matches)	515	515	515	515	412	412	412	412

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the matching surveys in columns 1-4. Results from the first worker followup survey in columns 5-8. 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 in columns 1-4 is the same standardized index as in Table 3. The dependent variable in columns 5-8 is a dummy equal to one if the worker was hired by the matched firm, and zero otherwise. This is the same outcome as in Table 6. 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: 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 2-4 and 6-8 also control for a dummy for whether the worker had a pass grade (C or above) on all five soft skills measured in the baseline assessments. 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. In the second row to proxy for worker skills we use the principal component of the five soft skills measured in the baseline assessments. In the third row, to proxy for owner ability we use the first principal component of the following skills measured at baseline: cognitive skills (measured through Raven matrices), years of education and the Big Five. In the fourth row to proxy for owner ability we use their score on a Raven matrices cognitive test they took at baseline. In the fifth row we create a dummy equal to one if the score on the Raven matrices cognitive test is on or above the median. Each regression also controls for the variable by which heterogeneous effects are considered, uninteracted with Treatment. All regressions further control for dummies for missing values in each of the independent variables.

Table A7.2: Heterogeneous impacts on match-level outcomes

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

Dependent variable:	Assessment of worker skills by owner (standardized index)			Worker was hired by the matched firm		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.101	.072	.249	-.040	-.204*	-.208*
	(.151)	(.220)	(.263)	(.046)	(.105)	(.107)
Interaction of Treatment with:						
Owner is high ability	.521***	.506***	.599***	.110*	.119*	.101
	(.170)	(.177)	(.201)	(.064)	(.062)	(.078)
Owner has high risk aversion	.124	.121	.071	.005	.008	.023
	(.168)	(.166)	(.162)	(.060)	(.060)	(.057)
Owner is female			.095			-.137
			(.274)			(.104)
Owner attended a VTI			-.284			-.034
			(.175)			(.076)
Owner has high English literacy			.308*			-.016
			(.181)			(.083)
Number of employees			-.072*			.004
			(.037)			(.013)
Log average monthly profits			-.010			.016
			(.031)			(.015)
Mean of dep. var. in Control group	.000	.000	.000	.116	.116	.116
Controls for interactions of sector dummies with Treatment	No	Yes	Yes	No	Yes	Yes
***p*-value from F-test of joint significance of interactions of sector dummies with Treatment**	-	[.592]	[.509]	-	[.217]	[.211]
Number of observations (matches)	515	515	515	412	412	412

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. 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 in columns 1-3 is the same standardized index as in Table 3. The dependent variable in columns 4-6 is a dummy equal to one if the worker was hired by the matched firm, and zero otherwise. This is the same outcome as in Table 6. All regressions control for stratification variables (dummies for BRAC branch and sector) and 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; dummy for whether the owner has high ability; dummy for whether the owner has high risk aversion. To proxy for owner ability we use the first principal component of the following skills measured at baseline: cognitive skills, years of education and the Big Five. Firm owners with a value of the first principal component on or above the median are assigned to the high ability group. Risk aversion was measured through a gamble choice game with real money, based on Attanasio et al. (2012). More details on the elicitation of risk aversion are in the Supplemental Material. Managers with a risk aversion at the median or above are assigned to the high risk aversion group. The regressions in columns 3 and 6 further control for log average monthly profits and for a dummy for whether the owner has above median English literacy. The regressions further control for dummies for missing values in each of the independent variables.

Table A8.1: Heterogeneous impacts on expectations, outside options, search behavior and beliefs - By whether met any firms

OLS coefficients; standard errors in parentheses are robust in columns 3, 4, 7, 9, 10, 11, and clustered at the worker level in the other columns

	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	Any casual work in the last week	Attended further education or training in the last year	Looked for a job in the public/ngo sector in the last year	Looked for a job in the last year	Average self-assessed soft skills grade (1 to 5 scale)	Average perceived returns to soft skills (0 to 10 scale)	Average perceived constraints in signaling skills (1 to 5 scale)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(i) Treatment	11.5***	.271*	.156*	.107**	-.056	.062**	.135***	-.016	.014	.100	-.090
	(4.18)	(.162)	(.088)	(.046)	(.037)	(.026)	(.049)	(.033)	(.051)	(.154)	(.121)
(ii) Did not meet any firms	-7.07	.004	-.103	-.084	.014	-.049	-.066	-.009	.013	-.207	-.170
X Treatment	(6.52)	(.233)	(.128)	(.067)	(.053)	(.039)	(.073)	(.050)	(.067)	(.222)	(.182)
***p*-value from test: (i) + (ii) = 0**	[.360]	[.100]	[.562]	[.631]	[.257]	[.649]	[.195]	[.521]	[.531]	[.491]	[.047]
Mean of dep. var. in Control	114.7	5.53	-.001	.624	.323	.118	.268	.749	4.35	8.06	2.73
Controls for baseline value of outcome	Yes	Yes	Yes	No	Yes	No	No	Yes	No	No	Yes
Uses data from first followup	Yes	Yes	No	No	Yes	Yes	No	Yes	No	No	No
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,330	1,349	666	668	1,350	1,348	674	1,350	673	673	673

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. The regressions in columns 1, 2, 5, 6 and 8 further control for a dummy for second follow-up since they use data from both follow-ups. The dependent variable in the other columns is available only at second follow-up. 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 variables throughout are exactly the same as in Tables 4 and 5. For the definition of the dependent variables see Tables 4 and 5. 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. 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 A8.2: Heterogeneous impacts on employment and earnings - By whether met any firms

OLS coefficients; standard errors in parentheses are clustered at the worker level

Dependent variable:	Any work as wage employee in the last week	Any work as self-employed in the last week	Main activity in last week is wage employment, self-employment or education/training	Weekly hours worked in last job as employee	Weekly hours worked in last job as self-employed	Any paid work in the last month	Total earnings in the last month (USD)	
Sample of workers:	**All**	**All**	**All**	**All**	**All**	**All**	**All**	**Conditional on employment**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
(i) Treatment	.073*	-.027	.048	4.19	-3.91*	.018	7.03	8.13
	(.040)	(.036)	(.039)	(2.67)	(2.37)	(.035)	(4.39)	(4.95)
(ii) Did not meet any firms	-.089	.049	.003	-4.44	5.67*	-.065	-6.69	-1.79
X Treatment	(.059)	(.048)	(.056)	(3.98)	(3.30)	(.050)	(6.48)	(7.29)
***p*-value from test: (i) + (ii) = 0**	[.706]	[.486]	[.200]	[.931]	[.436]	[.175]	[.943]	[.235]
Mean of dep. var. in Control	.428	.211	.600	37.0	16.0	.750	47.2	63.1
Controls for baseline value of outcome	Yes	Yes	No	No	No	Yes	Yes	Yes
Uses data from first followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Uses data from second followup	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,350	1,350	1,350	1,347	1,349	1,338	1,329	988

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. All regressions control for stratification variables (dummies for BRAC branch and sector), a dummy for second follow-up 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; 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 variables throughout are exactly the same as in Tables 8 and 9. For the definition of the dependent variables see Tables 8 and 9. 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. 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, 2 and 6. Similarly, in columns 7-8 we consider as baseline value of the outcome expected earnings at baseline. All regressions further control for dummies for missing values in each of the independent variables.

Table A9: Heterogenous impacts on education and training

OLS coefficients, standard errors clustered at the worker level in parentheses

Dependent variable:	Attended further education or training in the last year	
	(1)	(2)
Treatment	-.010	-.021
	(.026)	(.041)
High predicted ability for schooling X Treatment	.090**	
	(.039)	
High completed formal education X Treatment		.079*
		(.047)
Mean of dep. var. in Control group	.118	.118
Controls for baseline value of outcome	No	No
Uses data from first and second followup	Yes	Yes
Number of observations	1,348	1,348

Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the worker follow-up surveys are reported. All regressions control for stratification variables (dummies for BRAC branch and sector), for dummies for month of interview, and for a dummy for second follow-up. In addition, all regressions control for the following worker characteristics measured at baseline: the average grade on the five soft skills measured in the baseline assessments (standardized); age and age squared; dummy for female; duration (in years) of the vocational training program the worker was attending at baseline; dummy for any past work experience. The variable "High completed formal education" takes value one if the worker had a value of completed formal education at baseline equal to the sample median or above. The variable "High predicted ability for schooling" is constructed using four variables from the worker baseline survey: completed years of formal education; cognitive skills (measured through a 10-item Raven matrices test); a dummy for whether the worker intends to seek additional formal education or training in the future; the number of years of education that the worker would like to attain in the future. Predicted schooling ability is constructed by converting each of the four 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. We then create a dummy equal to one if the worker had a value of predicted schooling ability equal to the sample median or above. Each regression also controls for the variable by which heterogeneous effects are considered, uninteracted with Treatment. All regressions further control for dummies for missing values in each of the independent variables.

Table A10: Impacts on hiring behavior of firms

OLS coefficients; robust standard errors in parentheses

	Panel A: Screening				Panel B: Search		Panel C: Employment	
Dependent variable:	Average perceived returns to soft skills (1 to 5 scale)	Perceived constraints in screening soft skills (1 to 5 scale)	Asks for certificate/reference letter during interview	Log duration of typical job interview in minutes	Firm has a vacancy	Looks for workers using formal channels	Log number of employees	Log ideal number of employees
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-.080	-.024	.015	-.071	-.029	-.005	-.029	-.010
	(.119)	(.109)	(.043)	(.070)	(.051)	(.020)	(.041)	(.050)
Owner ability (standardized) X Treatment	.058	-.175*	.055	.116*	.080	.001	.052	.093*
	(.102)	(.098)	(.042)	(.064)	(.053)	(.021)	(.044)	(.048)
Mean of Dep. Var. in Control	8.07	3.55	.783	53.5	.429	.048	2.60	5.18
Controls for baseline value of outcome	No	No	No	Yes	No	Yes	Yes	Yes
Number of observations (firms)	371	369	369	364	369	369	367	361

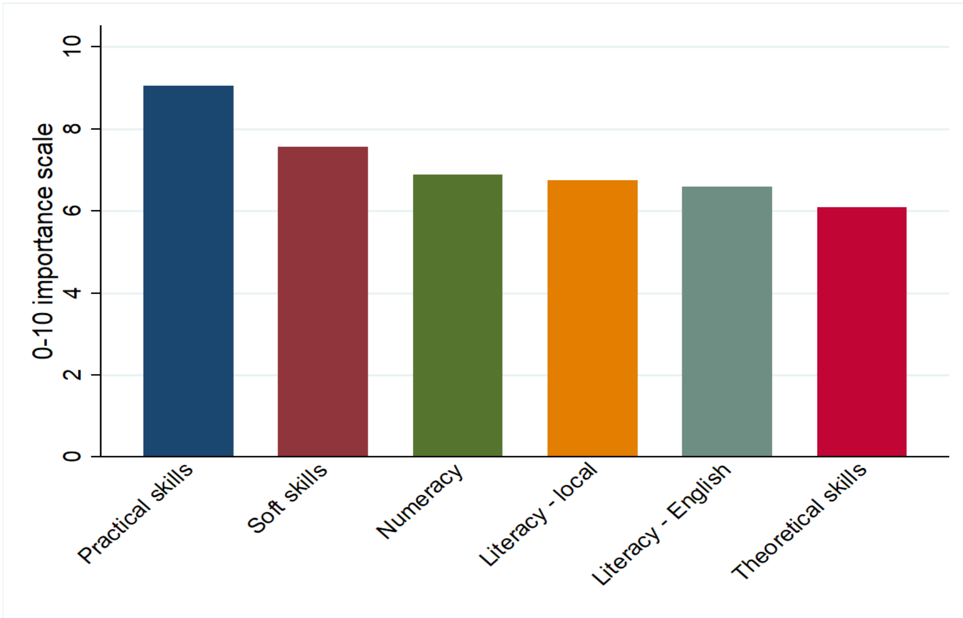
Notes: *** (**) (*) denotes significance at the 1% (5%) (10%) level. Results from the firm follow-up survey are reported. All regressions control for stratification variables (dummies for BRAC branch and sector) as well as for dummies for month of interview. All regressions also control for the following 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. To proxy for owner ability we use the first principal component of the following skills measured at baseline: cognitive skills, years of education and the Big Five. Each regression also controls for the variable by which heterogeneous effects are considered, uninteracted with Treatment. The dependent variable in column 1 is constructed as follows: firm owners were asked how important it would be that their ideal employee had a good level of each of the five soft skills measured in the baseline assessments and disclosed on the certificates, using a 0-10 scale, where 0="Not important at all" and 10="Extremely important". We compute the average perceived importance of the five soft skills. The dependent variable in column 2 is constructed as follows: firm owners were asked to report their perceived importance of constraints related to observing soft skills at recruitment, using a 1 to 5 scale, where 1="Not a problem at all" and 5="A very important problem". 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 certificates. In column 3 the dependent variable is a dummy that takes value one if the owner reported asking for certificates/reference letters during the typical interview with workers not known to the owner from before. In column 4 the dependent variable is the log of the duration of the typical job interview with workers not known to the owner from before, in minutes. In column 5 the dependent variable is a dummy that takes value one if the firm owner reported currently having a vacancy. In column 6 the dependent variable is a dummy that takes value one if the owner reported looking for workers using formal channels, that is, either: (i) advertising on posters/mass media/internet; (ii) through middlemen or (iii) through other NGO/government programs. In column 7 the dependent variable is the log of the number of employees plus 1. In column 8 the dependent variable is the log of the number of ideal employees plus 1. The top 1% of values for interview duration, number of employees and ideal number of employees are excluded.

Table A11: 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	44.64
2 Change in annual earnings per individual in year 2	44.64
3 NPV change in annual earnings per individual	**72.57**
Sensitivity to different assumptions about discount rate	
3.1 *Discount rate = 20%*	68.20
3.2 *Discount rate = 30%*	60.75
4 Benefits/Cost ratio	**3.80**
Sensitivity to different assumptions about discount rate	
4.1 *Discount rate = 20%*	3.57
4.2 *Discount rate = 30%*	3.18

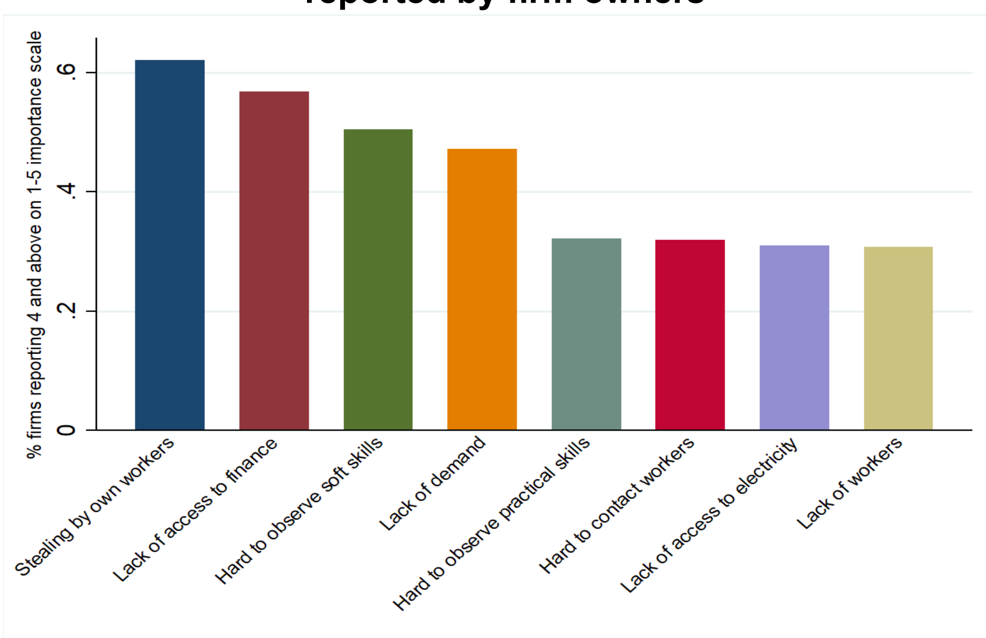
Notes: The total cost per individual at year zero includes: (i) cost of developing and administering the skill tests (9.19 USD); (ii) cost of producing and distributing certificates (6.40 USD); (iii) overheads (3.50 USD). Monetary amounts are deflated and expressed in terms of the price level in January 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 ($3.72), and multiplying that by 12.

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



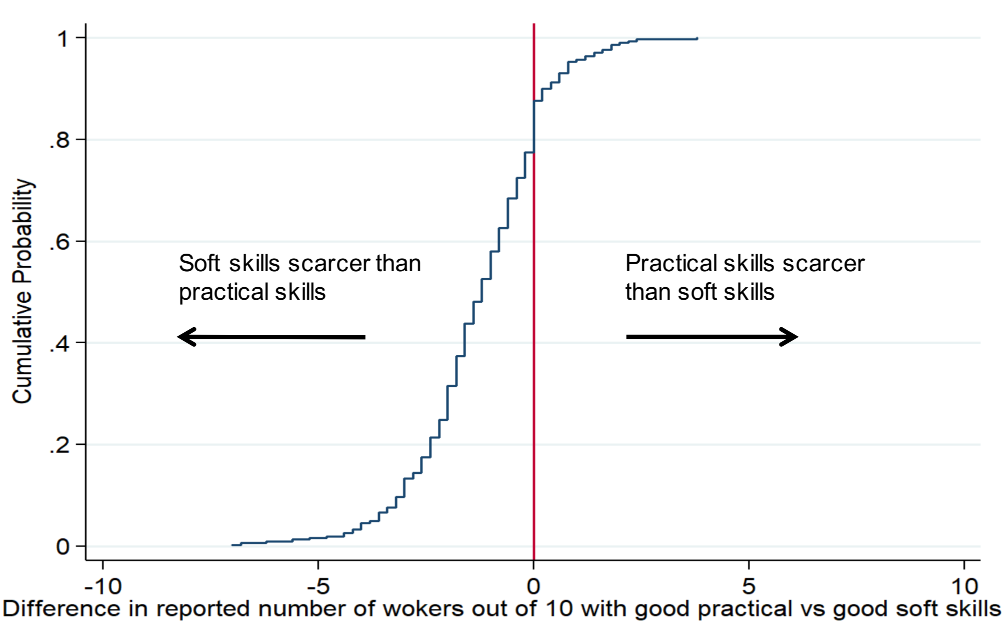
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 0-10 scale, where 0 = "Not important at all", and 10 = "Extremely important", the importance of various skills for their operations. The figure reports the average importance given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

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



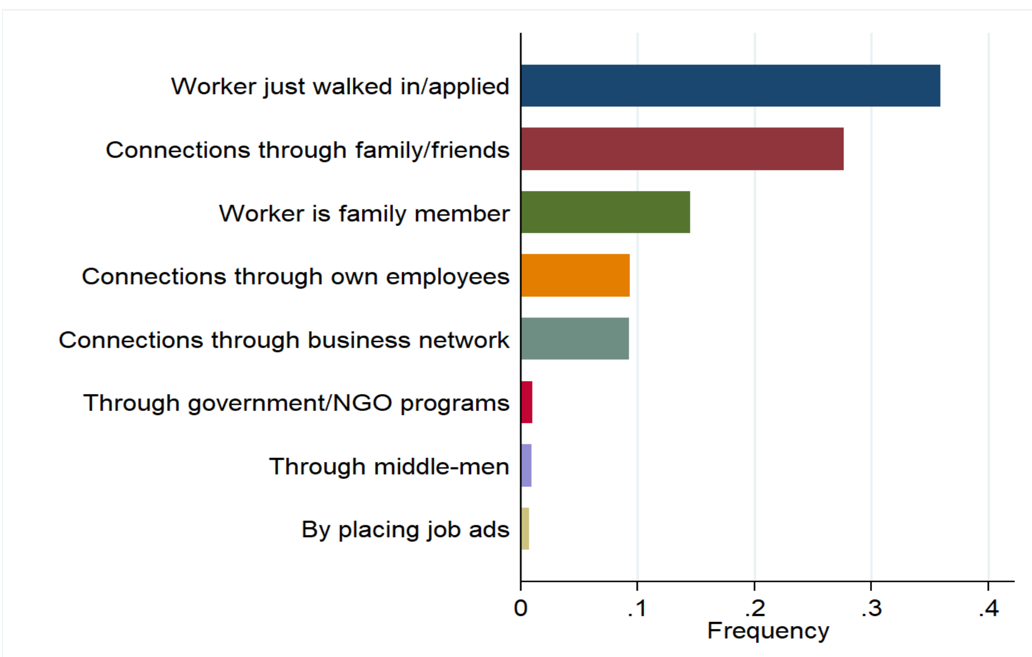
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 1-5 scale, where 1 = "Not important at all", and 5 = "Extremely important", the importance of these potential constraints. The figure reports the percentage of firm owners that answered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

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



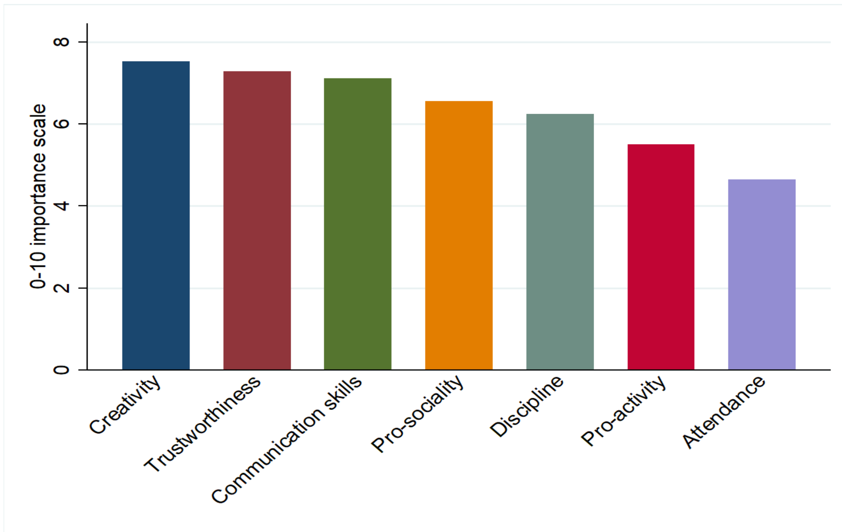
Notes: The figure reports the CDF of within-firm owner differences in the number of potential workers out of 10 reported as having a good level of practical skills vs. a good level of soft skills. To estimate the reported number of workers with a good level of soft skills we compute the average of the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

Figure A4: Recruitment channel of current employees



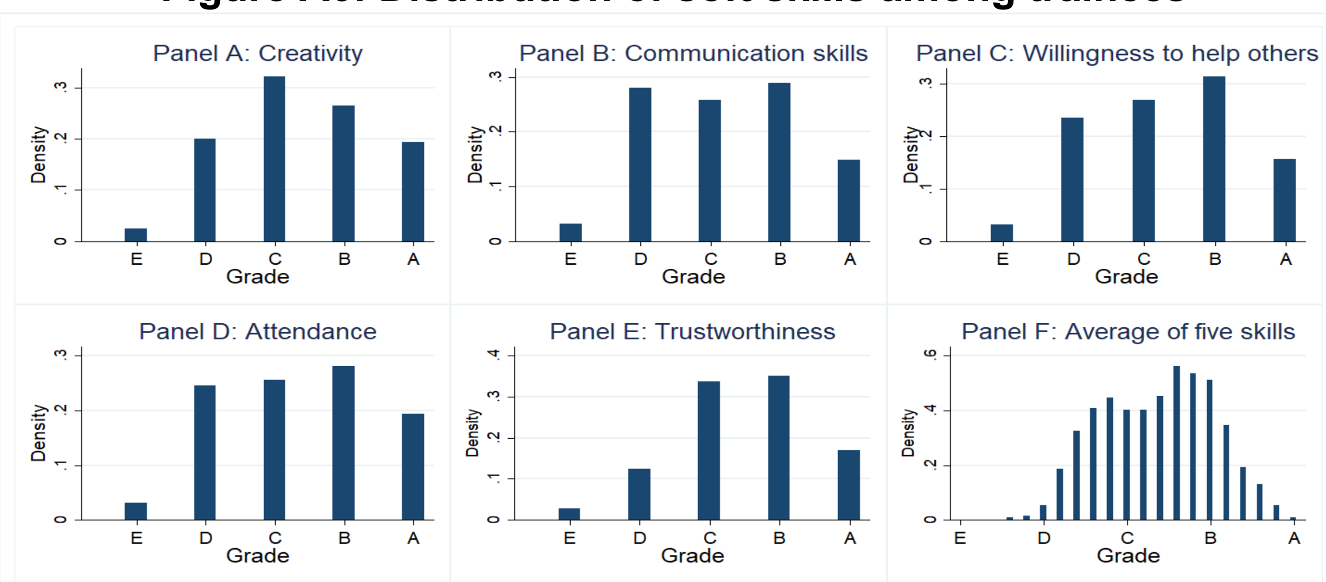
Notes: The figure reports the frequency of recruitment channels for the workers employed at baseline in the sample of firms included in the intervention.

Figure A5: 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 A6: Distribution of soft skills among trainees



Notes: The sample includes the 787 trainees taking part to the matching and signaling intervention.

Figure A7: Skills certificates

Panel A: Treatment

ID XXX

brac

Acknowledgment

This is to acknowledge that

NAME OF TRAINEE

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

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

Date: 02/04/2015

Dr Jenipher Twebaze Musoke
Coordinator, Research,
BRAC Africa Programme

Bhuiyan Muhammad Imran
Country Representative
BRAC Uganda

The BRAC-IFS Youth Employment Job Placement Project

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

BRAC

Plot 90 Busingiri Zone	*T: +256 (0) 414 270978*	*Registered in Uganda*
Off Entebbe road Nyanama	*: +256 (0) 712 111322*	*As BRAC Uganda*
PO Box 31817 (Clock Tower)	*E: bracuganda@brac.net*	*Registration Number*
Kampala Uganda	*W: www.brac.net*	*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 A7: Skills certificate (continued)

Panel B: Control

ID XXX

brac

Acknowledgment of Participation

This is to acknowledge that

NAME OF TRAINEEE

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

Date: 02/04/2015

…………………………	…………………………
Dr Jenipher Twebaze Musoke	*Mr Bhuiyan Muhammad Imran*
Coordinator, Research,	*Country Representative*
BRAC Africa Programme	*BRAC Uganda*

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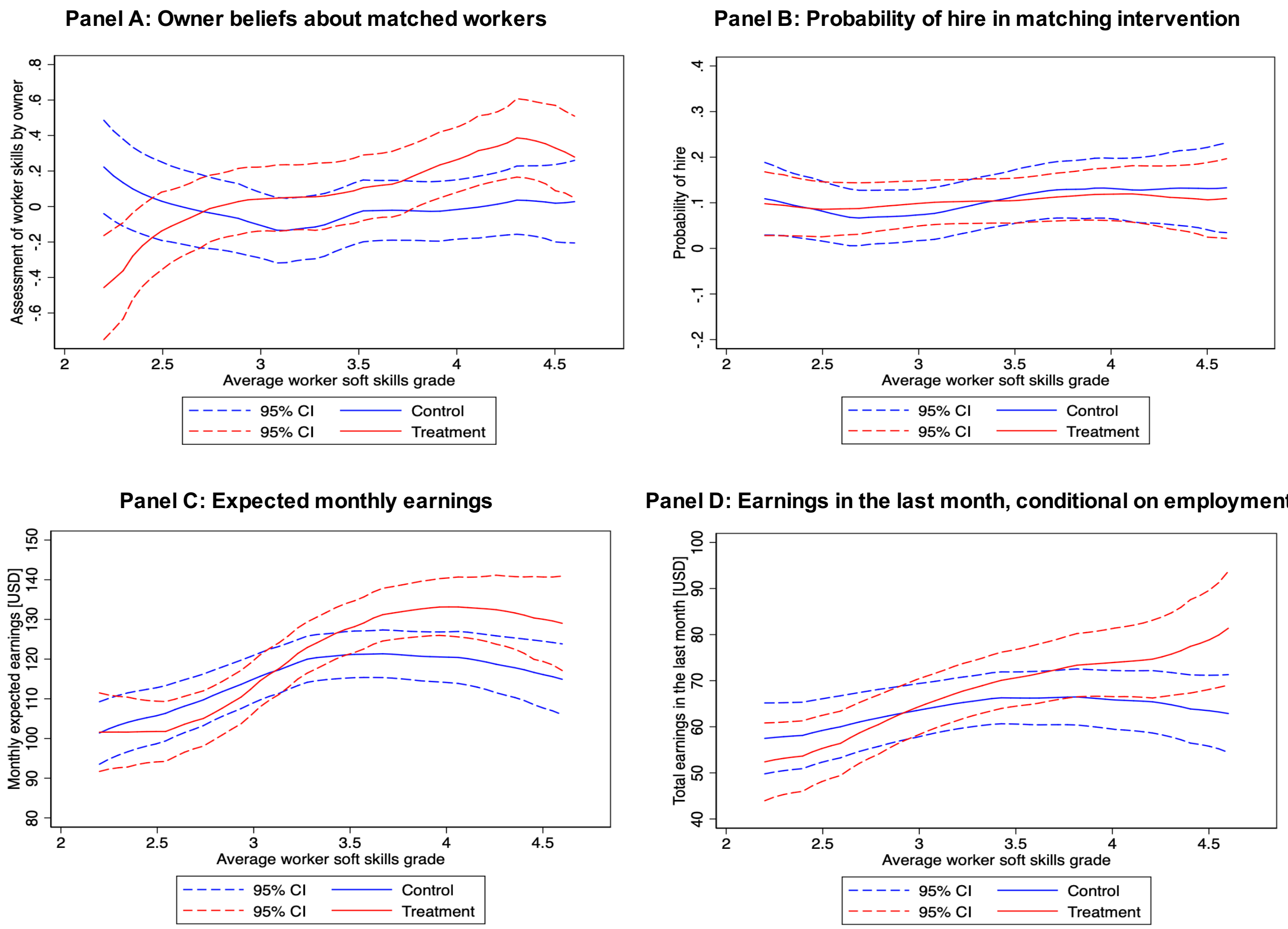
Figure A8: Non-parametric treatment effects along the skill distribution



Note: The Figure reports non-parametric regressions of four outcomes on the average soft skills grade of the worker, by Treatment. No other controls are included. Workers in the bottom 1% or top 1% of average soft skills grades are excluded from the sample. Panel A uses data from the matching surveys, and the dependent variable is the same standardized index of managers' assessments as in Table 3. Panel B uses data from the worker first follow-up survey, and the dependent variable is a dummy equal to one if the worker was hired by the matched firm, and zero otherwise. This is the same outcome as in Table 6. Panel C uses data from the worker follow-up surveys, and the dependent variable is expected monthly earnings. This is the same outcome as in column 1 of Table 4. Panel D uses data from the worker follow-up surveys, and the dependent variable is total earnings in the last month prior to the survey. This is the same outcome as in Table 9, column 7. 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 monthly earnings and total earnings are excluded. Panel D limits the sample to workers in paid employment at follow-up.

Figure A9: 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 Appendix Table A11, and is equal to 3.80. This is the grey 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.