Information Frictions in the Labor Market: Evidence from a Field Experiment in Uganda*

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Abstract

We study how lack of information on the skills of workers affects both employers and job seekers. To do so, we design and implement a field experiment in the Ugandan labor market: through the provision of certifications, we vary whether new information on the *soft skills* of workers is disclosed to both managers and workers during job interviews. We show that both sides of the market react to the information: managers of higher ability update their beliefs on worker's skills, while workers with higher skills revise their outside options upwards. Guided by these facts, we develop a screening model with two-sided updating. The model predicts *non-linear* impacts of the certifications on job offers and hires along the skill distribution, due to differential effects on worker's outside options. In line with these predictions, we find the largest employment gains for workers in the *middle* of the skill distribution. Our estimates of the Internal Rate of Return (IRR) of the intervention range between 9-29%, implying positive welfare gains for the average participant. Motivated by the heterogeneous impacts, we use the model to determine the welfare effects of introducing a mandatory certification policy on soft skills across the entire skill distribution.

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1 Introduction

The efficient functioning of labor markets crucially depends on the information available to both employers and workers. The ability of firms to acquire information on the skills of workers can impact hiring decisions, wage setting, promotions and terminations. The ability of workers to signal their skills can affect the likelihood of getting a job, worker mobility and the ex ante decision to acquire human capital. Studying information frictions on the skills of workers is thus important, as these can affect the efficient allocation of resources, and ultimately aggregate productivity and output.

Job interviews are a key opportunity for firms to learn about the skills of job candidates, and for workers to show their skills. Interviews are also valuable insofar as workers can learn about the value of their signals by observing the reaction of hiring managers to their CVs and certifications. Despite the potentially important role of job interviews in generating information in the labor market, the literature on employer learning has mostly focused on how firms learn about the skills of workers *on the job*, rather than *at recruitment*.[1]

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

Our main contribution is to study how *both* firms and workers respond to new information during job interviews. On the firm side, we identify whether managers update their beliefs on the skills of workers. On the worker side, we study whether certifications impact the worker's perceived outside options. We then develop a screening model to link the updating of firms and workers to labor market outcomes, focusing on job offers and hires, and compare the model predictions to the reduced form impacts of the intervention on these outcomes. We use our estimates of the program impacts, along with the model, to perform a cost-benefit analysis of the intervention.

Our setting is an informal labor market, where information frictions can be particularly important, given the lack of institutions certifying the skills of workers. The sample includes young workers fresh out of vocational education and looking for jobs, and Small and Medium

[1]See, for example, Jovanovic (1979), Farber and Gibbons (1996), Altonji and Pierret (2001) and Kahn and Lange (2014). A related literature focuses on the implications of the learning process about workers being potentially asymmetric between the incumbent firm and outside firms. See, for example, Schönberg (2007), Pinkston (2009) and Kahn (2013).

Enterprises (SMEs) looking for workers. Young workers might be particularly affected by these frictions, given their lack of work experience. Similarly, SMEs in this context are likely not to have access to sophisticated screening technologies, and so might be less able to screen workers, compared to larger firms operating in more developed labor markets.

A pre-intervention survey of SMEs reveals a number of key facts that inform our research design. First, soft skills are reported by managers as important but difficult to observe at recruitment. In addition, managers believe soft skills to be relatively scarce among potential applicants. The survey further reveals that it is common for managers to recruit workers without any prior connection or referral. Therefore, an intervention in which the firms and workers paired for job interviews do not know each other is likely to be informative of the regular recruitment process in this labor market. Finally, we document substantial heterogeneity in managerial ability, by showing that firm owners of higher cognitive ability are significantly more profitable.[2] Based on this, in the empirical analysis we consider heterogeneous effects by manager's ability, to learn about the interaction between managerial ability and the size of the information friction.

We identify the specific soft skills to be revealed in Treatment certificates based on the stated preferences of firm owners in the baseline survey over which skills they would find most useful to receive information on during interviews. We focus the information revelation on five skills: attendance, communication skills, creativity, trustworthiness and willingness to help others. We measure these skills through initial assessments at the vocational institutes where the workers are enrolled, before graduation.

We collect data on the outcomes of over 500 face-to-face job interviews, and use this to study how both firms and workers react to the information disclosure. We collect information on: (i) stated beliefs of firm owners on the skills of applicants, (ii) job offers and hires in the intervention, (iii) outcomes of both workers and firms in the first year post-intervention.

We document that the workers that participate to the intervention are *positively* selected on soft skills, relative to the eligible population. This is explained by the recruitment process for the experiment: all eligible workers were informed that, if they accepted to participate to the intervention, information from their skills assessments could be disclosed to firm owners during job interviews. This positive selection indicates that workers with higher skills believe they have more to benefit from participating to the signalling intervention.

Our first set of results relate to whether firm owners and workers respond to the certifications by updating their beliefs. We find that high ability firm owners revise *upwards* their beliefs on the skills of the matched workers. The positive effect is stronger for workers

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

with higher skills, and we find no evidence of negative updating for workers with relatively lower skills. The positive selection of workers into the experiment, along with the low priors of managers on the distribution of soft skills among workers, explains why the certificates create mostly positive news for high ability owners. On the contrary, low ability owners do not react to the information disclosure, in terms of their beliefs.

We show that workers with higher skills react to the certifications by positively updating their outside options, as proxied by their reservation wages and probability to be back in training one year post-intervention. This suggests that these workers are trying to transition to better jobs, where their skills might have higher returns. The results are driven by matches with high ability firm owners: we find no evidence that workers matched to low ability owners update their outside options. This confirms that the updating process during job interviews is *symmetric*: if firm owners do not react to the certifications, workers do not react either. A job interview thus provides an opportunity for workers to learn about the value of their signals through the firm's reaction. This highlights one important way in which heterogeneity in the managerial ability of firm owners directly links to the size of the information friction.

The results on firm and worker updating are linked together through a screening model in which firms use interviews to avoid hiring workers with *very low* soft skills. The certifications reduce adverse selection, but can increase the cost for the firm of hiring workers with high skills, through changes in their outside options. The model predicts *non-linear* impacts of the intervention on job offers and hires: workers at the low end of the distribution are hurt by the information revelation. Workers in the middle of the distribution benefit more than workers at the top, because they do not revise their outside options upwards.

We document substantial heterogeneity in the ability of firm owners to screen workers in Control, where no additional information is revealed: in matches involving a high ability owner, workers with higher skills are more likely to get a job offer and to be hired, relative to workers with lower skills. On the contrary, worker skills do not predict offers and hires in Control matches with low ability firm owners.

We find that the revelation of information on skills significantly increases overall job offers and hires for workers in the *middle* of the skill distribution: the probability of employment at the matched firm increases from 2.3% to 5.4% for this group of workers, once information on their skills is revealed. The treatment effect is driven entirely by matches with high ability managers: in this case workers in the middle of the distribution become 8pp more likely to be hired, relative to similar workers in Control. On the other hand, workers with higher soft skills do not get more offers and are not more likely to be hired, once more information on their skills is revealed.

These results confirm the model predictions. Certifications increase job offers and hires for workers in the middle of the distribution because firm owners positively update their beliefs on the skills of workers: if firm owners are trying to avoid hiring workers with *very low* soft skills, then once they learn through the certificates that they are not facing these lowest types, their propensity to hire increases. The null effects of the certifications for workers with higher skills can be explained by their reaction to the information: while the revelation of information increases the expected productivity of these workers at the matched firm, it also increases their outside options, and thus their expected cost for the firm.

The intervention has persistent effects on the employment outcomes of workers, as documented in the one-year follow-up survey. Given the overall low level of offers and hires in the intervention, the gains in employment in the post-intervention period are best explained by workers having found employment in other firms. Coherently with this, we find no evidence of a sustained increase in firm size for the firms affected by the intervention.

We use the documented impacts on both workers and firms to conduct a cost-benefit analysis of the intervention. We find that even under conservative assumptions the lower bound of the welfare gains for program participants is positive: our estimates of the Internal Rate of Return (IRR) to the certification intervention range between 9% and 29% for the average worker. In line with the reduced form impacts on labor market outcomes, the cost-benefit analysis shows the largest gains for workers in the middle of the skill distribution.

Finally, we discuss the implications of introducing a mandatory certification policy on soft skills. This is an interesting comparison because the welfare implications could be very different in the mandatory case: we documented that those workers that decided not to participate to the intervention are negatively selected on soft skills, and so, as highlighted by the model, they could be *negatively* affected by the certifications. In particular, we assume that all the eligible workers that selected out of the intervention are the *very low* types that firms are trying to avoid hiring. We then show how the IRR would change under various assumptions on any *negative* effects of the certification policy on these workers. While our estimates of the IRR become lower, these remain positive for the average worker in most of our counterfactual scenarios. This indicates that a mandatory certification policy on soft skills is likely to be welfare improving in this context. However, by making some workers better off, and some worse off, the policy would also increase inequality among workers.

Our work relates to two main strands of the literature in labor and development economics. First we contribute to the literature on job testing (Autor and Scarborough (2008), Hoffman et al. (2015)). This literature studies how *firms* use the results of screening tests to make hiring decisions, and finds that the introduction of job tests improves the ability of managers to hire workers with *higher* skills.[3] Information revelation is typically one-sided in

[3] A related literature studies the role of temporary employment firms (e.g. Autor (2001)), online outsourc-

job tests, as the firm is revealed the results of the test, while workers are not. This creates an informational advantage for the firm, that can be used to extract rents from workers. We contribute by showing that a signalling intervention in which *both* the worker and the firm are informed about the results of skills assessments can have non-linear impacts on the probability of employment, through changes in the outside options of workers.

Second, we contribute to the literature on observable worker performance or experience in the labor market (Terviö (2009), Pallais (2014), Barach (2015), Stanton and Thomas (2015)).[4] A closely related paper is Pallais (2014), who shows that disclosing more information about worker's abilities increases hiring and welfare in the context of an online labor market. The main mechanism behind her results is that hiring generates public information on the skills of workers, as firms have to disclose a public rating of worker performance. This creates an externality, which leads firms to hire too few inexperienced workers. We focus on a different source of inefficiency: adverse selection in hiring. We show that the provision of information is likely to increase overall hiring and welfare even if firms fully obtain the benefits of the information they generate. In addition, we contribute by showing that *both* workers and firms react to the information revelation, and by documenting the gains from information to both sides of the labor market.[5]

The rest of the paper is organized as follows: Section 2 describes the sample and the selection into the experiment. Section 3 presents the intervention. Section 4 discusses the empirical strategy. Section 5 presents the impacts on beliefs. Section 6 develops a screening model to map the impacts on beliefs to predictions on offers and hires. Section 7 describes the results on offers and hires. Section 8 discusses the cost-benefit analysis of the intervention, together with the welfare implications of introducing a mandatory certification policy on the skills of workers. Section 9 concludes.

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ing agencies (e.g. Stanton and Thomas (2016)) and referrals (e.g. Brown et al. (2016), Burks et al. (2015), Pallais and Sands (2015)) in screening workers in the labor market.

[4]A related literature studies the signalling value of post-secondary educational credentials in the labor market. See, for example, Deming et al. (2016), and MacLeod et al. (2015).

[5]Another related literature studies labor market frictions and youth employment in developing countries. This literature has focused on the importance of networks and referrals for hiring (Beaman and Magruder (2012), Beaman et al. (2015)). A number of recent papers evaluate matching interventions between unemployed youth and firms, finding mixed results on employment (Groh et al. (2015), Hardy and McCasland (2015)). We contribute by testing for the importance of information frictions on the skills of workers in explaining the limited impacts of such matching programs. A related paper is Abebe et al. (2016) who show that certifying the hard skills of young workers with little education in Ethiopia improves their labor market outcomes, and more so for workers with higher skills. Finally, our study contributes to the literature on the returns to cognitive and non-cognitive skills (see, for example, Bowles et al. (2001), Heckman et al. (2006) and Deming (2015)).

2 Sample Selection and Descriptives

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

2.1 Firm Census and Selection into the Final Research Sample

Firms were initially identified by means of a census of SMEs conducted in 17 urban areas of Uganda, covering all four regions of the country.[6] To be included in the census, firms had to: (i) be operating in one of the following six sectors: carpentry, catering, hairdressing, motor-mechanics, tailoring, welding; (ii) employ at least 2 workers (in addition to the owner).[7] The census identified 1086 eligible SMEs.

Table 1 reports summary statistics from the census. Panel A shows that the median firm employs 4 workers and has been operating for 5 years. The typical owner has experience recruiting and managing workers. Most firms are registered with the local authority. The gender distribution of owners is slightly skewed towards males. Panel B shows that hairdressing is the most common trade, making up 30% of the sample.[8] Panel C shows that most firms are located in the region of Kampala, the capital city.

We decided to focus on SMEs for two reasons. First, the majority of the labor force in Uganda and in developing countries more broadly is employed in small firms (Hsieh and Olken (2014)). SMEs operating in our six sectors are very common in urban Uganda, and most Vocational Training Institutes (VTIs) offer courses in these sectors. Second, SMEs in this context might be particularly affected by information frictions at recruitment given their limited access to screening technologies and recruitment agencies or platforms.

At the end of each interview in the census, firm owners were asked whether they would be interested in participating to the BRAC Job Placement Program: they were told that as part of the program BRAC would facilitate job interviews with recent graduates from VTIs, interested in looking for employment in their sector and region. Importantly, firm owners were not told anything about the skills measurement and signalling component of the

[6] The urban areas are: Bugembe, Buwenge, Fortportal, Gulu, Kamuli, Kasese, Kibuli, Kireka, Kyengera, Makindye, Mbale, Mbarara, Nansana, Njeru, Nyendo, Ojwina and Zana. The census took place within a 4km radius from the local BRAC branch.

[7] The reason why we set the minimum firm size at 2 workers was to restrict the sample to firms that had experience in the labor market and were used to hiring workers. We did not impose eligibility conditions related to whether the current employees were family members of the owner.

[8] This is in line with findings from other censuses of SMEs in urban Uganda (cfr. Alfonsi et al. (2016)).

intervention.[9] Information was recorded on whether firms were interested in participating. All interested firms were then administered a baseline survey: the 422 firms that completed the survey and confirmed their interest in the program form our final research sample.

Since we have background information on all the firms included in the census, we can study the determinants of selection into the final research sample. We regress an indicator variable for whether the firm was included in the final research sample, on the main observable characteristics collected in the census. Table A1 reports the results: smaller firms are more likely to be interested, and the sector dummies for carpentry, motor-mechanics and welding are positive and significant, relative to hairdressing (the omitted category).[10] Interest in the program can be interpreted as an indicator of unmet demand for labor, and so Table A1 suggests that this might be higher in smaller firms and in firms in male-dominated sectors.

2.2 Worker Census

We defined as eligible for the project all trainees currently enrolled at 15 partner Vocational Training Institutes (VTIs) in one of the six sectors covered by the project, and expected to graduate in time for the placement intervention (i.e. by February 2015).[11] We conducted an initial survey of all the 1011 eligible trainees. The survey was administered before any information was given to trainees about the BRAC Job Placement Program.

Table 2 reports basic descriptives for the eligible workers: the median trainee is 20 years old, has completed 11 years of education before enrolling at the VTI, and is undertaking a 2-year course. Training focuses on practical skills: while instructors might talk about the importance of soft skills, there are no formal training sessions on such skills. Most students receive a certificate at the end of training with the results of assessments on their practical skills, while soft skills are not certified in any way by the institutions. The sample is slightly skewed towards males. About a quarter of the sample has any previous work experience, and less than 10% has already secured a job at the end of the training. 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. For

[9]The number of graduates assigned to each firm was left unspecified. Also, details about the VTIs workers could come from were not disclosed at this stage. BRAC is the largest NGO in Uganda, and is well known in the local communities for its programs targeting youth skills and firm financing. The BRAC Job Placement Program described in this paper is a new pilot program, which builds on previous programs targeting youth employment. Given that all firms in the census were operating within 4km of the local BRAC branch, they were most likely already aware of BRAC and its operations. So concerns related to the credibility of BRAC are not first order.

[10]No information was collected on whether the firm had a vacancy at the time of the census, so it is not possible to study how this predicts selection.

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

these reasons we can expect the typical trainee to look for jobs in SMEs after training.[12] The distribution of trainees over sectors shows that the most popular training courses are hairdressing and motor-mechanics.

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, that has the second lowest median age in the world (UNAIDS 2010). Second, young workers might be particularly affected by informational frictions at recruitment, given their lack of work experience.

In addition to basic socio-demographics, the survey included two measures of skills: (i) a cognitive test, and (ii) a Big 5 questionnaire, aimed at measuring the soft skills of trainees along five dimensions: agreeableness, conscientiousness, extraversion, neuroticism and openness to experience.[13] More details on the measurement and distribution of these skills are given in Appendix B2.

2.3 Selection of Workers into the Final Research Sample

After completing the survey, all trainees in the census were informed about the BRAC Job Placement Program. Importantly, they were described both the matching and signalling components of the intervention. They were told that BRAC would offer to schedule job interviews with potential employers among SMEs. In addition, they were told that BRAC would conduct additional skills measurements – on both cognitive and soft skills – and that the information collected during the assessments might be disclosed to potential employers during the matching process. Trainees were then asked whether they were interested in participating to the intervention.[14] All interested trainees were administered a baseline survey and were included in the skills assessment exercizes: the 787 trainees that answered the survey confirming their interest in the intervention, and have complete information on the results of the main skills assessments, comprise the final research sample for the intervention.

Since workers were informed about the signalling component of the intervention, a natural question is to what extent this affected their selection into the final research sample: if workers are aware of their skills, then we can expect that workers with a higher level of skills might have higher perceived returns to participating to the signalling intervention.

Table 3 provides evidence on this, by reporting the results of regressions where an indicator for being included in the final research sample is regressed on the skills and other

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

[13] The Big 5 are five basic dimensions of personality. See John and Srivastava (1999) for a review of the main concepts and methods related to the definition and measurement of the Big 5 traits.

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

background characteristics of all the trainees in the census. Column 1 shows that while there is no correlation between cognitive skills and interest in the program, the Big 5 factors are significant predictors in this basic specification that does not control for additional worker characteristics: as shown at the foot of the Table, the joint F-test of significance of the Big 5 variables has associated *p-value*=.002. The selection is *positive*, so that workers with a higher level of soft skills are more likely to be included in the final sample.[15] The Big 5 variables remain jointly significant at the 1% level in Column 2, once a number of controls are added to the regression.

To study where in the skill distribution the selection is coming from, we focus on the three Big 5 skills that remain significant in Column 2 of Table 3: agreeableness, conscientiousness and neuroticism, and study their distribution, by whether the worker was included in the final research sample. The results are reported in Figure 2, which shows that all three distributions are shifted to the right for workers that self-selected in the final research sample. This confirms that the positive selection is all along the skill distribution. In particular, Figure 2 highlights that workers with a *very low* level of soft skills are less likely to participate. We further probe this finding by calculating the average of the Big 5 traits for each worker, and plotting the distribution of this summary variable, by whether the worker is interested in the intervention. The results are reported in Figure A2, and again confirm that workers at the lower end of the distribution are selecting out of the program.

This self-selection implies that the sample of workers that are matched to firms are *positively* selected on soft skills. Firm owners were not informed about this selection process of trainees into the applicant pool, so if they expect the matched workers to be a random sample of VTI graduates, then the revelation of information might provide more positive news than it would do if the sample of graduates was representative of the VTI population. At the same time, since workers at the low end of the distribution are selecting out of the intervention, we will not be able to estimate the impacts of the intervention for these workers. However, we will use evidence on the importance of soft skills in our sample of firms, along with a model, to characterize what the impacts of the information revelation would be at the low end of the skill distribution.

2.4 Key Facts about SMEs at Baseline

We conducted a baseline survey of the SMEs included in the final research sample, to learn about the key features of the labor market and the way these firms operate. We now present five key facts that emerge from the survey. These are used to justify our research design

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

and empirical strategy. More details on each of these key facts, as well as the supporting evidence, can be found in Appendix B1.

1. Soft skills are perceived by firm owners as having relatively high returns. In particular, soft skills are reported as more important than numeracy, literacy or theoretical skills. This matches evidence that soft skills have high returns in the US labor market (Heckman et al. (2006), Deming (2015)).

2. Firm owners report stealing by their employees and difficulties in observing the soft skills of workers among their main perceived constraints. Employee stealing is reported as the most important constraint, among a list that includes also access to finance, one of the main constraints that the literature has focused on.[16] Difficulties in assessing the soft skills of workers are reported as more important than lack of demand, access to electricity, difficulties in finding workers, or screening on practical skills.

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

4. It is common for firms to recruit workers that just show up and ask for a job, without any prior connection or referral: we document that over one-third of the workers employed at baseline in our sample of firms were hired in this way.

5. There is substantial heterogeneity in the managerial ability of firm owners: we find that firm owners with higher cognitive ability have significantly higher profits per worker, and so we interpret this as an indication of higher managerial ability.

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 damage to the firm by, for example, stealing. Since soft skills are reported as difficult to observe, then this can create adverse selection in hiring. Given the relatively low priors of firm owners on the distribution of soft skills among workers, the third key fact suggests that the signalling intervention might be especially beneficial for those workers that can now credibly signal they are not among those types that firm owners are afraid of hiring. The fourth key fact indicates that an intervention that aims to introduce workers to firms they have no prior contact with is likely to be informative of the regular hiring process in the

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

labor market. The final key fact suggests that the cognitive ability of firm owners might be an important source of heterogeneity in the results: firm owners with higher ability seem to be better managers, and so we might expect them to respond differently to the intervention.

3 Intervention and Research Design

We now describe the three components of our intervention. The first is a screening component, whereby information was collected on the soft skills of workers in our final research sample while they were still enrolled at the VTIs. The second is a matching component: this took place after the workers had graduated from the VTIs, and consisted in scheduling job interviews with the firms participating in the study. The third is the introduction of *experimental variation* in whether information from the screening assessments was disclosed to *both* workers and firms during the matching process, through the provision of certifications.

3.1 Worker Screening

The term "soft skills" encompasses a wide range of skills related to work ethic and interpersonal behavior. During our initial focus groups with firm owners,[17] we identified seven specific soft skills that were reported as potentially relevant in our context: creativity, communication, discipline, attendance/time-keeping, trustworthiness, willingness to help others/prosociality and pro-activity.[18] These are the skills we target in our screening activities.

From the discussion in the previous section it should not be surprising that trustworthiness was mentioned as an important soft skill. Also, existing research suggests that firms value discipline and that absenteeism is a widespread phenomenon in developing countries.[19] Creativity is to be intended both as the ability to generate new ideas, but also as the ability to come up with creative solutions to problems. This is something relevant for *all* trades in our study, as workers are often asked to use in an ingenious way the limited tools available. In addition, it is common for employees to work in teams, and to take care of customers. This can explain why skills such as communication, willingness to help others and pro-activity

[17] We included in the focus groups SMEs operating in the same sectors considered for our study.

[18] We decided not to focus the signalling component of the intervention on the Big 5, as our piloting exercises revealed that certifications reporting the results of such skills would have been more difficult to understand for firm owners and workers. So we preferred to focus on skills that could be more easily mapped into behavior, such as communication skills, as these were found to be easier to convey through certifications. We discuss the correlation between the specific soft skills we focus on and the Big 5 later in this section.

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

were mentioned as relevant during the focus groups.

To measure those skills that are easier to assess for an external examiner – attendance, communication skills, willingness to help others and pro-activity – we use teacher surveys. To measure creativity and trustworthiness, which are harder to assess based on in-class behavior, we create our own assessments: to elicit creativity, we develop a battery of questions with the help of a local psychologist; to elicit trustworthiness, we make trainees play trust games with real money. Appendix B2 reports more details on the skills assessment procedures.

The information revelation was limited to five skills, to address any concerns related to attention constraints of firm owners and workers.[20] We base our choice of the skills to be revealed on Treatment certificates on the stated preferences of firm owners in the baseline survey over which skills they would find most useful to receive information on during interviews. Specifically, we focus on: creativity, communication skills, trustworthiness, willingness to help others and attendance. More details on how the five skills are selected can be found in Appendix B3. To facilitate the reporting of information on skills, we follow the Ugandan education system, and grade each skill on a A-E scale. Grades were given using an absolute scale, and so were *not* curved within our sample. Appendix B4 reports the details of the grading procedure.

It is important to note how the grading procedure links back to the positive selection on the Big 5 traits documented in Section 2: since the grades are not curved within our sample, if they are positively correlated to the Big 5 traits, then this would suggest that our grade distribution is *positively skewed* relatively to the entire VTI population. Table A4 reports the results of regressions where the dependent variables are the five soft skills measured in the assessments, and the independent variables are the Big 5 measures.[21] We document a positive correlation between the Big 5 traits and most of the other soft skills. In particular, conscientiousness and agreeableness, which predict selection in the final sample, are also positively correlated to, respectively, trustworthiness and creativity. This suggests that our sample of workers is positively selected not just on the Big 5, but also on at least some of the soft skills that are revealed during the intervention. Figure A10 reports the distribution of grades for the five soft skills: indeed, we see that extremely few workers in our sample are awarded the lowest grade of "E" on any of the skills.

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

[21] The regressions also control for: a dummy for female, age and age squared at baseline, a dummy for whether the worker has any work experience, duration of VTI course, prior formal education level, dummies for VTI attended (14 dummies), sector of training dummies (5 dummies), a dummy for whether the trainee scored on or above the median on a 10-item Raven matrices cognitive test.

3.2 Randomization and Matching

The second component of the intervention involved scheduling job interviews between workers and firms, that is, determining the matching allocations. This was done in three steps: (i) firms and workers were allocated to "submarkets", defined over sector and region; (ii) firms and workers were randomly allocated to Treatment and Control groups within each submarket; (iii) workers and firms were randomly matched within each submarket and treatment group. Appendix C reports details of the randomization and matching procedure.

The matching allocations produced a total of 1230 scheduled worker-firm matches: 616 in Treatment, and 614 in Control. The median firm (worker) was matched with 3 workers (1 firm). There are no cross-treatment matches: Treatment firms (workers) only meet Treatment workers (firms). On the other hand, Control firms (workers), only meet Control workers (firms). Both Treatment and Control got the matching component. The difference between the two groups lies only in the signalling component.

The randomization produced a balanced sample. Table A5 reports balance checks on the firm side, and shows that our sample is balanced on 9 of the 10 variables considered.[22] Importantly, the sample of firm owners is balanced on their score of the cognitive test. Also, when we regress an indicator variable for whether the observation is in Treatment, on all the 10 variables considered, we are not able to reject the null hypothesis that the regressors are all jointly insignificant in predicting Treatment assignment. This is shown by the *p-value* on the joint F-test at the bottom of Column 2 in the Table (*p-value*=.520). Finally, Table A6 shows that the randomization produced a balanced sample also on the worker side, both in terms of worker characteristics (Panel A and B), as well as matching assignments (Panel C). Importantly, the sample is balanced on skills, as shown in Panel B.

3.3 Skills Signalling

Only workers and firms in the Treatment group receive the signalling component of the intervention: for these workers, we created transcripts reporting their grades on the five soft skills. Panel A of Figure A11 shows an example. The order of the skills on the transcripts was randomized. On the back page, the transcripts reported a brief description of the skills assessment procedure,[23] as well a guidelines to interpret the grades. To stress the credibility of the certification, the front page reported the signatures of two high BRAC officials.[24]

[22]The normalized difference between Treatment and Control for the other variable (i.e. whether the owner has received training from a VTI) is small (.12). This limits concerns related to the sample being unbalanced on this specific variable.

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

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

To control for any effects of simply releasing any new transcripts, a placebo transcript was produced for workers in Control. An example is shown in Panel B of Figure A11: the document simply states that the trainee was willing to be put in contact with potential employers. It reports no information at all about the grades on the soft skills or about the skills assessment procedure (the word "skills" is not mentioned anywhere on the document). Otherwise, the transcript is identical to the Treatment one.

The exact timing of the revelation of the transcripts was the following (the procedure was the same in Treatment and Control). On the interview day, the worker was first met by the BRAC staff at the local BRAC branch, where the BRAC staff showed the transcript and explained its content to worker. The worker was also informed that the firm owner would be shown the same transcript at the start of the interview. The worker then had the option to select out, in case she had changed their mind about wanting to meet the firm owner. In practice, only 2 workers met the BRAC staff but decided not to proceed to the job interviews. The worker was then introduced to the matched firm by the BRAC staff, who made sure that the firm owner was also shown the transcript. The transcript was then left to the worker to keep. After the initial introduction, the firm owner and the worker were left to interact as they pleased, and the BRAC staff played no further role in the interview.

4 Empirical Strategy

Our empirical analysis proceeds in two stages. First, we study whether firm owners and workers react to the disclosure of information on skills by updating their beliefs. Second, we study how the updating of firms and workers turns into impacts on the probability that a worker receives a job offer and is hired by the matched firm.

We allow for heterogeneous effects by worker skills and firm owner ability: we can expect the impacts of the intervention to depend on the skills of workers; in addition, the last key fact discussed in Section 2.4 suggests that heterogeneity by the ability of firm owners could be important. We reduce the skills dimensionality problem through principal component analysis: workers with a first principal component of the five soft skills reported on the certifications at the median or above are assigned to the *High Score* group, while workers that are in the lower half of the distribution are assigned to the *Low Score* group.[25] We use the score of firm owners on a Raven matrices cognitive test to divide them into a *High Ability*

further limits any concerns about the credibility of the certifications.

[25]We prefer to use principal component analysis to factor analysis as our primary objective is to reduce the dimensionality of a set of correlated variables, and not to test a model of how latent factors affect observed variables. In practice, using factor analysis rather than principal component analysis produces very similar results.

group (if they scored on or above the median), and a *Low Ability* group (if they scored below the median).

4.1 Main Regression Specification

Our main estimating equation takes the following form:

$$y_{ij} = \beta_0 + \beta_1 HS_i + \beta_2 LS_i \times T_{ij} + \beta_3 HS_i \times T_{ij} + \gamma \mathbf{X}_i + \delta \mathbf{X}_j + \alpha \mathbf{Int}_{ij} + \nu_{ij} \tag{1}$$

Where y_{ij} is the outcome of the worker i, firm j match, for example whether the job interview resulted in a job offer to the worker. HS_i is a dummy equal to one if the worker ranked at the median or above in the distribution of the first principal component of skills. LS_i is a dummy equal to one if the worker ranked in the lower half of such distribution (this variable is defined as $1 - HS_i$). T_{ij} is a Treatment group indicator. $\mathbf{X}_i$ is a vector of baseline worker controls.[26] $\mathbf{X}_j$ are baseline firm controls.[27] $\mathbf{Int}_{ij}$ are interview controls.[28]

There are three main coefficients of interest: β_1 can be interpreted as the (conditional) difference in the outcome variable between *High Score* workers and *Low Score* workers in Control. β_2 and β_3 instead allow us to detect differences between Treatment and Control: β_2 is the difference in outcomes between Treatment and Control for *Low Score* workers; β_3 is the difference in outcomes between Treatment and Control for *High Score* workers. We interpret a significant estimate of β_2 or β_3 as the impact of the revelation of information during the matching for the different types of workers. To test for the impact of Treatment on the *average* worker, we run similar regressions, which control for the HS_i and T_{ij} dummies separately, and do not include their interactions. The treatment effect on the average worker is then given by the coefficient on the T_{ij} dummy in these regressions.[29]

Our main results are estimated on the full sample of workers and firms originally *assigned* to be matched, by giving a value of zero to the dependent variables for those matches that were never carried out. In doing this, our estimates of β_1, β_2 and β_3 recover Intention To Treat (ITT) parameters. One important exception is when the independent variable are the stated beliefs of firm owners on the skills of the matched workers, as obviously these are

[26] These include: dummy for female, age and age squared, dummy for any work experience, VTI course duration, completed formal education, dummies for three largest VTIs, dummy for whether trainee scored on the median or above on the cognitive test administered at baseline.

[27] These include: dummy for female owner, business age and age squared, number of employees, dummies for region of operation, dummies for sector of operation.

[28] These include: dummies for month of interview, number of firms matched to worker, number of workers matched to firm, dummy for whether the worker was matched in a submarket different from the preferred one.

[29] The exact specification is the following: $y_{ij} = \zeta_0 + \zeta_1 HS_i + \zeta_2 T_{ij} + \gamma \mathbf{X}_i + \delta \mathbf{X}_j + \alpha \mathbf{Int}_{ij} + \nu_{ij}$.

recorded only for those matches that actually took place. So for this outcome we report results on the selected sample of matches that took place (these are 42% of the matches that were originally scheduled). In this case the parameters should be interpreted as the Average Treatment Effect (ATE) of the intervention. Estimates on this selected sample are subject to any potential bias resulting from non-random selection into the sample of workers and firms that actually meet. We address this concern in Section 7.3 later in the paper. We do not find evidence of significant bias when explicitly controlling for selection through a control function approach. This justifies showing the results on the selected sample without controlling for selection for this specific outcome.

Since we are interested in heterogeneous effects by owner ability, we run regressions like (1) separately for the sample of matches with *High* and *Low Ability* owners.[30] To test whether the coefficients are different across the two samples, we estimate a fully interacted model where each variable in equation (1) is interacted with a dummy equal to one if the owner has high cognitive ability. We find some evidence of selective attrition from the experiment, based on Treatment and our measures of soft skills. To correct for this, in the specific samples where selective attrition is documented, our regressions are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge (2010). More details on attrition and how we correct for it can be found in Appendix D.

5 Impact of the Intervention on Beliefs

We begin the empirical analysis by studying whether the intervention leads firm owners to update their beliefs on the skills of the matched workers, and whether workers react to the information by updating their stated outside options. As documented in Section 2.4, soft skills are reported as important but difficult to observe by firm owners. So we can expect the signalling intervention to impact their beliefs on the skills of workers. On the other hand, if workers attach a positive signalling value to the certifications, or learn more information about the returns to their skills through the intervention, then this could affect their outside options. Therefore, it is possible that *both* sides of the labor market update their beliefs as a result of the information revelation.

[30] In 92% of the cases the job interviews were conducted by the firm owners themselves. This justifies focusing on the cognitive ability of the firm owner as a main potential source of heterogeneous effects.

5.1 Do Firm Owners Respond to the Information?

At the end of each job interview, firm owners were asked whether they thought the worker they had just met was similar or different to the workers that typically come looking for a job at their firm. In case owners thought the worker was different, they were also asked to report why that was the case. We use this information from the matching surveys to construct a dummy equal to one if the firm owner reported the matched worker as *more skilled* than usual applicants. We run OLS regressions analogous to (1) using this as dependent variable. As discussed above, this variable is available only for those matches that actually took place, and so these regressions are run on this selected sample of observations.

Table 4 reports the results. As shown in Column 1, there is no impact of the intervention on this outcome, when the full sample of matches is considered. However, this result masks important heterogeneous impacts. Columns 2 and 3 report sample splits for matches with *High* and *Low Ability* owners. Focussing on Column 2, we notice that *High Ability* owners interpret the revelation of information as *positive news* for the average worker. This is shown by the positive and significant estimate of the treatment effect on the average worker reported in Row (v). The positive updating is stronger for *High Score* workers, which are 14pp more likely to be reported as more skilled than the usual applicant, relative to similar workers in Control, a result significant at the 5% level. This corresponds to a more than twofold increase. The estimate of β_2 in Column 2 is also positive and large, though not significant. There is no evidence that the revelation of information leads *High Ability* owners to revise their beliefs downwards for *Low Score* workers. If anything, the updating is positive also for these workers. On the other hand, Column 3 shows that there is no impact at all of the signalling intervention on the beliefs of *Low Ability* owners. The differences between *High* and *Low Ability* owners are significant at the 5% level, as shown by the *p-values* in Column 4, which are estimated from a fully interacted model. This suggests that *Low Ability* owners are not able to interpret or do not value the information on the transcripts.

It is important to link back the positive updating of *High Ability* owners to the selection of workers into the experiment and to firm owner priors: earlier in the paper we documented that workers in the final research sample are *positively selected* on soft skills. In addition, we discussed that firm owners have relatively *low priors* on the distribution of soft skills in the population. As they were never informed about the positive selection of workers into the experiment, then this can explain why the transcripts are interpreted as mostly positive news by firm owners. These results are in line with the findings of the job testing literature (Autor and Scarborough (2008), Hoffman et al. (2015)) which shows how the introduction of job tests increases the ability of managers to identify and select workers with *higher* skills.

5.2 Do Workers Respond to the Information?

We now turn to documenting the impacts of the intervention on the perceived outside options of workers. We can expect such an impact because a copy of the transcript was left to the workers to keep after the job interview. So if workers believe that the transcript has a positive signalling value in the labor market, or if they learn additional information on the returns to their skills through the intervention, then this could affect their outside options.

We use data from the worker follow-up survey, conducted one year after the intervention. We focus on three proxies for outside options: (i) whether the worker is currently enrolled in training or education; (ii) the reported (monthly) reservation wage;[31] and (iii) whether the main activity in the week prior to the survey is casual work. The advantage of using the reservation wage (compared to total earnings) as a proxy for the outside option is that this is available for *every worker*, regardless of their employment status. Observations are at the worker level, and we are interested in documenting heterogeneous effects by whether workers met *High* or *Low Ability* owners. Since some workers met more than one firm, we restrict attention to workers who were assigned to meet either only *High Ability* owners, or only *Low Ability* owners. So we exclude workers that were matched to a mix of the two owner types.[32] We then estimate an equation analogous to (1) but at the worker level.

The results are reported in Table 5: in Columns 1-4 the dependent variable is a dummy equal to one if the worker is currently enrolled in education or training; in Columns 5-8 the dependent variable is the log of the monthly reservation wage; in Columns 9-12 the dependent variable is a dummy equal to one if the main activity in the week prior to the survey was casual work. We find that the signalling intervention leads *High Score* workers who met *High Ability* owners to revise their outside options upwards. Our estimate of β_3 in Column 2 indicates that *High Score* workers who met *High Ability* owners in Treatment are 15pp more likely to be back in training or education one year after the intervention, relative to similar workers in Control, a result significant at the 5% level. This corresponds to almost a threefold increase. In addition, as shown in Column 6, the same group of workers report monthly reservation wages that are about 22% higher than similar workers in Control, a result significant at the 10% level. Finally, the results in Column 10 reveal that *High Score* workers who met *High Ability* owners are significantly less likely (at the 1% level) to be involved in casual labor as their primary activity. These results are consistent with these workers trying to transition to better jobs, where their skills might have higher returns.[33]

[31] This is measured by asking workers the minimum monthly wage they would need to accept a job.

[32] The workers that are excluded are about one third of the sample. Including these workers in the analysis by adding them to the group that only met *High Ability* owners does not change the results substantially. This confirms that meeting at least one *High Ability* owner is sufficient for the updating to take place.

[33] Another way to test if the certifications affected the outside options of workers would be to study their

We find no evidence of an impact of the intervention on the outside options of *Low Score* workers, even those meeting *High Ability* owners. Our estimate of β_2 in Column 2 is small and insignificant. In Column 6 and 10, we notice that the impact of the certifications is significant for the average worker, as shown in Row (v). However, this effect is driven by workers with a higher level of skills: the estimate of β_2 is not significantly different from 0 in either of these specifications. Interestingly, we find no impacts of the signalling intervention on the outside options of *High Score* workers who met only *Low Ability* firm owners.[34] This suggests that workers respond to the new information only if they are matched to firm owners that react to such transcripts in the first place. This is a plausible result in this context: the transcripts that we introduce are a *new* certification, and so if workers are uncertain of their value, they might take the reaction of firm owners as an indication of their importance or credibility. Alternatively, workers might interpret the positive response of firm owners to the certifications as a positive signal about the returns to these skills in the labor market. Our design and data do not allow us to disentangle these two explanations.[35]

These results highlight one important difference with the job testing literature. Since the outcomes of job tests are typically not disclosed to applicants, the literature on job testing does not consider possible responses of workers to the information contained in the screening tests (Autor and Scarborough (2008), Hoffman et al. (2015)). By contrast, our analysis shows a significant response on *both* the firm side and the worker side to the information embedded in screening assessments, once these are made available during interviews.

6 Mapping the Impacts on Beliefs to Offers and Hires

In the previous section, we showed the following reactions to the information revelation: (i) firm owners respond by revising *upwards* their beliefs on the skills of workers; (ii) workers with higher skills respond by increasing their perceived outside options. In this section, we develop a simple screening model with two-sided updating that links the response of firms

impact on wages earned at the matched firm. However, as shown later in the paper, only 40 workers in total started a job at the matched firm, and so this limits the scope of any analysis on wages (or tenure) conditional on hiring. Nevertheless, in Table A10 we report details of the wages earned in the first week of work, for those workers that did start a job at the matched firm. While any conclusions based on this Table are subject to the caveat of the very small sample size, the data reveals that wages are higher in Treatment, and especially so for *High Score* workers. So this evidence is once again in line with an impact of the intervention on the outside options of workers, which is reflected in their wages at the matched firm.

[34] The difference in impacts between workers matched to *High* and *Low Ability* owners is significant for the education outcome, as shown by the *p-values* in Column 4, but we lose precision in estimating heterogeneous treatment effects for the other two outcomes, as shown by the *p-values* in Columns 8 and 12.

[35] It is less likely that workers learn about their skills through the intervention: we documented that workers with higher skills are more likely to participate, thus suggesting that they are already aware of their skills.

and workers in terms of beliefs, to impacts on job offers and hires. The model formalizes the adverse selection problem, and makes precise how the certificates can generate *non-linear* impacts on job offers and hires along the skill distribution, due to the reaction of workers in terms of their outside options. At the same time, the model highlights how the revelation of information would impact those workers at the very low end of the distribution, that have selected out of the intervention. We use the predictions of the model to interpret the reduced form impacts of the intervention on job offers and hires, and to inform the welfare implications of introducing a mandatory certification policy on soft skills later in the paper.

6.1 Model Setup

We consider a one-period model. The economy is populated by risk-neutral workers and firms. Firms are homogeneous. There are three types of workers: H workers have high skills, and produce output $y_H > 0$; L workers have low skills, and produce output $y_L > 0$, with $y_H > y_L$; *VL* workers have very low skills, and produce *negative* output $y_{VL} = -d$. The interpretation is that VL workers can steal, can upset customers by having poor communication skills, or can create tension in the firm by not being able to relate to the other co-workers. For ease of exposition, we assume that each worker type represents one third of the worker population. Workers know their skills, while firms do not. H workers have outside option $u_H > 0$; L workers $u_L > 0$, with $u_H > u_L$; and VL workers $u_{VL} = 0$. Outside options are exogenously given, and represent the continuation value of workers.[36] The outside options take into account the information available to workers on the returns to their skills, and their ability to signal their skills in the labor market. Workers and firms are randomly paired for job interviews. This follows our experimental design, where random matching is implemented. Each firm only meets one worker. Each worker only meets one firm. There is no rematching.

Interviews generate a signal S on the skills of the worker. The signal can be: *Good* (G), *Bad* (B) or *Very Bad* (VB): $S \in \{G, B, VB\}$. The worker and the firm observe the same signal. This is in line with our experimental design, where firms and workers in Treatment are *both* revealed the same information during the job interview. Let q be the probability that the signal is correct, that is: $q = P(G|H) = P(B|L) = P(VB|VL)$, with $q \in [1/3, 1]$. If $q = 1/3$, signals are not informative; the closer q is to 1, the more informative signals are about the worker type. Each worker can send one correct signal, and two wrong signals. For example, in the case of H workers the two wrong signals are the B and VB signals. Assume that the two wrong signals are equally likely, so that the probability that a worker sends

[36] For instance, the outside options can be interpreted as employment opportunities in other sectors of the economy, self-employment, migration, or further education.

each of them is simply: $(1-q)/2$. Further assume that the value of q and the share of worker types are common knowledge. Given signal S, firms compute the posterior probability of the worker type, using Bayes' rule.[37] Since the share of each worker type is equal to $1/3$, q is also equal to the posterior probability.

Upon meeting a worker, firms experience a match-specific monetary taste shock θ for the worker, with $\theta \in [\underline{\theta}, \overline{\theta}]$, and $E[\theta] = 0$. After observing signal S and shock θ, a firm decides whether to make a job offer to the worker. Workers must be paid at least their outside option to accept a job. Firms know the outside options of the different worker types (though they might be uncertain of which worker type they have in front). Assume that firms can set piece-rate wages to the amount w such that H and L workers are indifferent between accepting this piece-rate wage and taking their outside option.[38] That is, assume that $wy_H = u_H$ and $wy_L = u_L$. Further assume that workers have limited liability, so VL workers earn $w = 0$ but cannot compensate the firm for the loss of output. Define $\pi_i = (1-w)y_i$, where $i \in \{H, L, VL\}$. Upon observing signal S, firms compute the expected profits from making a job offer to the worker, $E[\Pi|S]$. These take the following values, depending on whether a G, B or VB signal is realized:

$$E[\Pi|G] = q\pi_H + \frac{(1-q)}{2}[\pi_L + \pi_{VL}] \tag{2}$$

$$E[\Pi|B] = q\pi_L + \frac{(1-q)}{2}[\pi_H + \pi_{VL}] \tag{3}$$

$$E[\Pi|VB] = q\pi_{VL} + \frac{(1-q)}{2}[\pi_H + \pi_L] \tag{4}$$

Equation (2) says that with probability q the worker who sends the G signal is indeed a H type, in which case she generates profits π_H; with probability $(1-q)/2$ she is a L type, in which case profits are π_L; finally, with probability $(1-q)/2$ she is a VL type, and so creates *negative* profits π_{VL}. Equations (3) and (4) can be described similarly. Firms make a job offer if $E[\Pi|S] + \theta > 0$, and reject the worker otherwise. Note that as long as $q > 1/3$, then $E[\Pi|G] > E[\Pi|B] > E[\Pi|VB]$, because $\pi_H > \pi_L > \pi_{VL}$. Therefore, firms make the highest expected profits from workers who send a G signal; they make lower profits from workers who send a B signal; and make the lowest profits from workers who send a VB signal.[39]

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

[38] 74% of the workers employed in our sample of firms at baseline are paid piece-rate.

[39] Because of symmetric information, the model predicts that all offers should be accepted. To rationalize rejected offers in the data, it suffices to assume that offers are accepted with exogenous probability δ. For instance, workers might get a negative health shock or a family problem that prevents them from starting

6.2 Model Predictions

We first present the predictions on job offers along the skill distribution in Control. We then discuss the comparative statics with respect to Treatment. To guide the exposition, Figure 3 reports a graphical representation of the predictions. Starting from Control, the model set up delivers one immediate prediction.

Prediction C1. *If firm owners are not able to extract informative signals on skills in Control, that is, if $q = 1/3$, then all worker types are equally likely to receive a job offer. On the other hand, if firm owners can extract informative signals, that is, if $q > 1/3$, then H workers are more likely than L workers to receive a job offer, and L workers are more likely than VL workers to receive a job offer.*

Panel A of Figure 3 shows this prediction in the case that $q = 1/3$: if firms are not able to screen, then all worker types are equally likely to send any signal, and so worker skills do not predict the probability of job offer. Panel B shows the prediction when $q > 1/3$: if firms owners are able to extract informative signals, then H workers are more likely to send a G signal, compared to L and VB workers. Since firms make the highest profits from workers who send a G signal, then this explains why H workers have the highest probability of job offer. A similar reasoning explains the rest of the prediction.

Moving to the comparative statics with respect to Treatment, in the previous section we showed that the certificates impact the beliefs of firm owners on the skills of the matched workers. At the same time, we found evidence of a positive treatment effect on the outside options of workers with higher skills. In line with these results, we interpret the Treatment as having two effects in the model. First, it eliminates the uncertainty on skills, so that $q = 1$ in Treatment. Second, it increases the outside option of H workers to $u_H^{'} = u_H + \epsilon$. We interpret the impact on the outside option as coming from the positive signalling value of the certifications, or from workers updating their beliefs on the returns to their skills. We first discuss the comparative statics assuming that the Treatment only impacts q. We then show how these would change if the Treatment also increases the outside option of H workers.

Prediction T1. *If the only impact of the Treatment is to eliminate the uncertainty on skills, so that $q = 1$, then H workers are more likely to get an offer in Treatment, than in Control, and VL workers are less likely to get an offer in Treatment, than in Control. The prediction is ambiguous for L workers: offers to L workers can increase or decrease in Treatment, relative to Control.*

work. The introduction of this additional parameter would not alter the firm's problem substantively.

Panel C of Figure 3 shows this prediction, in the case in which job offers increase for L workers. The result for H workers follows from the fact that since firms make higher profits from H workers, compared to the other worker types, then H workers must have a higher probability of receiving a job offer once the uncertainty on their skills is revealed. A reasoning opposite to this explains why offers decrease for VL workers: these workers create a loss to the firm, so once the firm can perfectly identify these workers, we should see the probability that they receive an offer get closer to 0, and be exactly 0 if we assume that $\overline{\theta} < -y_{VL}$, so that taste shocks for the worker cannot compensate for the loss of output.

The reason why the prediction on L workers is ambiguous is the following. In Control, there is a chance that a L worker sends a G signal, which would increase her chances of getting a job offer. In Treatment, L workers have no chance of sending a G signal, so this creates a negative effect on their probability of receiving a job offer. At the same time, in Treatment firm owners are sure that L workers will not destroy output, and so this creates a positive effect on their probability of receiving an offer, relative to Control (where there is a chance that L workers send a VB signal). Whether the probability of receiving an offer increases or decreases in Treatment, relative to Control, depends on the differences in output produced by the three worker types. Intuitively, the higher the damage caused by VL workers, the more the revelation of information will put upward pressure on the probability that L workers receive an offer. On the other hand, the larger the difference in output between H and L workers, the more the revelation of information will put downward pressure on the probability of offer for L workers.

We now consider how the comparative statics would change if the Treatment also induces a positive impact on the outside options of H workers, which we assume to be observed by the firm. This change only affects that part of Prediction T1 that concerns H workers, while the rest of the prediction is unaffected.

Prediction T2. *If the Treatment eliminates the uncertainty on skills, so that* $q = 1$*, and increases the outside option of* H *workers to* $u_H^{'} = u_H + \epsilon$*, then* H *workers are more likely to get an offer in Treatment, than in Control if the increase in the outside option is small enough.* VL *workers are less likely to get an offer in Treatment, than in Control. The prediction is ambiguous for* L *workers: offers to* L *workers can increase or decrease in Treatment, relative to Control.*

Panel D of Figure 3 shows this prediction in the case that the treatment effect on the outside option of H workers cancels out any positive effects from the information revelation. The impact of the Treatment on the outside option of H workers does not affect their output at the firm, but increases their wages: H workers need to be paid a higher piece-rate than before to accept the offer. Depending on the size of the outside option shock, the probability that H workers receive an offer in Treatment may not increase, and may actually decrease, relative to Control.[40] Panel D of Figure 3 makes clear how the certifications can create a *non-linear* impact on job offers: workers at the low end of the distribution are hurt by the information revelation, while workers in the middle benefit more than workers at the top, because they do not revise their outside options upwards.

If through the self-selection of workers into the experiment the final sample that is matched to firms does not include VL types, then we should not expect to observe a fall in offers at the low end of the skill distribution. This case is reported in Panel E of Figure 3. In addition, note that if firm owners are not able to read or interpret the transcripts, then we should not expect any changes in Treatment, relative to Control.

Finally, the model makes clear that the overall effects of information on welfare are ambiguous: the additional information allows firms to identify better workers, but can also increase the cost of hiring H workers, through the impact on outside options. At the same time, while the information can help workers with higher skills, it can hurt workers at the low end of the distribution. Whether the information revelation results in an overall increase in welfare is therefore an empirical question. We return to this in Section 8 below.

7 Results on Offers and Hires

We now turn to presenting reduced form evidence on job offers and hires in the intervention. We first map the model to data using non-parametric analysis. We then add parametric restrictions, and estimate equations analogous to (1) for offers and hires.

7.1 Mapping the Model to Data through Non-parametric Analysis

The model states that the impact of the intervention on job offers and hires should vary depending on the skills of workers. Therefore, we begin by running non-parametric regressions of the impact of Treatment along the skill distribution. Specifically, we estimate:

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

$$y_{ij} = m(Score_i) + \epsilon_{ij} \tag{5}$$

Where y_{ij} is a dummy equal to one if the worker i, firm j match resulted in a job offer, and $Score_i$ is the first principal component of the five skills revealed on the transcripts. We use local polynomial regression, and estimate (5) separately for Control and Treatment, on match-level data from the worker follow-up survey. The sample includes all workers and firms that were assigned to meet in the intervention, and so we give a value of zero to the outcome of those matches that were never carried out.

Figure 4 reports the results. The blue line corresponds to the estimate of the $m(\cdot)$ function in Control, while the red line is the estimate for Treatment. Starting from Control, we notice that the probability of receiving an offer is flat and close to zero in the lower half of the skill distribution, but increases in the top half of the distribution. This is in line with Prediction C1 from the model, which states that if interviews provide informative signals, workers with higher skills should be more likely to get an offer in Control, relative to workers with lower skills. In addition, the fact that the probability is flat in the bottom half of the distribution suggests that if VL workers are in the sample, then they are being pooled with workers in the middle of the distribution: if firm owners were able to separate out the very low types, then we should expect the probability of receiving an offer to get closer to 0 at the bottom of the distribution, as shown in Panel B of Figure 3, but this is not the case.

The certificates result in an increase in the probability of offer in the bottom half of the distribution – except perhaps at very low levels of skills. This positive effect is in line with firm owners having updated positively on the quality of workers with relatively lower skills, once the information is revealed. Mapping to the model, this result supports Prediction T1, which states that it is possible that the Treatment increases the probability that L workers get an offer. In addition, this finding suggests that our sample of workers does not include those very low types that firm owners are trying to avoid hiring: once the information makes clear to firm owners that they are not facing these lowest types, they increase their job offers in the bottom half of the distribution.

The null treatment effect at the top of the skill distribution gives support to Prediction T2, which states that the impact of Treatment on offers for workers with high skills is ambiguous, given the effects on their outside option. The documented positive updating of firm owners on the quality of workers with high skills confirms that the null treatment effects for these workers cannot be explained by firm owners not having changed their beliefs.

Figure 5 reports the results of estimation of (5) for *High* and *Low Ability* owners separately. The left Panel shows that the patterns described so far are even stronger in matches with *High Ability* owners. On the other hand, the right panel suggests that the picture for *Low Ability* owners is much less conclusive: while *Low Ability* owners do make offers to workers, it is unclear whether these depend on skills, and whether they respond at all to the introduction of the Treatment. So this again confirms that *Low Ability* owners are less able to screen in Control, and less able to interpret or value the information on the transcripts.

7.2 Parametric Analysis to Test for Heterogeneous Impacts

To test whether the differences documented in the non-parametric analysis are significant, we estimate equations analogous to (1) on the full sample of scheduled matches. Table 6 reports the results: in Columns 1-4 the dependent variable is a dummy for whether a job offer was made; in Columns 5-8 it is a dummy for whether the match turned into a hire.

Focusing first on behavior in Control, Column 1 shows that when firm owners are pooled together, *High Score* workers are 3.4pp more likely to be made an offer by the firm, relative to *Low Score* workers, but the estimate is not statistically significant. However, this effect masks substantial heterogeneity depending on firm owner ability: Column 2 reveals that *High Ability* owners are 8pp more likely to make job offers to *High Score* workers, than to *Low Score* workers, a result significant at the 5% level. This is a large increase, considering that only 1.1% of matches between *Low Score* workers and *High Ability* owners turn into offers in Control. On the other hand, Column 3 confirms that this effect is totally absent for *Low Ability* owners. Overall, the results in Control confirm the evidence from the previous subsection that firm owners prefer workers with higher skills, and are in line with *High Ability* owners being better able to extract information on the skills of workers during interviews, relative to *Low Ability* owners.

Turning to the impact of Treatment, Column 1 confirms a positive treatment effect for *Low Score* workers, while there is no effect for *High Score* workers: our estimate of β_2 is positive and significant, while the estimate of β_3 is insignificant and small. Columns 2 and 3 show that the impacts are driven entirely by matches with *High Ability* owners: Column 2 shows that *Low Score* workers are 12pp more likely to receive a job offer in Treatment, than in Control, a result significant at the 1% level. Column 3 instead reveals that there is no impact of the Treatment on the probability of offers in matches with *Low Ability* owners. The Treatment effect averaged over the skill distribution is positive and significant in both the pooled sample, as well as the sample of *High Ability* owners, as shown in Row (v). These findings confirm that the heterogeneous impacts discussed in the previous subsection are significant, and are consistent with the predictions of the model.

The results for offers carry through to hires, as shown in Columns 5-8. As shown in Column 5, the Treatment increases the probability that *Low Score* workers are hired from

2.3% to 5.4% in the pooled sample of matches, while there is no treatment effect for *High Score* workers. Columns 6-8 confirm that these effects are once again driven by *High Ability* owners, while they are absent for *Low Ability* owners. Comparing the results on offers and hires, we notice that while some offers are rejected, there is no systematic pattern in rejection rates, neither across skills groups, nor across Treatment groups. This provides support to the modelling assumption of exogenous rejections.

Linking our results to the literature, the positive treatment effect on offers and hires for workers in the bottom half of the distribution is consistent with evidence from the US on how more information at recruitment helps job candidates from social groups perceived as having generally low outcomes. For instance, Wozniak (2015) finds that the introduction of drug testing among US employers led to a significant increase in hiring of low-skilled black men, thus reducing the discrimination of employers over this group of workers.

The lack of positive treatment effects on hires at the top of the skill distribution is in stark contrast with the findings of the literature on job testing (e.g. Autor and Scarborough (2008), Hoffman et al. (2015)), which documents how the introduction of job tests increases the ability of firms to hire workers with *higher* skills. However, the results of job tests are typically not disclosed to workers, and so this creates an informational advantage for firms that can be used to extract rents from workers. Our results highlight the very different effects of a signalling intervention in which both sides of the labor market are given more information about the skills of workers, through the impacts on worker's outside options.

Finally, the positive treatment effects on offers and hires suggest that information frictions at recruitment may be an important explanation for why labor market programs matching workers to firms have produced very modest results in developing countries.[41] Our results are in line with the findings of Hardy and McCasland (2015) who claim that the positive impact on employment of a government-sponsored apprenticeship program in Ghana was mainly due to firms using worker self-selection into the program as a screening mechanism to identify high ability workers.

7.3 Controlling for Sample Selection and Robustness

In our preferred specification for job offers and hires, we assign a value of zero to the outcome of job interviews that were never carried out. The estimates of β_1, β_2 and β_3 can then be interpreted as ITT estimates. One potential concern with this approach is that outcomes y_{ij} are actually observed only for the subsample of job interviews that takes place. This is

[41] For instance Groh et al. (2015) show that a matching intervention in Jordan produced non-significant results on employment for young college graduates.

especially true when y_{ij} are the beliefs of firm owners on the skills of workers, as these are clearly only available for those matches that actually took place.

A potential sample selection issue arises because only 42% of the scheduled matches were carried out, and the randomization was conducted on all the workers and firms *assigned* to be matched. Indeed, we find evidence that observable worker characteristics, including skills and Treatment, predict whether a match happens, thus suggesting that the final sample is unbalanced. More evidence on non-random selection is reported in Appendix E.

To quantify the extent to which non-random selection is likely to bias our results, we first run the estimation on the selected sample that meets, without controlling for selection. This recovers an estimate of the Average Treatment Effect (ATE), which might be biased from non-random selection. We then run the regressions on the selected sample, controlling for selection: if the coefficients are stable across the two specifications, then this is evidence that any selection bias is likely to be small; if the coefficients move substantially, then this indicates that selection bias if significantly affecting our results. To control for selection, we estimate a two-sided selection model, using a control function approach in the spirit of Newey (2009). We use as instruments the characteristics of the enumerators that were assigned to contact the firms and workers for the intervention. The estimates on the selected sample controlling for selection recover an unbiased and consistent estimate of the ATE, under the assumption that the model is correctly specified and so, conditional on the control functions, the final sample is balanced and there is no remaining non-random selection on unobservables. More details on the selection model are given in Appendix E.

Figure 6 reports the results without controlling for selection (middle panel), and controlling for selection (right panel). For comparison, results on the full sample assigned to meet are also reported (left panel). Specifically, point estimates and confidence intervals of β_1, β_2 and β_3 are reported for the sample of matches with *High Ability* owners. The estimates in the left panel are from a regression in which about half of the observations receive a zero in the outcome variable as the match was not carried out. This explains why the coefficients increase substantially when moving to the middle panel. The important point from the Figure is that the magnitude of the coefficients is stable when moving from the middle panel, which does not control for selection, to the right panel, which controls for selection. The standard errors increase substantially when controlling for selection: this is not surprising given the small sample, as the control function estimation is inefficient. Appendix E gives more details on the results on the selected sample, with and without controlling for selection.

Finally, Appendix F reports a number of robustness checks on our main results on job offers and hires. In particular, we show robustness of our main conclusions to: (i) repeating the analysis using firm reports rather than worker reports; (ii) running the analysis at the firm level rather than at the match level; (iii) using the average of the grades on the five soft skills as a proxy of worker type, rather than the first principal component; (iv) correcting for non-random attrition from the experiment using the bounding procedure described in Lee (2009); (iv) correcting for multiple hypothesis testing through the procedure discussed in Romano and Wolf (2016).

7.4 Alternative Mechanisms

We discuss the plausibility of two alternative mechanisms for the results on offers and hires. First, in a directed search model,[42] offers to workers with low skills could increase in Treatment because the firm uses the additional information to offer them lower wages, relative to Control. The predictions from this alternative model are not consistent with our findings: in Table 5 there is no evidence that the Treatment induces *Low Score* workers to report lower reservation wages. If anything, *Low Score* workers report slightly *higher* reservation wages in Treatment, than in Control. The same is true for wages earned at the matched firm: Table A10 shows that wages for *Low Score* workers are higher in Treatment, than in Control.

Second, if the placebo transcripts used in Control had a negative effect on outcomes, for example because firm owners are not used to seeing certifications without grades, then there would be a concern that we are overstating the impact of Treatment. We provide two pieces of evidence to limit this concern. First, it is common for training institutions in this labor market to issue certificates without grades, for example certificates of training completion. It is also common for workers to show such certifications during interviews: 30% of workers in Control brought vocational certifications to the interviews of their own initiative, and in half of the cases the certification did not report any specific grades. In addition, the BRAC staff that supervised the job interviews in Control reported no cases of firm owners asking why there were no grades on the placebo certificates, or expressing other concerns about them. If anything, our worry at the start of the project was that the placebo transcripts might be interpreted as a *positive* rather than a neutral signal (i.e. as a form of endorsement of the trainee by BRAC), so that any impacts of the Treatment would be underestimated.

8 Cost-Benefit Analysis

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

[42]On directed search models, see the survey paper by Rogerson et al. (2005).

very different in the mandatory case: we documented that those workers that decided not to participate to the intervention are *negatively* selected on soft skills, and so, as made clear by the model, they could be negatively affected by the certifications.

To conduct the cost-benefit analysis, we first study whether the employment gains documented at the matching stage resulted in any long-lasting impacts on the labor market outcomes of workers. This is important as the cost-benefit analysis heavily depends on the duration of impacts. We use evidence from the worker follow-up surveys. In particular, Table 7 reports the results of OLS regressions analogous to (1) but at the worker level. In Columns 1-4 the dependent variable is a dummy equal to one if the worker reports wage employment as her main activity in the week prior to the survey. In Columns 5-8 the dependent variable is the log of *1+total earnings* in the month prior to the survey, from any activity. This outcome takes value zero if the worker had no earnings in the last month.

Column 2 shows that the impacts of the intervention are persistent for workers that met *High Ability* firm owners: *High Score* workers in Control are 18pp more likely to be in wage employment at follow-up, relative to *Low Score* workers, a result significant at the 10% level. This corresponds to about a 71% increase in the probability of employment. Also, *Low Score* workers in Treatment are about 18pp (71%) more likely to be in wage employment, relative to similar workers in Control. As shown in Table 6, the overall level of hiring in the intervention was low. In addition, very few workers are still employed at the matched firm at follow-up. So most of the gains are coming from workers having found employment in other firms. Coherently with our findings in Table 6, we see no differences in the employment rates of *High Score* workers in Treatment, relative to Control. The findings in Column 3 further confirm that there are no long-lasting differences in the employment rates of workers who met a *Low Ability* owner, depending on their skills. This is true both in Control and Treatment.

Columns 5-8 show that we find no significant impacts of the intervention on total earnings. However, the estimates are very noisy, so we cannot rule out relatively large impacts for any of the groups. Comparing these results with the ones reported in Table 5 reveals that the higher reservation wages of *High Score* workers matched to *High Ability* owners in Treatment have not yet translated into a significant increase in earnings. As we documented that these workers are also significantly more likely to be back in education, the higher reservation wage might take into account the expected future returns to the additional training.

8.1 Cost-Benefit Analysis for Program Participants

We use our estimates of the program impacts on the worker side from Tables 5 and 7 to conduct the cost-benefit analysis of the intervention. We assume that the intervention has zero impact on the firm side. This is supported by the analysis in Appendix G, which shows that we do not find evidence of long-lasting impacts of the intervention on firm size. To the extent that firms experience gains that are not reflected in changes in total number of employees, such as any gains from the signalling intervention having generated more productive matches, then assuming zero impact on firms is likely to be a lower bound. Our data does not allow us to investigate any such gains.[43]

We documented the following impacts for workers matched to *High Ability* owners: (i) an increase in the probability of being back in education for *High Score* workers; (ii) an increase in reservation wages for *High Score* workers; (iii) an increase in the probability of employment for *Low Score* workers. We use these impacts to estimate the total gains of the program, and produce the cost-benefit analysis under various assumptions on the evolution of impacts over time.[44]

Column 1 of Table 8 reports the cost-benefit analysis of the intervention, in January 2015 US$. Panel A reports the external parameters for the calculation. Costs include any expenses incurred to create and distribute the certifications.[45] We set the remaining productive life of individuals at 38 years, computed as average life expectancy in Uganda (58 years) minus average age of the individuals at baseline (20 years).

Rows 1-3 of Panel B report estimates of the change in annual earnings as a result of the program, averaged over the different worker types. The change is zero for workers matched to *Low Ability* owners. We assume that for *High Score* workers matched to *High Ability* owners the gains are zero in year 1 and 2, and then for year 3 onwards the gains are equal to the estimated change in reservation wages (+22%) times the initial probability of employment, which is not impacted by the program and so is held constant at the level in Control. That is, we assume that the changes in contemporaneous reservation wages reflect future changes in actual earnings, that will be realized after the end of the additional training. We assume that this will take two years, as that is the typical length of the training courses workers are

[43]The intervention could have produced a negative impact on firms if the gains from the increased information were outweighed by the increased cost of hiring *High Score* workers with a certificate. As shown in Table A10, we do not observe large differences in the wages of *High Score*, relative to *Low Score* workers in Treatment. This suggests that the possibility of a negative impact of the intervention on firms is less likely.

[44]Here we focus on the costs and benefits of implementing the certification component of the intervention. We do not consider the costs nor the benefits of implementing the matching component of the intervention, as this did not vary between Treatment and Control, and so cannot be evaluated separately.

[45]The total cost per individual at year 0 include: (i) cost of developing and administering the skill tests ($9.19); (ii) cost of producing and disbursing certificates ($6.40); (iii) program management costs ($3.50).

enrolled in. As we are assuming that the returns to training act *only* through changes in wages, and not through changes in employment, we can interpret our calculation as a likely lower bound on the gains for this group of workers.

For *Low Score* workers matched to *High Ability* owners we consider two scenarios: (i) they experience a gain equal to the observed change in employment (+18pp) times the reservation wage reported in Control (which is not impacted by the program); (ii) they experience no gain at all from the program. Case (i) is likely to be an upper bound on the gains to these workers, as we found no impacts on actual earnings at follow-up (though the estimates on earnings are noisy); on the other hand, (ii) is likely a lower bound for such gains, given the positive documented impact on the probability of employment, and the lack of any negative effects on the reservation wage. We assume that these gains are realized from year 1 onwards. The actual numbers in Rows 1-3 of Panel B are the weighted average of the impacts described above, where the weights are the share of workers in each group.[46]

Moving to the results, Row 5 shows that under our initial set of assumptions, the program results in a benefits/cost ratio of 5.35 for the average participant. This remains safely above 1 when each of the following scenarios is considered: (i) the discount rate is doubled from the initial level of 5% to 10% (Row 5.1); (ii) benefits are assumed to last for only 10 years (Row 5.2) or 5 years (Row 5.3); (iii) there is no impact at all for *Low Score* workers matched to *High Ability* owners (Row 5.5). In line with our reduced form impacts, the cost-benefit analysis for the average worker masks substantial heterogeneity by worker type. This is shown in Panel A of Figure 7, which reports the benefits/cost ratios for the average worker, and then for *Low Score* and *High Score* workers separately. We notice that the overall gains are more than twice as high for *Low Score* workers than for *High Score* workers. The benefits/cost ratio for *Low Score* workers is close to 2 even in the most conservative scenario in which benefits only last 5 years. Instead, the ratio falls below 1 for *High Score* workers in this case. This confirms that the gains in employment experienced by *Low Score* workers outweigh the benefits experienced by *High Score* workers in terms of changes in reservation wages.

We summarize the cost-benefit analysis for the average participant by calculating the Internal Rate of Return (IRR), i.e. the interest rate at which the Net Present Value of the project would be zero. As reported in Row 6 of Column 1 of Table 8, under our initial set of assumptions, the IRR is 29%. As shown in Row 6.4, this falls to 9% under the most conservative scenario in which there is no impact for *Low Score* workers matched to *High Ability* owners. Taken together, the results of the cost-benefit analysis suggest that the program we evaluate is likely to generate substantial returns for the average participant.[47]

[46] An additional assumption in the cost-benefit analysis is that the documented gains do not come at the expenses of other workers that are displaced as a result of the program.

[47] To put these numbers into context, we can compare the IRR of this program to the ones from other

8.2 Welfare Implications of a Mandatory Certification Policy

In Column 2 of Table 8 we explore how the cost-benefit analysis would change if the intervention was extended to include also those workers that initially decided not to participate. The analysis in Section 2 suggests that these workers are *negatively* selected on soft skills. Therefore, we use the model and assume that all the workers that selected out are the *very low* types that firms are trying to avoid hiring, and so would be hurt by the policy, in terms of their employment and earnings. We show the sensitivity of the cost-benefit analysis to different assumptions on how much these workers would lose from participation in the program. The analysis in Column 2 can then be informative of a likely *lower bound* on the welfare impacts of introducing a mandatory certification policy on the soft skills of workers.

22% of the eligible workers selected out of the program, at the start of our intervention. We assume that in the absence of the intervention they would have had the same labor market outcomes as *Low Score* workers in Control. We also assume that had they been forced to participate to the intervention, they would have experienced a reduction in employment equal to the gains in employment for *Low Score* workers in Treatment. That is, we assume a negative treatment effect for such workers of 18pp. Coherently with the evidence from the experiment that impacts are found only for workers matched to *High Ability* owners, we further assume that only half of the workers that selected out would have experienced this loss (as the sample of firm owners is approximately equally split between *High* and *Low Ability* owners). We also show how the results would change in the more extreme case in which the probability of employment went down to exactly zero for these workers.

The results in Column 2 show that the cost-benefit analysis still produces mostly positive results for the average worker under these more conservative scenarios. As shown in Row 6 of Column 2 of Table 8, the IRR under our main set of assumptions remains relatively high, at 14%, and is still 11% for the average worker in the very conservative case in which the negative treatment effect brings employment to zero for those workers that selected out (Row 6.3). This shows that even under conservative assumptions the program is likely to generate a relatively high return for the average worker. However, as shown in Panel B of Figure 7, the mandatory certification policy generates highly *non-linear* welfare effect along the skill distribution: workers at the low end of the distribution are hurt by the policy, while those in the middle of the distribution gain the most.

The results of our welfare discussion are in line with evidence in Pallais (2014), who shows that disclosing more information about worker's abilities increases hiring and welfare

active labor market policies, such as training programs. For example, using follow-up data for up to 10 years since the original intervention, Attanasio et al. (2015) calculate that a vocational training program (focused on practical skills) in Colombia had an IRR of 22.1-29.5% for young women.

in the context of an online labor market. The main mechanism behind her results is that hiring generates public information on the skills of workers, as firms have to disclose a public rating of worker performance. This creates an externality, which leads firms to hire too few inexperienced workers. Our results instead originate from information solving a different inefficiency: adverse selection in hiring. The information helps firms separate worker types, and this increases their propensity to hire. In addition, workers benefit through the impacts on their outside options.

Regarding the incentives to finance a similar intervention, while certain groups of workers are benefitting from it, they might be credit constrained. On the incentives of the government to implement this type of program, our analysis shows that while this intervention is likely to increase overall welfare, some workers might lose, in terms of employment and earnings. In addition, by making some workers better off, and some worse off, the policy would increase inequality among workers. The decision to finance this program would then depend on the specific welfare function that the government is trying to maximize. Therefore, our cost-benefit analysis makes the important point that simply looking at the benefits/cost ratio or the IRR for the average worker can be very misleading in cases when policies are likely to benefit a certain part of the population, while hurting other participants: considering the impacts along the distribution of participants is necessary to fully characterize the welfare implications of implementing this type of policies.

9 Conclusion

We study the implications of lack of information on the skills of workers during recruitment, for both workers and firms. We do so by designing and implementing a field experiment in the Ugandan labor market that has two main components: (i) a *matching* component, whereby firms and workers are randomly matched for job interviews, and (ii) a *signalling* component, by introducing experimental variation in whether information on the *soft skills* of workers, such as work ethic and interpersonal skills, is disclosed to both sides of the labor market, through the provision of certifications.

Our main finding is that both managers and workers respond to the new information: the certificates lead managers to revise their beliefs on the skills of workers, while workers respond by updating their outside options. We further show how the updating of firms and workers impacts the propensity of firms to hire, as well as the allocation of jobs to workers with different skills. Our results highlight how the gains from removing the information friction can originate both from an increased probability of employment conditional on making a job application, as well as from the reallocation of labor across jobs in the economy.

Throughout the analysis, we show that only managers of higher ability react to the information in terms of their beliefs and hiring choices. This provides direct evidence of the role that firms can play in reducing the size of the information friction, through their ability to screen. In addition, the information acquisition is *symmetric* during job interviews: workers respond to the information only if managers also respond. These results have important implications for the modelling of labor markets in developing countries, by highlighting the role of two-sided symmetric learning during job interviews.

Our results have important implications for labor market policy. Our discussion on the welfare effects of a mandatory certification on soft skills shows that this is likely to produce positive welfare gains for the average worker. However, the policy would have *non-linear* impacts along the skill distribution, and the gains would be heterogeneous: workers at the low end of the distribution would be hurt by the policy, while the positive gains would be largest for workers in the middle of the distribution. Therefore, by making some workers better off and some worse off, the policy would also increase inequality among workers.

Finally, our findings raise a number of questions that are left for future research. For instance, why do vocational training institutes rarely assess and certify of their own initiative the soft skills of workers? One possible explanation is that introducing such assessments would affect the *selection* of workers into vocational institutes. In particular, certifying soft skills might not be profit-maximizing if this reduces the demand for these programs by workers with low soft skills. While the literature has focused on establishing the labor market returns to vocational training programs in middle and low income countries (Attanasio et al. (2011), Attanasio et al. (2015), Hirshleifer et al. (2015), Alfonsi et al. (2016)) little is known about what determines the selection of students into training programs. At the same time, while there is a growing literature on referrals (Beaman and Magruder (2012), Burks et al. (2015), Brown et al. (2016), Pallais and Sands (2015)) and on the signalling role of attending college (MacLeod et al. (2015), Deming et al. (2016)) less is known about the labor market signalling value of attending vocational training programs in informal labor markets in developing countries. Answering these questions would provide important new insights into the functioning of both labor markets and education markets in low income countries, and so is something worth attempting further in future research.

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Appendix

A Selection of Vocational Training Institutes

This section describes the selection of Vocational Training Institutes (VTIs) for the intervention, and presents summary statistics, by the VTI participation status.

To select the VTIs for the project we contacted 24 large, reputable institutions taken from the list of VTIs registered with the Directorate of Industrial Training. We only considered VTIs training at least 50 workers about to graduate in the sectors considered in our project, and operating in areas where our project was to be implemented. Contacted institutions included VTIs that BRAC had worked with before for other skills training programs, as well as new potential partner VTIs.[48]

VTI principals were told that, if they accepted to participate, BRAC would promote the Job Placement Program with students at their institutions. VTI principals were informed about all components of the program: they were made aware that BRAC would be conducting skills measurements on their trainees using a variety of methods, including in-class observations, and that some of this information could be disclosed to firm owners during the job placement component of the intervention.

The project was well received: only 5 of the 24 VTIs reported not being interested to participate. We followed up with the interested VTIs and established partnerships with 15 of them.[49] Table A2 reports basic VTI descriptives by whether the VTI took part in the project or not.[50] The Table shows that we were successful at identifying large, well established VTIs: the average institution taking part in the project has been operating for about 35 years, offers 11 different types of courses, and has about 350 students currently enrolled. The Table further shows that there are no significant differences in means between the institutions that took part in the program, and those that did not. This suggests that such basic observable characteristics do not drive selection into the project, although any conclusions from Table A2 are subject to the caveat of the sample being very small.

[48]We contacted 24 VTIs in total as we expected this would be a sufficient number to achieve our target sample of workers.

[49]There were 4 VTIs that were interested to participate but were not included in the final sample for the project: this was due to their trainees graduating at a time of the year not compatible with our intervention.

[50]This information is missing for 3 VTIs.

B Data Appendix

This section first presents more details about the key facts discussed in Section 2.4. We then provide more information about the skills assessments conducted at the VTIs for the intervention.

B.1 Key Facts about SMEs at Baseline: Additional Details

The first key fact is that soft skills are perceived as having relatively high returns. Firm owners were asked to rate on a 0 to 10 scale[51] how important different skills are in their firms. Figure A3 reports the average importance given to each skill.[52] While practical skills are reported as having the highest returns, soft skills are reported as the second most important skill, and more important than numeracy, literacy or theoretical skills.

The second key fact is that firm owners report stealing by their employees and difficulties in observing the soft skills of workers among their main perceived constraints. Owners were asked to rate the importance of a range of potential constraints on a 1 to 5 scale.[53] 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 A4 shows that employee stealing is reported as the most important constraint. At the same time, difficulties in assessing the soft skills of workers are reported as more important than lack of demand, access to electricity, difficulties in finding workers, or screening on practical skills.

The third key fact is that firm owners have relatively low priors on the distribution of soft skills among workers. Firm owners were asked to report how many potential workers out of 10 they thought had (i) a good level of practical skills, and (ii) a good level of soft skills. We compute the difference between these two variables for each owner, and plot the resulting CDF in Figure A5. The Figure shows that as many as 80% of firm owners think that practical skills are relatively more common among potential workers.

The fourth key fact is that it is common for firms to recruit workers that just show up and ask for a job, without any prior connection or referral. Figure A6 shows that over one-third of the workers employed at baseline were hired in this way.

Finally, the fifth key fact is that there is substantial heterogeneity in the managerial ability of firm owners. Owners were asked to answer the same cognitive test administered

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

[52]Firm owners were asked to rate the importance of each of the Big 5 skills separately, and so in Figure A3 and Figure A4 we label as "soft skills" the average importance given to the Big 5 skills.

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

to the trainees. Figure A7 reports the distribution of scores.[54] Owners with a score equal or above the median are assigned to the *High Ability* group, while owners with a score below the median form the *Low Ability* group. We regress profits per worker on a range of firm and owner characteristics[55] and plot the residuals, splitting the sample by the firm type. This is done in Figure A8, which shows that *High Ability* owners have higher residual profits per worker. We interpret this as an indication of higher managerial ability.

B.2 Skills Assessments at the VTIs: Additional Details

We now describe the assessment procedures used to measure the skills of trainees at the VTIs, and present the distributions of the main skill measures.

Information on the cognitive skills and Big 5 traits of all eligible trainees were conducted at the VTIs during the initial worker census. To measure cognitive skills, we used a 10-item Raven matrices test. To measure the Big 5 traits we used a 10-item questionnaire. Questions were translated in Ugandan with the help of a local psychologist. Panel A of Figure A1 reports the distribution of cognitive skills and of the Big 5 traits.

We used teacher surveys to measure the soft skills of trainees on the following dimensions: attendance, discipline, communication skills, proactivity and willingness to help other students in class.[56] Teachers were asked to use a 0-10 scale to rate the skills of their trainees, in absolute terms (and so not relatively to the class).

BRAC field staff conducted in-class observation of worker behavior. This took the form of random spot-checks, whereby our field staff showed up unannounced at the VTIs and attended a number of classes, noting down details of student behavior.[57] The main purpose of the spot-checks was to verify the information provided in the teacher surveys, by comparing this to in-class behaviors that could be objectively noted by external observers. So for attendance/time-keeping, BRAC staff were asked to record whether trainees were observed as present and on-time in class; for willingness to help others, we recorded whether they were observed voluntarily helping other students; for proactivity, we recorded whether they were observed asking questions to the teacher; for discipline, we recorded whether they were

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

[55]The complete set of controls includes: number of employees at baseline, a dummy for whether the owner is female, business age and age squared, a dummy for whether the owner attended a VTI in the past, years of education of the owner, a dummy for whether the business is registered, sector and region dummies.

[56]The instructors taking part in the survey had been teaching the trainees in our sample for 12 months on average by the time of the survey. Teachers were not asked to rate the creativity and trustworthiness of trainees as we wanted to keep the teacher surveys focused on those skills that we thought could be easier to assess for an external examiner.

[57]Spot-checks were conducted weekly while trainees were still enrolled at the VTIs. The median trainee was observed in 6 different spot-checks.

reproached by the teacher for inappropriate behavior in class, such as making noise.[58] For each trainee, we can then construct the percentage of classes in which she was observed by BRAC staff as engaging in a particular type of behavior.

Panel B of Figure A1 shows that, reassuringly, there is a positive and significant correlation between the measures from the teacher-surveys, and the corresponding observed behaviors from the spot-checks. As teacher surveys have fewer missing values, we use information from the teacher surveys to develop our final measures of the following skills: attendance, proactivity, discipline, willingness to help others and communication skills.

To measure creativity, we developed an 8-item questionnaire, together with the School of Psychology at Makerere University in Kampala. We construct a creativity index by calculating the unweighted average of answers to the 8 questions. The distribution of the creativity index is reported in Panel C of Figure A1. The index goes from 1 to 5, with a higher value of the index corresponding to higher creativity. Finally, to measure trustworthiness, we used a version of a standard trust game, played by trainees with real money.[59] Every trainee played exactly the same game. Specifically, trainees were told that an anonymous sender (not among the other trainees at the VTIs) had decided to send them 1000 Ugandan shillings (about 30 USD cents), and that BRAC had tripled this amount, so that every trainee had received 3000 Ugandan shillings (or approximately 90 USD cents). Trainees were then asked how much (if anything) they were willing to send back to the original sender, to reciprocate for the initial transfer received. A higher amount sent back is interpreted as higher trustworthiness. Panel C of Figure A1 reports the distribution of the amounts sent back.

B.3 Information that Managers would Like to See about Trainees

To select the five skills to the revealed on Treatment certificates, we used information from the firm baseline survey on the stated preferences of firm owners over which skills they would like to receive more information on during interviews. Specifically, firm owners were asked to rate on a 0-10 scale[60] how important it would be to receive additional information on the seven soft skills included in our assessments, if they were to interview trainees that had just completed training at VTIs.[61] Figure A9 reports the average importance given to each skill. For the information revelation, we choose the top 4 skills: creativity, communication, trustworthiness

[58]BRAC staff were not asked to rate the communication skills of trainees, as it was not straightforward to link communication skills to an objective type of behavior observable during a short time-period in class.

[59]The main reference for the trust game is Berg et al. (1995).

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

[61]Specifically, firm owners were told that we would be observing trainees while at the VTIs, and that our skills assessments would be based on the behavior of trainees while at the VTIs.

and willingness to help others, plus attendance. This allows us to use attendance as a "placebo", and to potentially check whether it is given a lower weight during recruitment, relative to the other skills.

B.4 Grading Procedure

To grade the workers on each of the five soft skills, we begin from the skills assessed in the teacher surveys: communication, willingness to help others and attendance, and observe the distribution of grades given by teachers. Teachers were explicitly asked to grade workers using an *absolute* scale, and so not to curve the grades within the sample participating to the study. We then match the grade distribution of creativity and trustworthiness (which are not measured from the teacher surveys) to the average grade distribution of the other three skills. Figure A10 reports the distribution of grades for the five skills. Table A3 reports the pairwise correlation coefficients among the grades.

C Randomization and Matching Procedure

This section describes the procedure used to randomize workers into Treatment and Control, and to implement the matching allocations for the job interviews.

We decided to match firms and workers only within sectors, as it is reasonable to assume that workers would mainly look for jobs in the same sector of training. In terms of how many workers to match to each firm, the median firm from the baseline normally interviews 3 applicants before a vacancy is filled. Our aim was to replicate a "hiring round" in our sample of firms, and so we tried to keep the number of workers matched to each firm as similar as possible to the number of applicants they would normally interview absent our intervention. At the same time, to keep our intervention logistically feasible, we imposed as a constraint that a worker could not be matched to more than 5 different firms. Therefore, we developed a matching rule that in each sector fixed the number of workers matched to each firm to exactly 3 workers, as long as this was compatible with all workers in that sector being matched with at least 1 firm, and with at most 5 different firms.

The main challenge with implementing this rule is that the number of workers and firms varies substantially across sectors in our research sample. We were able to implement the assignment with exactly 3 workers per firm in catering and hairdressing. Firms in tailoring were assigned instead to meet 4 workers each, and firms in motor-mechanics (the sector with the highest ratio of workers to firms) were assigned 6 workers each. On the other hand, firms in carpentry and welding (sectors with a relatively low supply of graduates) were matched to 2 workers each.[62]

In practice, we decided to implement the worker-firm matchings at the submarket level, where a submarket is defined as an urban area-sector combination. So firms in a given submarket were only matched to workers assigned to the same submarket. This was done to reflect the local nature of informal labor market search, appropriate to this context: while workers are mobile in principle, evidence shows that transportation costs limit significantly the ability of workers to move for job search in similar informal contexts (Abebe et al. (2016), Franklin (2015)).[63]

To assign workers to urban areas for the matchings, we followed as much as possible their stated preferences at baseline: workers were asked about their first, second and third most preferred urban areas where they would like to look for employment after the end of training. Each trainee was initially assigned to her most preferred urban area. This initial allocation resulted in a few submarkets that had either "excess supply" or "excess demand" of workers, relative to the ranges needed for the allocation rule described above to be implementable. So workers were randomly moved across urban areas for the purpose of the matching assignments, until every submarket had a number of assigned workers and firms falling in the ranges required by the assignment rule. When moving workers across submarkets, their second and third stated preferences were taken into account as much as possible. While some workers had to be matched in an urban area different from their preferred one, such cases were limited: 72% of workers were matched in their preferred urban area, and only 9% were matched in a urban area outside their three most preferred ones.

Once the allocation of workers and firms to submarkets was finalized, we randomly assigned workers and firms to a Treatment group and a Control group of approximately equal sizes. So the randomization produced four groups: (i) Treatment workers, (ii) Control workers, (iii) Treatment firms; (iv) Control firms. The randomization was stratified by submarket.

Finally, in the third step, we implemented the matching allocations: within each submarket and treatment group, firms and workers were matched randomly, according to the assignment rule described above. So, for example, hairdressing firms in the Kasese urban area were randomly matched with exactly 3 workers who had received hairdressing training and were assigned to be matched in the Kasese area. The 3 workers were then matched with between 1 and 5 hairdressing firms in Kasese.

[62] All firms in welding were matched to 2 workers, with the exception of firms in Fortportal, Gulu, Kasese, Mbale, Mbarara, Nyendo, Ojwina which were assigned only 1 worker).

[63] To further limit any concerns related to transportation costs, BRAC paid for the travel costs of workers to the location of interview on the day of the matchings.

D Correcting for Non-random Attrition

We now turn to the discussion of non-random attrition from the experiment, by the time of the follow-up surveys. Our main concern is attrition related to Treatment or to our measures of worker soft skills.

In Table A8 we test for selective attrition both at the individual firm/worker level and at the match-level, by running OLS regressions where the dependent variable is a dummy equal to one if the outcome of a given firm, worker, or match is observed at follow-up. The results show that there is evidence of selective attrition in the firm follow-up survey, when the level of observation is the firm: in particular, Column 1 reveals that *High Ability* owners that were assigned to meet only *Low Score* workers are easier to track in Treatment, relative to Control. On the other hand, Column 2 shows that there is no evidence of selective attrition among *Low Ability* owners. Columns 3 and 4 show that there is no evidence of selective attrition in the worker follow-up survey, when observations are at the worker level. Columns 5 and 6 reveal that there is no selective attrition in the firm follow-up data, when the unit of observation is the match. Finally, Column 7 shows that in the worker follow-up matches involving *High Score* workers and *High Ability* owners are easier to track in Treatment than in Control. On the other hand, as shown in Column 8, there is no selective attrition in matches with *Low Ability* owners. Based on this evidence, we correct for selective attrition when the analysis is conducted at the firm level using the the firm follow-up survey, and when it is conducted at the match level using the worker follow-up survey.

To correct for attrition in the match-level worker follow-up and in the individual-level firm follow-up, we use the Inverse Probability Weighting (IPW) procedure described in Wooldridge (2010). This essentially models attrition as a function of observable characteristics. In the first step, a Probit regression is estimated to predict the probability that a given observation is included in the follow-up sample. The Probit regression includes the same list of controls used in the regression specifications for our main outcomes. We also add instrumental variables to help correct the non-random attrition. To predict attrition from the match-level worker survey we use the gender of the enumerators assigned to contact the workers for the follow-up survey. Workers were assigned to enumerators randomly. To predict attrition from the individual-level firm follow-up survey we use: age of the enumerator and age squared; score of the enumerator on an initial ability test and score squared; gender of the enumerator assigned to contact the firms at follow-up. Firms were assigned to enumerators randomly. We allow the regressors to differently predict attrition depending on the cognitive skills of firm owners, and so every variable in the first step regression is interacted with the *High Ability* dummy. A joint F-test on the significance of the enumerator gender and its interaction with the *High Ability* dummy in predicting attrition from the match-level worker dataset has *p-value*=.020. The same test on the IVs for attrition from the individual-level firm follow-up survey has *p-value*=.006. Therefore, our IVs are significant predictors of being in the sample at follow-up, and so they can help in reducing the bias from selective attrition. In the second step regressions, observations are weighted by the inverse of their predicted probability of being in the follow-up sample (calculated from the first step).

An alternative way to correct for non-random attrition is to calculate bounds on the treatment effect of interest using the procedure described in Lee (2009). The key intuition is the following. The procedure first trims the sample such that the proportion of observed respondents is the same in both Treatment and Control groups. Then the upper bound and the lower bound on the treatment effects are calculated assuming that either the respondents with the worst outcomes or the ones with the best outcomes are more likely to attrit (but not a combination of the two). We implement a conditional version of this procedure for our main outcomes of job offers and hires in the intervention, where we are able to control for gender of the worker to tighten the bounds.

E Correcting for Sample Selection Bias

This section discusses the potential bias arising from non-random selection into the final sample of workers and firms that meet for the job interviews. It then presents the estimation of a two-step econometric model of sample selection.

We wish to estimate the treatment effect of the signalling intervention on the outcomes of the job interviews between firms and workers. In particular, interest lies in estimating the following regression equation:

$$y_{ij} = \beta_0 + \beta_1 HS_i + \beta_2 LS_i \times T_{ij} + \beta_3 HS_i \times T_{ij} + \gamma \mathbf{X}_i + \delta \mathbf{X}_j + \alpha \mathbf{Int}_{ij} + u_i + u_j + \nu_{ij} \quad (6)$$

The main components of this equation are described in Section 4.1. For the discussion in this section, it is useful to decompose the error term into three components. The unobservables u_i, u_j capture, respectively, worker and firm characteristics that are not observed to the econometrician, but that might have an impact on the outcome of job interviews: u_i include things such as the physical appearance of the worker; examples of u_j are how intensively the firm owner was looking for workers at the time of the intervention. ν_{ij} include interview specific unobservables: an example is whether a customer came to the business during the interview, so that the firm owner got distracted and gave less attention to the worker. ν_{ij}

can be thought of as "transitory shocks", and are very different in nature from u_i or u_j, which are permanent, but unobserved, worker and firm characteristics.

Outcomes y_{ij} are observed only for job interviews that took place. This can raise a sample selection problem, because: (i) only 42% of the 1230 scheduled job interviews were carried out; and (ii) the randomization into Treatment and Control groups was performed on the sample of workers and firms *assigned* to the intervention. So there is no guarantee that the sample that did participate to the job interviews is balanced in terms of characteristics (including skills) across experimental groups.

Sample selection bias can arise from both the worker and firm side, since a job interview takes place only if both sides agree to meet. If the decision of workers to show up at the interview depends on unobserved factors related to their potential performance in an interview (e.g. their self-confidence), then this creates non-random sample selection on the worker side. If firm owners accept to interview workers because of unobserved characteristics such as their labor demand, then this gives rise to non-random sample selection on the firm side. Non-random sample selection on either side introduces standard selection bias in OLS estimation of (6) on the sample of matches for which y_{ij} is observed.

To provide evidence on the presence of non-random sample selection, we regress a dummy equal to one if the interview took place, on the same set of worker, firm and interview controls as in equation (1), using the sample of *scheduled* job interviews. If our main regressors of interest, that is, HS_i, $LS_i \times T_{ij}$ and $HS_i \times T_{ij}$, are found to be significant predictors, then this is indicative that the final sample of job interviews that took place is unbalanced in terms of the main observables of interest. In turn, this would suggest that the sample selection problem is likely to be non-negligible.

Table A7 reports the results of such regressions. Column 1 reports estimates on the full sample of scheduled job interviews. In Columns 2 and 3 the sample is split between interviews scheduled with *High* and *Low Ability* firm owners, respectively. Column 1 shows that job interviews involving workers with high soft skills are more likely to have been carried out. The splits in Columns 2 and 3 further reveal that the predictive power of our main observable characteristics is concentrated in matches with *High Ability* firm owners. In particular, the results in Column 2 reveal that the positive impact of the skills index on the probability of interviews with *High Ability* firm owners taking place is found only in the Control group, and not in the Treatment group. On the other hand, observable worker characteristics do not predict whether a job interview took place in the sample of matches involving *Low Ability* owners. The results in Table A7 suggest that concerns related to non-random sample selection might be important, and motivate writing down an econometric model to correct for sample selection bias in the estimation of equation (6).

E.1 An Econometric Model of Sample Selection

We now develop and estimate a two-step econometric model to control for sample selection in the estimation of equation (6).

E.1.1 Timing of Events and Information Set

To model the selection process, we lay out the timing of the implementation steps leading up to a job interview taking place or not, and discuss the information set available to both the worker and the firm at each of these steps:

1. Step 1: A few days before the matching intervention, BRAC staff contacted all workers and firms included in the initial randomization, to inform them that the intervention was just about to start. Workers and firms were asked to confirm their interest in participating to the job interviews, and this was recorded by BRAC staff. Importantly, at this stage firms and workers were not given any information about whom they had been matched with, nor about the Treatment group they had been assigned to.

2. Step 2: Job interviews were carried out between the workers and firms that had confirmed their interest, following the initial matching allocations. The transcripts were shown to both the worker and the matched firm only conditional on the job interview taking place. Workers and firms that had lost interest in the program were not informed about their treatment status, and were not shown any transcript.

Conditional on meeting at least one worker in Step 2, only 7% of firms did not meet one of the other matched workers because of loss of interest in conducting the additional interviews. Also, only 3% of the workers that met at least one firm did not meet one of the other matched firms because of lack of interest in doing the additional interviews. This confirms that selection into the final sample of matched workers and firms operates mainly through the *extensive margin* decision of whether to participate at all, rather than through the intensive margin of which interviews to take part in, conditional on participation.

E.1.2 Selection Equations and Bias Analysis

Following the discussion in the previous subsection, the decision of workers and firms to participate in the program can be modelled as follows:

$$m_i = \eta \mathbf{Z}_i + \epsilon_i \tag{7}$$
$$m_j = \theta \mathbf{Z}_j + \epsilon_j \tag{8}$$

$$D_i = 1 \quad if\ m_i > 0; \quad D_i = 0 \quad if\ m_i \leq 0 \tag{9}$$
$$D_j = 1 \quad if\ m_j > 0; \quad D_j = 0 \quad if\ m_j \leq 0 \tag{10}$$

Where m_i is a latent variable representing the propensity of workers to participate. This is a linear projection of observable worker characteristics $\mathbf{Z}_i$ and unobserved (to the econometrician) worker component ϵ_i. Note that the participation decision of workers does not depend on (observed or unobserved) firm characteristics. This reflects a feature of the implementation described in Step 1 above. The latent variable representing the participation decision of firms, m_j, is modelled in a similar way, and so does not depend on worker characteristics.

Workers are willing to participate to the job interviews when the value of m_i is greater than 0. In this case, $D_i = 1$. Similarly, for firms $D_j = 1$ only if $m_j > 0$. For the job interview to take place, *both* worker i and firm j must be willing to participate. Therefore, interview outcomes y_{ij} are observed only if *both* $D_i = 1$ and $D_j = 1$. We have:

$$y_{ij} = \gamma \mathbf{X}_i + \delta \mathbf{X}_j + \alpha \mathbf{Int}_{ij} + u_i + u_j + \nu_{ij} \qquad if\ D_i = 1\ and\ D_j = 1 \tag{11}$$
$$y_{ij} = not\ observed \qquad if\ D_i = 0\ or\ D_j = 0 \tag{12}$$

Where, to simplify exposition, the HS_i, LS_i dummies, and their interaction with the Treatment indicator, have been included in $\mathbf{X}_i$.

The assumption that the participation decision of workers only depends on worker characteristics implies that ϵ_i is uncorrelated with both u_j and ν_{ij}. The corresponding assumption on the firm side implies that ϵ_j is uncorrelated with both u_i and ν_{ij}. To summarize, the implementation protocol suggests the following two assumptions on the correlation between unobservables:

Assumption U1: $E[u_i|\epsilon_j] = E[u_i] = 0$, and $E[u_j|\epsilon_i] = E[u_j] = 0$.

Assumption U2: $E[\nu_{ij}|\epsilon_j] = E[\nu_{ij}|\epsilon_i] = E[\nu_{ij}|\epsilon_j, \epsilon_i] = E[\nu_{ij}] = 0$.

We can study selection bias by taking the expectation of (11) conditional on the covariates and the interview taking place. To simplify exposition, define $\mathbf{C}_{ij}$ as the set of worker, firm and interview covariates, so that we can rewrite the conditional expectation of interest as: $E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1]$, as follows:

$$\begin{aligned} E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1] = \gamma\mathbf{X}_i + \delta\mathbf{X}_j + \alpha\mathbf{Int}_{ij} &+ E[u_i|D_i = 1, D_j = 1] \\ &+ E[u_j|D_i = 1, D_j = 1] \\ &+ E[\nu_{ij}|D_i = 1,\ D_j = 1] \end{aligned} \tag{13}$$

Assumption U1 implies that we can rewrite:

$$\begin{aligned} E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1] = \gamma\mathbf{X}_i + \delta\mathbf{X}_j + \alpha\mathbf{Int}_{ij} &+ E[u_i|D_i = 1] \\ &+ E[u_j|D_j = 1] \\ &+ E[\nu_{ij}|D_i = 1,\ D_j = 1] \end{aligned} \tag{14}$$

Using Assumption U2, we can rewrite:

$$\begin{aligned} E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1] = \gamma\mathbf{X}_i + \delta\mathbf{X}_j + \alpha\mathbf{Int}_{ij} &+ E[u_i|D_i = 1] \\ &+ E[u_j|D_j = 1] \end{aligned} \tag{15}$$

Using (7), (8), (9) and (10), we can rewrite equation (15) as:

$$\begin{aligned} E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1] = \gamma\mathbf{X}_i + \delta\mathbf{X}_j + \alpha\mathbf{Int}_{ij} &+ E[u_i|\epsilon_i > -\eta\mathbf{Z}_i] \\ &+ E[u_j|\epsilon_j > -\theta\mathbf{Z}_j] \end{aligned} \tag{16}$$

Equation (16) makes clear that the presence of selection bias is driven only by the potential correlation between u_i and ϵ_i on the worker side, and u_j and ϵ_j on the firm side. For OLS on the selected sample to produce unbiased and consistent estimates, we would need to assume that $E[u_i|\epsilon_i] = E[u_i] = 0$, and $E[u_j|\epsilon_j] = E[u_j] = 0$. This is a strong assumption, and is equivalent to claiming that selection into the final sample of matched workers and firms is random. If we are not willing to invoke this extra assumption, then OLS estimation on the selected sample will produce biased estimates: the last two terms on the right hand side of (16) are likely to be non-zero, and so as long as the observables in $\mathbf{Z}_i$ and $\mathbf{Z}_j$ include (or are correlated to) at least some of the elements in $\mathbf{C}_{ij}$, then omitting the last two terms of (16) will lead to standard omitted variable bias.

E.1.3 Bias Correction with a Control Function Approach

We can correct for sample selection in estimation of (6) by controlling for the selection terms highlighted in (16). If joint normality of the unobservables in the selection and main equation is assumed, then it is easy to show that (16) can be rewritten as:

$$E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1] = \gamma\mathbf{X}_i + \delta\mathbf{X}_j + \alpha\mathbf{Int}_{ij} + \rho_w\sigma_w\lambda(\eta\mathbf{Z}_i) + \rho_f\sigma_f\lambda(\theta\mathbf{Z}_j) \tag{17}$$

Where ρ_w and ρ_f are the correlation coefficients between the unobservables in the main equation and in the selection equation, σ_w and σ_f are the standard deviations of the unobservables in the main equation, relative to the standard deviations of the unobservables in the selection equation (which are normalized to 1), and $\lambda(\cdot)$ is the inverse Mille ratio: $\lambda(\cdot)=\frac{\phi(\cdot)}{\Phi(\cdot)}$. If one is willing to assume joint normality of the errors, then a two-step procedure similar to the one developed by (Heckman 1974; 1977; 1979)can be used to recover consistent estimates of the parameters γ, δ and α.

Equation (17) can be written in more general form, without invoking the joint normality assumption:

$$E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1] = \gamma\mathbf{X}_i + \delta\mathbf{X}_j + \alpha\mathbf{Int}_{ij} + g(\eta\mathbf{Z}_i) + h(\theta\mathbf{Z}_j) \tag{18}$$

Where $g(\cdot)$ and $h(\cdot)$ are non-parametric functions. We follow an approach similar to Newey (2009) and replace $g(\cdot)$ and $h(\cdot)$ with series approximations, as follows:

$$\begin{aligned} E[y_{ij}|\mathbf{C}_{ij}, D_i = 1, D_j = 1] = \gamma\mathbf{X}_i + \delta\mathbf{X}_j + \alpha\mathbf{Int}_{ij} + \zeta_1\lambda(\eta\mathbf{Z}_i) + \zeta_2\lambda(\eta\mathbf{Z}_i)\cdot(\eta\mathbf{Z}_i) + \\ + \zeta_3\lambda(\theta\mathbf{Z}_j) + \zeta_4\lambda(\theta\mathbf{Z}_j)\cdot(\theta\mathbf{Z}_j) \end{aligned} \tag{19}$$

Where $\lambda(\cdot)=\frac{\phi(\cdot)}{\Phi(\cdot)}$. This is more robust than simply assuming joint normality of the unobservables. In the next subsection, we go through the details of estimating equation (19). The estimates from this model recover the ATE, under the assumption that the model is correctly specified and so, conditional on the control functions, the final sample is balanced and there is no remaining non-random selection on unobservables.

E.1.4 Estimation Using a Two-step Procedure

One key aspect of our set-up is that sample selection is allowed to be *two-sided*, so that y_{ij} is observed only if *both* the worker and the firm agree to meet. This implies that there might be firms that were interested to participate, but for which y_{ij} is never observed, because the workers matched to them were not available for interview. The same applies on the worker side. Therefore, we cannot simply recover the selection indicators D_i and D_j using information on *which* job interviews were carried out. More information on the *interest* of workers and firms in participating to the intervention is necessary for estimating the model described above without making additional assumptions.

In Step 1 of the implementation, information was recorded on whether workers and firms were interested to participate in the job interviews. We use this information to create the selection indicators: we define $D_i = 1$ for workers that met at least one firm, or that reported being interested to participate when contacted by BRAC staff just before the start of the intervention. The variable D_j is defined in a similar way on the firm side.

To achieve identification of the parameters in equation (19), the observables $\mathbf{Z}_i$ and $\mathbf{Z}_j$ in the selection equations should contain at least one continuous regressor excluded from the main equation (6). For this, we use the characteristics of the BRAC staff who contacted the firms and workers for the intervention, in particular their gender and years education. Workers and firms were assigned randomly to BRAC staff, so these variables are exogenous. These are also excludable from the main regression equation because the BRAC staff who contacted the respondents in Step 1 were not necessarily the same that oversaw the job interviews in Step 2 of the implementation. The significance of our excluded regressors in predicting interest in the intervention in Step 1 can be tested empirically. The two-step estimation procedure is implemented as follows:

1. Regress D_i on $\mathbf{Z}_i$ by Probit using the *full sample* of workers initially assigned to be matched. $\mathbf{Z}_i$ includes the instruments and the same worker controls as in (6), apart from the soft skills dummies and the Treatment indicator (as the transcripts with the results of skills assessments were revealed to the workers only conditional on the job interviews taking place). We store the predicted values $\hat{\eta}\mathbf{Z}_i$ and compute an estimate of the inverse Mills ratio for workers $\lambda(\hat{\eta}\mathbf{Z}_i)$.

2. Do the same for firms, by regressing D_j on $\mathbf{Z}_j$ by Probit. $\mathbf{Z}_j$ contains the instruments and the same firm-side control variables included in (6), apart from the Treatment indicator (as the firms were shown the transcripts only conditional on the interviews taking place). Since in the analysis we split the sample of matches depending on the ability of the firm owner, every variable in the firm-side selection equation is interacted

with the *High Ability* dummy.

3. Estimate equation (19) by OLS separately for *High* and *Low Ability* firm owners, using the estimated inverse Mills ratios and predicted values from the previous step to correct for sample selection bias. Standard errors are calculated using 500 bootstrap replications, with resampling clustered by firm, to account for the fact that firms were assigned to meet more than one worker.

E.1.5 Estimation Results

We apply this two-step procedure to the analysis of our main outcomes of interest: job offers and hires. These are observed only if the interview between the worker and the firm took place.

Estimation of the selection equations confirms that the BRAC staff instruments are significant predictors of interest in the intervention: the joint F-test of significance of the instruments in the worker selection equation gives *p-value*=.002. On the firm side, the joint F-test of the instruments has associated *p-value*=.050.

Table A9A reports the results of the two-step model. As shown in the lower half of the Table, the inverse Mills ratios and their interaction with the predicted values from the selection equation in the first step are not statistically significant in the second step: for example, Column 1 shows that the *p-value* from the joint F-test on the worker inverse Mills ratio and its interaction with the predicted values from the worker selection equation is .845. Similarly, the *p-value* from the joint F-test of significance of the firm inverse Mills ratio and its interaction with the predicted values from the firm selection equation is .851. Finally, the *p-value* from the joint F-test of significance of all worker and firm selection terms in the second stage is .951. The selection terms continue to remain statistically insignificant predictors in the second step, throughout Table A9A. This suggests that sample selection bias is not substantial in these regressions.

We further evaluate the results from the two-step model by comparing them with estimation of equation (6) on the sample of matches that took place, without controlling for selection. The results are reported in Table A9B. Comparing Table A9A with Table A9B shows that controlling for selection does not produce significant changes in the point estimates in the second stage: for example, focusing on our estimate of β_1, we notice that in Column 1 of Table A9A this is .253, while in Table A9B this changes to .223. This point can be appreciated graphically in Figure 6, where the point estimates from Column 1 of Tables A9B and A9A are reported next to each other. The stability of coefficients across the two Tables provides additional evidence that selection into the final sample of matches is not creating substantial bias in these regressions. However, controlling for selection substantially increases the standard errors of the estimates: this is not surprising, as the control function approach used to correct for sample selection is clearly less efficient.

F Additional Robustness Checks

We now turn to the discussion of additional robustness checks. One concern is measurement error on the outcomes of job interviews. This arises because the rate of job offers and hires is higher in the firm than in the worker follow-up survey.[64] We believe measurement error to be lower in the worker follow-up for al least two reasons: (i) while the median firm was matched to 3 workers, the median worker was only matched to 1 firm, so possible recall errors related to the respondent getting confused about the different job interviews are lower on the worker side; (ii) in 13% of the cases, the person that answered the firm follow-up survey is different from the owner that conducted the job interviews as part of the intervention.[65] This raises additional concerns over measurement error from the firm side, that are not present on the worker side, since reports about the outcome of job interviews from the worker follow-up necessarily come from the workers themselves. This explains why for our preferred specification we use data from the worker follow-up. We repeat the analysis using firm reports in Table A9C. The main results are in line with the worker reports. The only effect that is different from our main specification and is worth pointing out is that *High Score* workers are significantly less likely than *Low Score* workers to be hired by *Low Ability* owners, though they are *not* less likely to receive a job offer. This would suggest that *High Score* workers are more likely to reject offers from *Low Ability* firms. This would be consistent with our main interpretation of the results: *High Score* workers know that they have higher skills, and so prefer not to work for *Low Ability* owners, for instance because these firms are less profitable and have lower growth potential.

Another potential concern relates to correlated observations. In estimating equation (1) at the match level, we are implicitly assuming that job interviews are *iid* observations. However, the median firm was matched to 3 workers.[66] To ease possible concerns related

[64]In the firm follow-up survey, 21.8% of the matches turned into a job offer, while this percentage is 8.4% in worker follow-up data. The correlation coefficient between offers in the two datasets is .475. Reassuringly, the information from the two datasets lines up much more when looking at hires: 4.93% of matches turned into a hire according to the firm reports, while this percentage is 4.1% in the worker reports (the correlation in this variable is .836 across the two datasets).

[65]This is due to the original owner having left the business or to someone else other than the owner having answered the follow-up survey, for instance because it was not possible to contact the owner at the time of the survey.

[66]The median worker was matched to one firm, so concerns related to observations not being independent across workers are less strong.

to observations at the match level not being independent, we estimate versions of equation (1) at the firm level, where we condition on whether the firm met at least one *High Score* worker.[67] The results are reported in Table A9D, and are not substantially different from our core results presented in Table 6. Consistently with the results in Table A9C, we do again find that *High Score* workers matched to *Low Ability* owners are more likely to reject job offers.

In our preferred specification we use principal component analysis to aggregate the five skills reported on the certifications. Table A9E shows that our core results are robust to using the average of the grades of the workers on the five soft skills to separate them into the *High Score* and *Low Score* groups. This eases potential concerns related to the specific procedure used to aggregate the information on skills and reduce the dimensionality problem.

An additional robustness check relates to alternative ways to correct for non-random attrition from the experiment. A conservative way is to calculate bounds on the effect of interest, using the procedure described in Lee (2009). More details on this procedure are given in Appendix D. Table A9F reports the bounds on the main effects of interest. Focusing on job offers, we notice that in matches involving *High Ability* owners, both the upper and lower bound on β_1 are highly significant. The upper bound on β_2 is also highly significant, and the lower bound is positive and large in magnitude, but not significant. The bounds on β_3 are small and not significant. None of the bounds for *Low Ability* owners are significant. Taken together, the evidence from Table A9F suggests that our main results are reasonably robust to this conservative procedure to correct for attrition.

One final concern relates to multiple testing, as in our main regression specification we are simultaneously testing *three* coefficients of interest: β_1, β_2 and β_3. To verify the robustness of the results to multiple testing, we adjust the individual *p-values* on each coefficient estimate using the procedure developed in Romano and Wolf (2016). The results are reported in Table A9G, and show that our results are robust to accounting for the fact that we are testing three hypotheses simultaneously. When adjusting the *p-values* in the fully interacted model to test for different impacts across the sample of *High Ability* and *Low Ability* owners, we are unable to reject the equality of the three coefficients of interest between the two groups of owners, as shown in Columns 3 and 6. Given the relatively small sample our results are based on, then this last result does suggest some caution in interpreting the split between *High Ability* and *Low Ability* owners.

[67] An alternative way to account for the fact that observations are correlated would be to use clustered standard errors in the estimation of match-level regressions, where each cluster is a submarket (i.e. a sector-region cell). While the number of clusters would not be a concern (we have 81 submarkets), there is substantial variation in the size of each submarket. So we prefer to use robust standard errors in the estimation and show robustness of the results to correlations across observations by running firm-level regressions.

G Post-intervention Firm Outcomes

We study whether the intervention resulted in any long-lasting impacts on firms using data from the firm follow-up survey, conducted six months after the matching and signalling intervention. The results are reported in Table A11. The independent variable is the log of *1+total number of workers* employed at follow-up. This takes value zero for firms that have no employees at follow-up. Since the intervention involved matching workers to firms, this is a natural outcome variable to focus on. The Table reports the results of OLS regressions analogous to (1), but estimated at the firm level. As the median firm was matched to three workers, we create an indicator variable for whether the firm was matched to at least one *High Score* worker, and a corresponding indicator variable for whether the firm was matched to only *Low Score* workers. We then interact both indicators with the Treatment dummy. The results show that we do not detect any significant long-lasting impacts on firm size, although the estimates are imprecise, as the sample of firms is small. The lack of impacts is consistent with the low overall take-up of the intervention documented in Table 6.

While overall attrition is low in the firm follow-up (we were able to track about 90% of the sample), information on profits and revenues is missing for about half the sample, who refused to provide this information. Therefore, we are not able to reliably estimate impacts on these additional outcomes.

Table 1: Firm descriptives

Sample: all eligible firms from the census

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

Notes: The Table uses data from the initial census of 1086 firms conducted for the job placement interventions. The census was conducted in 17 urban areas of Uganda, and targeted all firms employing at least 2 employees and operating in six sectors: carpentry, catering, hairdressing, motor-mechanics, tailoring and welding.

Table 2: Worker descriptives

Sample: all eligible trainees from the census

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

Notes: The Table uses data from the census of the 1011 workers eligible to participate in the job placement interventions. The census took place at 15 partner Vocational Training Institutes throughout Uganda, and included all workers currently receiving training in one of the following six sectors: carpentry, catering, hairdressing, motor-mechanics, tailoring, welding, and expected to graduate by February 2015.

Table 3: Selection into the program - Workers

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Worker included in the final research sample [Yes=1]	
	(1)	(2)
A. Skills		
Cognitive test score	.009	-.001
	(.006)	(.006)
Extraversion	.025*	.017
	(.014)	(.014)
Agreeableness	.037**	.034**
	(.016)	(.016)
Conscientiousness	.023	.027*
	(.017)	(.016)
Neuroticism (reversed scale)	.025	.027*
	(.016)	(.015)
Openness to Experience	-.013	.007
	(.018)	(.018)
B. Other worker characteristics		
Age		-.016
		(.033)
Age squared		.000
		(.001)
Female		.199***
		(.046)
Completed prior education		.010
		(.008)
Course duration		-.010
		(.018)
Ever employed		-.070**
		(.033)
Mean of dependent variable	.908	.908
Sector of training dummies	No	Yes
P-value on F-test of joint significance of Big 5 variables	[.002]	[.005]
P-value on F-test of joint significance of sector dummies		[.004]
R-squared	.026	.095
Number of observations (workers)	851	851

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

Table 4: Results on firm owner beliefs about matched workers

OLS regression coefficients, robust standard errors in parentheses

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

Dependent variable:	Owner thought matched worker was MORE SKILLED than usual applicant [Yes=1]			
Sample of firm owners:	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (2) = (3)**
	(1)	**(2)**	**(3)**	**(4)**
(i) High Score Worker	-.013	.034	.054	[.850]
	(.047)	(.068)	(.077)	
(ii) Low Score Worker X Treatment	-.030	.081	.000	[.389]
	(.041)	(.066)	(.067)	
(iii) High Score Worker X Treatment	.038	.140**	-.067	[.027]
	(.044)	(.064)	(.068)	
Mean of Dep. Var. in Control Low Score	.121	.111	.130	
(iv) High Score - Low Score in Treatment	.055	.093	-.014	
	(.047)	(.074)	(.057)	
(v) Treatment effect on average worker	.004	.116**	-.030	[.041]
	(.031)	(.049)	(.051)	
Worker, firm, match and interview controls	Yes	Yes	Yes	
Number of observations (matches)	444	228	216	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the matching surveys are reported. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: number of employees, a dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 - High Score. Firm owners are split in High and Low Ability Owners using their score on the cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners; owners that scored below the median are assigned to the Low Ability group. In Column 1 the sample includes all matches with non-missing information on the variables used for the analysis. In Column 2 it includes only matches with a High Ability Owner. In Column 3 it is restricted to matches with Low Ability Owners. The p-values in Columns 4 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as: (i) + (iii) - (ii).

Table 5: Results on worker outside options

OLS regression coefficients, robust standard errors in parentheses

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

Dependent variable:	**Currently enrolled in education/training [Yes=1]**				**Log(monthly reservation wage) [USD]**				**Main activity in last week is casual work [Yes=1]**			
Sample of workers matched to:	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (2) = (3)**	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (6) = (7)**	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (10) = (11)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
(i) High Score Worker	-.026	-.076	-.036	[.576]	-.029	-.008	-.085	[.659]	.047	-.017	.076	[.312]
	(.032)	(.058)	(.042)		(.081)	(.132)	(.113)		(.045)	(.059)	(.070)	
(ii) Low Score Worker X Treatment	.017	-.016	.009	[.708]	.059	.141	-.022	[.370]	-.017	-.077	-.019	[.451]
	(.033)	(.051)	(.045)		(.079)	(.113)	(.143)		(.037)	(.049)	(.059)	
(iii) High Score Worker X Treatment	.046	.154**	-.018	[.043]	.102	.220*	.016	[.201]	-.085**	-.154***	-.077	[.371]
	(.037)	(.074)	(.042)		(.075)	(.124)	(.101)		(.043)	(.046)	(.072)	
Mean of Dep. Var. in Control Low Score	.044	.063	.030		98.6	91.3	103.9		.096	.104	.091	
(iv) High Score - Low Score in Treatment	.003	.094	-.063		.014	.071	-.047		-.022	-.093**	.018	
	(.041)	(.070)	(.050)		(.084)	(.113)	(.133)		(.038)	(.040)	(.062)	
(v) Treatment effect on average worker	.031	.069	-.005	[.159]	.079	.181**	-.003	[.136]	-.050*	-.116***	-.048	[.265]
	(.024)	(.043)	(.031)		(.055)	(.084)	(.090)		(.028)	(.034)	(.050)	
Worker, match and interview controls	Yes	Yes	Yes		Yes	Yes	Yes		Yes	Yes	Yes	
Number of observations (workers)	437	205	232		431	201	230		437	205	232	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the worker follow-up survey are reported. To correct for selective attrition, observations are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge [2000], where the IPW instrument is the gender of the enumerator assigned to contact the worker for the follow-up survey, and where the observable predictors of attrition are interacted with the High Ability Owners dummy. Standard errors are adjusted for heteroskedasticity. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of first match dummies (2 dummies), number of firms assigned to be matched to the worker during intervention, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. The sample includes workers that were assigned to meet either all High Ability owners or all Low Ability owners (these are 65% of the total sample of workers). In Columns 1, 5 and 9 the sample includes all workers assigned to meet either only High Ability owners or only Low Ability owners. In Columns 2, 6 and 10 the sample is restricted to workers assigned to meet only High Ability owners. In Columns 3, 7 and 11 the sample is restricted to only workers assigned to meet Low Ability owners. The dependent variable in Columns 5-7 is the log of the average monthly reservation wage. Respondents were asked to report their reservation wage for a job that required them to commute for (i) 10 minutes; (ii) 30 minutes or (iii) 60 minutes each way. We use the average of the three reported reservation wages. Observations with a value in the top 1% of the average reservation wage are excluded. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary earnings are then converted in January 2015 USD. The p-values in Columns 4, 8 and 12 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owners dummy. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as: (i) + (iii) - (ii).

Table 6: Results on offers and hires in the intervention

OLS IPW regression coefficients, robust standard errors in parentheses

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

Dependent variable:	Worker was made a job offer by the matched firm [Yes=1]				Worker was hired by the matched firm [Yes=1]			
Sample of firm owners:	All	High Ability Owners	Low Ability Owners	P-value (2) = (3)	All	High Ability Owners	Low Ability Owners	P-value (6) = (7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(i) High Score Worker	.034	.080**	-.032	[.039]	.012	.052**	-.039	[.021]
	(.028)	(.032)	(.044)		(.019)	(.024)	(.031)	
(ii) Low Score Worker X Treatment	.062**	.120***	.018	[.060]	.031*	.080***	-.006	[.032]
	(.026)	(.034)	(.043)		(.019)	(.027)	(.029)	
(iii) High Score Worker X Treatment	.011	.006	.040	[.539]	.003	.006	.023	[.660]
	(.026)	(.033)	(.044)		(.018)	(.027)	(.026)	
Mean of Dep. Var. in Control Low Score	.051	.011	.084		.023	0	.042	
(iv) High Score - Low Score in Treatment	-.016	-.034	-.009		-.017	-.021	-.011	
	(.029)	(.039)	(.047)		(.021)	(.029)	(.030)	
(v) Treatment effect on average worker	.036*	.056**	.029	[.494]	.017	.038*	.008	[.285]
	(.018)	(.024)	(.032)		(.013)	(.020)	(.020)	
Worker, firm, match and interview controls	Yes	Yes	Yes		Yes	Yes	Yes	
Number of observations (matches)	909	467	442		909	467	442	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the worker follow-up survey are reported. To correct for selective attrition, observations are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge [2000], where the IPW instrument is the gender of the enumerator assigned to contact the worker for the follow-up survey, and where the observable predictors of attrition are interacted with the High Ability Owner dummy. Standard errors are adjusted for. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a 10-item Raven matrices cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Columns 1 and 5 the sample includes all matches with non-missing information on the variables used for the analysis. In Columns 2 and 6 it includes only matches with a High Ability Owner. In Columns 3 and 7 it is restricted to matches with Low Ability Owners. The p-values in Columns 4 and 8 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as: (i) + (iii) - (ii).

Table 7: Results on post-intervention labor market outcomes

OLS IPW regression coefficients, robust standard errors in parentheses

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

Dependent variable:	Main activity in last week is wage-employment [Yes=1]				Log(total earnings in last month) [USD]			
Sample of workers matched to:	All	High Ability Owners	Low Ability Owners	P-value (2) = (3)	All	High Ability Owners	Low Ability Owners	P-value (6) = (7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(i) High Score Worker	-.003	.183*	-.091	[.049]	-.419	-.160	-.496	[.543]
	(.071)	(.094)	(.102)		(.272)	(.389)	(.392)	
(ii) Low Score Worker X Treatment	.053	.182*	-.001	[.214]	-.180	-.075	-.509	[.427]
	(.067)	(.103)	(.105)		(.251)	(.390)	(.381)	
(iii) High Score Worker X Treatment	.062	.015	.104	[.527]	.066	.290	-.111	[.456]
	(.067)	(.101)	(.096)		(.264)	(.378)	(.381)	
Mean of Dep. Var. in Control Low Score	.368	.250	.455		36.0	36.2	35.9	
(iv) High Score - Low Score in Treatment	.006	.017	.014		-.173	.205	-.098	
	(.074)	(.117)	(.105)		(.289)	(.439)	(.403)	
(v) Treatment effect on average worker	.057	.098	.052	[.483]	-.063	.106	-.305	[.288]
	(.048)	(.075)	(.074)		(.181)	(.268)	(.277)	
Worker, match and interview controls	Yes	Yes	Yes		Yes	Yes	Yes	
Number of observations (workers)	437	205	232		425	198	227	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the worker follow-up survey are reported. To correct for selective attrition, observations are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge [2000], where the IPW instrument is the gender of the enumerator assigned to contact the worker for the follow-up survey, and where the observable predictors of attrition are interacted with the High Ability Owners dummy. Standard errors are adjusted for. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of first match dummies (2 dummies), number of firms assigned to be matched to the worker during intervention, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. The sample includes workers that were assigned to meet either all High Ability owners or all Low Ability owners (these are 65% of the total sample of workers). In Columns 1 and 5 the sample includes all workers assigned to meet either only High Ability owners or only Low Ability owners. In Columns 2 and 6 the sample is restricted to workers assigned to meet only High Ability owners. In Columns 3 and 7 the sample is restricted to only workers assigned to meet Low Ability owners. The dependent variable in Columns 5-7 is the log of 1+total earnings in the last month from any work activity. This has value 0 for workers that had no income in the last month. Observations with a value of this variable in the top 1% of the distribution are excluded. All monetary amounts are deflated and expressed in terms of the price level in January 2015, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary earnings are then converted in January 2015 USD. The p-values in Columns 4 and 8 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owners dummy. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as (i) + (iii) - (ii).

Table 8: Cost-benefit analysis

	All workers included in the experiment (High Score and Low Score) (1)	All eligible workers (High Score, Low Score and workers that selected out) (2)
Panel A. External parameters		
Total cost per individual at year 0 [USD]	19.10	19.10
Remaining expected productive life of beneficiaries	38 years	38 years
Social discount rate = 5%		
Panel B. Estimated expected annual earnings benefits [USD]		
1 Change in annual earnings per individual in year 1	4.24	1.41
2 Change in annual earnings per individual in year 2	4.24	1.41
3 Change in annual earnings per individual in year 3 and until year 38	6.29	3.00
4 NPV change in annual earnings per individual from year 1 to 38	102.23	47.68
Sensitivity to different assumptions about program impacts		
4.1 *Employment of workers that selected out goes to 0*	102.23	36.93
4.2 *No impact on earnings for Low Score workers*	30.67	-8.03
5 Benefits/cost ratio (assuming benefits last 38 years)	**5.35**	**2.50**
Sensitivity to different discount rates/time horizons		
5.1 *Social discount rate = 10%*	3.02	1.39
5.2 *Benefits last 10 years from program date*	2.34	1.06
5.3 *Benefits last 5 years from program date*	1.23	0.53
Sensitivity to different assumptions about program impacts		
5.4 *Employment of workers that selected out goes to 0*		1.93
5.5 *No impact on earnings for Low Score workers*	1.61	-0.42
6 IRR (assuming benefits last 38 years)	**0.29**	**0.14**
Sensitivity to different time horizons		
6.1 *Benefits last 10 years from program date*	0.26	0.06
6.2 *Benefits last 5 years from program date*	0.12	-0.13
Sensitivity to different assumptions about program impacts		
6.3 *Employment of workers that selected out goes to 0*		0.11
6.4 *No impact on earnings for Low Score workers*	0.09	NA

Notes: The total cost per individual at year 0 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 remaining expected productive life of beneficiaries is computed as average life expectancy in Uganda (58 years), minus average age in the sample at baseline (20 years). The change in annual earnings in year 1 is computed as the change in the probability of wage employment times the stated reservation wage, relative to Control group. These impacts are taken from Table 7 and Table 5, respectively. The changes are estimated for each of the following groups of workers separately: (i) High Score workers matched to High Ability owners; (ii) High Score workers matched to Low Ability owners; (iii) Low Score workers matched to High Ability owners; (iv) Low Score workers matched to Low Ability owners. The impacts reported in the Table are an average of the impacts for these groups of workers, weighted by the shares of workers in each category. For High Score workers matched to High Ability firms we assume that the impacts on reservation wages translate into actual earnings starting in year 3. This reflects the fact that we also observe an impact of the intervention on the probability that this group of workers is back in education, and most training programs last two years. In Column 2, we show how the cost benefit analysis would change under different assumptions about how the workers that selected out of the program would be affected by an expansion of the program that makes certifications compulsory for everyone. For the main calculations, we assume that half the workers that selected out would experience a reduction in earnings equal to the increase in earnings experienced by the Low Score workers matched to High Ability owners. We assume that the intervention would have no impact for the remaining half of the sample that selected out, as Low Ability owners did not react to the certificates. We assume that the cost per person of implementing the program would not change for workers that selected out from the experiment. The dummy variable High Score is constructed taking the first principal component of the 5 skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners by using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group.

Table A1: Selection into the program - Firms

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Firm included in final research sample [Yes=1]
	(1)
A. Owner and firm characteristics	
Owner is female	.028
	(.048)
Number of employees	-.047***
	(.012)
Number of employees squared	.002***
	(.001)
Business has trading licence	-.011
	(.069)
Age of business	.003
	(.006)
Age of business squared	-.000
	(.000)
B. Sector	
Carpentry	.284***
	(.065)
Catering	-.029
	(.053)
Motormechanics	.137*
	(.071)
Tailoring	.067
	(.065)
Welding	.172***
	(.063)
C. Region	
North	.025
	(.057)
East	-.103***
	(.039)
West	-.040
	(.045)
Mean of dependent variable	.383
Number of observations (firms)	957

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The Table uses data from the initial census of firms conducted for the job placement interventions. The excluded sector is hairdressing. The excluded region is Kampala. Firms in the top 1% of number of employees are excluded from the regression.

Table A2: Basic VTI descriptives

Means, standard deviations in parentheses
P-value from test of equality of means in brackets

	VTI took part in the project	VTI did not take part in the project	P-value from test of equality of means [took part=did not take part]
	(1)	(2)	(3)
Number of VTIs	14	7	
A. VTI characteristics			
Years since establishment	35.2	24.7	[.194]
	(30.3)	(9.86)	
Public institution	.214	.143	[.593]
	(.426)	(.378)	
Number of training courses offered	10.9	9.57	[.281]
	(5.46)	(3.64)	
Number of students currently enrolled	349	604	[.958]
	(201)	(447)	
B. Region			
Kampala	.500	.571	[.562]
	(.519)	(.535)	
North	0	0	
	(0)	(0)	
East	.214	.286	[.557]
	(.426)	(.488)	
West	.286	.143	[.443]
	(.469)	(.378)	

Notes: The Table uses data from the 21 VTIs that were contacted about the intervention and for which information is available. There are two additional VTIs that did not take part in the project for which this information is missing, and one additional VTI that took part in the project for which this information is missing. Column 3 reports p-values from two-sided t-tests for all variables apart from the dummy variables (region dummies and private vs public dummy), for which the p-value is from Fisher exact test.

Table A3: 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 A4: Correlation between Big 5 traits and soft skills

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Attendance	Communication skills	Trustworthiness	Willingness to help others	Creativity
	(1)	(2)	(3)	(4)	(5)
Extraversion	.002	.060*	-.043	.008	.003
	(.032)	(.033)	(.038)	(.033)	(.040)
Agreeableness	.007	.028	-.049	.022	.094*
	(.037)	(.036)	(.043)	(.039)	(.049)
Conscientiousness	.037	.024	.089**	.012	.032
	(.039)	(.036)	(.043)	(.041)	(.048)
Neuroticism (reversed scale)	-.006	-.001	.041	.043	.032
	(.034)	(.034)	(.042)	(.040)	(.044)
Openness to Experience	.008	-.010	-.000	-.006	.054
	(.043)	(.042)	(.049)	(.046)	(.056)
Worker controls	Yes	Yes	Yes	Yes	Yes
Number of observations (workers)	678	678	678	678	678

Notes: *** (**) (*) indicates 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. 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). Worker controls (all measured at baseline) include: a dummy for female, age and age squared, a dummy for any work experience, duration of VTI course, prior formal education level, dummies for the VTI attended (14 dummies), sector of training dummies (5 dummies), a dummy for whether the trainee scored on or above the median on a 10-item Raven matrices cognitive test.

Table A5: Baseline balance on firm characteristics

Means, standard deviations in parentheses

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

P-value on F-tests in braces

	Control Firms	Treatment Firms	P-value from t-test [Treatment=Control]
	(1)	(2)	(3)
Number of firms	211	211	
A. Owner characteristics			
Owner is female	.360	.374	[.499]
	(.481)	(.485)	
Owner age [Years]	35.7	36.1	[.392]
	(8.71)	(8.54)	
Owner completed years of education	10.6	10.2	[.410]
	(3.23)	(3.37)	
Owner has received training from a VTI	.422	.341	[.073]*
	(.495)	(.475)	
Owner scored at median or above on cognitive test	.538	.554	[.962]
	(.499)	(.498)	
B. Firm characteristics			
Business is registered	.915	.919	[.983]
	(.280)	(.273)	
Number of employees	2.98	2.79	[.423]
	(2.91)	(2.47)	
Age of business [Years]	6.73	6.97	[.629]
	(5.28)	(6.52)	
Average monthly revenues [USD]	548	565	[.520]
	(642)	(677)	
Average monthly profits [USD]	214	213	[.927]
	(270)	(228)	
F-test of joint significance from column regression		{.520}	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Data is from the 422 firms included in the final research sample. Firms in the top 1% of the profit and revenues distribution are excluded from the balance checks on such variables. The t-tests are from OLS regressions of the variable of interest on a constant, treatment dummy and strata fixed effects. There are 81 strata, where a stratum is a sector-urban area combination. 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 strata fixed effects. Standard errors are robust in such regression.

Table A6: Baseline balance on worker characteristics

Means, standard deviations in parentheses

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

P-value on F-tests in braces

	Control Workers	Treatment Workers	P-value from t-test [Treatment=Control]
	(1)	(2)	(3)
Number of workers	397	390	
A. Basic worker characteristics			
Age [Years]	20.3	20.6	[.115]
	(2.43)	(2.81)	
Female	.504	.492	[.802]
	(.501)	(.501)	
Completed prior education [Years]	10.2	10.3	[.442]
	(1.88)	(1.85)	
Course duration [Years]	1.50	1.48	[.968]
	(.898)	(.857)	
Ever employed	.194	.218	[.726]
	(.396)	(.413)	
B. Skills			
Attendance [1-5 scale]	3.39	3.34	[.893]
	(1.13)	(1.14)	
Communication skills [1-5 scale]	3.23	3.25	[.492]
	(1.08)	(1.13)	
Creativity [1-5 scale]	3.38	3.43	[.445]
	(1.11)	(1.11)	
Trustworthiness [1-5 scale]	3.49	3.53	[.470]
	(1.01)	(.974)	
Willingness to help others [1-5 scale]	3.34	3.32	[.929]
	(1.10)	(1.07)	
Cognitive test score [0-10 scale]	5.23	5.21	[.506]
	(2.49)	(2.43)	
C. Matching assignments			
Matched to more than one firm	.295	.315	[.396]
	(.456)	(.465)	
Matched in other district than first preference	.280	.269	[.485]
	(.449)	(.444)	
Matched in district not among first three preferences	.078	.103	[.236]
	(.269)	(.304)	
F-test of joint significance		{.809}	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Data is from the 787 trainees included in the final research sample. The t-tests are from OLS regressions of the variable of interest on a constant, treatment dummy and strata fixed effects. There are 81 strata, where a stratum is a sector-urban area combination. 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 strata fixed effects. Standard errors are robust in such regression.

Table A7: Predictors of whether match was carried out

OLS regression coefficients, robust standard errors in parentheses

Dependent variable:	Match was successfully carried out [Yes=1]			
Sample of matches:	All	Matches with High Ability Owner	Matches with Low Ability Owner	P-value (2) = (3)
	(1)	(2)	(3)	(4)
(i) High Score Worker	.082*	.134*	.029	[.284]
	(.048)	(.069)	(.070)	
(ii) Low Score Worker X Treatment	.058	.038	.085	[.617]
	(.044)	(.063)	(.068)	
(iii) High Score Worker X Treatment	-.058	-.131**	.036	[.061]
	(.043)	(.059)	(.067)	
Mean of De. Var. in Control	.421	.413	.429	
(iv) High Score - Low Score in Treatment	-.034	-.035	-.020	
	(.048)	(.068)	(.071)	
(v) Treatment effect on average worker	-.002	-.057	.061	[.076]
	(.031)	(.044)	(.050)	
Worker, firm, match and interview controls	Yes	Yes	Yes	
Number of observations (matches)	1006	525	481	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the matching surveys are reported. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners by using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Column 1 the sample includes all matches with non-missing information on the variables used for the analysis. In Column 2 it includes only matches with a High Ability Owner. In Column 3 it is restricted to matches with Low Ability Owners. The p-values in Columns 4 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The Treatment effect on the average firm is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the Matched with at least one High Score dummy. The High Score - Low Score in Treatment effect is defined as (i) + (iii) - (ii).

Table A8: Attrition

OLS regression coefficients, robust standard errors in parentheses

	Firms		Workers		Matches (firm data)		Matches (worker data)	
Dependent variable:	Firm in sample at firm follow-up [Yes=1]		Worker in sample at worker follow-up [Yes=1]		Match in sample at firm follow-up [Yes=1]		Match in sample at worker follow-up [Yes=1]	
Sample:	High Ability Owners	Low Ability Owners	Matched to High Ability Owners	Matched to Low Ability Owners	High Ability Owners	Low Ability Owners	High Ability Owners	Low Ability Owners
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(i) Matched with at least one High Score Worker	.122	-.053						
	(.082)	(.078)						
(ii) Matched only with Low Score Workers X Treatment	.190**	-.138						
	(.095)	(.096)						
(iii) Matched with at least one High Score Worker X Treatment	-.068	.022						
	(.053)	(.068)						
(iv) High Score Worker			-.030	-.073	-.000	-.025	-.051	-.051
			(.071)	(.059)	(.017)	(.031)	(.046)	(.037)
(v) Low Score Worker X Treatment			.029	-.032	.015	.005	.036	-.026
			(.065)	(.060)	(.014)	(.024)	(.044)	(.035)
(vi) High Score Worker X Treatment			.028	.087	-.013	.029	.093**	.021
			(.073)	(.058)	(.014)	(.024)	(.037)	(.037)
Mean of De. Var. in Control group	.864	.962	.814	.892	.991	.968	.872	.944
Firm baseline controls	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Worker baseline controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Match and interview controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	193	162	244	269	525	481	525	481

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Data is from baseline and follow-up surveys. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was matched in a different urban area than the preferred one. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners by using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. The Matched with at least one High Score dummy takes value 1 if the firm was matched to at least one worker that had High Score=1. The Matched only with Low Score dummy takes value 1 if the firm was matched only with Low Score workers. Firm owners are split into high and low ability owners by using their score on a 10-item Raven matrices cognitive test. Firm owners that scored on or above the median are defined as high ability owners, while owners that scored below the median are assigned to the low ability group.

Table A9A: Results on offers and hires - Two-step selection model

OLS IPW regression coefficients, bootstrap standard errors in parentheses

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

	Job Offers			Hires		
Depdendent variable:	**Worker was made a job offer by the matched firm [Yes=1]**			**Worker was hired by the matched firm [Yes=1]**		
Sample of firm owners:	**High Ability Owner**	**Low Ability Owner**	**P-value (1) = (2)**	**High Ability Owner**	**Low Ability Owner**	**P-value (4) = (5)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
(i) High Score Worker	.253*	-.092	[.078]	.133	-.085	[.097]
	(.144)	(.144)		(.105)	(.090)	
(ii) Low Score Worker X Treatment	.333**	-.018	[.040]	.204*	-.009	[.113]
	(.142)	(.119)		(.112)	(.084)	
(iii) High Score Worker X Treatment	.010	.054	[.835]	.035	-.007	[.781]
	(.125)	(.163)		(.098)	(.113)	
Mean of Dep. Var. in Control Low Score	.032	.213		0	.106	
(iv) High Score - Low Score in Treatment	-.070	-.020		-.035	-.084	
	(.133)	(.160)		(.093)	(.103)	
(v) Treatment effect on average worker	.142	.015	[.352]	.104	-.008	[.302]
	(.101)	(.103)		(.082)	(.075)	
P-value from joint F-test on worker inv. Mills ratio and interactions with predicted values	[.845]	[.655]		[.679]	[.229]	
P-value from joint F-test on firm inv. Mills ratio and interactions with predicted values	[.851]	[.656]		[.737]	[.339]	
P-value from joint F-test on worker and firm inv. Mills ratio and interactions with predicted values	[.951]	[.796]		[.842]	[.331]	
Worker, firm, match and interview controls	Yes	Yes		Yes	Yes	
Number of observations (matches)	170	176		170	176	
First Stage						
P-value from joint F-test on excluded variables in Worker selection equation			[.002]			
P-value from joint F-test on excluded variables in Firm selection equation			[.050]			

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the worker follow-up survey are reported. The Table reports the results of the estimation of a two-sided two-step selection model. To correct for selective attrition, observations are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge [2000], where the IPW instrument is the gender of the enumerator assigned to contact the worker for the follow-up survey, and where the observable predictors of attrition are interacted with the High Ability Owner dummy. Standard errors are bootstrapped, where the re-sampling is clustered by firm, with 500 replications. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a 10-item Raven matrices cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners by using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Columns 1 and 4 the sample includes only matches with a High Ability Owner. In Columns 2 and 5 the sample is restricted to matches with Low Ability Owners. The p-values in Columns 3 and 6 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The instruments in the first step are the years of education and gender of the enumerator assigned to contact, respectively, the matched worker and the matched firm at the time of the intervention. The F-tests in the second step are on the inverse Mills ratios and their interactions with the predicted values from the first step. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as: (i) + (iii) - (ii).

Table A9B: Results on offers and hires - Selected sample

OLS IPW regression coefficients, robust standard errors in parentheses

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

	Job Offers			Hires		
Depdendent variable:	Worker was made a job offer by the matched firm [Yes=1]			Worker was hired by the matched firm [Yes=1]		
Sample of firm owners:	High Ability Owners	Low Ability Owners	P-value (1) = (2)	High Ability Owners	Low Ability Owners	P-value (4) = (5)
	(1)	(2)	(3)	(4)	(5)	(6)
(i) High Score Worker	.223**	-.109	[.016]	.124*	-.117	[.031]
	(.088)	(.105)		(.072)	(.085)	
(ii) Low Score Worker X Treatment	.321***	-.022	[.009]	.195**	-.021	[.032]
	(.098)	(.086)		(.079)	(.062)	
(iii) High Score Worker X Treatment	.025	.057	[.815]	.029	.011	[.850]
	(.084)	(.105)		(.068)	(.072)	
Mean of Dep. Var. in Control Low Score	.032	.213		0	.106	
(iv) High Score - Low Score in Treatment	-.073	-.031		-.042	-.085	
	(.103)	(.107)		(.075)	(.075)	
(v) Treatment effect on average worker	.139**	.013	[.182]	.093*	-.007	[.152]
	(.065)	(.068)		(.052)	(.047)	
Worker, firm, match and interview controls	Yes	Yes		Yes	Yes	
Number of observations (matches)	170	177		170	177	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the worker follow-up survey are reported. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Columns 1 and 4 the sample includes only matches with a High Ability Owner. In Columns 2 and 5 the sample is restricted to matches with Low Ability Owners. The p-values in Columns 3 and 6 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as: (i) + (iii) - (ii).

Table A9C: Results on offers and hires - Firm reports

OLS regression coefficients, robust standard errors in parentheses

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

	Job Offers			Hires		
Dependent variable:	**Worker was made a job offer by the matched firm [Yes=1]**			**Worker was hired by the matched firm [Yes=1]**		
Sample of firm owners:	**High Ability Owners**	**Low Ability Owners**	**P-value (1) = (2)**	**High Ability Owners**	**Low Ability Owners**	**P-value (4) = (5)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
(i) High Score Worker	.150**	.008	[.094]	.060**	-.059*	[.009]
	(.063)	(.057)		(.030)	(.034)	
(ii) Low Score Worker X Treatment	.139**	.020	[.134]	.058**	-.017	[.094]
	(.055)	(.057)		(.028)	(.034)	
(iii) High Score Worker X Treatment	-.088*	-.049	[.625]	-.004	.007	[.784]
	(.053)	(.059)		(.030)	(.025)	
Mean of Dep. Var. in Control Low Score	.194	.172		.019	.074	
(iv) High Score - Low Score in Treatment	-.077	-.061		-.002	-.035	
	(.062)	(.057)		(.031)	(.028)	
(v) Treatment effect on average worker	.013	-.014	[.649]	.024	-.006	[.340]
	(.039)	(.044)		(.020)	(.022)	
Worker, firm, match and interview controls	Yes	Yes		Yes	Yes	
Number of observations (matches)	520	463		520	463	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results are from the firm follow-up survey. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Columns 1 and 4 the sample includes only matches with a High Ability Owner. In Columns 2 and 5 the sample is restricted to matches with Low Ability Owners. The p-values in Columns 3 and 6 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as: (i) + (iii) - (ii).

Table A9D: Results on offers and hires - Firm-level evidence

OLS regression coefficients, robust standard errors in parentheses

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

	Job Offers			Hires		
Dependent variable:	**Firm made at least one job offer among matched workers [Yes=1]**			**Firm hired at least one of the matched workers [Yes=1]**		
Sample of firm owners:	**High Ability Owners**	**Low Ability Owners**	**P-value (1) = (2)**	**High Ability Owners**	**Low Ability Owners**	**P-value (4) = (5)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
(i) Matched with at least one High Score Worker	.307**	-.161	[.014]	.178**	-.214**	[.003]
	(.131)	(.137)		(.085)	(.099)	
(ii) Matched only with Low Score Workers X Treatment	.467***	.121	[.131]	.283***	.015	[.106]
	(.148)	(.174)		(.104)	(.129)	
(iii) Matched with at least one High Score Worker X Treatment	-.078	-.029	[.709]	.014	.041	[.731]
	(.083)	(.102)		(.059)	(.053)	
Mean of Dep. Var. in Control Low Score	.273	.440		.045	.200	
(iv) High Score - Low Score in Treatment	-.238*	-.311*		-.092	-.188	
	(.134)	(.159)		(.099)	(.114)	
(v) Treatment effect on average firm	.040	.013	[.131]	.072	.034	[.106]
	(.073)	(.090)		(.052)	(.055)	
Firm and interview controls	Yes	Yes		Yes	Yes	
Number of observations (firms)	189	153		189	153	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the firm follow-up survey are reported. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared, dummies for sector of operation (5 dummies); dummies for region of operation (3 dummies). Interview controls include: month of interview dummy. The dummy variable High Score is constructed taking the first principal component of the 5 skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. The Matched with at least one High Score dummy takes value 1 if the firm was matched to at least one worker that had High Score=1. The Matched only with Low Score dummy takes value 1 if the firm was matched only with Low Score workers. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Columns 1 and 4 the sample includes only High Ability Owners. In Columns 2 and 5 the sample is restricted to Low Ability Owners. The p-values in Columns 3 and 6 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The Treatment effect on the average firm is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the Matched with at least one High Score dummy. The High Score - Low Score in Treatment effect is defined as (i) + (iii) - (ii).

Table A9E: Results on offers and hires - Alternative skills aggregation

OLS IPW regression coefficients, robust standard errors in parentheses

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

	Job Offers			Hires		
Dependent variable:	Worker was made a job offer by the matched firm [Yes=1]			Worker was hired by the matched firm [Yes=1]		
Sample of firm owners:	High Ability Owners	Low Ability Owners	P-value (1) = (2)	High Ability Owners	Low Ability Owners	P-value (4) = (5)
	(1)	(2)	(3)	(4)	(5)	(6)
(i) High Score Worker	.072**	-.050	[.024]	.043**	-.013	[.151]
	(.029)	(.045)		(.022)	(.032)	
(ii) Low Score Worker X Treatment	.117***	.047	[.218]	.072***	.006	[.081]
	(.035)	(.044)		(.027)	(.026)	
(iii) High Score Worker X Treatment	.010	.020	[.844]	.013	.014	[.990]
	(.033)	(.041)		(.027)	(.028)	
Mean of Dep. Var. in Control Low Score	.011	.084		0	.034	
(iv) High Score - Low Score in Treatment	-.035	-.076		-.015	-.005	
	(.041)	(.047)		(.032)	(.027)	
(v) Treatment effect on average worker	.056**	.033	[.576]	.038*	.010	[.310]
	(.025)	(.032)		(.020)	(.019)	
Worker, firm, match and interview controls	Yes	Yes		Yes	Yes	
Number of observations (matches)	467	442		467	442	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the worker follow-up survey are reported. To correct for selective attrition, observations are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge [2000], where the IPW instrument is the gender of the enumerator assigned to contact the worker for the follow-up survey, and where the observable predictors of attrition are interacted with the High Ability Owner dummy. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was not matched in the preferred urban area. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the sum of the 5 skills reported on the certificates and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners by using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Columns 1 and 4 the sample includes only matches with a High Ability Owner. In Columns 2 and 5 the sample is restricted to matches with Low Ability Owners. The p-values in Columns 3 and 6 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy. The Treatment effect on the average worker is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the High Score dummy. The High Score - Low Score in Treatment effect is defined as: (i) + (iii) - (ii).

Table A9F: Results on offers and hires - Lee [2009] bounds

Lee [2009] bounds for attrition correction, standard errors in parentheses

		Job Offers		Hires	
	Dependent variable:	Worker was made a job offer by the matched firm [Yes=1]		Worker was hired by the matched firm [Yes=1]	
	Sample of firm owners:	High Ability Owners	Low Ability Owners	High Ability Owners	Low Ability Owners
		(1)	(2)	(3)	(4)
(i) High Score Worker	Upper bound	.084***	.033	.053***	.006
		(.024)	(.053)	(.020)	(.039)
	Lower bound	.076***	-.000	.053***	-.021
		(.024)	(.042)	(.020)	(.030)
(ii) Low Score Worker X Treatment	Upper bound	.107***	.061	.065***	.007
		(.033)	(.041)	(.024)	(.031)
	Lower bound	.055	.035	.017	.001
		(.054)	(.040)	(.036)	(.025)
(iii) High Score Worker X Treatment	Upper bound	-.008	.013	-.004	.012
		(.035)	(.056)	(.028)	(.037)
	Lower bound	-.051	.006	-.035	.000
		(.042)	(.042)	(.038)	(.024)
Number of observations (matches)		533	489	533	489

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the worker follow-up survey are reported. The Table reports Lee [2009] bounds for attrition correction. We are able to control for the gender of the worker to tighten the bounds. The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group.

Table A9G: Results on offers and hires - Romano and Wolf [2016] p-values

OLS IPW regression coefficients, robust standard errors in parenthesis

Romano and Wolf [2016] p-values in braces: unadjusted on the left; corrected for multiple testing on the right

	Job Offers			Hires		
Dependent variable:	Worker was made a job offer by the matched firm [Yes=1]			Worker was hired by the matched firm [Yes=1]		
Sample of firm owners:	High Ability Owners	Low Ability Owners	P-value (1) = (2)	High Ability Owners	Low Ability Owners	P-value (4) = (5)
	(1)	(2)	(3)	(4)	(5)	(6)
(i) High Score Worker	.080	-.032	{.100 ; .424}	.052	-.039	{.060 ; .267}
	(.032)	(.044)		(.024)	(.031)	
	{.012 ; .020}	{.634 ; .840}		{.041 ; .070}	{.368 ; .659}	
(ii) Low Score Worker X Treatment	.120	.018	{.076 ; .468}	.080	-.006	{.041 ; .307}
	(.034)	(.043)		(.027)	(.029)	
	{.003 ; .004}	{.673 ; .840}		{.022 ; .036}	{.859 ; .859}	
(iii) High Score Worker X Treatment	.006	.040	{.715 ; .946}	.006	.023	{.798 ; .938}
	(.033)	(.044)		(.027)	(.026)	
	{.849 ; .849}	{.438 ; .840}		{.825 ; .825}	{.458 ; .673}	
Mean of Dep. Var. in Control Low Score	.011	.084		0	.042	
Worker, firm, match and interview controls	Yes	Yes		Yes	Yes	
Number of observations (matches)	467	442		467	442	

Notes: Results from the worker follow-up survey are reported. To correct for selective attrition, observations are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge [2000], where the IPW instrument is the gender of the enumerator assigned to contact the worker for the follow-up survey, and where the observable predictors of attrition are interacted with the High Ability Owner dummy. P-values are calculated using the bootstrap Romano and Wolf [2016] procedure, where bootstrap replications are conducted with 5000 replications. Firm controls (measured at baseline) include: number of employees, dummy for female owner, business age and business age squared. Worker controls (measured at baseline) include: dummy for female, age and age squared, dummy for any work experience, duration of VTI course, prior formal education level, dummies for the largest VTIs (3 dummies), dummy for whether the trainee scored on or above the median on a cognitive test. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of match dummies (2 dummies), number of workers assigned to be matched to the firm, number of firms assigned to be matched to the worker, dummy for whether the worker was matched in a different urban area than the preferred one. Interview controls include: dummies for month of interview (1 dummy). The variable High Score is constructed taking the first principal component of the 5 soft skills reported on the transcripts. The variable High Score takes value 1 if the worker ranked at the median or above on the sum of the 5 skills reported on the certificates and 0 otherwise. The Low Score variable is defined as 1 – High Score. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Columns 1 and 5 the sample includes all matches with non-missing information on the variables used for the analysis. In Columns 2 and 6 the sample includes only matches with a High Ability Owner. In Columns 3 and 7 the sample is restricted to matches with Low Ability Owners. The p-values in Columns 4 and 8 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owner dummy.

Table A10: Total Earnings in First Week Worked at Matched Firm

	Mean (1)	SD (2)
(i) Low Score in Control	1.56	2.60
(ii) High Score in Control	4.04	6.99
(iii) Low Score in Treatment	4.11	6.02
(iv) High Score in Treatment	4.49	8.84

Notes: Data is from the worker follow-up survey. All monetary amounts are in USD. The sample includes the 36 workers that were hired in the intervention and for whom earnings data are available. The observation with the highest earnings is excluded from the analysis. The variable High Score takes value 1 if the worker ranked at the median or above on the sum of the 5 skills reported on the certificates and 0 otherwise. The variable High Score takes value 1 if the worker ranked at the median or above on the sum of the 5 skills reported on the certificates and 0 otherwise. The Low Score variable takes value 1 if the worker ranked below the median on the sum of the 5 skills reported on the certificates and 0 otherwise.

Table A11: Results on post-intervention firm outcomes

OLS IPW regression coefficients, robust standard errors in parentheses

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

Dependent variable:	**Log(number of employees)**			
Sample of fims:	**All**	**High Ability Owners**	**Low Ability Owners**	**P-value (2) = (3)**
	(1)	**(2)**	**(3)**	**(4)**
(i) Matched with at least one High Score Worker	.023	-.080	.036	[.474]
	(.080)	(.098)	(.130)	
(ii) Matched only with Low Score Workers X Treatment	-.025	-.147	.128	[.138]
	(.094)	(.126)	(.136)	
(iii) Matched with at least one High Score Worker X Treatment	-.004	.049	.006	[.675]
	(.056)	(.079)	(.086)	
Mean of Dep. Var. in Control Low Score	2.68	2.76	2.58	
(iv) High Score - Low Score in Treatment	.044	.116	-.091	
	(.080)	(.115)	(.109)	
(v) Treatment effect on average firm	-.010	.007	.037	[.770]
	(.049)	(.071)	(.073)	
Firm, match and interview controls	Yes	Yes	Yes	
Number of observations (workers)	314	169	145	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Results from the firm follow-up survey are reported. To correct for selective attrition, observations are weighted using the Inverse Probability Weighting (IPW) procedure described in Wooldridge [2000], where the IPW instruments are: age of the enumerator and age squared, score of the enumerator on an ability test and score squared, gender of the enumerator; and where the observable predictors of attrition are interacted with the High Ability Owners dummy. Standard errors are adjusted for heteroskedasticity. Firm controls (measured at baseline) include: log of 1+number of employees, dummy for female owner, business age and business age squared. Match controls include: dummies for sector of match (5 dummies), dummies for region of match (3 dummies), month of first match dummies (2 dummies), number of workers assigned to be matched to worker during intervention. Interview controls include: dummies for month of interview (1 dummy). The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component and 0 otherwise. The Low Score variable is defined as 1 – High Score. The Matched with at least one High Score dummy takes value 1 if the firm was matched to at least one worker that had High Score=1. The Matched only with Low Score dummy takes value 1 if the firm was matched only with Low Score workers. Firm owners are split into High and Low Ability Owners using their score on a cognitive test administered at baseline. Owners that scored on or above the median are defined as High Ability Owners, while owners that scored below the median are assigned to the Low Ability group. In Column 1 the sample includes all firms. In Column 2 it is restricted to High Ability firm owners. In Column 3 it is restricted to Low Ability firm owners. The dependent variable is the log of 1+number of employees at the time of the survey. The variable takes value 0 for firms with no employees. The p-values in Column 4 are from similar OLS regressions estimated on the full sample and where each independent variable is interacted with the High Ability Owners dummy. The Treatment effect on the average firm is the estimated coefficient on the Treatment indicator in a similar OLS regression of the outcome on Treatment, controlling for the Matched with at least one High Score dummy. The High Score - Low Score in Treatment effect is defined as (i) + (iii) - (ii).

Figure 1: Project timeline

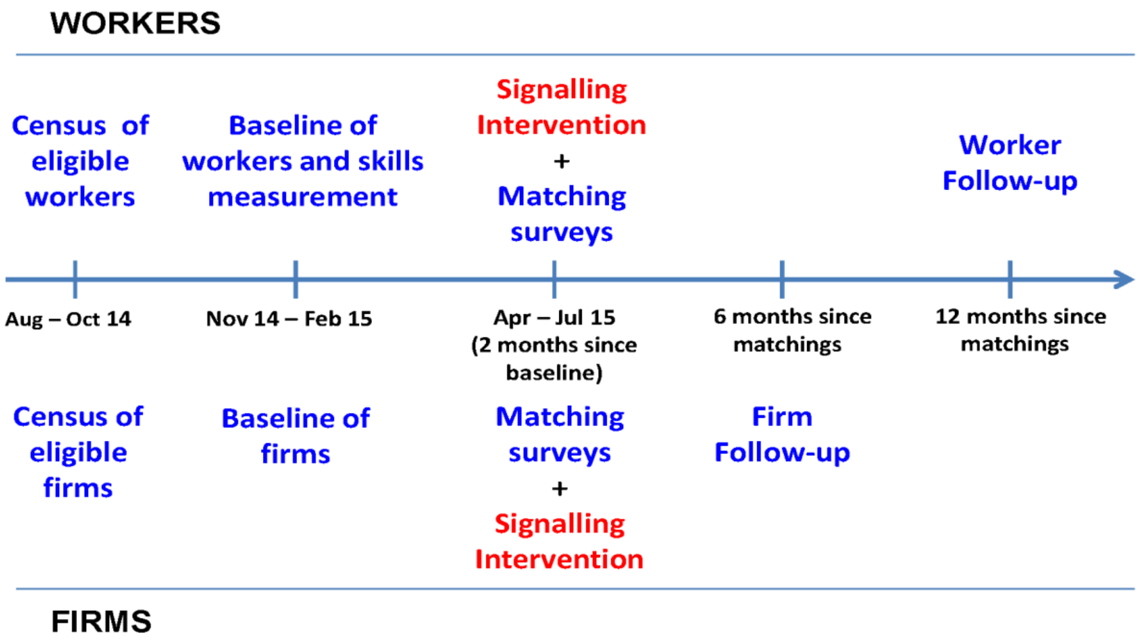
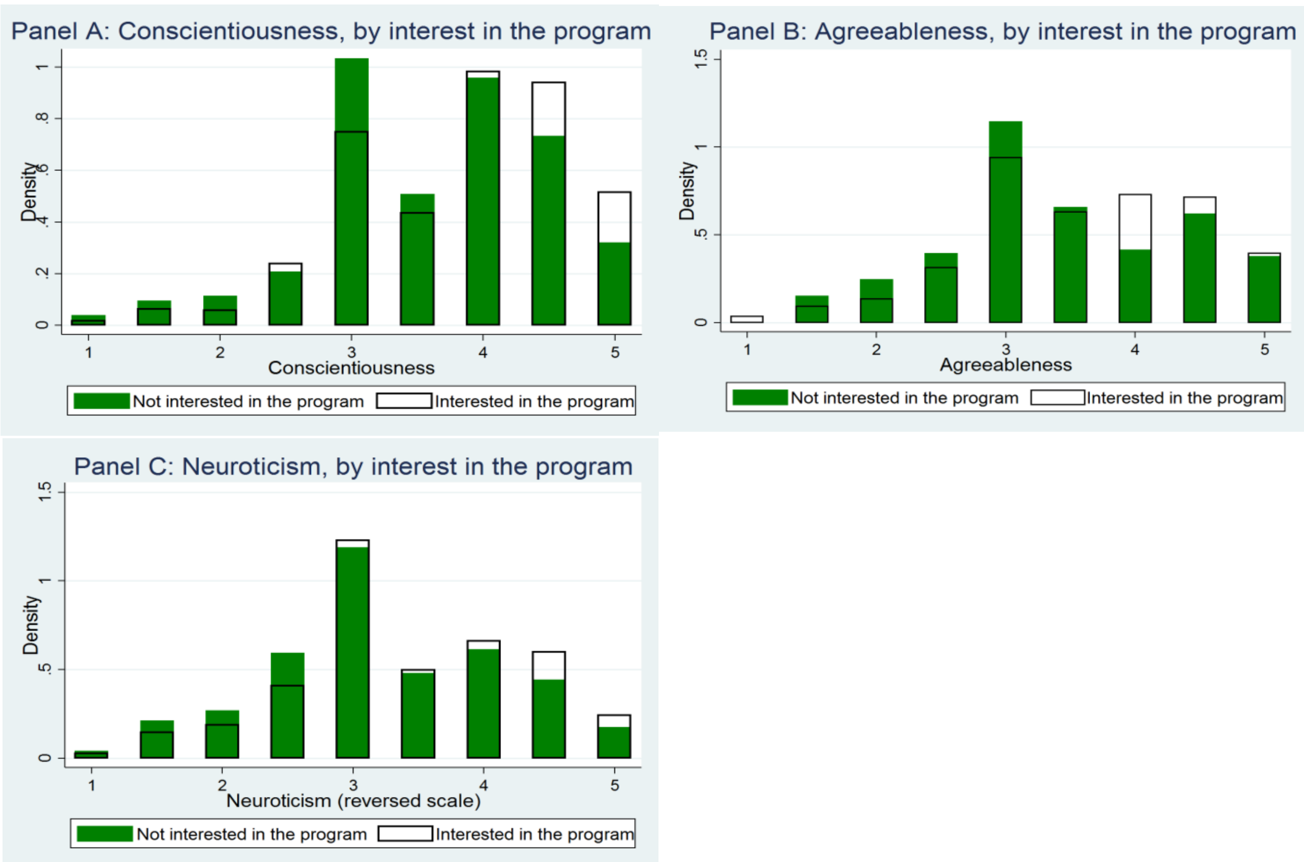


Figure 2: Distribution of worker soft skills, by participation in the program



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

Figure 3: Model predictions

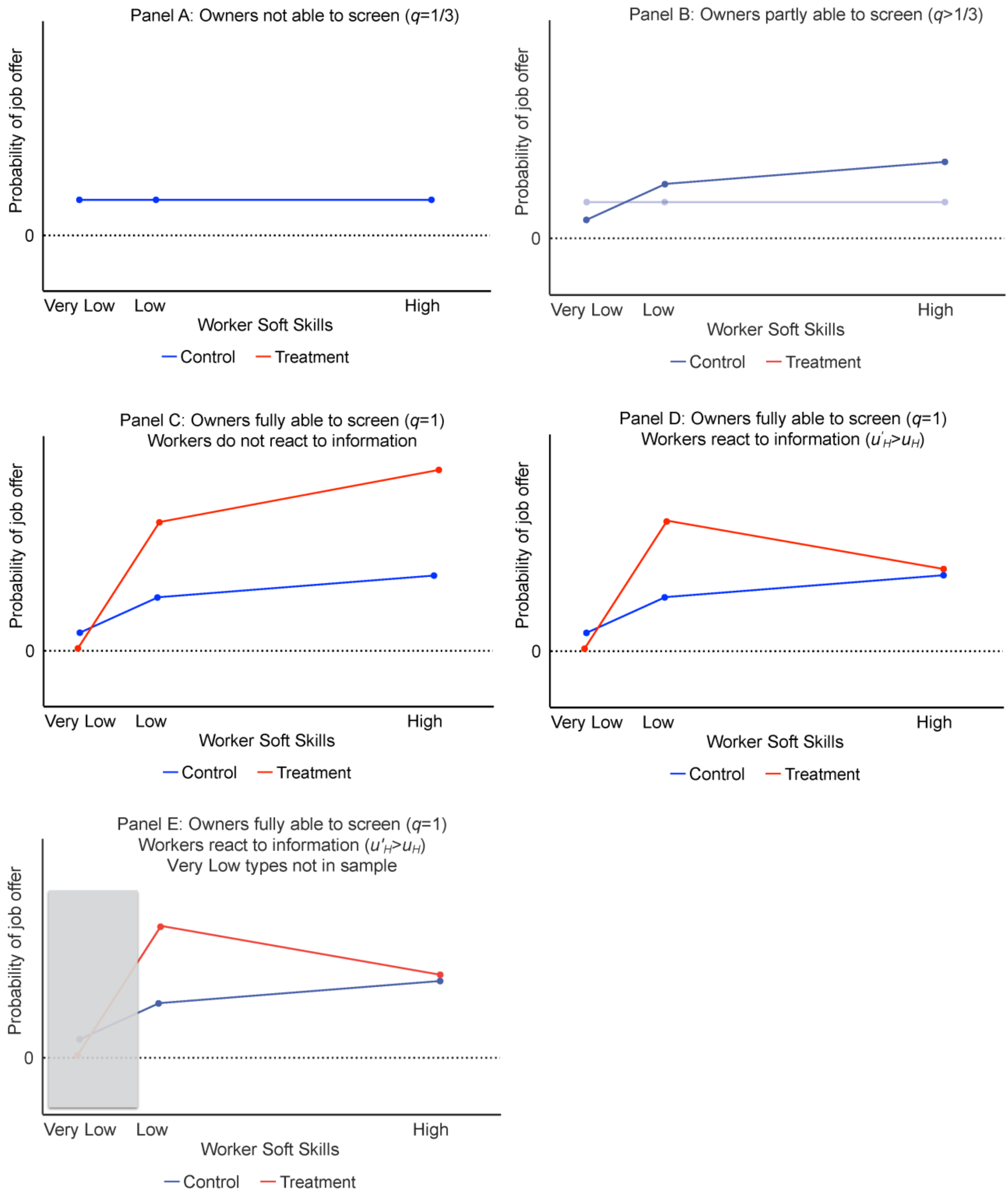
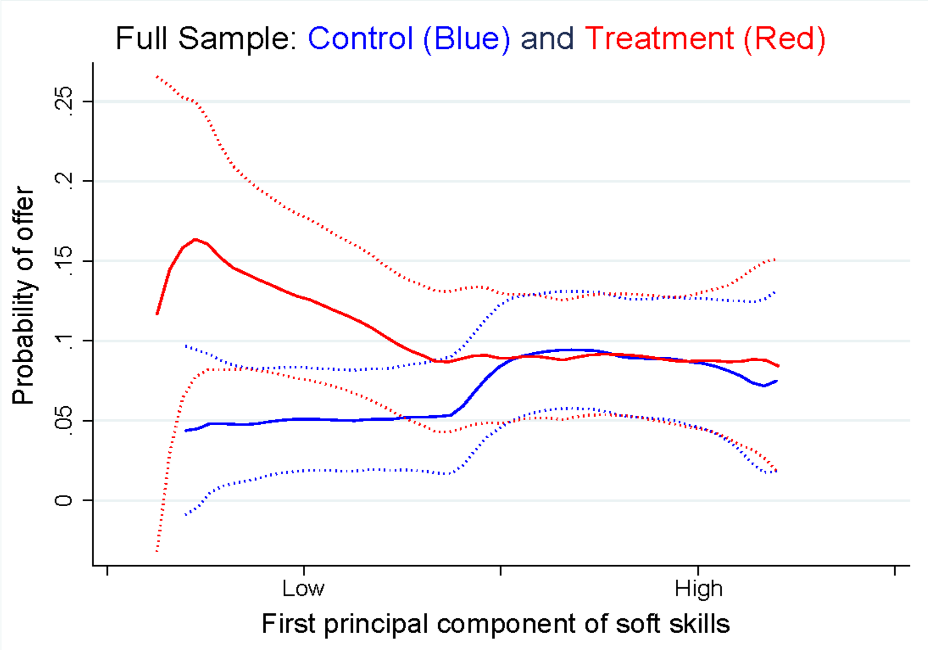
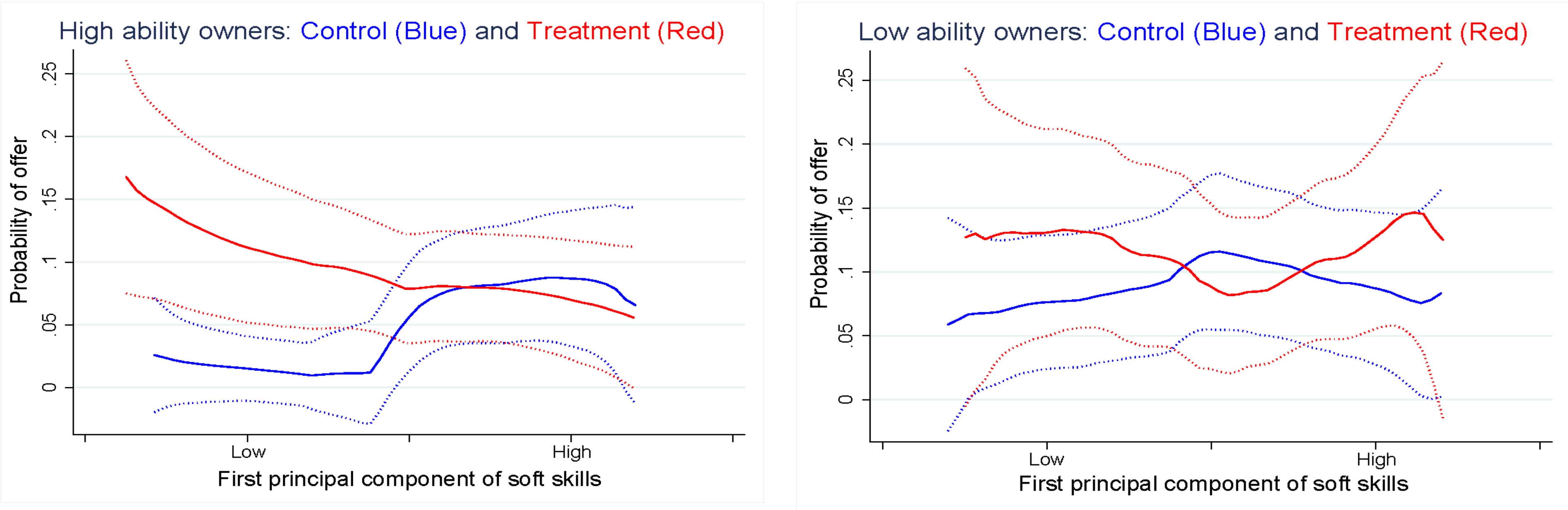


Figure 4: Results on offers in the intervention - Non-parametric estimation



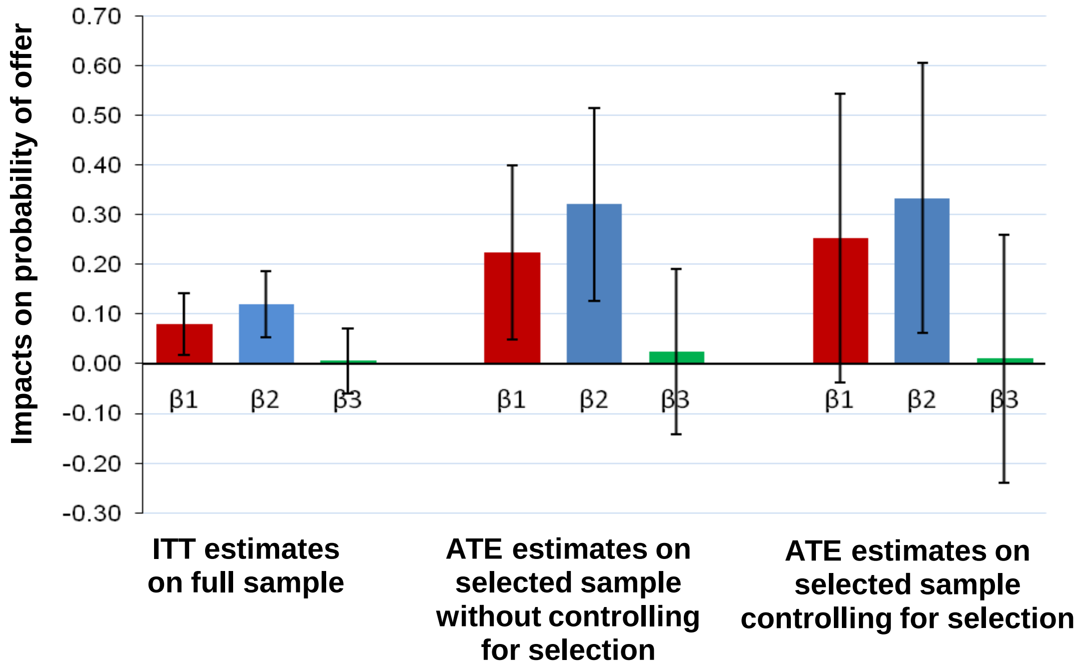
Notes: The Figure reports estimates from kernel-weighted local polynomial regressions of the probability of offer on the first principal component of soft skills. The solid lines correspond to the estimates. The dotted lines are 95% confidence intervals. The estimates in blue are for the Control group. The ones in red are for the Treatment group.

Figure 5: Results on offers in the intervention - Non-parametric estimation, by owner ability



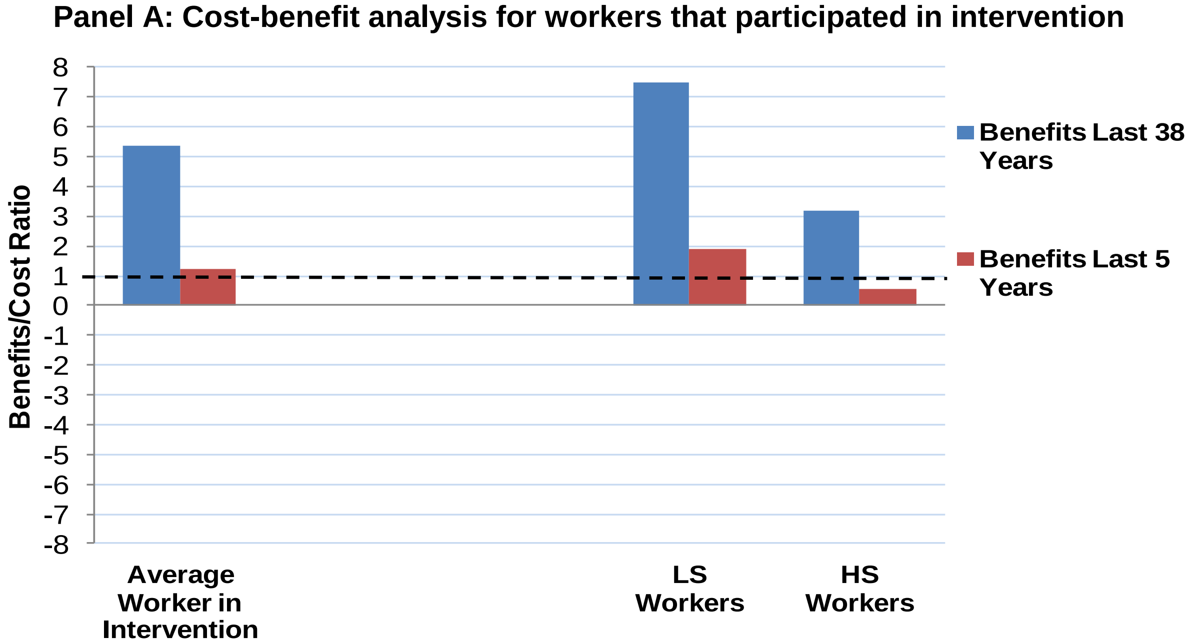
Notes: The Figure reports estimates from kernel-weighted local polynomial regressions of the probability of offer on the first principal component of soft skills. The solid lines correspond to the estimate. The dotted lines are 95% confidence intervals. The estimates in blue are for the Control group. The ones in red are for the Treatment group. The left panel uses the subsample of matches with High ability firm owners. The right panel uses the subsample of matches with Low ability owners. Firm owners are split into high and low ability owners by using their score on a 10-item Raven matrices cognitive test. Firm owners that scored on or above the median are defined as High ability owners, while owners that scored below the median are assigned to the Low ability group.

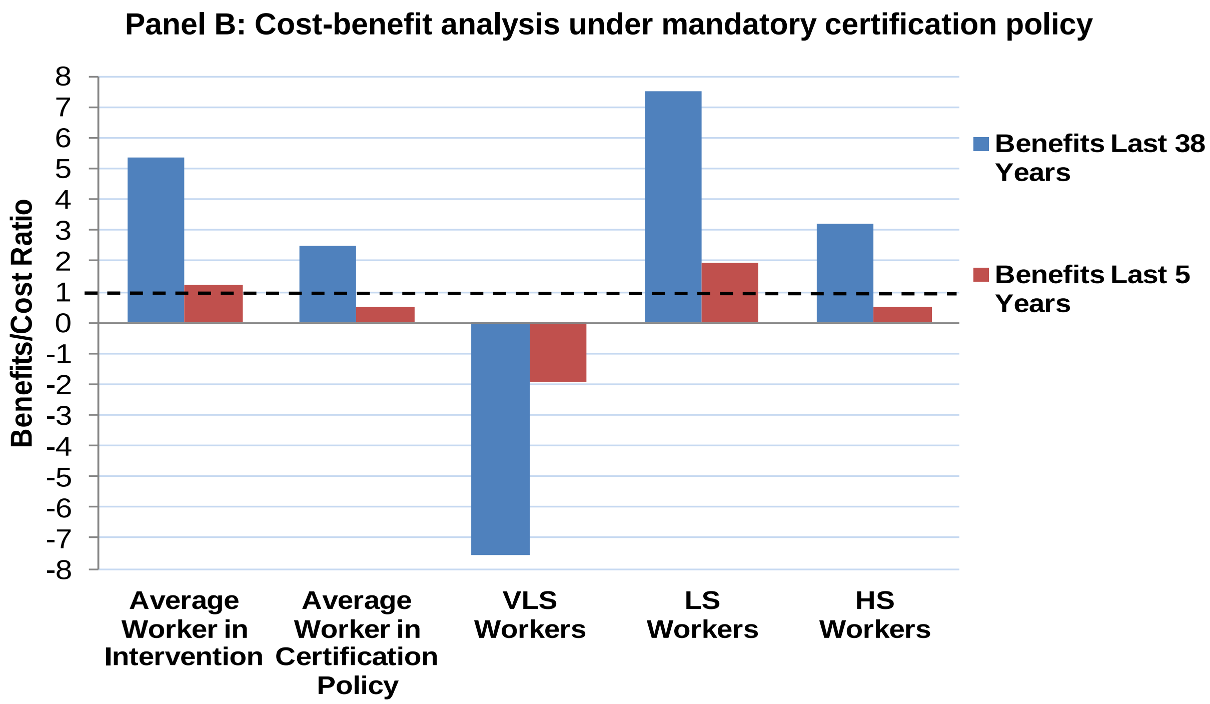
Figure 6: Controlling for sample selection



Notes: The Figure reports the estimated impacts on the probability of a job offer for the sample of matches with High Ability owners. These are taken from Table 6 (left panel), Table A9B (middle panel) and Table A9A (right panel). The red bars correspond to the estimates of the coefficients on the High Score dummy; the blue bars to the coefficients on the interaction between the Low Score dummy and Treatment; the green bars to the coefficients on the interaction between the High Score dummy and Treatment. The black bars correspond to 95% CIs around the point estimates.

Figure 7: Cost-benefit analysis

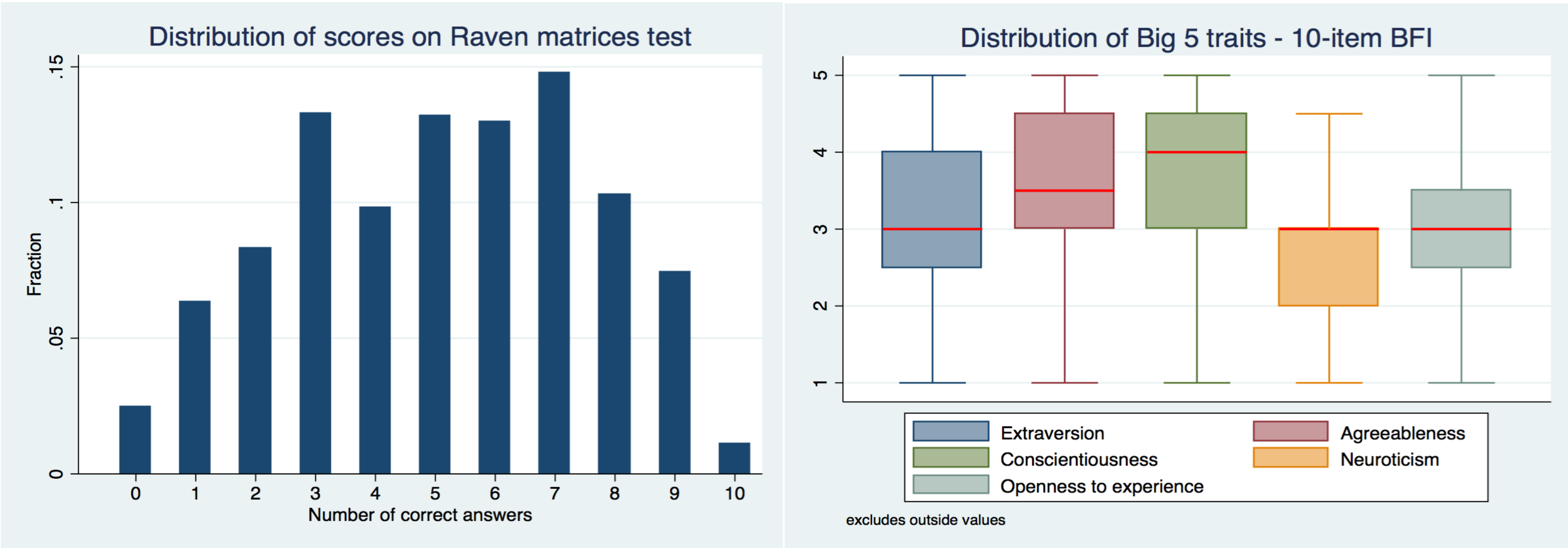




Notes: The Figure reports the Benefits/Cost ratios of the intervention for different groups of workers. The Benefits/Cost ratio is defined as the NPV of the intervention (computed using a 5% discount rate) over the total costs of the intervention at year 0. The estimates are taken from the calculation in Table 8. Panel A reports the estimates for the sample of workers that participated to the intervention. Panel B considers how the cost-benefit analysis would change if those workers that initially selected out of the intervention were forced to participate. The "Average Worker in the Intervention" corresponds to the average program participant. The "Average Worker in Certification Policy" corresponds to the average worker eligible for the intervention. "VLS" indicates "Very Low Score" workers, that is, those workers that decided not to participate to the intervention. "LS" indicates "Low Score" workers. "HS" indicates "High Score" workers. The variable High Score takes value 1 if the worker ranked at the median or above on the first principal component of the five soft skills and 0 otherwise. The Low Score variable is defined as 1 – High Score.

Figure A1: Skills measurement on workers

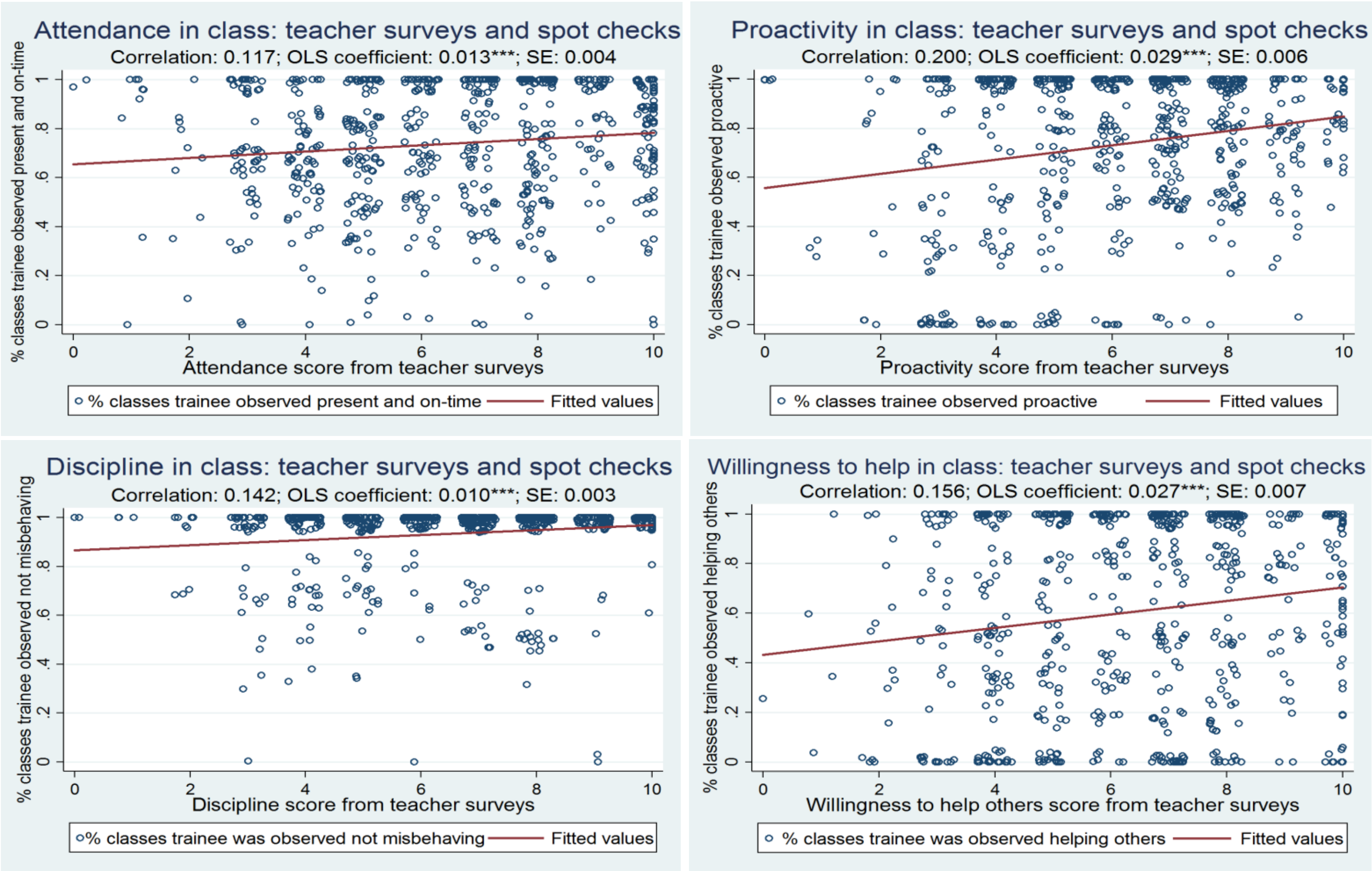
Panel A: Distribution of cognitive skills and of Big-5 traits for trainees in initial census



Notes: The sample for both panels includes all trainees surveyed in the initial census. The left panel reports the frequency histogram of the scores on the 10-item Raven matrices test administered. The right panel reports Box and Whisker plots of the distribution of the Big-5 traits, measured using a standard 10-item questionnaire, with explanations and examples adapted to the Ugandan context. The red line is the median.

Figure A1: Skills measurement on workers (continued)

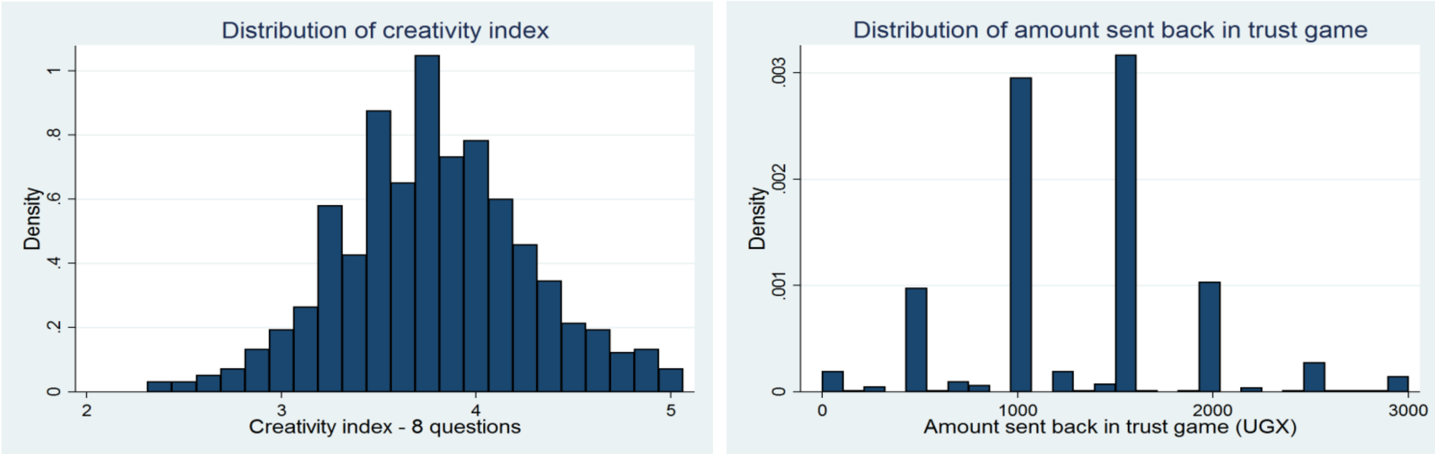
Panel B: Correlation between teacher surveys and spot-checks



Notes: The sample in all panels includes all workers interested in the matching intervention and so included in the baseline survey. The graphs are two way scatter plots with jittering. The red lines are fitted values from a univariate OLS regression of the variable on the y-axis on the variable on the x-axis. The sample sizes are the following: Attendance from spot-checks: 626 trainees; Attendance from teacher surveys: 787 trainees; Pro-activity from spot checks: 619 trainees; Pro-activity from teacher surveys: 787 trainees; Discipline from spot checks: 620 trainees; Discipline from teacher surveys: 787 trainees; Willingness to help others in class from spot-checks: 619 trainees; Willingness to help others in class from teacher survey: 787 trainees; Spot-checks were conducted weekly and the median trainee is observed in 6 spot-checks.

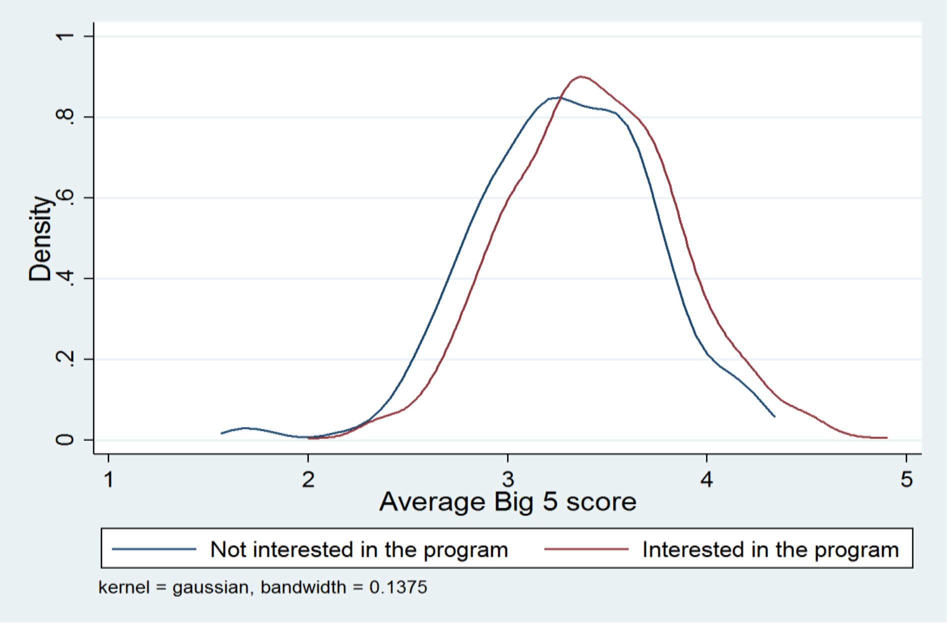
Figure A1: Skills measurement on workers (continued)

Panel C: Creativity and Trustworthiness



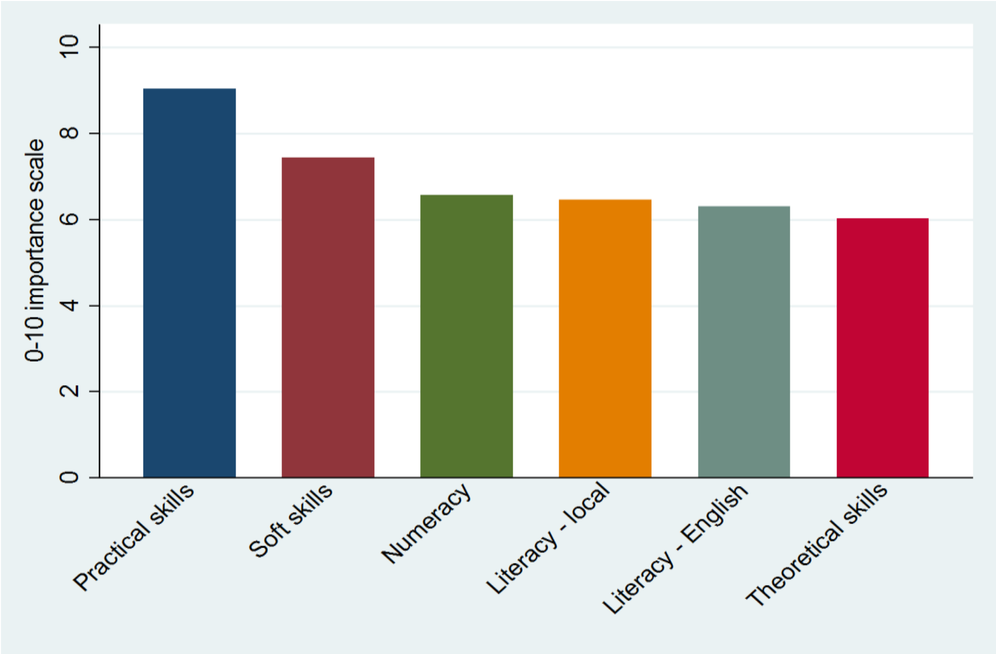
Notes: The sample in all panels includes all 787 workers interested in the matching intervention and so included in the baseline survey. The left panel reports the distribution of the creativity index. This is constructed as the unweighted average of answers to an 8-item battery of questions designed to measure creativity. Creativity is increasing in going from 1 to 5. The right panel reports the distribution of the amount (in Ugandan shillings) that trainees sent back in the trust game they played. A higher amount is interpreted as an indication of higher trustworthiness.

Figure A2: Distribution of worker soft skills, by participation in the program



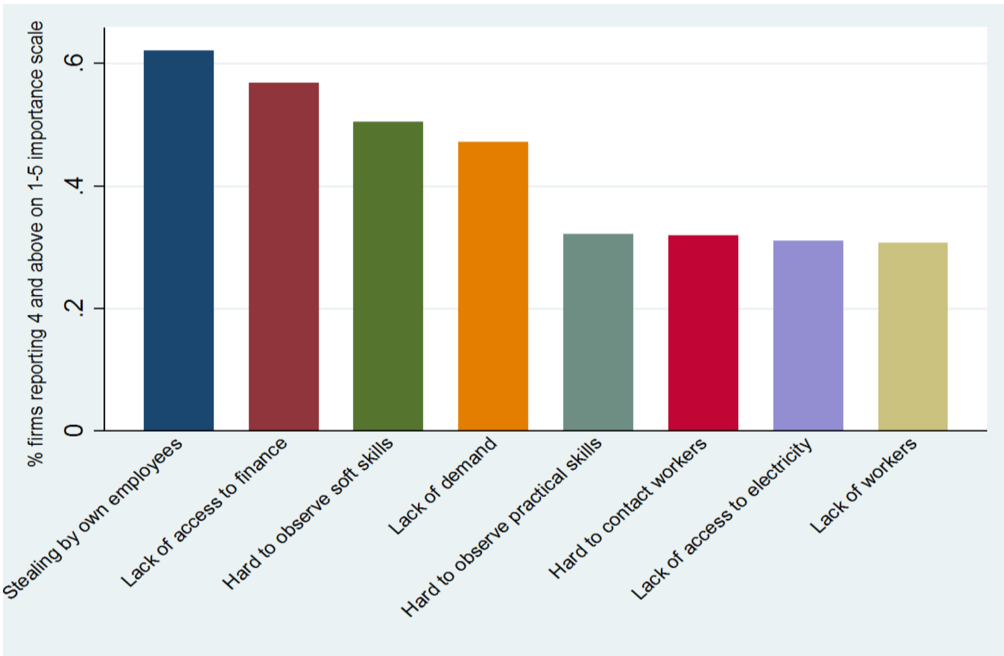
Notes: The Figure reports kernel density estimates of the distribution of soft skills among the sample of eligible workers, split by whether the worker was included in the program or not. Soft skills are measured as the average score on the Big-5 traits from a 10-item questionnaire.

Figure A3: Perceived returns to various skills



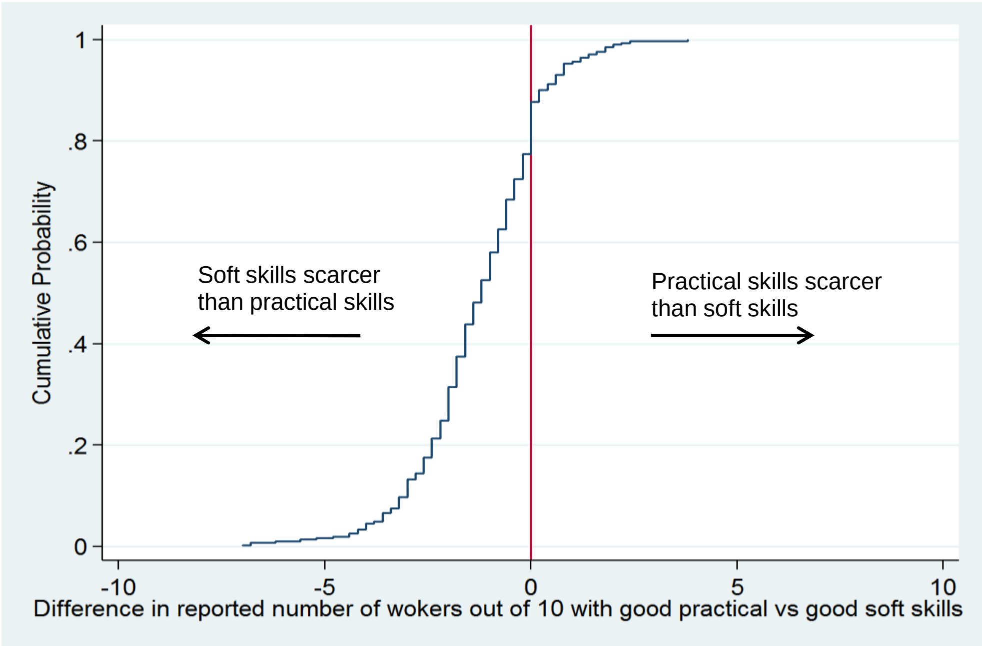
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 0-10 scale, where 0 = "Not important at all", and 10 = "Extremely important", the importance of various skills for their operations. The Figure reports the average importance given 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 A4: Perceived importance of various constraints



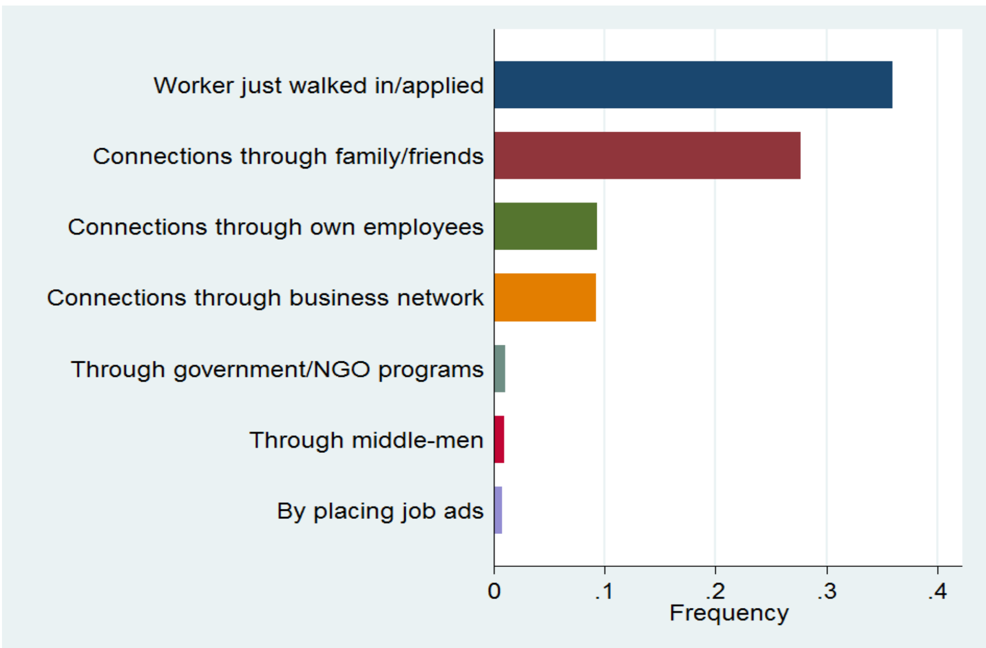
Notes: Data is from the baseline survey of the 422 firms interested in being matched. Firm owners were asked to rate on a 1-5 scale, where 1 = "Not important at all", and 5 = "Extremely important", the importance of these potential constraints. The Figure reports the percentage of firm owners that answered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

Figure A5: Relative perceived scarcity of practical vs soft skills in the worker population, as reported by firm owners



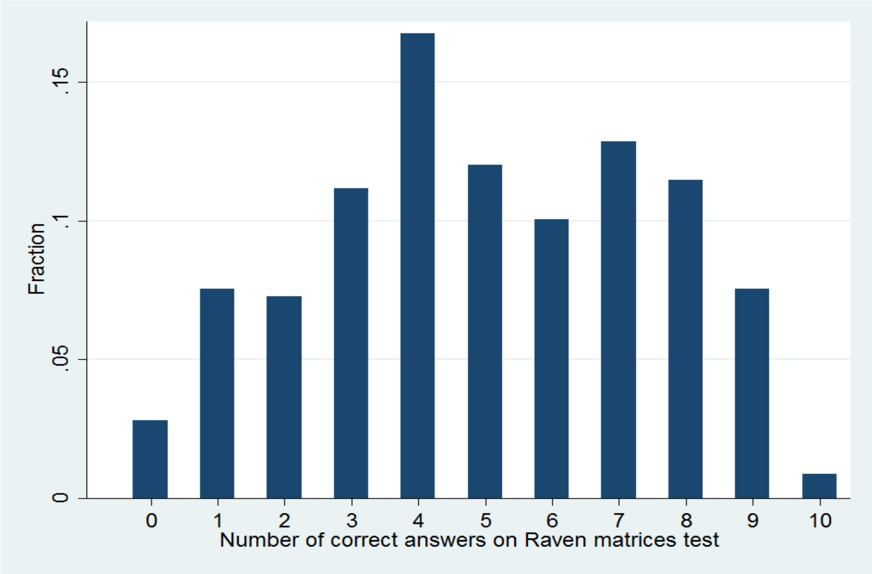
Notes: The Figure reports the CDF of within-firm owner differences in the number of potential workers out of 10 reported as having a good level of practical skills vs. a good level of soft skills. To estimate the reported number of workers with a good level of soft skills we compute the average of the 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 A6: Recruitment channel of current employees



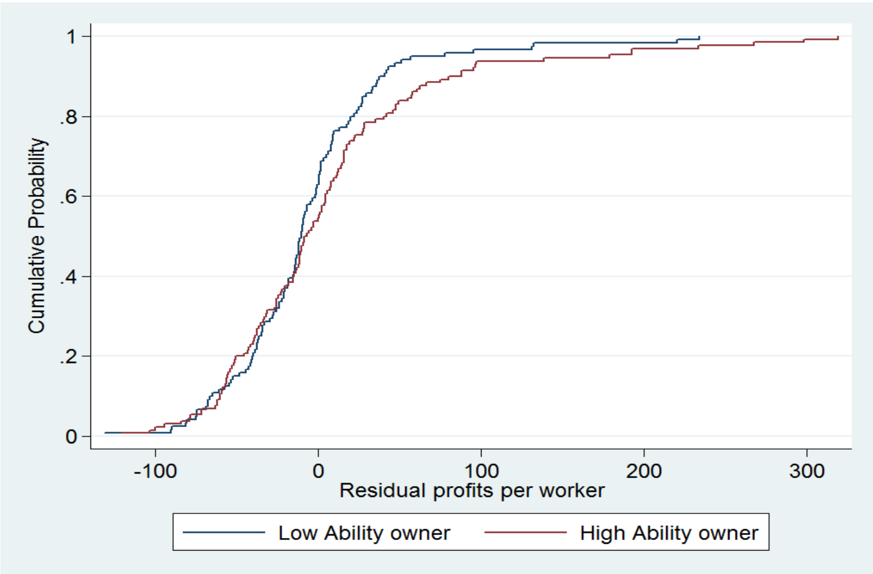
Notes: The Figure reports the frequency of recruitment channels for the workers employed at baseline in the sample of firms included in the intervention.

Figure A7: Distribution of cognitive ability among firm owners



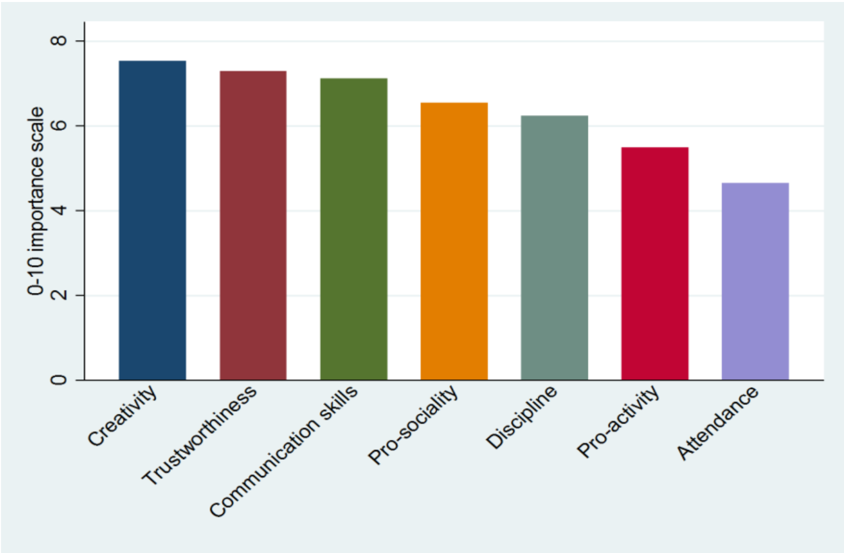
Notes: Data is from the firm baseline survey. The Figure reports the frequency histogram of the scores on the 10-item Raven matrices test administered to firm owners at baseline

Figure A8: Distribution of residual profits per worker, by firm owner type



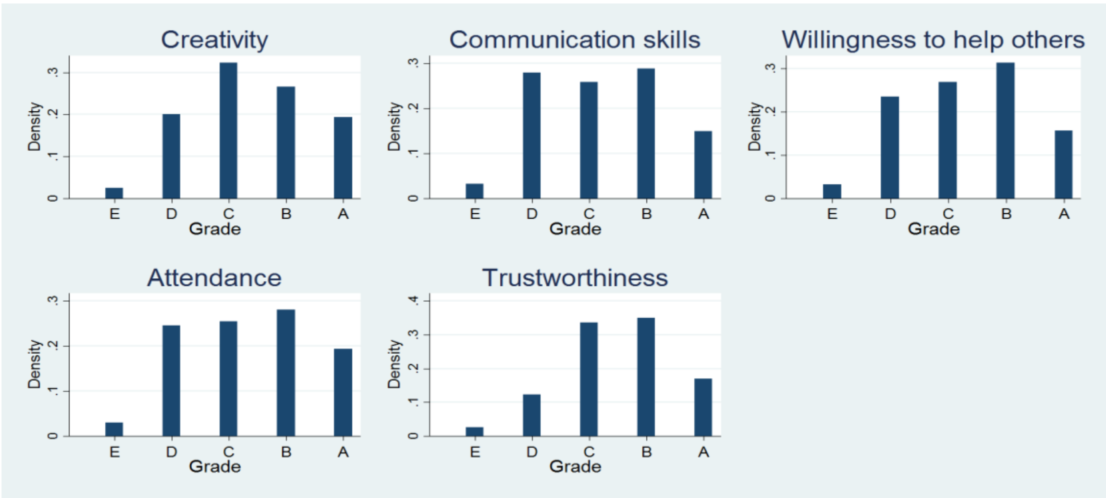
Notes: The sample includes all firms interviewed at baseline, for which the score of the firm owner on the cognitive test is available. The Figure plots the residuals from an OLS regression of profits per worker on the following covariates: owner's gender, owner's years of education; a dummy=1 if the owner attended a VTI in the past; number of employees; age of the business; age of the business squared; dummies for region of operation (three dummies); dummies for sector of operation (five dummies). Standard errors are robust in this regression. The Figure plots the CDF of residual profits per worker, by whether the owner is classified as a Low ability owner, or a High ability owner. Firm owners are split into High and Low Ability owners by using their score on a cognitive test administered at baseline. Firm owners that scored on or above the median are defined as High Ability owners, while owners that scored below the median are assigned to the Low Ability group.

Figure A9: 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 A10: Distribution of soft skills among trainees



Notes: Data is from the 787 trainees offered the matching intervention.

Figure A11: Skills signalling

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 A11: Skills signalling (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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