The Search for Good Jobs: Evidence from a Six-year Field Experiment in Uganda*

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Abstract

We present evidence from a six-year field experiment with young job seekers in urban labor markets in Uganda. We study how standard labor market interventions impact their search behavior, long run labor market outcomes, and how these long run outcomes are mediated through search behavior. The interventions we consider are the offer of vocational training, vocational training combined with job assistance, and job assistance only. Training is offered in sectors with high wage firms. Job assistance comprises a light touch offer to match workers for job interviews with such high wage firms. At baseline, youth are unskilled yet overly optimistic about their job prospects. Relative to controls, those offered vocational training become even more optimistic, search more intensively and direct their search towards higher quality firms. However, for youth additionally offered job assistance, expectations and search effort are revised downwards as call back rates from firms the workers are matched to, are far lower than their prior expectation. These differential search strategies impact long run outcomes: vocational trainees without job assistance have higher employment rates, longer employment spells, and end up in higher quality jobs and firms than workers additionally offered job assistance. Our analysis highlights how the potential for labor market entrants to be exuberant or discouraged both matter for long run outcomes through job search behavior. We discuss implications for the design and targeting of labor market interventions meant to help young people find good jobs. *JEL Classification: J64, O12.*

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1 Introduction

Labor markets play a critical role in driving economic development. We study the process by which young workers search for jobs in urban labor markets in a low-income setting: Uganda. In common with many developing countries, Uganda faces a challenge of having large cohorts of young people transitioning into the labor market each year, in search of meaningful work. Hence understanding how labor market interventions can aid young people find good jobs is at the top of the policy agenda.

We present evidence from a field experiment tracking young labor market entrants over six years. The experiment is designed to study how individual search strategies and long run outcomes are impacted through the following standard labor market interventions: (i) the offer of vocational training; (ii) the offer of vocational training combined with a light touch job assistance intervention when training is completed; (iii) light touch job assistance only. The study timeline allows us to map how these interventions cause shifts in short run search strategies, and how this then translates into long run labor market outcomes.

Labor market entrants were recruited into our study from across Uganda, through the offer of potentially receiving six months of sector-specific vocational training in one of eight sectors: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering. In line with many labor market programs, the eligibility criteria targeted disadvantaged youth [Attanasio *et al.* 2011, Card *et al.* 2011]. We received 1400 valid applications from young people with limited labor market experience and scope to learn about their job prospects through the process of job search. At baseline, these youth have poor labor market histories, rely on informal contacts to find work, and mostly hold casual jobs. They lack skills and likely face credit constraints to invest in the kind of vocational training we offered.

The sectors we offered training in are associated with 'good jobs' that offer regular employment in high wage, high productivity firms. They constitute an important source of wage employment for youth in Uganda: at baseline, 25% of employed workers aged 18-25 work in them. We view the sectors we offered training in as providing a chance to workers to progress up the job ladder beyond the kinds of itinerant casual work they are reliant on at baseline.

The firms involved in the job assistance component of the experiment comprise 1281 firms operating in 15 urban labor markets, including Kampala. We selected firms: (i) operating in one of the eight manufacturing and service sectors in which we offered sector-specific vocational training; (ii) having between one and 15 employees (plus a firm owner).

Our field experiment follows an over subscription design and is structured as follows. Individuals are first randomly assigned to receive an offer of vocational training or not. Over two thirds of workers take-up of the offer of vocational training, and 90% then complete training courses. In earlier work we show such intense and sector-specific training has large measurable impacts on worker skills, with the experimentally identified private returns in these urban labor markets to be 20-30% [Alfonsi *et al.* 2020].

At a second stage of randomization, we offer light-touch job assistance. Workers were asked whether they wanted their details to be passed onto the kinds of firms in our experiment: nearly all agreed. Firms were then presented shortlists of workers that were either: (i) vocationally trained, or; (ii) unskilled, but had demonstrated labor market attachment in the sense that they had been willing to undertake six months of intense training. Workers were randomly matched to firms and there were a maximum of two workers presented to firms on each list. We presented stylized CVs of workers to firms. In case (i), firms knew what sector the worker had been trained in, but not that training had been paid for by BRAC. The firm could hire neither, one or both (and of course remained free to hire workers from outside the evaluation sample).

Although workers were randomly assigned to each treatment arm at the point of application, they were only informed about any potential job assistance once vocational trainees had completed their courses. This ensures there is no differential compliance with vocational training based on the future offer of job assistance. We thus also document that sector specific skills accumulation is not statistically different between those offered vocational training and those offered vocational training and job assistance. Among those not assigned to vocational training, the job assistance intervention takes place when vocational trainees are graduating from their courses.

Our design thus assigns workers to one of four groups as Figure 1 summarizes: (i) the offer of vocational training (T1); (ii) the offer of vocational training and job assistance (T2); (iii) job assistance (T3); (iv) controls (C).

We show that at baseline, although workers have relatively accurate beliefs over the earnings distribution if they could progress into jobs in good sectors, they are overly optimistic about the job offer arrival rate from employers in these good sectors – such optimism has been documented among job seekers in the US [Spinnewijn 2015, Mueller *et al.* 2021, Potter 2021], Ethiopia [Abebe *et al.* 2021a] and South Africa [Banerjee and Sequeira 2021]. These beliefs are then central to understand how workers might react to the offer of job assistance.

From the worker's perspective, the key outcome generated from the job assistance intervention is whether the firm they are matched to decides to call back the worker, inviting them to interview. To understand how workers might react to call backs (or a lack thereof), we track the evolution of worker beliefs from baseline to the eve of job assistance to workers being announced. We see a sharp bifurcation in beliefs over this period between those randomized in and out of vocational training. Trainees become ever more exuberant over their job prospects: at the point of graduating (but before any announcement of job assistance is made), the median trained worker believes there is a 30% chance in the next month of receiving a job offer from the kinds of good employer we consider – this is far higher than employment rates actually experienced by those only offered vocational training over the same time period, as well as a nationally representative survey in Uganda fielded close to our baseline (UNHS 2012/3) suggests could be plausible flow rates into regular employment for skilled workers.

Among those randomized out of training, they continue to search for work over the next six months, but with little improvement in their job prospects. Employment rates remain constant and they remain reliant on casual work. Over these six months of search, they gradually revise down their beliefs over the job offer arrival rate from firms operating in the kinds of high-wage sectors we consider. On the eve of job assistance being announced to unskilled youth, the median youth believes there is a 20% chance in the next month of receiving a job offer from an employer in our study sectors.

The job assistance intervention is thus implemented to these groups of increasingly exuberant youth that were offered vocational training, and increasingly realistic youth that were randomized out of vocational training. Among vocational trainees the actual call back rate is far lower than their prior belief (16% vs. 30%). Among those randomized out of the offer of vocational training, call back rates are in line with prior beliefs (18% vs. 20%).

We show call backs are actually determined by a lack of vacancies and other firm characteristics. Worker characteristics do *not* determine call backs – this is unsurprising because in our design firms are presented with two workers that are, by construction, similar on observables. There is little basis on which to prefer one over another.

Our null hypothesis is that workers are perfectly informed, and fully understand call backs are not determined by worker characteristics. They then rationally infer there to be zero information from any single call back (or lack thereof) about their own job prospects. Under this null, the search strategies of workers – irrespective of whether they have earlier been vocationally trained or not – should be unaffected by the offer of job assistance.

Our alternative hypothesis is that some share of workers are imperfectly informed. For trained workers the lower than expected call back rate causes them to revise down their beliefs about their own job prospects. Such misattribution can occur because of the combination of two factors: (i) labor market entrants are not well informed at baseline, and trainees become even more optimistic relative to their realistic prospects as they complete their training; (ii) there are no market substitutes for the job assistance intervention, so the offer to match to good firms can be a highly salient and unique opportunity for them to find meaningful work. Under this alternative, job assistance generates bad news for the average trained worker. Trained workers without job assistance are insulated from this news, and so begin their job search with the increasingly exuberant beliefs documented earlier.

For workers randomized out of the offer of training, the low rate of call backs – in line with their priors – might provide credible confirmation of their poor labor market prospects, prompting them to change search job behavior relative to controls.

Our first set of results document how these labor market interventions cause exogenous changes to the search strategies used by workers in the year after training is completed and/or job assistance implemented.

First, comparing workers offered vocational training to controls (T1 vs C), we find the former group further revise upwards their beliefs over the job offer arrival rate and the distribution of expected earnings. Comparing these to actual labor market outcomes for youth in nationally representative household survey data from the UNHS, we see that they become increasingly optimistic on the job offer arrival rate, while in terms of expected earnings their beliefs move in line with the skills premium offered for trained young workers in urban labor markets. These skilled workers also search more intensively along multiple margins (time devoted to job search and channels used), and they engage in directed search towards more productive firms.

Second, workers offered the combination of vocational training and light touch job assistance also change job search strategies. However, relative to those only offered vocational training, they revise down their beliefs over the job offer arrival rate and distribution of expected earnings (especially the left tail of expected earnings that in some models is proportional to their reservation wage), search less intensively, and search over lower quality firms. These differences in behavior between those offered vocational training with and without job assistance runs counter to the null that workers are fully informed of what drives call backs. Their behavior is consistent with them reacting to the lower than expected call back rate by revising down their beliefs over their own job prospects, and these differences in search strategy reflecting multiple margins along which young workers are discouraged relative to those only offered vocational training.

Finally, workers only offered job assistance – relative to controls – react to the confirmation of their poor job prospects by borrowing small amounts, with the stated purpose of using such finance to set up in self-employment.

Our second batch of results examine whether the labor market interventions – through experimentally induced changes in search behaviors – translate into long run differences in outcomes for workers, up to five years after training is completed and/or job assistance provided.

We find that relative to controls, those offered vocational training are more likely to be employed, to transition from casual work into regular work, to be employed in good sectors, and end up in better jobs and in higher quality firms. In contrast, workers offered both vocational training and job assistance do significantly worse that those only offered training on a range of labor market dimensions up to six years later: on the extensive margin they are less likely to work in regular jobs, on the intensive margin, they work significantly fewer months in regular jobs, and in terms of sectoral allocation, they work less time in one of the eight sectors in which we offered training. They end up at worse quality firms, have lower earnings, experience longer unemployment spells, and shorter employment spells.

Taken together, the results highlight detrimental long run impacts of job assistance on those also offered vocational training: while those only offered vocational training transition up the job ladder from casual to regular work, this transition into good jobs is significantly slower for those also provided job assistance.

To quantify these long run differences, we construct a holistic index of labor market success combining information on the extensive and intensive margins of employment in good jobs, earnings, employment spells, and characteristics of jobs and firms workers end up being employed at. This broad measure of long run labor market success significantly increases by $.115\sigma$ for those offered vocational training relative to controls. For those additionally offered job assistance, the index increases by less than half the amount ($.051\sigma$), and the two estimates are significantly different ($p = .001$). In short, light touch job assistance to those offered vocational training undoes half of what is achieved through vocational training alone.

Finally, workers only offered job assistance (that might confirm to them their poor job market prospects), are significantly more likely to enter self-employment, in line with their stated intention three years earlier. On the holistic index of labor market success we find, in line with earlier meta-analyses [Card *et al.* 2017, McKenzie 2017], the impact of light touch job assistance is muted ($.020\sigma$) and not significantly different to controls.

We use mediation analysis to decompose these treatment effects into those ascribable to skills and the dimensions of search strategy considered. Among workers offered vocational training, sector-specific skills are the most important mediator: 20% of the long run impact on labor market outcomes is directly mediated by skills. Among search behaviors, the most prominent mediators is the proxy for the reservation wage – the minimum expected earnings from employment in a study sector (10%). The second most important search behavior is the belief over the job offer arrival rate, explaining 8% of the long run effect.

Among workers additionally offered job assistance, sector-specific skills play the most important role in mediating long run outcomes. These skills – that do not differ between those offered vocational training and those offered vocational training and job assistance – explain the same increase in our holistic measure of labor market success for both groups of worker. The role of search behaviors in mediating long run labor market success is far more prominent for those only offered vocational training than those additionally offered job assistance – the reason being that workers additionally offered job assistance are discouraged in a variety of dimensions of their search behavior, and so end up with search strategies closer to controls overall.

We discuss the external validity of our findings by considering: (i) the scalability of the interventions and alternative kinds of information that could be provided; (ii) firms that workers were matched to; (iii) targeted workers, where we establish the homogeneity of impact across workers with differing abilities and psychological traits.

Job search is a classic question in labor economics, with fifty years of work since the seminal papers by McCall [1970] and Mortensen [1970]. We use this body of work to motivate our experimental design that replicates standard labor market interventions used around the world. Our survey measurement tools are also designed to reflect the fundamental insights of this long-standing literature, that job search is an inherently multi-dimensional process. Hence we measure multiple margins along which workers search strategies can be impacted by interventions. We make three novel contributions.

First, we experimentally identify the role that prominent forms of active labor market policy –

skills provision and job assistance – play in determining search strategies used by young workers, and how these map into long run labor market outcomes. We provide one of the few economic analysis on individual labor market dynamics that combines experimental variation in policies young workers are exposed to, data on multiple dimensions of search strategies – beliefs and expected earnings, search intensity, and the nature of directed search – with long run labor market outcomes including information on individual job offers, employment, earnings, bargaining, spells, and the characteristics of jobs and firms matched to.[1]

Second, we build on a nascent experimental literature evaluating similar labor market programs of training and job assistance in low-income countries [Beam 2016, Groh *et al.* 2016, Abebe *et al.* 2021a, 2021b, Acevedo *et al.* 2020, Carranza *et al.* 2020, Banerjee and Sequeira 2021]. We bridge between this work and a recent literature on behavioral job search that shows job-seekers tend to be over-optimistic about their job finding rates and this delays exit from unemployment [Spinnewjin 2015, Arni 2015, Conlon *et al.* 2018, Mueller *et al.* 2021]. We link these literatures by providing the insight that because labor market interventions impact beliefs and search strategies, light-touch job assistance can backfire, reducing short-run job search effort and long-run labor market success if workers misinterpret the lack of call backs from potentially good employers. Exploiting our cross-cutting experimental design, we document how these unintended consequences are most pronounced for those also offered vocational training, who upon completion of their training courses hold exuberant beliefs over their own job prospects. In contrast, unskilled workers do not get discouraged by such light touch job assistance, and marginally improve some labor market outcomes as a result.[2]

Third, understanding the heterogeneous effects of job assistance across job seekers has implications for the design and targeting of such interventions, many of which have had weak impacts in high- and low-income settings [Card *et al.* 2017, McKenzie 2017]. We show the provision of vocational training leads to individuals holding exuberant beliefs and these can drive forward job search effort and result in better long-term labor market outcomes. Trying to debias more skilled individuals through even light touch job assistance – or potentially other informational interventions and nudges – can backfire. However, the opposite might hold for unskilled workers. Our results suggest low skill workers are able to access credit markets to finance self-employment. Providing them credible confirmation of their poor job prospects might then be more effective

[1] Two other papers providing granular analysis of job search are Arni [2015] and Fluchtmann *et al.* [2020]. Arni [2015] uses a field experiment on job assistance (a coaching intervention), provided to 327 older job seekers (aged 45 to 62) in Switzerland. The intervention increased job finding rates by 9pp, driven by a reduction in reservation wages and an increase in search efficiency. Fluchtmann *et al.* [2020] provide descriptive evidence from Danish job seekers using administrative data: they find as unemployment duration rises there are only marginal changes in the types of jobs applied for, but greater adjustments along job search channels used.

[2] Banerjee and Sequeira [2021] show that job search subsidies worsen labor market outcomes of job seekers in South Africa as workers fail to find better jobs during the subsidy period and settle for lower-paying jobs closer to home. Our study differs in that: (i) their study considers one year impacts only; (ii) we focus on a lighter touch job assistance intervention, and (iii) we document the heterogeneous effects of job assistance across workers, discussing the sources of such heterogeneity.

that providing them access to microcredit for example.

This paper is part of a larger project studying workers and firms in urban labor markets in Uganda. We have used data from this project in earlier work [Alfonsi *et al.* 2020]. There our focus was on comparing labor market returns to vocational training versus firm-sponsored apprenticeships, contrasting the effects of those two training routes on human capital accumulation and labor market outcomes. As we make clear throughout, there is some overlap with our results on skills (Table 4) and labor market outcomes (Tables 9 and 10). However, the previously considered apprenticeship treatment plays no role in the current study. Instead, this paper fully exploits treatment arms related to light-touch job search assistance that were not studied in our earlier work. Most importantly, job search strategies were not considered in our earlier work, but these lie at the heart of the current analysis.

Section 2 describes our context, experimental design and data. Section 3 describes search behavior of controls and the evolution of worker beliefs from baseline until the offer of job assistance is to be announced. Section 4 presents treatment effects on job search strategies. Section 5 shows how the interventions map into persistent differences in labor market outcomes across workers, using mediation analysis to show the relative importance of skills and search strategies. Sections 6 and 7 discuss the external validity and policy implications of our findings. Section 8 concludes. Additional design details and research ethics are discussed in the Appendix.

2 Context, Design and Data

2.1 Context

Our study context is urban Uganda. As in most urban labor markets in low-income countries, multiple frictions are relevant including: (i) skills mismatch, where youth enter labor markets with skills not well suited to the needs of firms [Frederiksson *et al.* 2018]; (ii) credit, so workers cannot finance human capital investments to correct for such mismatch even if these generate private returns; (iii) information, where labor market entrants lack knowledge of how to search, and firms lack information on worker histories or certifiable skills [Alfonsi *et al.* 2020, Abebe *et al.* 2021b].

To get a descriptive sense of such market imperfections in our context, we use the Uganda National Household Survey (UNHS) from 2012/3 (so from around the time of our baseline), to derive the share of young people engaged in casual jobs, and in more regular jobs, by age. Casual work is hard to define precisely. Throughout, we classify casual work as jobs in which workers are typically hired on a daily basis, as well as agricultural labor. This is in line with a standard definition of casual jobs being those in which there is no obligation on either the worker or the firm side to supply or demand labour on a regular basis.[3]

[3]Casual work thus includes the following kinds of jobs: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing, slashing compounds, and any type of agricultural labor such as farming,

Panel A of Figure A1 then shows that for all ages between 18 to 25, young workers remain reliant on casual work, with there only being a slow increase in them accessing regular work as they age.[4] To show the inability of workers to invest in their human capital, Panel B shows how skills vary by age, again using the UNHS data. Fewer than 6% of young workers make any investment in training or higher education post labor market entry. Finally, Panel C shows how skills raise the likelihood of being in regular work at each age – yet, the majority of skilled youth still do not find regular work. In other words, the labor market fails to clear even for high-skilled youth.

Our interventions relax some of these fundamental constraints: our treatment offering workers vocational training relaxes credit constraints workers face in acquiring valuable skills, and our job assistance treatment reduces information frictions that might otherwise prevent some worker-firm matches forming.

Vocational Training Institutes Our study is a collaboration with the NGO BRAC, who implemented all treatments, and five reputable vocational training institutes (VTIs). Each VTI could offer standard six-month training courses in eight sectors: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering.[5]

Workers Individuals were recruited into our experiment throughout Uganda, using an advertised offer for eligible applicants to potentially receive six months of sector-specific vocational training at one of our partner VTIs. The first row of Table A1 shows applicant characteristics: 57% are men, they are aged 20 on average, and almost none have previously undertaken vocational training.[6]

Table 1 shows labor market histories at baseline among our sample. Focusing on the first row for controls, employment rates at baseline are 40% for these youth, with insecure casual work being the most prevalent labor activity. Unconditionally, average monthly earnings from regular work are $5 (so including zeroes), corresponding to around 10% of the Ugandan per capita income at the time. Conditional on work, earnings rise to $13 per month. Hence these individuals remain unlikely to be able to self-finance the kind of investment into vocational training we offer (that costs over $400). To see the representativeness of our sample, Table A1 compares them to those aged 18-25 in the UNHS data from 2012/3. The intervention appears well targeted towards

animal rearing, fishing and agricultural day labor.

[4]This dynamic is in contrast with the traditional view of labor markets in higher-income settings, where the first years after entry are typically a productive period for young workers, characterized by rapid wage growth as they frequently switch towards better paying jobs [Topel and Ward 1992].

[5]The VTIs we worked with: (i) were founded decades earlier; (ii) were mostly for-profit; (iii) trained hundreds of workers with an average student-teacher ratio of 10; (iv) in four VTIs, our worker sample shared classes with regular trainees.

[6]The program was advertized using standard channels, and there was no requirement to participate in other BRAC programs. The eligibility criteria were: (i) being aged 18-25; (ii) having completed at least (most) a P7 (S4) level of education (corresponding to 7-11 years); (iii) not being in full-time schooling; (iv) a poverty score, based on family size, assets owned, type of building lived in, village location, fuel used at home, number of household members attending school, monthly wage, and education level of the household head. Applicants were ranked 1-5 on each dimension and a total score computed. A geographic-specific threshold score was used to select eligibles.

disadvantaged youth: our sample is similar on age, gender and previous experience of vocational training, but worse off at baseline in terms of wage employment and earnings. This remains so when we compare to youth in the UNHS who report being labor market active.

Firms To draw a sample of potential employers for the job assistance intervention, we first conducted a firm census in 15 urban labor markets throughout Uganda, including Kampala. We selected firms: (i) operating in one of the eight manufacturing and service sectors in which we offered sector-specific vocational training; (ii) having between one and 15 employees (plus a firm owner). Our sample comprises 1281 small and medium sized enterprises, employing 3735 workers in aggregate at baseline.[7] Firms are not selected on the basis of them having a vacancy, but at baseline, 92% of them reported being willing to expand in the near future, with 52% stating they would be willing to do so by hiring workers. However firms report being size constrained because they are unable to find: (i) skilled workers (67%); (ii) trustworthy workers (57%); (iii) unskilled workers (28%). Our job assistance intervention relaxes constraints on firm's ability to match with workers with sector-specific skills or a high degree of labor market attachment.

Job Search and Recruitment in Urban Labor Markets Table 2 provides descriptive evidence on how youth in our control group normally find jobs, and recruitment processes used once they match with potential employers. Given our focus on whether interventions allow workers to move up the job ladder, it is useful to split these descriptives related to search and recruitment into those for casual and regular jobs.

Panel A shows job characteristics. The first row reiterates that at baseline workers are reliant on casual work, especially including forms of subsistence self-employment. Employment spells are short: individuals work three to four months each year. Regular jobs offer longer hours per day, similar days per week of work, and earnings that are almost three times higher. Panel B shows methods of job search: the majority of youth rely on informal contacts through friends/family, especially for regular jobs. They are more likely to use direct walk-ins to firms when searching for regular jobs. Fewer than 2% of workers report finding work through posted job adverts. The informal nature of labor markets is reiterated in Panel C on firm recruitment strategies. As this information is obtained via our firm-side surveys, we can only provide this for regular jobs. This reinforces the idea the worker-firm matching process is informal, relying on personal contacts or walk-ins rather than posted-ads. We later examine how our interventions impact such margins of search effort and search channels used by youth. Panel D describes firm's screening technologies. Interviews, references and skills tests are more common for regular jobs, although even there, the minority of workers report being screened using those methods.

[7] On average these firms have been in operation for almost 7 years, have monthly profits of $217, and have a capital stock valued at $1209. Among firm owners, 53% are women, they are on average age 35 and have 11 years of education (far higher than our sample of workers).

2.2 Design

Figure 1 shows the oversubscription design of our field experiment. Eligible individuals were first randomly assigned to either receive vocational training or not. Within those assigned to training, a further random assignment into two groups took place. The first group was assigned to six months of training at one of our partner VTIs, and then upon graduation, transitioned into the labor market to search for jobs unassisted. This is the business-as-usual training model, where VTIs are paid to train workers, but not to find them jobs. The second group of trained workers were upon graduation from the VTI, offered light touch job assistance by BRAC.

As shown in the lower branch of Figure 1, workers randomized out of the offer of training were also randomly assigned into two groups: (i) at the same time as those assigned to vocational training were graduating from VTIs, these unskilled workers were either: (i) offered the same kind of light touch job assistance, or; (ii) held as a control.

We assigned workers to each treatment arm using a stratified randomization where strata are region of residence, gender and education.

Although workers were randomly assigned to each treatment arm at the point of their initial application, they were only informed about any potential job assistance once vocational trainees had completed their courses. This helps avoid lock-in or threat-effects on search [Black *et al.* 2003], and also ensures job assistance and call backs for those randomized into and out of the offer of vocational training take place simultaneously. This leaves open the possibility that those not assigned to vocational training might have found employment before the job assistance offer. A six month tracker survey fielded just prior to the offer of job assistance helps shed light on this. While this confirms that 16% of controls are in some work activity at the time, most remain reliant on casual jobs and over 90% report that they remain interested in any job placement opportunity offered by BRAC.

The pairwise intent to treat comparisons we focus on are: (i) T1 vs C: the impact of the offer of vocational training; (ii) T2 vs T1: the differential impact of the offer of job assistance on those previously offered vocational training; (iii) T3 vs C: the impact of the offer of job assistance on those randomized out of vocational training.

Vocational Training The vocational training intervention provides workers six months of sector-specific training in one of eight sectors. Our intervention partner BRAC covered training costs, at $470 per trainee. Courses were held from Monday through to Friday, for six hours per day; 30% of course content was dedicated to theory, 70% to practical work covering sector-specific skills and managerial/business skills. VTIs signed contracts with BRAC to deliver these standard training courses to workers. They were monitored by regular and unannounced visits by BRAC staff to ensure workers were present and being trained. For each worker, VTIs were paid half the training fee at the start of training, and half at the end, conditional on them having trained the worker. This staggered timing of payments ensured workers nearly always completed the full course of training conditional on enrolment.

Job Assistance Our job assistance intervention is light-touch. Workers were first asked whether they wanted their details to be passed onto the kinds of firms in our firm-side survey: nearly all agreed (among both those earlier offered vocational training and those randomized out of that offer). Firms were then presented shortlists of workers that were either: (i) vocationally trained, or; (ii) unskilled, but had demonstrated labor market attachment in the sense that they had been willing to undertake six months of intense training. There were a maximum of two workers randomly assigned to firms on each list. In case (i), firms knew what sector the worker had been trained in, but not that training had been paid for by BRAC. We presented stylized CVs of workers to firms (fitting a common template). The firm could hire neither, one or both (and of course remained free to hire workers from outside the evaluation sample). The median worker was matched to a single firm from our firm-side survey. The random worker-firm match assignments were restricted to take place between firms operating in the same sector as the worker had been trained in (T2), or had expressed an initial desire to be trained in (T3). Worker and firm also had to be located in the same region to increase the feasibility of the match.[8]

The Appendix describes in more detail how worker-firm match assignments were practically implemented, including the exact scripts used to communicate the process to workers and firms. These were designed to try and minimize uncertainty both parties had over the process of job assistance being provided by BRAC. In addition, firms were not provided contact details of workers – they had to come through BRAC officers. Hence our results are not due to firms recalling workers or workers using storable offers [Katz 1986, Katz and Meyer 1990]. The job assistance program only involves BRAC officers and workers, with VTI employees playing no role. As VTIs do not normally match workers to firms, there are no pre-existing ties between VTIs and firms.

2.3 Data

Timeline and Surveys Figure 2 shows the six-year study timeline from 2012 to 2018. The baseline worker survey took place from June to September 2012 just after applications for vocational training were received. Among those taking-up the offer of training (T1, T2), we next surveyed them at the end of their six month course. We use this to measure their posterior beliefs over their labor market prospects just as they complete training but prior to having knowledge over job assistance being offered. Among those randomized out of training, we next surveyed them just as vocational trainees were completing their courses, and use this to assess the opportunity

[8] Meta-analyses of job assistance programs [Card *et al.* 2017, McKenzie 2017] emphasize that their typical element involves engineered worker-firm meetings, to help overcome search frictions. These meetings can either be directed (as in our match offer treatments that are directed towards firms in sectors where workers were originally offered training) or undirected, such as through the use of job fairs [Beam 2016, Abebe *et al.* 2021a].

cost of attending six months of vocational training. These two rounds of data collection are under Phase 1 of the timeline shown in Figure 2.[9]

For workers involved in job assistance treatments, we record key outcomes from worker-firm matches that take place (job offers, offer refusals etc.).

Workers were tracked 24, 36, 48 and 68 months after baseline (12, 24, 36 and 56 months after the end of training/job assistance). The worker surveys were designed to measure multiple dimensions of search behavior suggested by classes of job search models. This allows us – perhaps uniquely – to measure panel data on individuals over six years, on multiple dimensions of search behavior, such as expected earnings and job offer arrival rates, search effort and channels, desired firm and job characteristics, as well as detailed labor market outcomes on job offers, employment, earnings, hours, wages, bargaining, spells, job and firm characteristics. We couple this data with measures of time invariant worker traits such as their cognitive ability and psychological traits, to shed light on whether such traits interact with training and job assistance to determine search behavior and outcomes.

Balance, Compliance and Attrition Table 1 shows the labor market characteristics of workers in each treatment arm. Table A2 shows other background characteristics. In both cases, the samples are well balanced, and normalized differences in observables are small.

We noted earlier that among those offered job assistance, there is near full compliance in that all workers agree for their details to be passed onto potential employers. However, to see the extent to which these differences are driven by compliers with the vocational training treatment, we first note that 68% of individuals take-up the offer of training, with over 95% of them completing training conditional on enrolment. Table A3 shows correlates of compliance with the offer of vocational training, namely whether the worker completed their training course. We see that: (i) 65% of individuals comply with vocational training; (ii) this is no different between those offered only vocational training and those later also offered job assistance – this is as expected because job assistance is only announced upon training completion, so because individuals were unaware of the later job assistance offer at the point of enrolling into vocational training, compliance with training is independent of the expected returns from job assistance; (iii) women and the more educated are less likely to comply; (iv) the correlates of compliance do not differ between those

[9] A second smaller round of applications and baseline surveys (17% of the overall sample) were conducted in May and June 2013. The majority of trainees from the first round of applicants started training in January 2013, as shown in the timeline. For logistical reasons, a smaller group received training between April and October 2013. The trainees from the second round of applications received vocational training between October 2013 and March 2014. VTI surveys were collected towards the end of the training period while trainees were still enrolled at the VTIs. Workers from the second round of applicants were not included in the Tracker Survey. There were two rounds of job assistance and vocational training + job assistance interventions, in line with the two batches of first round trainees from the vocational training institutes. The first round took place in August-September 2013. The second round took place in December 2013-February 2014. Our specifications control for implementation round dummies, and the results are robust to dropping workers in the second round.

offered only vocational training and those who later also offered job assistance.[10]

Only 15% of workers attrit by the 68-month endline. In the Appendix we describe correlates of worker attrition, confirm attrition is uncorrelated to treatment, and that there is no evidence of differential attrition across treatments based on observable characteristics (Table A4).

3 Search Behavior and Expectations

3.1 Controls

To understand how worker's search strategies might shift in response to these labor market interventions, we first detail search behavior among controls. We do so in three steps describing: (i) how their search effort varies over our six-year study period; (ii) their baseline expectations over the earnings distribution if they were to move up the job ladder and be employed in their most preferred study sector; (iii) their baseline expectations over the job offer arrival rate from firms in these study sectors.

Search Effort Figure 3 shows how employment and search effort change over time among controls. Panel A focuses on the extensive margin of employment and job search. In these labor markets informal/casual work are commonplace, so the notion of unemployment is somewhat vague. We define individuals to be unemployed if they are not involved in any work activity. Those engaged in casual work or unpaid work in family businesses are considered to be employed.

Over the fours years from first follow-up, the share of workers reporting being unemployed at some point in the year falls from 90% to 70%. However, the share of workers reporting looking for a job never rises above 60%. Panel B shows the intensive margin: in the year prior to baseline, workers spend around nine months unemployed, yet spend less than one month looking for work. While the days spent searching rise over time, they never get close to matching the time these young workers actually spend without work year on year.

To place our offer of job assistance into wider context, we can also use controls to get a sense of the frequency of job applications made. We only collected this at the final follow up, six years after baseline. We find among controls the average number of job applications made in the preceding year is 4.7, rising to 8.1 applications among those that were non-employed for that entire time. In short, job seekers make fewer than one application per month.

This apparent misallocation of time can be due to workers either being discouraged – with their poor labor market outcomes being a self-fulfilling prophecy – or as a result of them being optimistic over the returns to search effort. Workers might react differently to our interventions

[10] The main reasons for not taking up the training offer were family reasons (35%), followed by distance to the VTI (15%). Only 13% reported not taking up because they had found a job. With this design, we would need to caveat any comparison of the response to job assistance between workers offered vocational training or not (T2 vs T3), but that is not our focus.

depending on which of these is more relevant. We thus dig deeper into the issue by presenting evidence on the beliefs control youth hold over their own job prospects.

Expected Earnings Motivated by job search models emphasizing workers learn about the wage offer distribution [Wright 1986, Burdett and Vishwanath 1988], we start by examining worker's expected earnings if they were employed in the good sectors that we offered vocational training in. We elicit these beliefs for the worker's most preferred sector (for those taking up the offer in T1 and T2, this nearly always corresponds to the sector in which they receive training).

To establish a benchmark for these beliefs, the first two box-whisker plots in Figure 4A show the entire distribution of *actual* monthly earnings of controls at baseline, split for casual and regular work (for each type of work, we show the 10th, 25th, median, 75th and 90th percentiles of the earnings distribution). As expected, the distribution of earnings from regular employment is right-shifted relative to earnings in casual employment (where the majority of workers report being unpaid).

We next show expected earnings at baseline if, given their skill set, individuals were to move up the job ladder and be employed in their most preferred study sector. These beliefs are derived for all controls, irrespective of their search effort or employment status, and hence are not driven by compositional changes.[11] We asked controls their minimum and maximum expected earnings if offered a job in their preferred study sector. We asked them the likelihood their earnings would lie above the midpoint of the two, and fit a triangular distribution to derive their expected earnings. The next three box-whisker plots in Figure 4A show the distribution of minimum, maximum and expected earnings of controls in these good jobs. We see an intuitive ranking across expectations, with greater dispersion in the expected maximum earnings. Average expected earnings are higher than actual earnings from the kinds of regular work that controls are engaged in at baseline – indeed, the median earnings in actual regular work at baseline lies below the 25th percentile of expected average earnings if the worker could move into their most preferred sector. Hence controls recognize jobs in our study sectors are better than the kinds of work they have previously experienced.

To assess the accuracy of these beliefs, the final batch of box-whisker plots takes earnings data from workers actually employed in these eight study sectors, using the sample of firms tracked in our study. We show earnings for: (i) unskilled workers; (ii) recent hires; (iii) skilled workers. The first two are plausible counterfactuals for controls if they were to immediately transition into good sectors. We observe a fair degree of overlap between the distribution of expected earnings and the actual earnings of unskilled and newly hired workers in these sectors. In short, controls

[11] Only individuals who report a zero probability of finding a job in their most preferred good sector in the next 12 months are excluded from the sample. For employed workers, we ask them to consider a scenario if their firm shut down and they were to transition to a job in their most preferred study sector. These beliefs are elicited at baseline, pre-treatment but after individuals have been recruited into the evaluation sample through the oversubscription design. They might then reflect an element of expecting to be trained.

have reasonably accurate beliefs about the wage offer distribution should they move up the job ladder. Biased beliefs on this margin do not appear to explain why they devote too little time to job search (Figure 3).[12]

Examining correlates of these earnings expectations, we find no evidence that gender, age or recent labor market experiences predict these minimum, maximum or expected earnings. It is as if the distribution of entry level earnings in these good sectors is almost common knowledge among labor market entrants.

Expected Job Offer Arrival Rate The second margin of beliefs relevant for search is over the job offer arrival rate, akin to workers learning about their own job prospects [Falk *et al.* 2006, Gonzalez and Shi 2010]. At baseline we asked controls what was their expected probability of finding a job in these study sectors in the next month, six months and year. The job offer acceptance rate is over 90%, so this question essentially corresponds to worker beliefs over the job offer arrival rate. The distribution of these beliefs are shown in the first three box-whisker plots in Figure 4B. Reassuringly, these are right-shifted as we increase the time horizon considered. However, despite youth non-employment rates close to 60% and a reliance on casual jobs, the median belief held among unskilled controls is they have a 20% chance of receiving a job offer from firms in these good sectors within a month, 40% within the next six months, and 60% within the next year.

We assess the accuracy of these beliefs using two approaches. First, we compare them to actual youth employment rates in regular jobs. Panel C of Figure A1 shows this using the UNHS data, that is fielded close in time to our baseline. For unskilled youth, employment rates in regular jobs are around 20%, and only rise by a further 10% for workers two years older, and plateau thereafter. This is far lower than the baseline belief held by the median control worker of a 60% job offer arrival rate from firms in good sectors in the next year.[13]

Second, we examine how controls revise their expectations between baseline and first follow-up, as they engage in job search over those two years. The next three box-whisker plots in Figure 4B show the distribution of revised expectations over job offer arrival rates at first follow-up. These are revised downwards: the median expectation held among controls is they have a 10% chance of receiving a job offer from a firm in a good sector within a month, 20% within the next six months, and 40% within the next year. Controls are therefore gradually becoming more realistic over time as they search.

[12] We note a positive earnings gradient in skills in these firms, and the actual earnings distribution for skilled workers overlaps far less with the expected wages of unskilled control workers if they were to be able to move into these firms.

[13] In making a comparison to the UNHS we are of course contrasting the stock of young workers in the economy with regular jobs to the flow probability our evaluation sample workers express about entry into regular jobs. As a result, we might expect the economy-wide flow of young workers into regular jobs to be even lower than the stock measured in the UNHS.

To see how quickly their expectations are converging to reality, we calculate the *actual* likelihood of finding a good job over exactly these horizons using data from the second follow-up survey, fielded a year later. These are shown in the remaining box-whisker plots in Figure 4B. These are still far lower than worker expectations over the job offer arrival rate, with the divergence increasing with the time horizon considered: 7% of workers actually find a job within a month, 10% do so within six months, and 13% do so within a year. Such persistent optimism can potentially explain the lack of search effort described earlier, and thus contribute to slow exit rates out of non-employment.[14]

These results complement a growing literature on the *persistence* of optimistic beliefs [Benabou and Tirole 2002, Compte and Postelwaite 2004, Van den Steen 2004]. More specifically, we add to the evidence that displaced workers are optimistic over job offer arrival rates both in the US [Spinnewijn 2015, Mueller *et al.* 2021, Potter 2021], and in lower-income labor markets including Ethiopia [Abebe *et al.* 2021a] and South Africa [Banerjee and Sequeira 2021].

3.2 How Expectations Evolve Until Job Assistance is Announced

We next zoom in on the time window between the baseline and eve of when any announcement of job assistance is made, considering the evolution of beliefs over this crucial period for our study. We contrast the evolution of beliefs of those assigned to vocational training to those for control youth. This is critical for understanding how workers react to information generated by the job assistance intervention, so informing two of our ITT comparisons: (i) T2 vs T1: the offer of job assistance on those previously offered vocational training relative to those only offered vocational training; (ii) T3 vs C: the offer of job assistance on those randomized out of vocational training.

For those assigned to vocational training, we measure their expectations just as they complete their training course, but prior to any offer of job assistance being announced. For controls, as described we measure beliefs at baseline and first follow-up. We make a simplifying assumption that beliefs evolve linearly over time, so that on the eve of job assistance being announced, beliefs would have changed half way from what is measured at baseline and first follow up. Nothing hinges on this assumption of linearity, it is only made to interpolate a specific belief at the time the job assistance is offered. A similar exercise could be conducted by interpolating reasonable non-linear monotonic changes in beliefs.

Expected Earnings We first consider the evolution of expectations over the earnings distribution in our study sectors. Figure 5A shows the distribution of beliefs on the minimum and maximum expected earnings from being employed in their most preferred sector for: (i) all workers at baseline; (ii) controls; (iii) vocational trainees. For controls, beliefs over the earnings distribution

[14] Examining correlates of beliefs over job offer arrival rates, women tend to be more optimistic over all horizons, and older workers less optimistic. Having worked or earnings in the past month do not robustly correlate to these beliefs. There is only a weak positive gradient between beliefs over the job offer arrival rate and actual search.

hardly change. This is as expected – controls have relatively accurate beliefs at baseline, and little new information is gained over six months of job search. Among workers graduating from vocational training, both distributions of minimum and maximum expected wages shift rightward, with an especially pronounced upward shift in the distribution of maximum earnings. This reflects their self-recognition of high returns to their newly acquired skills.

Figure 5A shows the entire distribution of beliefs held. To formally test differences in means of these distributions between workers in treatment arms or over time, Table 3 shows the mean and standard deviation of each expectation, pooling together those assigned to vocational training (T1, T2) and those assigned out of vocational training (T3, C). Rows R1 and R2 show expectations at baseline, while Rows R3 and R4 show expectations on the eve of job assistance being announced. At the foot of the Table 3 we report p-values on tests of equality of expectations, between groups at the same moment in time (Row 1=Row 2, Row 3=Row 4), and within workers in a given treatment arm over time (Row 1=Row 3, Row 2=Row 4).

Columns 1 to 3 focus on the minimum, maximum and expected earnings. We see that: (i) at baseline there are no significant differences in expectations across workers assigned to vocational training or not (Row 1=Row 2); (ii) there are significant changes in beliefs over time among workers assigned to vocational training (Row 1 = Row 3); (iii) there are no significant changes in beliefs over time among workers randomized out of vocational training (Row 2 = Row 4); (iv) hence, in line with the patterns shown in Figure 5A, on the eve of job assistance being offered, there are significant differences in beliefs between those offered vocational training and those randomized out of it (Row 3 = Row 4).

To probe further how uncertainty over earnings in good sectors changes over time, we can construct the coefficient of variation as a measure of the dispersion or uncertainty of expected earnings (again assuming a triangular distribution), shown in Column 4 of Table 3. We see that from baseline to the eve of job assistance being announced there are relatively minor changes in uncertainty among those assigned to vocational training (Row 1 = Row 3). Hence just on the eve of job assistance being offered, the precision of beliefs does not differ significantly between those with and without the offer of vocational training (Row 3 = Row 4).

Expected Job Offer Arrival Rate Figure 5B shows how beliefs over the job offer arrival rate evolve among those assigned to vocational training and the control group. For controls, we saw earlier they hold optimistic beliefs on this margin at baseline, but gradually become more realistic as they search. The beliefs of vocational trainees move sharply in the *opposite* direction: they revise upwards their belief over the job offer arrival rate at each horizon, with the gap in beliefs between trainees and controls opening up considerably at the six and 12 month horizons: over those horizons, there is no overlap in the interquartile range of beliefs among the two groups of workers. Close to graduating, 25% of trainees believe they will receive a job offer in their most preferred good sector with certainty in the next twelve months.[15]

To see whether these differences are significant, Column 5 of Table 3 shows that this belief: (i) significantly rises among those assigned to vocational training (Row 1 = Row 3); (ii) significantly falls among those randomized out of vocational training (Row 2 = Row 4). On the eve of job assistance being offered, beliefs along this margin of the job offer arrival rate thus significantly differ between workers offered vocational training and those that are not (Row 3 = Row 4).

How realistic are these beliefs of these newly skilled workers? We can benchmark them in two ways using out of sample and in-sample data. First, we refer back to the evidence from the UNHS survey in Figure A1. Panel C shows the likelihood skilled workers are in regular jobs, by age. At each age this is higher than for unskilled workers (in proportionate terms these employment rates are near double). However, their levels remain low: around 35% of 20-21 year olds have regular jobs, and this rises to only 40% for those aged 22-23. This is far from the beliefs held by trainees as they complete vocational training.[16]

Second, we can consider the actual rate at which vocational trainees work in the one of the study sectors in the 12 months from the end of their courses, as measured in our second follow up. As discussed in more detail later, 30% of vocational trainees end up working in one of the eight study sectors over this time frame (and in line with the UNHS evidence). We can see from the last set of bars in Figure 5B that this is far below the median or even the 10th percentile of beliefs held by these workers as they completed training. It is because of this huge wedge between expectations and reality that we describe these workers as overly optimistic or exuberant.

Call Back Rates and their Determinants For workers offered job assistance, the key outcome is whether they receive a call-back, i.e. an invitation to meet the firm owner.[17]

How do actual call back rates compare to worker's prior beliefs? As Figure 5B shows, on the eve of job assistance being announced, the median trained worker believed there was a 30% chance they would receive a job offer from a good firm in the next month. In actuality, only 16% of skilled workers receive a call back. Among controls, the median worker had a prior belief of there being a 20% chance they would receive a job offer from a firm in a good sector in the next month. In actuality, 18% of unskilled workers receive a call back, thus confirming their prior.

[15] The perceived skills workers have at the completion of the vocational training course are significantly and positively correlated with these expected job offer arrival rates at 6 and 12 months.

[16] Are these outcomes from the UNHS a good counterfactual for what would occur to the vocational trainees? There are opposing forces for the comparison between our sample and those in the UNHS. On the one hand, our workers are more disadvantaged than the average youth in Uganda, because of the eligibility criteria used. On the other hand the kinds of VTIs they attend are higher quality than the average VTI attended by youth in Uganda. Moreover, we can compare actual labor market outcomes over the short run for those assigned to vocational training: we see that although their employment rates improve, in the short run there is no change in the likelihood they have engaged in regular work (remaining close to 30% as for controls).

[17] The entire process from when assistance is announced until when workers are invited to interview is around two weeks (although workers never called back would obviously only later realize this). While this can cause short run postponements of search, we measure impacts on search behavior a year later.

To understand how the average worker in each treatment arm might react to these call back rates, we need to be precise on the correlates of call backs. Recall that each firm is paired with two workers, who are either both unskilled or both skilled. Columns 1 and 2 of Table A5 show correlates of call backs to compliers with the offer of vocational training, Columns 3 and 4 present analogous specifications for call backs to those randomized out of vocational training. The specifications control for: (i) worker and firm characteristics; (ii) worker characteristics and firm fixed effects (exploiting that each firm is presented with two workers). At the foot of each Column we report p-values on the joint significance of worker and firm covariates.

Two important results emerge. First, worker characteristics do not predict call backs, for either group of workers – the p-values on the joint test of significance of worker covariates vary from .399 to .658 across specifications. This is unsurprising: firms are presented with two workers that are, by construction, similar on observables. Hence the design of the job assistance intervention almost fully removes the possibility that worker characteristics determine call backs.[18]

Second, call backs are predicted by firm characteristics. In particular, trained workers are more likely to be called back if they are matched to firms that would like to expand (and so have a vacancy), and where owners report being constrained by an inability to find trustworthy workers. Hence in line with other studies, the key limiting factor on worker-firm matches actually taking place is firms willingness to meet workers, rather than reservation prestige driving worker refusals to meet firms [Groh *et al.* 2016].

Reaction to Call Backs As described earlier and detailed further in the Appendix, the job assistance treatment was clearly explained – using fixed scripts – to workers and firms. Given the wording, workers were fully aware their details were being handed over to only a few firms, and those firms would be within a small geographic area (a 4km radius of the local BRAC branch office, and only urban areas within that radius). Workers should therefore understand there is no additional informational content in any given call back (or lack thereof) over and above information about the labor market they may acquire through their own job search activities, as this is one draw from a restricted set of firms.

Our null hypothesis is that workers are perfectly informed, and understand call backs are not determined by their characteristics. They rationally infer there to be zero information from any given call back (or lack thereof) because: (i) they do not learn anything about the labor market (as this is one draw from a restricted sample of firms), and, (ii) they do not learn anything about their own labor market prospects (as workers characteristics do not determine call-backs). Under this null, the search strategies of workers – irrespective of whether they have earlier been vocationally trained or not (T2 vs T1, T3 vs C) – are unaffected by the offer of job assistance.

On the other hand, the offer of job assistance from a highly reputable NGO such as BRAC would

[18] Our design thus contrasts with the audit studies literature, that explicitly manipulates worker characteristics to determine which drive call backs.

be highly salient to workers – perhaps especially so among those workers that were completing the BRAC sponsored vocational training courses. We described earlier how control worker typically submit less than one job application per month. In these labor markets where job search often involves workers trying to approach firm owners directly, the treatment represents an almost unique opportunity for their details to be passed onto good firms, enabling them to get to the front of the job queue with such firms, and for firm owners to at least consider their credentials seriously.

Our alternative hypothesis is thus that some share of workers are imperfectly informed and so misinterpret what drives call backs in the experiment. In that case, for trained workers the lower than expected call back rate (30% vs. 16%) causes them to revise down their beliefs about their own job prospects. Such misattribution can occur because of the combination of two factors: (i) labor market entrants are not well informed at baseline, and trainees remain optimistic relative to their realistic prospects as they complete their training (Figure 5); (ii) there are no market substitutes for the job assistance intervention, and so the offer can be viewed as a salient and unique opportunity for them to find meaningful work. Under this alternative, job assistance generates bad news for the average trained worker.

While we do not aim to micro-found misattribution, we note it is consistent with job seekers being subject to the gambler's fallacy, in which they become discouraged as they overinfer their own job prospects from one bad draw [Rabin and Vayanos 2010], and with a large body of theoretical literature that studies why individuals can hold unrealistically positive views of their own prospects [Carrillo and Mariotti 2000, Benabou and Tirole 2002, Santos-Pinto and Sobel 2005, Grossman and van der Weele 2017, Koszegi *et al.* 2021].

Hence between trained workers with and without job assistance (T2 vs. T1), under this alternative a key distinction is that trained workers with job assistance receive bad news on their own job prospects, just at a time when they are transitioning into the labor market and meeting potential employers. Trained workers without job assistance are insulated from this news, and so begin their job search with the increasingly exuberant beliefs shown in Figure 5.

For workers randomized out of the offer of training, their priors are in line with call back rates (20% vs. 18%). Hence, even under the alternative hypothesis, there is no reason why they should alter search behavior. However, because call backs generated in the experiment are not the kind of signal they receive during regular job search, the low rate of call backs provides credible confirmation of their poor labor market prospects. This can prompt them to change search job behavior relative to controls (T3 vs. C). Ultimately this however remains an empirical question, that we now turn to.

4 Job Search

4.1 Empirical Method

We analyze how labor market interventions offering vocational training and job assistance impact search strategies. These effects are measured at first follow-up, 24 months after baseline and a full year after trainees have graduated, and call backs made, so using outcome data from Phase 2 of the timeline in Figure 2. For worker i assigned to treatment group j in strata s, we estimate ITT effects using the following specification:

$$y_{is1} = \sum_j \beta_j T_{ij} + \gamma y_{i0} + \lambda_s + u_{ist}, \tag{1}$$

where y_{is1} is the search behavior of interest at first follow up ($t = 1$), T_{ij} is a dummy for the treatment arm that worker i is assigned to, y_{i0} is the baseline value of that outcome (where available), λ_s are strata fixed effects. All regressions control for the implementation round and dummies for month of interview. We present robust standard errors as randomization is at the individual level, but also report p-values adjusted for randomization inference [Young 2019] and multiple hypothesis testing to account for the three treatment effects estimated in (1), using the step-down procedure of Romano and Wolf [2016].

The ITT coefficients of interest are: (i) β_1 (T1 vs C): the impact of the offer of vocational training on worker search strategies; (ii) $\beta_2 - \beta_1$ (T2 vs T1): the impact of the offer of job assistance on those offered vocational training; (iii) β_3 (T3 vs C): the impact of the offer of job assistance on those randomized out of the offer of vocational training.[19]

We present findings on search strategies for all workers irrespective of their employment status, ensuring results are not driven by composition effects. Hence these treatment effects should be interpreted as combining: (i) impacts on search behavior while not employed; (ii) impacts through on-the-job search. Table A6 summarizes short run labor market treatment effects (measured at first follow up). We see no short run divergence in outcomes between those offered vocational training with and without job assistance. Those offered vocational training are 6 to 9pp more likely than controls to have worked in the last month (Column 1), are around 16pp more likely to have worked in one of the study sectors (Column 2), and work about a month longer in one of the study sectors (Column 3). There are muted impacts on earnings, self-employment or the quality of firms employed at, as measured through an index of firm characteristics.[20]

[19] Spillover and general equilibrium effects have been much discussed in the literature on job assistance [Crepon *et al.* 2013]. In our setting such spillovers are unlikely to be relevant. Considering a labor market as defined by a sector-region, then in each labor market from our original firm census we measure there to be 156 employed workers and 40 firms, and only a small fraction of these are engaged in our study.

[20] We construct the index so that higher values correspond to firms that are likely more productive or profitable because they: (i) have more employees; (ii) are formally registered; (iii) provide training; (iv) provide other material employee benefits to workers.

4.2 Skills

Sector Specific Skills In our earlier work using data from this project, Alfonsi *et al.* [2020], we presented results on how the offer of vocational training translates into human capital accumulation. We briefly reiterate those results, and extend them to document skills accumulation for those offered job assistance.

We first consider a sector-specific skills test we developed in conjunction with skills assessors and modulators of written and practical occupational tests in Uganda. Each test comprises seven questions (with a combination of multiple choice and more complex questions being used). Figure A2 shows an example of the skills test for the motor mechanics sector. Workers had 20 minutes to complete the test, and we convert answers into a 0-100 score. If workers answer questions randomly, their expected score is 11. The test was conducted on all workers (including those assigned to as controls) at second and third follow-up, so measuring persistent skills accumulation. There is no differential attrition by treatment into the test.[21]

Before administering the test, we asked a filtering question to workers on whether they had *any* skills relevant for sectors in our study. The dependent variable in Column 1 of Table 4 is a dummy equal to one if the worker reported having skills for a sector, where we report the β_j estimates from specification (1). Focusing on the first row that shows treatment effects for workers offered vocational training, we see they are significantly more likely than controls to report having sector-relevant skills, as measured two and three years after the vocational training is provided. As reported at the foot of the table, 61% of controls report having skills for some sector, and reassuringly this rises to 87% for those offered vocational training.

All workers that reported having sectoral skills took the test: others (mostly controls) were assigned a score of 11 assuming they would answer the test at random. Column 2 shows workers offered vocational training significantly increase their measurable skills. Relative to controls, they increase sector-specific skills by 21% (or $.29\sigma$ of test scores).

The next specification estimates the ATE on sector specific skills acquired, so replacing treatment assignment with treatment take-up, where take-up is defined as a dummy equal to one if the worker completed vocational training. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. We bootstrap standard errors using 1,000 replications. Column 3 shows that among those that take-up training, skills accumulation is even greater, increasing by 28% over controls (or $.37\sigma$ of

[21] We developed the sector-specific skills tests over a two-day workshop with skills assessors from the Directorate of Industrial Training (DIT), the Uganda Business and Technical Examinations Board (UBTEB) and the Worker's Practically Acquired Skills (PAS) Skills Testing Boards and Directorate. To ensure the test would not be biased towards merely capturing theoretical/attitudinal skills taught only in VTIs, workshop modulators were instructed to: (i) develop questions to assess psychomotor domain, e.g. trainees ability to perform a set of tasks on a sector-specific product/service; (ii) formulate questions to mimic real-life situations (e.g. "if a customer came to the firm with the following issue, what would you do?"); (iii) avoid using technical terms used in VTI training. We pre-tested the skills assessment tool both with trainees of VTIs, as well as workers employed in firms in the eight sectors we study (and neither group was taken from our evaluation sample).

test scores). In Alfonsi *et al.* [2020] we estimate the steady state labor market returns to these skills to be 20-30%.[22]

The Table also sheds light on whether the offer of job assistance has additional impacts on skills. We see that: (i) workers offered vocational training and job assistance have no different skills accumulation to those only offered vocational training; (ii) among those randomized out of vocational training, there are no differences in skills between those with and without job assistance.

Other Dimensions of Human Capital Table A7 shows the offer of vocational training or job assistance do not impact other dimensions of human capital or worker traits: (i) among youth offered vocational training, there are no differences in the big-5 personality traits, cognitive ability (as constructed from a 10-question version of the Raven's progressive matrices test) and other psychological traits between those with and without job assistance; (ii) among those randomized out of vocational training, there are also no differences in the big-5 personality traits, cognitive ability and other psychological traits between those with and without job assistance. This battery of results helps rule out any of our later findings on search behaviors or labor market outcomes are driven through the interventions impacting these margins of human capital or traits.

4.3 Expectations

Key to our analysis is how worker expectations over their own labor market prospects respond to labor market interventions, a full year after training is completed and job assistance offered. These results are in Table 5. Columns 1 to 3 show treatment effects on the distribution of expected earnings if workers were able to transition into their most preferred study sector job.

Focusing first on those offered vocational training, we see they significantly revise upwards their minimum expected earnings, their maximum expected earnings are revised upwards by a greater extent, and their expected earnings shift forward by $25.4/month, corresponding to a 44% rise over the expectations of controls. In many job search models, the minimum expected wage helps pin down the reservation wage of a worker (because a potential employer would not incur the cost of making an offer she knows will be rejected by the worker who prefers to continue to remain non-employed and search). The fact that this proxy for the reservation wage shifts upward with the offer of vocational training suggests workers are adjusting search strategies.[23]

[22] This is all consistent with other evidence we collected from workers towards the end of their training. When asked about their satisfaction with their course, 76% were extremely happy/very happy with the experience; 86% were extremely happy/very happy with the skills gained; 96% reported skills acquisition as being better than or as expected, and 56% reported that six-months of training was sufficient for them to learn the desired skills.

[23] Given the importance of reservation wages in job search models, much has been discussed in the literature on how reservation wages might change with the duration of unemployment benefits. In our context if we focus on the control group for whom employment rates remain at 40% between baseline and first follow-up, we see little significant change in reservation wages over time. This lack of updating among controls is consistent with evidence from high-income settings on reservation wages directly [Krueger and Mueller 2016, Le Barbanchon *et al.* 2018], or on search activity [DellaVigna *et al.* 2020, Marinescu and Skandalis 2021].

Column 4 shows that there is no overall change in the dispersion of expectations as measured by the coefficient of variation. Finally, Column 5 shows they also revise upwards their belief over the job offer arrival rate in the next year (by 1.84 on a 0-10 scale).

These ITT estimates are all robust to correcting for randomization inference or multiple hypothesis testing.

The next row shows the same outcomes among those also offered vocational training but who were, a year earlier, additionally provided job assistance (again relative to controls). At the foot of each Column we report the p-value on the equality of treatment effects on those offered vocational training with and without job assistance. We see that workers additionally offered job assistance have lower expected earnings from working in these good sectors – this difference is most pronounced at the minimum expected earnings, a proxy for their reservation wage ($p = .095$).

For workers additionally offered job assistance we also find they hold significantly less precise beliefs over earnings relative to those only offered vocational training ($p = .036$). Finally, Column 5 shows that they also significantly revise down their beliefs over the job offer arrival rate in good sectors, despite them being as skilled as those without any job assistance ($p = .082$).

In short, youth offered vocational training and job assistance are *discouraged* relative to youth only offered vocational training as measured by these various margins of expectation. On four out of five dimensions of belief, the general significant and downward revisions of beliefs for workers offered job assistance on top of vocational training, is in line with our alternative hypothesis, that the low call back rates they experience in the job assistance intervention represent bad news for them relative to their prior expectation at the time they completed vocational training (Figure 5).

It is worth reiterating that over the same one year time period, there are no differences in labor market outcomes between these two groups of worker (Table A6). Hence any feedback loop from employment outcomes to expectations is not driving this divergence in expectations across treatment arms.

The third row of Table 5 shows ITT estimates on the expectations of those only offered job assistance (again relative to controls). Their beliefs over expected earnings and the job offer arrival rate are unaffected. This is in line with the rate of calls backs among this group of (unskilled) workers being in line with their prior expectation.

Our results complement a nascent literature examining the process of worker's learning during job search, and are among the first to do so outside a US context [Krueger and Mueller 2016, Conlon *et al.* 2018, Mueller *et al.* 2021, Potter 2021].[24] A notable exception to this is Abebe

[24] Krueger and Mueller [2016] use panel data from unemployed job seekers in New Jersey to study the evolution of reservation wages over the unemployment spell. Conlon *et al.* [2018] document workers learning about the wage offer function during job search, again using US data. They document updating patterns that are inconsistent with Bayesian updating and estimate a partial equilibrium job search model with on the job search and learning. Potter [2021] develops and estimates a model of Bayesian learning about the arrival rate of offers in a job search model, again using US data. Mueller *et al.* [2021] show job seekers' beliefs are biased and under respond to unemployment spells, and then calibrate a model of job search to show how much they contribute to slower flows out of unemployment.

et al. [2021a] who show that among Ethiopian job seekers randomly assigned to attend job fairs (where few workers are actually hired), individuals also revise down their beliefs over their own labor market prospects.

4.3.1 Is This Really Misattribution?

We have no direct measure of workers misattributing information inferred from the lack of call backs in the job assistance intervention. We use two approaches to narrowing the interpretation that in response to job assistance, workers update their beliefs over their *own* job market prospects, and so do not behave according to a null of them being perfectly informed and understanding the lack of information in any given match offer.

First, low call back rates might cause workers to revise beliefs about the state of labor demand. Hence their changed expectations might reflect beliefs over market conditions, not their own prospects. To examine this we elicited worker beliefs over the following: (i) whether a lack of firms is a problem for job search; (ii) whether a lack of advertised jobs is a problem (signifying a lack of vacancies); (iii) whether workers have difficulties demonstrating their practical skills to employers; (iv) whether workers have difficulty showing their soft skills to employers. We combine these into one index using the approach of Anderson [2008] – this uses the data covariance matrix to construct a weighted sum of indicators in the group, and so gives less weight to items more correlated with each other. These indices are standardized to have mean zero and variance one in the control group at baseline, so estimates are interpreted as effect sizes.

Column 7 of Table 5 shows how the treatments impact this market beliefs index: we see no changes in beliefs over market conditions among any group of workers. Table A8 shows impacts on each dimension of the market beliefs index. For no treatment group do we find evidence of significant changes in beliefs about any dimension of labor market conditions.

Second, taking seriously call backs are not determined by worker characteristics (Table A5), we examine impacts on the expectations of workers with and without call backs. The results are in Table 6. Among those previously offered vocational training, those actually receiving a call back significantly revise upwards their earnings expectations relative to those that did not receive a call back. Despite call back rates being very low, the interaction of the treatment dummy with whether they receive a call back is positive and significant for both their expected maximum earnings and job offer arrival rate.

The magnitudes of these impacts of being called back are economically significant: the maximum expected earnings increase by over 80% and the belief over the job offer arrival rate increases by 50% for those offered vocational training and called back relative to those not called back. At the foot of each Column we test for the equality of beliefs between those only offered vocational training, to those also offered job assistance with and without call backs. We see that those called back have beliefs over the minimum, maximum and mean earnings that are not statistically different to those only offered vocational training (who we described as having exuberant beliefs) – although there is more uncertainly in their beliefs as measured by the coefficient of variation (Column 4). In contrast those not called back hold significantly different beliefs to those only offered vocational training in terms of the minimum, maximum and mean earnings, as well as the job offer arrival rate.

Among those only offered job assistance, we do not find robust evidence that being called back changes expectations, in line with those workers having on average accurate prior beliefs over call back rates on the eve of job assistance being announced.

Taken together the results show that in response to job assistance: (i) workers revise beliefs over their own job prospects, not over labor market conditions; (ii) beliefs updating depends on whether they are actually called back or not; (iii) for those receiving call backs, worker beliefs move in a direction that continues the trajectory beliefs were on between baseline and the eve of the announcement of job offers.

4.4 Search Intensity and Channels of Search

Introducing endogenous search effort has been a key extension of the canonical search model [Pissarides 2000, Shimer 2004]. We begin to examine how search intensity is impacted by our interventions by first considering the extensive margin of search. The result in Column 1 of Table 7 shows that workers offered vocational training are, relative to controls, significantly more likely to report having actively searched for a job. The magnitude of the effect is of economic significance: these workers increase search by 17.5pp, a 36% increase over controls. On the intensive margin vocational trainees report spending no more days searching for work (consistent with them experiencing shorter unemployment spells, as we later document), and they become more geographically mobile in their search.[25]

Those offered vocational training are also significantly more likely to report using direct walk-ins to firms (with no crowding out of their reliance on informal information from friends and family). The magnitude of the change is of economic significance: the 8.8pp rise corresponds to a 63% increase in the use of this search channel relative to controls.

Combining all margins of search intensity and channels into a single index, Column 6 shows this index of search behaviors rises significantly for those offered vocational training by $.089\sigma$.

Workers additionally offered job assistance have more muted responses on these dimensions of search a year later (their overall index rises by $.019\sigma$ and this is not different from zero). Perhaps most importantly, in Column 1 we see the impact on their extensive margin of search intensity is significantly lower than among those only offered vocational training ($p = .053$). While workers

[25] Our finding that the exogenous provision of skills expands the geographic basis of search complements other experimental evidence from low-income settings emphasizing that relaxing credit constraints leads to workers searching over a wider space [Franklin 2018, Banerjee and Sequeira 2020, Abebe *et al.* 2021b].

offered vocational training and job assistance do change behavior along some dimensions, these results are less robust to p-value adjustments for multiple hypothesis testing.

Workers only offered job assistance do not change search behavior among most margins except reporting spending fewer days actively searching for work.

Spinnewijn [2015] documents how US job seekers are bad at knowing that search is effective, that is, they underestimate the benefits of search. It is not straightforward to map from changes in search intensity to the returns to search effort because there are both income and substitution effects. Of course, it might be reasonable that among disadvantaged youth entering the labor market, the income effect dominates. If so, the significantly increased search intensity of those offered vocational training workers is consistent with them believing the returns to search effort have risen, while the more muted impacts among those also offered job assistance a year earlier, suggest they are discouraged from exerting search effort – in line with their revised beliefs.

4.5 Directed Search

Directed search is the notion that workers search over specific jobs/firms (or parts of the wage distribution) [Moen 1997, Shimer 1996, Acemoglu and Shimer 1999, Shimer 2005]. To examine if this dimension of search strategies is impacted by labor market interventions, we asked workers about characteristics of the *ideal* job and *ideal* firm they were searching for. We construct the ideal job index so that higher values correspond to jobs higher up the job ladder because they: (i) entail supervising others; (ii) have a high social status associated with them; (iii) enable workers to learn new job-specific skills; (iv) entail working with others (as opposed to working alone); (v) have a flexible schedule. The index is scaled so that treatment effects are interpreted as effect sizes. The result on the ideal job index is in Column 1 of Table 8: we see no evidence that either intervention impacts the ideal job characteristics workers are searching for. Table A9 confirms that no treatment impacts the ideal job searched for along any of the components of the index.

We construct the ideal firm index so that higher values correspond to more productive or profitable firms because they: (i) have more employees; (ii) are formally registered; (iii) provide training; (iv) provide other material benefits to employees. The treatment effects on the ideal firm index are shown in Column 2 of Table 8: we see significant evidence that workers offered vocational training change the kinds of firm they direct their search towards. Their ideal firm index rises by $.103\sigma$ (a result robust to p-value adjustments). Table A10 shows the firm characteristics driving this: these workers search for firms that can provide training and other material benefits.

Workers additionally offered job assistance a year earlier, search for firms that are no different to those targeted by control workers. This is also borderline significant to the types of firms that are ideally targeted by skilled workers ($p = .102$). These marginal differences in directed search tie closely to the marginal differences in reservation wage documented earlier (Table 5, Column 1). Those already hinted that while search strategies shift considerably among both groups of youth relative to controls, there are more subtle – yet important – distinctions in search behaviors between the two groups of individuals offered vocational training with and without job assistance.

4.6 Credit

The final dimension of job search strategy we consider builds on the idea that labor and credit markets are interlinked [Lentz and Tranaes 2005, Lise 2013].[26] We capture this interlinkage by constructing a credit index made up of the following components: (i) whether workers run down savings; (ii) increase borrowing; (iii) borrow to search for jobs; (iv) borrow for own business expenditures – i.e. set up in self-employment. Treatment effects on the index are shown in Column 3 of Table 8, with Table A11 showing the impacts on each component.

We see that for those offered vocational training – with or without the offer of job assistance – there is no response along these margins, and there is an overall null impact of these treatments on the credit index. However, for the first time we observe a margin of adjustment in search strategies used by workers only offered job assistance: their overall credit index rises significantly ($.090\sigma$). Table A11 reveals the channels for this: they are significantly more likely to borrow (Column 2), they do not use this to finance job search (Column 3), but rather report borrowing to finance own business expenditures in some form of self-employment (Column 4). The rate of borrowing for self-employment is double that of controls – and the average loan size among this treated group is $32 (so far below the $400 value of vocational training offered). We assess below whether their stated intention of borrowing for self-employment – as measured a year after job assistance is offered – actually translates into higher rates of self-employment in the long run.[27]

5 Labor Market Outcomes

The six-year study period allows us to map out how offers of vocational training and job assistance translate into labor market outcomes in the long run. We do so using outcomes over the last three survey waves, so 36 to 55 months after workers graduate from vocational training and/or are given job assistance. This corresponds to outcomes measured during Phase 3 of the timeline shown in Figure 2. We estimate the following ITT specification for worker i assigned to treatment group j

[26] Lentz and Tranaes [2005] model savings and job search as a joint decision problem. They show the conditions under which workers plan less *precautionary saving* when employed, and show that if utility is separable in consumption and search effort, then search intensity is monotonically *decreasing* with wealth. Lise [2013] introduces on-the-job search with optimal consumption/savings decisions. He shows that workers lower down the job ladder dissave because of two forces: they expect earnings to rise as they climb the ladder, and that the potential loss of income from unemployment is small (because they are low down the ladder).

[27] Business expenditures include expenses incurred to set up, or register a business, purchasing business assets or inputs, pay wages, etc.

in strata s in survey wave t:

$$y_{ist} = \sum_j \beta_j T_{ij} + \gamma y_{i0} + \lambda_s + \vartheta_t + u_{ist}, \tag{2}$$

where y_{ist} is the labor market outcome of interest in survey wave $t = 2, 3, 4$, ϑ_t is a survey wave fixed effect and all other controls are as previously described. We use robust standard errors as randomization is at the worker level, and also report p-values adjusted for randomization inference and multiple hypothesis testing to account for the three treatment effects estimated in (2).[28]

5.1 Employment

We begin in Table 9 by tracking standard measures of employment, and transitions into regular work. The first row shows the long run impacts of skills on these core labor market outcomes. Mirroring results described in Alfonsi *et al.* [2020], we find those offered vocational training: (i) are significantly more likely to work, with employment rates rising by 9.4pp or 15% over the long run average for controls (Column 1); (ii) this is not driven by an increase in the incidence of casual work (Column 2) but rather a transition for these youth towards regular employment, both on the extensive margin where regular employment rates rise by 11.3pp or 22% (Column 4), and on the intensive margin where these individuals spend 23% more months of the year engaged in regular work (Column 4). In terms of sectoral allocation, they double the months of the year they work in any one of the study sectors that offer good jobs (Column 5).

We summarize good employment outcomes by combining outcomes from Columns 3 to 5 into one index, using the Anderson [2008] approach and normalizing the index to be in effect sizes. The index is centered at zero for controls at baseline. This index outcome is shown in Column 6, and shows that relative to controls, for workers offered vocational training the employment index rises significantly by $.347\sigma$.

Strikingly, in the next row we see that for workers offered vocational training but also offered job assistance up to five years earlier, they have a significantly smaller improvement in their employment index of $.248\sigma$ ($p = .031$). The reason why the index is lower relative to those only offered vocational training is: (i) they are less likely to work in regular jobs ($p = .043$); (ii) on the intensive margin, they work significantly fewer months in regular jobs ($p = .011$); (iii) in terms of sectoral allocation, they work less time in one of the eight good sectors in which we offered training in ($p = .104$).[29]

The final row of Table 9 shows outcomes for those only offered job assistance. Relative to controls, their employment outcomes improve significantly along both extensive and intensive

[28] With a longer panel it would be appropriate to cluster standard errors by individual to account for correlated shocks within an individual over time.

[29] On other intensive margin measures we see no difference between skilled workers with and without job assistance in terms of the number of hours they work per day or the number of days they work per week.

margins. Naturally the magnitudes of impact are smaller than for those offered vocational training. Their employment index rises by $.117\sigma$, so around one third that of those offered vocational training and two thirds that of those offered vocational training and job assistance.

Our findings contribute to an ongoing debate about the persistence of intervention impacts in low-income contexts. While a body of work has suggested the combined provision of skills and assets can shift occupational choices in the long run for rural households [Banerjee *et al.* 2015, Bandiera *et al.* 2017], work in urban labor markets suggests the impacts of one-off high-valued transfers to underemployed youth fade over time [Blattman *et al.* 2020, Abebe *et al.* 2021b]. In contrast, we find persistent impacts of vocational training and job assistance.

Finally, we note that our results are not driven by gender: the impacts on the employment index are not statistically different between men and women for any treatment arm.

5.2 Earnings, Bargaining and Spells

Earnings are another key margin to consider. Column 1 of Table 10 shows that for those offered vocational training, total earnings rise by 26% over the long run average for controls. Columns 2 and 3 show the bulk of this rise comes from earnings from regular jobs (in line with the employment impacts in Table 9). Examining next earnings impacts for workers offered vocational training and job assistance, we see that: (i) total and regular earnings rise significantly over controls; (ii) the point estimates on both are smaller than for workers offered only vocational training, but these differences are not precisely measured and so not statistically significant.

At first sight it is slightly puzzling how, among those offered vocational training, the additional offer of job assistance has more pronounced impacts on employment outcomes (Table 9) than on earnings, despite the documented differences in search behavior between these two groups of youth. This is partly because earnings are noisily measured, but to probe the issue further we also consider the extent to which workers engage in *ex post* bargaining with firms they received job offers from. We consider bargaining over (i) wages; (ii) hours; (iii) location; (iv) additional benefits. We combine these into a bargaining index, and Column 4 of Table 10 shows treatment effects on this bargaining index. Only workers in one treatment arm are impacted: those offered both vocational training and job assistance, and they are significantly more likely to engage in *ex post* bargaining than those offered only vocational training ($p = .001$). Table A12 shows ITT effects on each component of this bargaining index and we see that these workers bargain over locations and additional benefits.[30]

Why would only those offered vocational training and job assistance many years earlier bargain

[30] We also see that 70% of workers in the control group report bargaining over wages (and this is not different among any group of treated workers). Hence the overall pattern of results is quite different to that found in US or German data where more than two thirds of workers report not being in a position to bargain over wages, but take offers as given [Wright *et al.* 2021]. Hence the urban labor markets we study are not well described within a competitive search framework, where wages/employment contracts are posted in advance and not negotiated.

harder with potential employers? One intuition is that workers bargain as their non-employment outside option improves [Jaeger *et al.* 2020]. Our experiment allows us to rule this out because workers only offered vocational training do not behave in the same way when they meet potential employers. We can also rule out that such workers are differentially skilled to those only offered vocational training (Table 4).

Rather, our results offer the novel possibility that the search process itself might influence how hard workers bargain *ex post* with firms. In particular, the frequency of job offers from good firms might determine bargaining behavior. To establish the frequency of opportunities workers have to bargain with potential employers, Columns 5 and 6 in Table 10 show treatment effects on (un)employment spells. We see that: (i) those offered vocational training have significantly shorter unemployment spells and significantly longer employment spells than controls; (ii) these impacts on spells are about half the magnitude for vocational trainees with job assistance, so their unemployment spells are significantly longer than for those only offered vocational training ($p = .023$) and their employment spells are significantly shorter ($p = .015$).

In short, vocational trainees with job assistance meet good employers less often, as they make a slower transition up the job ladder towards regular work. When they do, they bargain harder, and this helps explain how they close the earnings gap to those only offered vocational training (but not the employment gap documented in Table 9).[31]

5.3 Sorting into Jobs, Firms and Self-Employment

Our final batch of outcomes consider how our interventions impact labor market sorting. The degree to which labor market interventions induce positive assortative matching is important for understanding fundamental sources of inequality and the wider role of firms in the economy [Card *et al.* 2013, 2016, 2018]. We examine this by focusing on the characteristics of jobs and firms that workers end up at in their last employment spell in each survey wave, and the extent to which they engage in self-employment.

We collected information on job and firm characteristics to allow a direct comparison to the ideal job and firm characteristics workers expressed directing their search towards (Table 8). As before, we construct overall indices of job and firm quality, where higher indices correspond to jobs higher up the ladder and more productive firms. The results are in Table 11.

The first row shows that those offered vocational training end up in significantly higher quality jobs than controls – the job index rises by $.096\sigma$. The treatment effects on each component of the index are shown in Table A13: those offered vocational training end up in jobs that enable them to supervise others, have high status, and learn new job-specific skills.

In sharp contrast, we see for youth offered both vocational training and job assistance up to five years earlier, they end up in jobs not significantly different to those for controls. Their job index

[31] Employment spells are based on regular jobs as casual jobs are nearly always very temporary by nature.

rises by $.042\sigma$ but we cannot reject the null. Table A13 reveals their jobs are better than controls on some dimensions: providing new skills and allowing work with others, but these individuals do not move up the firm hierarchy in that they are not more likely to be supervising others.

Hence there is positive assortative matching between workers and jobs: those offered vocational training and so more highly skilled end up higher up the job ladder, but this progression is slower for those also offered vocational training but whose search strategies were altered because of the information generated by the job assistance intervention.[32]

The last row of Table 11 shows that workers with job assistance only end up in jobs with characteristics that are no different to controls.

Repeating the analysis for characteristics of firms that workers end up employed at Column 2 shows that: (i) among those offered both vocational training and job assistance, realized firm quality is significantly lower than those that were only offered vocational training ($p = .035$); (ii) indeed, vocational trainees with job assistance end up at firms of lower quality than controls; (iii) those only offered job assistance also end up in firms of lower quality than controls.

The treatment effects on each component of the index in Table A14 reveal that firm quality is lower for those offered vocational training and job assistance because they are significantly more likely to end up in informal firms and firms less likely to provide other benefits to workers. Realized firm quality is lower for workers with job assistance because they are more likely to end up employed in informal firms.

These results represent novel experimental findings on sorting patterns between workers, jobs and firms, and how these are shaped by labor market interventions in a low-income setting.

Our final result considers the extent to which workers move up the job ladder via self-employment in our study sectors. Column 3 of Table 11 shows that workers in all treatment arms are more likely than controls to engage in self-employment in our study sectors. As we saw earlier, the fact that long run non-employment rates even for skilled workers remain around 30% highlights that labor markets do not clear even for them [Banerjee and Sequeira 2021]. Hence the movement into self-employment even by those offered training might represent push factors arising from a lack of labor demand rather than workers preferring self-employment over other jobs. Indeed, we find no short run treatment effect on those offered vocational training on their stated desire to move into self-employment.[33]

For workers only offered job assistance, the magnitude of the impact on self-employment (4pp) corresponds to a near 66% increase over controls. This aligns perfectly with the stated intent of these workers, where we documented the only impact of job assistance on their search strategy was to start borrowing to start up in self-employment.

[32] Our results complement earlier findings from field experiments in low-income settings that job assistance raises job quality, although most of these have done so on narrower dimensions of job quality and over a shorter horizon [Beam, 2016, Franklin 2018].

[33] Blattman and Dercon [2018] present evidence on worker preferences over firm types using a field experiment. They find when barriers to self-employment are relaxed, workers prefer entrepreneurial to industrial labor.

5.4 Mediation Analysis

The six-year study period allows us to map out how labor market interventions translate into long run labor market outcomes via experimentally induced changes in job search strategies. We use mediation analysis to link our two sets of core results. Following Gelbach [2016], the basic intuition is that the treatment effect of intervention T on labor market outcome Y can be decomposed as operating through a set of K mediators each denoted m_k:

$$\frac{dY}{dT} = \sum_{k=1}^{K} \frac{\partial Y}{\partial m_k} \frac{\partial m_k}{\partial T} + R, \tag{3}$$

where R is the part of the treatment effect which cannot be attributed to any mediator. The method is invariant to the order in which mediators are considered, but does not represent causal mediation except under strong assumptions. However, because the same mediator is examined across multiple treatment arms and always in comparison to controls, the results can still be informative of the relative importance of different mediators.

The outcome we focus on is a holistic index of labor market success combining: (i) all components of the employment index; (ii) total earnings; (iii) the length of the last employment spell; (iv) all components of the indices of realized jobs and realized firms. The ITT treatment effects on this index are in Column 4 of Table 11. We see that on this broad measure of long run labor market success, there is a significant increase of $.115\sigma$ for vocational trainees. This increase is significantly larger than for those additionally offered job assistance ($p = .001$), for whom the index rises by less than half the amount ($.051\sigma$). In short, the impacts of job assistance on those offered vocational training are to undo half of what is achieved through vocational training alone.

Finally, on this holistic index of labor market success we find that in line with earlier studies, the overall long run impact of light touch job assistance is not significantly different to controls.

To see how specific dimensions of search strategy contribute to these impacts, we consider the following skill and search related mediators: the measured sector-specific skills of individuals, the reservation wage as measured by the minimum expected earnings, beliefs as captured by the expected probability of finding a job in their preferred good sector in the next year, search intensity as proxied by whether they have actively searched for a job in the last year, the ideal job index, the ideal firm index, and whether the individual is borrowing.

The result is in Figure 6. The x-axis shows the ITT estimate on the labor outcomes index for each treatment arm. The solid black bar shows the same ITT effect as reported in Column 4 of Table 11. Within each bar we show the contribution to this overall impact of each mediator, indicating the percentage of the overall ITT impact explained by the most prominent mediators.

Among workers offered vocational training, sector-specific skills are the most important mediator driving outcomes: 20% of the long run impact on labor market outcomes is directly mediated through skills. Among search behaviors, the most prominent mediator is the proxy for the reservation wage – the minimum expected earnings from employment in a study sector (10%). The second most important search behavior is the belief over the job offer arrival rate, explaining 8% of the ITT effect, followed by search intensity. For these youth, mediators related to directed search or credit markets play relatively little role in determining long run outcomes.

Among workers additionally offered job assistance, sector-specific skills and reservation wages both play important roles in mediating long run outcomes, explaining 41% and 17% of the overall labor outcomes index respectively. However, given the overall ITT to be explained is half the size ($.115\sigma$ vs. $.051\sigma$), the overall mediating importance of skills is the same for those offered vocational training, with or without job assistance. This is easily seen on Figure 6 by comparing across the ITT bars for these two groups of youth, and is as expected given the accumulation of sector-specific skills does not differ between these groups (Table 4). The overall pattern that emerges is that search-related mediators play less of a role in determining the long run labor market success of those offered both vocational training and job assistance – the reason being that these workers are discouraged in a variety of dimensions of their search behavior, and so end up with strategies closer to controls overall.

For workers only offered job assistance, no single mediator is prominent, although borrowing has a positive effect.[34]

Taken together these results provide novel evidence on how search strategies mediate the impacts of labor market interventions related to the provision of skills and/or the offer of job assistance on long run labor market outcomes. By providing such granular evidence, we help fill an important gap in the literature evaluating active labor market policies over the long run, that typically uses administrative data and so lacks such detailed information on the role that multiple dimensions of search behaviors play.

6 External Validity

Our field experiment has many elements and so it is useful to consider the external validity of each aspect: (i) the scalability of the interventions and alternative kinds of information that could be provided to workers; (ii) firms that workers were matched to; (iii) targeted workers.

[34] A large share of the impact on the labor outcomes index remains unexplained (R). This suggests either (i) in line with most models of job search, there are important interactions between the mediators, that the decomposition in (3) does not allow for; (ii) there are important unmeasured mediators. On (ii), an additional mediator to consider would be quality of the initial job/firm that individuals experience. The earlier results in Table A6 showed short run treatment effects on labor market outcomes (as measured at first follow-up). Most notably the quality of realized firms in the short run is no different to controls for any treatment arm (Column 5). This reinforces the notion that in our study, long run differences in labor market outcomes are driven by differences in job search strategies induced across workers, not the inherent quality of first jobs/firms experienced.

6.1 Scalability of Interventions and Alternatives

The vocational training offered is provided by pre-existing vocational training institutes throughout Uganda. They normally offer six-month sector specific training courses in our eight study sectors. This treatment thus represents a scalable market-based intervention. Clearly, our treatment offer of near fully subsidized vocational training relaxes credit constraints that would normally prevent young job seekers making such human capital investments. Our results show such constraints are a first order source of inefficiency in the urban labour markets studied, driving variation across workers in skills acquisition, job search strategies and long run labor market outcomes.

Our job assistance offer is relatively light-touch and thus potentially scalable. As there are no market substitutes for such offers, they relax information frictions preventing some worker-firm matches occurring. They might also be viewed by job seekers as providing a highly salient and unique opportunity to find meaningful employment because they: (i) allow them to bypass usual channels of job search (informal contacts or walk-ins) and get to the front of job queues; (ii) ensure potential employers are provided the CV of workers they are matched to, enabling the credentials of the worker to be evaluated.[35] Although unusual, these present opportunities that workers would like and have considered.

Natural alternatives to the kind of job assistance we have studied are to provide information about the state of labor demand, about the job prospects of the average young job seeker, or tailored to the specific circumstances of the individual [Altmann *et al.* 2018, Belot *et al.* 2019].[36] Such purely informational approaches link back to a long-standing discussion on what exactly individuals learn about during job search – aggregate demand conditions, as captured by learning the wage offer distribution [Wright 1986, Burdett and Vishwanath 1988] – or returns to their own abilities [Falk *et al.* 2006, Gonzalez and Shi 2010].

The general issue we highlight is that individuals might misunderstand or misattribute information provided to them. This lesson could apply to a broader class of information treatments than those we have considered, and links back to a long-standing emphasis on the need to consider the framing of job assistance offers, because what is perceived by workers matters as much as what is actually presented to them [Babcock *et al.* 2012].

[35] In addition, framing might matter: the match offer is organized by the reputable NGO BRAC. This force goes against the usual reason given for a lack of firm demand from match offers being because of stigma effects, where firms perceive workers with job search assistance being of low quality [Bell *et al.* 1999].

[36] Altmann *et al.* [2018] evaluate a light touch intervention providing unemployed German job seekers information about the job search process and the consequences of unemployment. Tracking workers for a year, they find positive impacts of the intervention on employment and earnings of those with the highest predicted risk of unemployment, while there is no impact for workers with low predicted risk of unemployment. Belot *et al.* [2019] evaluate the impact of providing job seekers in Scotland with tailored job search advice through a web-based tool that makes relevant suggestions to job seekers about occupations relevant for their profile. They find that the job-search tool broadens the job search activities of job-seekers (i.e. search across a wider range of occupations), and find that job interviews increase as a result, and this is driven by job seekers who initially search more narrowly.

6.2 Workers

Individuals in our evaluation are the kind of disadvantaged youth that many job training programs target [Attanasio *et al.* 2011, Card *et al.* 2011]. Given that in most developing countries youth unemployment rates are high and there are large cohorts of young job seekers entering the labor market each year, understanding the search behavior of these individuals, how interventions impact such behavior, and how this translates into labour market outcomes is important across contexts.

It is natural to consider if our results could apply in contexts where the same interventions were targeted to other job seekers. To shed light on this dimension of external validity, we consider heterogeneous treatment responses with regards to two individual characteristics: cognitive ability and psychological traits. We consider cognitive ability because search models represent an optimal stopping problem, so cognitive ability might determine how well worker behavior lines up with theoretical predictions. We measure cognitive ability using the worker score from a short 10-question version of Raven's progressive Matrices test, measured at first follow-up.

On psychological traits, behavioral models have emphasized the role that such time-invariant traits have for job search [DellaVigna and Paserman 2005, Falk *et al.* 2006, Caliendo *et al.* 2015, DellaVigna *et al.* 2017, 2020].[37] Three widely studied traits are self-esteem, locus of control, and neuroticism. Judge *et al.* [2002, 2003] argue they correlate to the same underlying construct, termed self-evaluation. This is a fundamental appraisal of one's worthiness, effectiveness, and capability. An individual with high self-evaluation is well adjusted, positive, self-confident, and believes in her own agency. Such individuals are more able to self-regulate and direct behavior towards goals such as job seeking.[38,39]

We classify individuals as high/low ability if their cognitive test score is above/below the median, and similarly divide individuals into high/low self-evaluation types. As shown earlier, cognitive ability and self-evaluation are not impacted by the treatments (Table A7). We thus take both as time invariant. They are also uncorrelated ($\rho = .06$ for the continuous measures).

[37] For example, patience [DellaVigna and Paserman 2005], self-confidence [Falk *et al.* 2006], internal locus of control [Caliendo *et al.* 2015], and reference dependence [DellaVigna *et al.* 2017, 2020] have all been documented to play an important role for search behavior, particularly for explaining non-monotonic search intensities around the point of benefit exhaustion in high-income settings.

[38] The extent to which an individual believes that her actions lead to the desired consequences is a person's locus of control (LOC). People who do not believe their own effort affects the probability of success (i.e. those with an external LOC) are unlikely to adopt new strategies to help them increase own effort. In contrast, those who believe their own effort is crucial for success (i.e., those with an internal LOC) are likely to learn new strategies to help them self-regulate their behavior and emotions to improve goal-directed effort. Self-esteem is the overall value that one places on oneself as a person. Neuroticism is the tendency to have a negativistic cognitive/explanatory style and to focus on negative aspects of the self. LOC has been found to matter directly for labor market outcomes: people with an internal LOC tend to achieve higher wages [Cebi 2007] and search for jobs more intensively because they believe investments in job search have higher payoffs [Caliendo *et al.* 2015]. Self-evaluation has also been shown to be a predictor of job satisfaction and job performance [Judge *et al.* 2003].

[39] The self-evaluation index is constructed in two steps: (i) among all the items measuring the three personality traits, we select the ones that correlate positively and strongly; (ii) we use principal component analysis to aggregate the items and construct a single index of the underlying trait. Neuroticism is measured at first follow-up, self-esteem and locus of control are measured at third follow-up.

Cognitive Ability Panel A of Figure 7 shows treatment effects on the labour outcomes index for high and low cognitive ability individuals. We see that within each treatment arm, the ITT impact on the long run labor outcome index is not different between those with high and low cognitive ability. Hence even within treatment arms involving job assistance, we find no evidence that low ability workers respond less than high ability workers.

This has two implications. First, our results have external validity to other contexts where the composition of targeted youth by ability differs. Second, the results reconfirm the notion that workers understood the nature of the job assistance intervention – otherwise we might have found those with low ability to have significantly different outcomes.

Self-evaluation Panel B shows the analysis split between workers of high and low self-evaluation. A similar pattern of results emerge: individual self-evaluation does not interact with long run outcomes for any treatment arm. This again suggests our results might extend to other samples of job seeker irrespective of this psychological trait.

6.3 Firms

A lack of labor demand is a key constraint in experiments involving matching workers to firms. In our context, low call back rates are driven by a lack of vacancies in firms (almost by construction, our design eliminates the possibility that worker characteristics determine call backs). The constraint is logistical in that in the period between when the firm sample is drawn, to when match offers made, there can be changes in demand conditions so that even if firms report hiring constraints as binding at baseline, this might no longer be the case by the time job assistance is actually implemented. An alternative approach to raise call back rates in light-touch job assistance would be to provide more information to firms. A class of papers have engineered matches between firms and job-seekers combined with the revelation of information to firms on workers' ability or skills [Pallais 2014, Groh *et al.* 2016, Carranza *et al.* 2020, Bassi and Nansamba 2021]. These find that matching with information positively impacts employment outcomes, with impacts varying across the skills distribution.

7 Policy Implications

Active labor market programs typically fall into two categories: those designed to raise worker productivity (say through skills provision or wage subsidies) and those designed to improve the worker-firm matching process (say through the kind of job assistance we have studied). As the second category of programs are relatively light touch, they can have substantially higher returns if designed and targeted optimally. McKenzie [2017] for example suggests the costs of job assistance are 1-2% of the cost of vocational training interventions.

Our study has three broad implications for the design and targeting of job assistance.

First, in line with research from other settings, we have documented how labor market entrants have biased beliefs [Spinnewijn 2015, Abebe *et al.* 2021a, Banerjee and Sequeira 2021, Mueller *et al.* 2021, Potter 2021]. A natural question is should policy makers design interventions to debias workers? Our results suggest a subtle answer, that depends on the skills of workers.

Among those offered vocational training and hence more skilled on average, there are returns to them searching while exuberant: they employ different search strategies than equally skilled workers that were also provided job assistance and discouraged as a result. In the long run, those offered vocational training without job assistance progress further up the job ladder than those also provided job assistance. Among those randomized out of vocational training – unskilled workers – the opposite is true: job assistance that credibly confirms their poor prospects unless they change behavior, causes them to adopt new strategies (borrowing for self-employment), and this enables them to do better on some labor market outcomes – especially those related to the extensive margin – than controls in the long run.

Second, and following from the last result, low skill workers are able to access credit markets to finance self-employment. Providing them credible confirmation of their poor prospects might then be more effective that providing them access to microcredit. This obviously relates to an emerging view that microcredit is itself not transformational in driving occupational choice [Banerjee *et al.* 2015], and that small resource transfers to finance job search might not impact outcomes [Abebe *et al.* 2021, Banerjee and Sequeira 2021].

Finally, our findings relate to wider policy discussions about how best to incentivize providers of vocational training. The default position for VTIs in most countries is they have no incentive to match workers to firms. However, it is often debated that government should provide performance-related pay to VTIs, incentivizing them to train *and* find workers employment. Our results suggest that incentive provision might not be enough: trying to match workers to firms is hard and requires additional information to be gained on both demand and supply conditions. This complements emerging findings that VTIs face severe information frictions even when trying to find their graduates employment [Banerjee and Chiplunkhar 2018].[40]

8 Conclusion

Of the 420 million young people in Africa today, more than 140 million are unemployed and another 130 million are underemployed and/or in working poverty [AfDB 2018]. More than 12 million young people enter the labor market each year seeking formal employment, with youth

[40] Banerjee and Chiplunkhar [2018] provide evidence that placement officers in vocational training institutes have very little information about the job preferences of graduating workers. They present results of a field experiment that provides them such information and find that placement officers come closer to efficiently matching candidates to job interviews. This leads to substantial improvement in job choices made by the candidates and subsequent employment outcomes for three to six months after initial placement.

unemployment rates in most African countries being higher today than in 2015. As a result, many countries throughout Sub Saharan Africa face the challenge of helping large cohorts of labor market entrants find good jobs. This is key not just from the microeconomic viewpoint of individual welfare, but also has macroeconomic consequences as the efficient matching of workers to firms determines labor productivity, the firm size distribution, the nature of macroeconomic cycles, and aggregate growth.

Our study speaks directly to this pressing policy challenge: we have presented results from a long term field experiment to shed light on how interventions related to the offer of training and job assistance shape search behavior and long run labor market success. In doing so, we add new evidence to a nascent literature studying labor market dynamics in low-income settings [Bick *et al.* 2018, Feng *et al.* 2020]. Our analysis provides a menu of key ingredients that need to be incorporated into job search models appropriate for such economies [Donovan *et al.* 2020, Rud and Trapeznikova 2021]. Indeed a natural next step is to take these results to develop and estimate a model of job search incorporating responses along multiple margins of search behavior to the interventions, including reservation wages, expectations, search intensity, search channels, job and firm sorting. This would push forward the current frontier of such structural models, where important recent contributions have considered the evolution of expectations with job search [Conlon *et al.* 2018, Mueller *et al.* 2021, Potter 2021]. Doing so will be critical to advancing our understanding of what are likely to be the most effective labor market policies to help cohorts of young workers find good jobs, and thus drive forward economic development.

A Appendix

A.1 Implementation of the Job Assistance Intervention

The job assistance treatments were implemented by job placement officers (JPOs) hired by BRAC specifically for our research project. They proceeded in four steps.

The JPO first contacted workers using the following script: *I am calling to inform you that you have been selected to receive assistance from BRAC in finding a job. I will be providing your name and some basic information about you to a number of firms in the area to see if they would be willing to hire you. If they are interested, I will let you know and put you in touch with the interested firms.*

If the worker agreed for their details to be forwarded, the JPO then contacted the relevant firms with a brief script that included, *As part of this programme I would like to introduce you to some workers who are interested in working as <trade>.*

The JPO would then show the firm owner the worker's information packet, explaining the information provided to them. JPOs were instructed not just to hand over the worker information packets. JPOs then recontacted firms with the script, *Are any of these workers people you would* *be willing to hire? ...please note that BRAC will not provide any financial assistance to you if you hire any of these workers. IF YES Great. I would like to arrange a meeting between the two of you sometime later this week. Before I call them, however, I want to make clear that you have no obligation to hire this worker. I am only the facilitator and cannot help you make the decision. Also, I want to make it clear that BRAC will not be able to provide any assistance to you if you hire the worker....After I have arranged the meeting, the decision on whether to hire this worker is yours. I will no longer be involved in the process and will only check in with you to ensure that the worker showed up for the meeting.*

If the firm agreed to meet a worker, the third step would be for the JPO to quickly arrange the meeting (within two weeks). Workers were reimbursed for travel expenses and provided lunch (not accommodation). It was also made clear to the worker that they would not be receiving additional financial assistance from BRAC (e.g. if offered a job, the worker would be responsible for travel expenses going forward). JPOs reiterated that BRACs only role is to facilitate the initial meeting.

As a fourth and final step, the JPO would have periodic follow-ups with the worker and firm.

A.2 Research Ethics

Following Asiedu *et al.* [2021] we discuss research ethics. On policy equipoise, both vocational training and job assistance are common in the policy space across developing countries including Uganda. There was a reasonable expectation that vocational training might produce larger net benefits than job assistance. Given scarce financial resources, it was not possible to offer vocational training to all original applicants. *Ex ante* there was no consensus on which workers would have benefitted more from these interventions, so that no participant had a greater claim to these scarce resources. Therefore, a scarcity argument justified randomization and the oversubscription design.

All interventions were implemented by BRAC. The researchers had no active role in the design and implementation of the vocational training intervention, which had already been offered by VTIs and BRAC for some time using similar modalities with previous cohorts of young workers. As BRAC training programs are typically oversubscribed, to implement this evaluation the researchers partnered with BRAC to randomly select applicants to be offered the intervention. The researchers played a more active role in the design of the job assistance component of the program. BRAC had been matching workers to firms for apprenticeship programs for some time prior to this study. The job assistance program evaluated in this paper deviates from the regular BRAC apprenticeship program in that: (i) firms did not receive a subsidy (neither monetary nor in-kind) to hire and train the matched workers; (ii) workers and firms were matched randomly.

Due care was taken by BRAC staff during the informed consent process to clarify the nature of the intervention to workers and firms. It was made clear to both parties that no financial or in-kind support would be provided to either the worker or the firm. Informed consent was obtained for all study participants prior to the study. The informed consent forms also described the research teams and met IRB requirements of explaining the purpose of the study, participant risks and rights, confidentiality, and contact information. Accessing the interventions and participation in surveys was voluntary for study subjects.

The interventions being studied did not pose particular risks or potential harms to participants. The study participants were potentially vulnerable as BRAC targeted disadvantaged youth. To address the vulnerability and low levels of literacy of study participants, particular care was taken in: (i) presenting informed consent material in the language of the respondent and using simple terms; (ii) training field staff and ensuring adherence to best practices during their interactions with study participants through intensive monitoring; (iii) ensuring that topics covered in the surveys were sensitive to the local cultural and social context of participants. Enumerator teams were recruited from the same geographical areas of participants to facilitate communication and understanding of the context. Participants' capacity to access future services was not reduced by participation in this study. Our data collection and data management procedures adhered to protocols around privacy and confidentiality. Participants were compensated for their time answering surveys with credit for mobile phone talk-time.

Research staff and enumerator teams were not subject to additional risks in the data collection process. None of the researchers have financial or reputational conflicts of interest with regards to the research results. No contractual restrictions were imposed on the researchers limiting their ability to report the study findings.

Summary findings from the study have been presented and discussed in multiple meetings with relevant policymakers and other stakeholders in Uganda. However, no activity for sharing results to individual participants is planned due to resource constraints. We do not foresee risks of the misuse of research findings.

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Table 1: Baseline Balance on Labor Market Histories

Means, robust standard errors from OLS regressions in parentheses
P-value on t-test of equality of means with control group in brackets
P-value on F-tests in braces

	Any work in the last month	Any regular wage employment in the last month	Any self employment in the last month	Any casual work in the last month	Total regular earnings in last month [USD]	Total regular earnings in last month [USD] | regular employment	F-test of joint significance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Control	.401	.120	.038	.296	5.11	13.0	
N=451	(.052)	(.026)	(.017)	(.051)	(1.29)	(2.41)	
Vocational Training	.389	.149	.034	.253	7.29*	19.1**	{.798}
N=390	(.032)	(.023)	(.013)	(.029)	(1.26)	(2.80)	
	[.985]	[.185]	[.761]	[.263]	[.062]	[.039]	
Vocational Training + Job Assistance	.360	.149	.050	.205*	5.25	15.1	{.772}
	(.034)	(.026)	(.015)	(.030)	(1.20)	(3.01)	
N=307	[.694]	[.228]	[.255]	[.065]	[.808]	[.945]	
Job Assistance	.367	.127	.057	.251	5.56	15.2	{.995}
N=283	(.034)	(.025)	(.016)	(.031)	(1.25)	(2.86)	
	[.373]	[.815]	[.211]	[.204]	[.728]	[.883]	

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. All data is from the baseline worker survey. Columns 1 to 6 report the mean of each worker characteristic, where standard errors are derived from an OLS regression of the characteristic of interest on dummy variables for the treatment groups. All regressions include strata dummies and a dummy for the implementation round. The comparison group in these regressions are Control workers. Robust standard errors are reported throughout. Column 7 reports the p-value from F-Tests of joint significance of all regressors from an OLS regression where the dependent variable is a dummy taking value 0 if the worker is assigned to the Control group, and 1 for workers assigned to the corresponding treatment group and the independent variables are the variables in Columns 1 to 5 (variable in Column 6 is dropped as it is missing for individuals who were not involved in any work activity in the month prior the survey). Robust standard errors are also calculated in these regressions. In Column 4 casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing compounds. Casual work also include any type of agricultural labor such as farming, animal rearing, fishing and agricultural day labor. In Column 5 workers who report doing no work in the month prior the survey (or only doing casual or unpaid work) have a value of zero for total earnings. The top 1% of earnings values are excluded. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 2: Jobs, Search and Recruitment

	Casual Jobs	Regular Jobs
A. Job Characteristics		
Worked in this activity in the last month	.257	.177
Self-employed	.663	.216
Number of months involved in activity in the last year	3.54	3.55
Hours worked in a typical day | employed	5.09	8.25
Days worked in a typical week | employed	5.14	5.50
Earnings in the last month | employed	10.5	24.7
B. Worker Job Search Methods		
Through friends/family member	.197	.463
Direct walk-in	.063	.251
Immediate family owns the business	.165	.063
Read job ad	.010	.017
C. Firm Recruitment Strategies		
Direct walk-in		.410
Through friends/family member		.407
Worker is a family member		.127
Posted job ad		.013
D. Screening		
Had to interview	.020	.178
Had to provide references	.032	.178
Had to take a skills test	.052	.259

Notes: The data used is from the baseline and the first follow-up surveys of workers (Panels A and B) and the baseline survey of firms (Panels C and D). The sample only includes workers and firms in the Control groups. In Panel A, the sample includes all workers for the following outcomes: involved in this activity in the last month, self-employed, and number of months involved in the activity in the last year. The remaining outcomes in Panel A are conditional on the worker being involved in a casual or regular work. For casual work, the list of activities indicated is exhaustive. Regular jobs include all other jobs that are not in the list of casual jobs, so the list is not exhaustive. Panel B shows the share of workers who have used the corresponding method to look for work in the year prior to the survey. Panels C and D show the share of employees hired through the corresponding method. The top 1% of earnings values are excluded. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 3: Evolution of Expectations

Means, standard deviations in parentheses

	Row		Expected Monthly Earnings [USD] Minimum	Maximum	Mean	Coefficient of Variation	Exp. prob of finding a job in the next year (0 to 10 scale)
			(1)	(2)	(3)	(4)	(5)
At Baseline	**R1**	**Assigned to Vocational Training (T1, T2)**	40.0	71.5	56.3	.107	5.59
			(35.0)	(58.6)	(44.8)	(.057)	(2.83)
	R2	**Not Assigned to Vocational Training (C, T3)**	42.1	74.6	58.6	.108	5.71
			(36.7)	(62.1)	(47.6)	(.060)	(2.90)
On Eve of Announcement of Job Assistance	**R3**	**Assigned to Vocational Training (T1, T2)**	57.3	101	79.3	.112	5.97
			(40.6)	(66.3)	(52.9)	(.057)	(2.66)
	R4	**Not Assigned to Vocational Training (C)**	42.9	72.5	57.8	.107	4.19
			(34.8)	(57.0)	(45.9)	(.058)	(2.72)
	p-value on tests of equality across rows: R1 = R2		*[.315]*	*[.368]*	*[.422]*	*[.681]*	*[.441]*
	R1 = R3		*[.000]*	*[.000]*	*[.000]*	*[.111]*	*[.015]*
	R2 = R4		*[.696]*	*[.568]*	*[.469]*	*[.507]*	*[.000]*
	R3 = R4		*[.000]*	*[.000]*	*[.000]*	*[.184]*	*[.000]*

Notes: The data used is from baseline, VTI surveys conducted towards the end of the training period while trainees were still enrolled at the vocational training institutes, and we extrapolate back from the first worker follow-up survey assuming a linear evolution of beliefs, what would have been beliefs among Controls at the same time as the VTI survey was being fielded. In Columns 3 and 4 we assume a triangular distribution to calculate the average and the coefficent of variation of expected monthly earnings. At the foot of each column we report p-values on the tests of equality of means: (i) between individuals assigned and not assigned to Vocational Training at baseline; (ii) between individuals assigned to Vocational Training at baseline and on the eve of job assistance announcement; (iii) between individuals not assigned to Vocational Training at baseline and on the eve of job assistance announcement; (iv) between individuals assigned and not assigned to Vocational Training at the eve of job assistance announcement.

Table 4: Sector Specific Skills

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Any relevant skills	Test score (ITT)	Test score (2SLS)
	(1)	(2)	(3)
Vocational Training	.256***	6.42***	8.29***
	(.023)	(1.21)	(1.60)
	{.000, .001}	*{.000, .001}*	-
Vocational Training + Job Assistance	.252***	7.44***	10.8***
	(.025)	(1.43)	(2.19)
	{.000, .001}	*{.000, .001}*	-
Job Assistance	.014	1.14	.803
	(.029)	(1.41)	(2.01)
	{.643, .610}	*{.428, 417}*	-
P-value: VT = VT + Job Assistance	*[.852]*	*[.488]*	*[.261]*
Mean in Control Group	.613	30.1	30.1
N. of observations	2,134	2,134	2,134

Notes: ****denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline, second and third worker follow-up surveys. All regressions include strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 we report a linear probability model on whether the respondent reports having any sector specific skills or not. In Columns 2 and 3 the dependent variable is the skills test score, from the test administered to workers in the second and third worker follow-ups. Column 2 reports OLS estimates, while in Column 3 we report 2SLS regressions, where we instrument treatment take-up with the original treatment assignment. In Column 3 standard errors are bootstrapped with 1000 replications. Take-up in is defined as the worker having completed the 6-months Vocational Training for the Vocational Training + Match Offer treatments, and as being called back in the Match Offer treatment. Workers that reported not having any sector specific skills are assigned a test score equal to what they would have got had they answered the test at random. Workers that refused to take the skills test are excluded from the regressions in Columns 2 and 3. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 5: Expectations

OLS regression coefficients, robust standard errors in parentheses

Randomization inference and Romano-Wolf adjusted p-values in braces

	Expected Monthly Earnings [USD]				Exp. prob of finding a job in the next year (0 to 10 scale)	Market beliefs index
	Minimum	Maximum	Mean	Coefficient of Variation		
	(1)	(2)	(3)	(4)	(5)	(6)
Vocational Training	17.7***	31.8***	25.4***	-.002	1.84***	-.048
	(3.06)	(4.85)	(4.37)	(.005)	(.205)	(.046)
	{.000, .001}	*{.000, .001}*	*{.000, .001}*	*{.661, 881}*	*{.000, .001}*	*{.305, .603}*
Vocational Training + Job Assistance	12.0***	23.6***	17.9***	.009	1.45***	-.054
	(3.28)	(5.37)	(4.67)	(.006)	(.217)	(.052)
	{.000, .002}	*{.000, .001}*	*{.000, .001}*	*{.108, .282}*	*{.000, .001}*	*{.301, .603}*
Job Assistance	3.21	6.04	3.47	-.000	.242	-.039
	(3.05)	(4.97)	(4.44)	(.007)	(.216)	(.053)
	{.327, .297}	*{.222, .236}*	*{.414, .449}*	*{.995, .986}*	*{.261, .286}*	*{.441, .603}*
P-value: VT = VT + Job Assistance	*[.095]*	*[.129]*	*[.105]*	*[.036]*	*[.082]*	*[.907]*
Mean in Control Group	42.9	72.5	57.8	.107	4.19	.028
N. of observations	952	946	801	797	1,171	1,231

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline, as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. Minimum, Maximum, Mean and coefficient of variation of Expected monthly earnings in Columns 1 to 4 refer to the workers' expected earnings in their preferred sector among the eight study sectors. In Columns 3 and 4 we assume a triangular distribution to calculate average and coefficient of variation of expected monthly earnings. Individuals who report a probability of finding a job in the next 12 months equal to zero are excluded from the sample in Columns 1 to 4. In Column 6 the outcome is an index of worker's labor market beliefs, constructed following Anderson's [2008] approach. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 6: Expectations by Call-back

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Expected Monthly Earnings [USD]				Exp. prob of finding a job in the next year (0 to 10 scale)
	Minimum	Maximum	Mean	Coefficient of Variation	
	(1)	(2)	(3)	(4)	(5)
Vocational training	17.7***	31.9***	25.4***	-.002	1.85***
	(3.07)	(4.86)	(4.38)	(.005)	(.206)
	{.000, .001}	*{.000, .001}*	*{.000, .001}*	*{.667, .927}*	*{.000, .001}*
Vocational training with job assistance	11.7***	20.8***	15.9***	.006	1.36***
	(3.47)	(5.67)	(4.91)	(.006)	(.228)
	{.000, .007}	*{.000, .003}*	*{.003,.012}*	*{.228, .719}*	*{.000, .001}*
Vocational training with job assistance x Called back	2.17	17.3*	11.6	.018	.706*
	(5.94)	(10.2)	(8.65)	(.014)	(.421)
	{.735, .718}	*{.111, .255}*	*{.215, .467}*	*{.204, .622}*	*{.127, .264}*
Job assistance	4.07	7.36	3.71	-.002	.137
	(3.21)	(5.31)	(4.74)	(.007)	(.228)
	{.201, .501}	*{.164, .317}*	*{.431, .695}*	*{.811, .927}*	*{.566, .561}*
Job assistance x Called back	-4.99	-7.55	-1.21	.009	.608
	(6.51)	(9.58)	(8.42)	(.017)	(.454)
	{.440, .687}	*{.450, .441}*	*{.883, .869}*	*{.616, .927}*	*{.206, .325}*
P-value: VT = VT + Job Assistance + call back	*[.503]*	*[.529]*	*[.807]*	*[.085]*	*[.596]*
P-value: VT = VT + Job Assistance + not called back	*[.093]*	*[.054]*	*[.050]*	*[.981]*	*[.039]*
Mean in Control Group	42.9	72.5	57.8	.107	4.19
N. of observations	952	946	801	797	1,171

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline, as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. Minimum, Maximum and the average, variance and coefficient of variation of Expected monthly earnings in Columns 1 to 5 refer to the workers' expected earnings in their preferred sector among the eight study sectors. In Columns 3, 4 and 5 we assume a triangular distribution to calculate average, variance and coefficient of variation of expected monthly earnings. Individuals who report a probability of finding a job in the next 12 months equal to zero are excluded from the sample in Columns 1 to 5. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 7: Search Effort and Channels

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Has actively looked for a job in the last year	Number of days has actively looked for a job in the last year	Has attempted to migrate to find a job	Main channel through which looked for a job is through family members/friends	Main channel through which looked for a job is by walking into firms and asking for a job	Search Index
	(1)	(2)	(3)	(4)	(5)	(6)
Vocational Training	.175***	.617	.084**	.053	.088***	.089**
	(.036)	(6.04)	(.033)	(.033)	(.028)	(.042)
	{.000, .001}	*{.921, .989}*	*{.012, .026}*	*{.112, .277}*	*{.003, .010}*	*{.037, .104}*
Vocational Training + Job Assistance	.097**	-.713	.060*	-.005	.056*	.019
	(.040)	(6.70)	(.036)	(.036)	(.030)	(.046)
	{.021, .030}	*{.914, .989}*	*{.101, .167}*	*{.886, .989}*	*{.072, .121}*	*{.662, .888}*
Job Assistance	-.036	-11.2*	-.036	-.000	-.004	-.003
	(.041)	(6.44)	(.033)	(.036)	(.028)	(.041)
	{.385, .372}	*{.083, .212}*	*{.270, .251}*	*{.996, 1.00}*	*{.899, .889}*	*{.942, .940}*
P-value: VT = VT + Job Assistance	*[.053]*	*[.845]*	*[.523]*	*[.125]*	*[.338]*	*[.146]*
Mean in Control Group	.490	41.7	.217	.270	.139	-.032
N. of observations	1,231	1,211	1,231	1,231	1,231	1,231

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. The variables in Columns 2 to 5 are set equal to zero if the worker did not actively look for a job in the last year. Column 6 combines all margins of search intensity and channels from Columns 1 to 5 into a single index following Anderson's [2008] approach. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 8: Directed Search, Credit

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Directed Search		
	Ideal Job Index	**Ideal Firm Index**	**Credit Index**
	(1)	**(2)**	**(3)**
Vocational Training	-.054	.103***	.040
	(.040)	(.036)	(.049)
	{.169, .313}	*{.004, .013}*	*{.410, .651}*
Vocational Training + Job Assistance	-.022	.030	-.035
	(.041)	(.039)	(.043)
	{.605, .593}	*{.454, .480}*	*{.420, .651}*
Job Assistance	-.064	.042	.090*
	(.042)	(.039)	(.048)
	{.139, .303}	*{.311, .480}*	*{.066,.190}*
P-value: VT = VT + Job Assistance	*[.465]*	*[.102]*	*[.133]*
Mean in Control Group	.020	-.046	-.021
N. of observations	1,231	1,215	1,231

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 the Realized Job Index has the components in Columns 1 to 5 of Table A9. In Column 2 the Realized Firm Index has the components in Columns 1 to 5 of Table A10. In Column 3 the Credit Index has the components in Columns 1 to 4 of Table A11. All indexes are constructed following Anderson's [2008] approach. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 9: Long Run Employment Outcomes

OLS regression coefficients, robust standard errors in parentheses

Randomization inference and Romano-Wolf adjusted p-values in braces

	Has done any work in the last month	Has done any casual work in the last month	Has done any regular work in the last month	Number of months of regular work in the last year	Number of months worked in one of the eight good sectors in the last year	Employment Index
	(1)	(2)	(3)	(4)	(5)	(6)
Vocational Training	.094***	.000	.113***	1.33***	1.94***	.347***
	(.021)	(.015)	(.022)	(.232)	(.207)	(.040)
	{.000, .001}	*{.993, .992}*	*{.000, .001}*	*{.000, .001}*	*{.000, .001}*	*{.000, .001}*
Vocational Training + Job Assistance	.063***	.005	.066***	.690***	1.54***	.248***
	(.023)	(.017)	(.024)	(.257)	(.228)	(.044)
	{.011, .010}	*{.758, .983}*	*{.009, .013}*	*{.008, .013}*	*{.000, .001}*	*{.000, .001}*
Job Assistance	.051**	-.003	.054**	.510**	.556***	.117***
	(.022)	(.017)	(.023)	(.246)	(.203)	(.040)
	{.024, .019}	*{.826, .983}*	*{.018, .015}*	*{.037, .034}*	*{.004, .004}*	*{.003, .003}*
P-value: VT = VT + Job Assistance	*[.152]*	*[.765]*	*[.043]*	*[.011]*	*[.104]*	*[.031]*
Mean in Control Group	.623	.169	.524	5.91	1.88	-.167
N. of observations	3,703	3,699	3,700	3,724	3,723	3,725

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the second, third and fourth worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 the outcome is a dummy equal to 1 if the respondent has done any work in the month prior the survey, including casual work. Casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing compounds. Casual work also includes any type of agricultural labor such as farming, animal rearing, fishing and agricultural day labor. In Column 4 the dependent variable is total earnings from any regular wage or self-employment in the last month. Individuals reporting no regular wage work or self-employment are assigned a value of zero. The top 1% of earnings values are excluded. In Column 5 the eight study sectors are: motor-mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical wiring and welding. The dependent variables in Columns 2 to 5 exclude casual work. In Column 6 the Labor Market Index has the components in Columns 3 to 5 and is constructed following Anderson's [2008] approach. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 10: Long Run Earnings, Bargaining and Spells

OLS regression coefficients, robust standard errors in parentheses

Randomization inference and Romano-Wolf adjusted p-values in braces

	Earnings in the last month [USD]	Earnings from casual jobs in the last month [USD]	Earnings from regular jobs in the last month [USD]	Bargaining index	Length of last unemployment spell (months)	Length of last employment spell (months)
	(1)	(2)	(3)	(4)	(5)	(6)
Vocational Training	11.0***	1.12	8.07***	.002	-1.24***	1.24***
	(2.52)	(.770)	(2.33)	(.023)	(.235)	(.234)
	{.000, .001}	*{.146, .357}*	*{.000, .003}*	*{.904, .917}*	*{.000, .001}*	*{.000, .001}*
Vocational Training + Job Assistance	6.11**	-.437	5.74**	.089***	-.667**	.619**
	(2.89)	(.870)	(2.69)	(.025)	(.259)	(.258)
	{.024, .074}	*{.613, .780}*	*{.028, .065}*	*{.000, .001}*	*{.013, .024}*	*{.020, .029}*
Job Assistance	3.27	.610	1.25	-.018	-.411	.452*
	(2.71)	(.957)	(2.47)	(.024)	(.250)	(.248)
	{.225, .224}	*{.503, .780}*	*{.617, .616}*	*{.460, .668}*	*{.081, .102}*	*{.054, .063}*
P-value: VT = VT + Job Assistance	*[.099]*	*[.102]*	*[.396]*	*[.001]*	*[.023]*	*[.015]*
Mean in Control Group	43.3	5.15	38.0	-.019	6.20	5.63
N. of observations	3,125	3,269	3,541	3,570	3,693	3,693

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the second, third and fourth worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 the dependent variable is total earnings from any casual and regular wage or self-employment in the last month. The top 1% of earnings values are excluded. The data used in Column 2 is from the second and third worker follow-up survey because casual earnings were not measured at fourth follow-up. In Column 4 the Wage Bargaining Index has the components in Columns 1 to 4 of Table A12 and is constructed following Anderson's [2008] approach. In Columns 3 and 4, the length of Last Employment and Unemployment spells refer to spells in which the respondent has been involved in the last year. For both outcomes, the maximum value is 12 months, which correspond to the respondent having been involved in the same employment or unemployment spell for the entire year. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 11: Realized Jobs, Realized Firms and Self-Employment

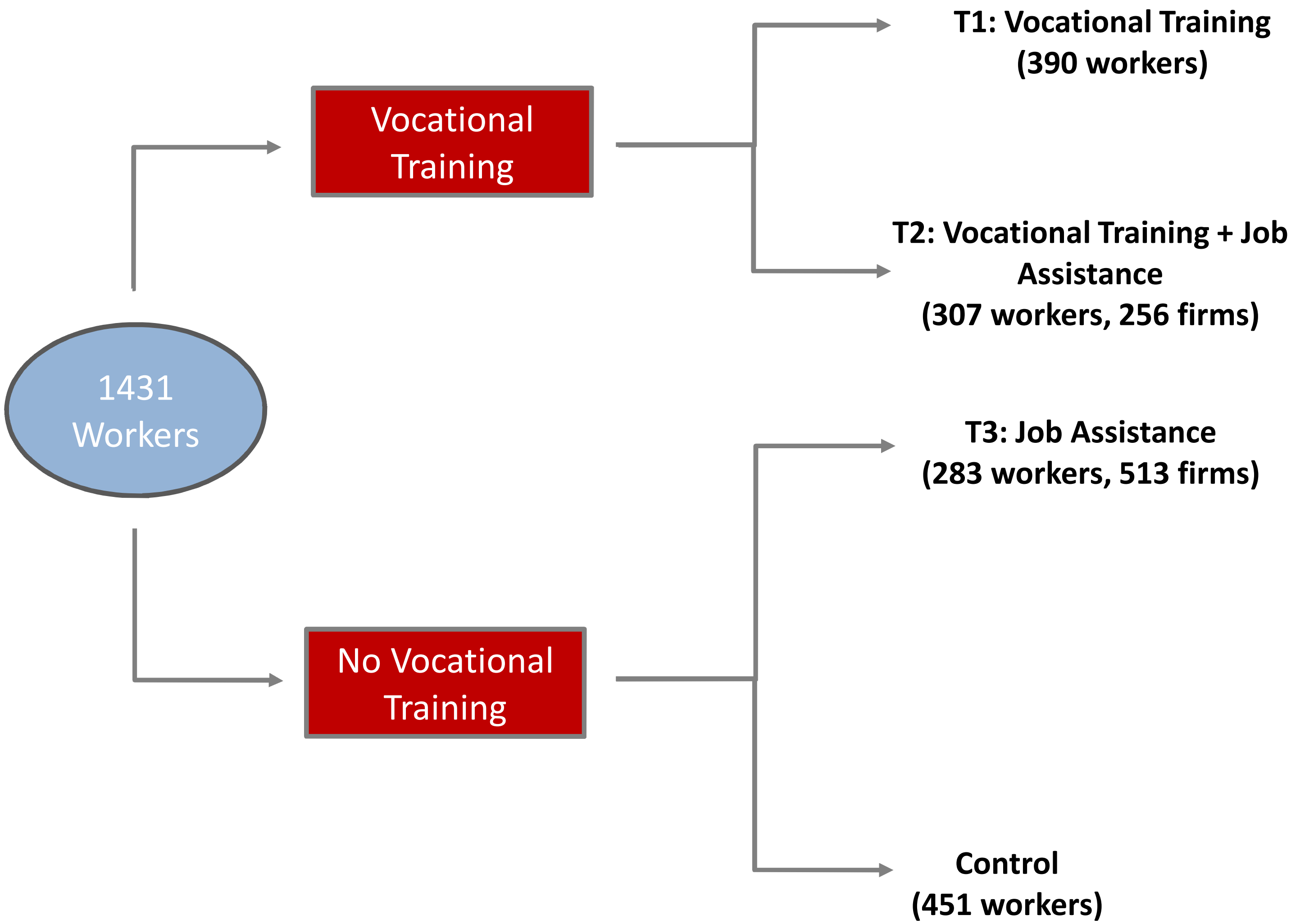
OLS regression coefficients, robust standard errors in parentheses

Randomization inference and Romano-Wolf adjusted p-values in braces

	Realized Job	Realized Firm	Has done any self-employment in one of the eight study sectors in the last month	Labor Outcomes Index
	(1)	(2)	(3)	(4)
Vocational Training	.096***	.003	.104***	.115***
	(.029)	(.028)	(.013)	(.018)
	{.000, .002}	*{.916, .910}*	*{.000, .001}*	*{.000, .001}*
Vocational Training + Job Assistance	.042	-.058*	.076***	.051***
	(.032)	(.031)	(.015)	(.020)
	{.202, .349}	*{.069, .106}*	*{.000,.001}*	*{.014, .021}*
Job Assistance	-.013	-.067**	.040***	.020
	(.030)	(.031)	(.013)	(.018)
	{.683, .672}	*{.021, .079}*	*{.004, .002}*	*{.288, .273}*
P-value: VT = VT + Job Assistance	*[.077]*	*[.035]*	*[.100]*	*[.001]*
Mean in Control Group	-.025	.045	.061	-.042
N. of observations	2,429	2,504	3,699	3,725

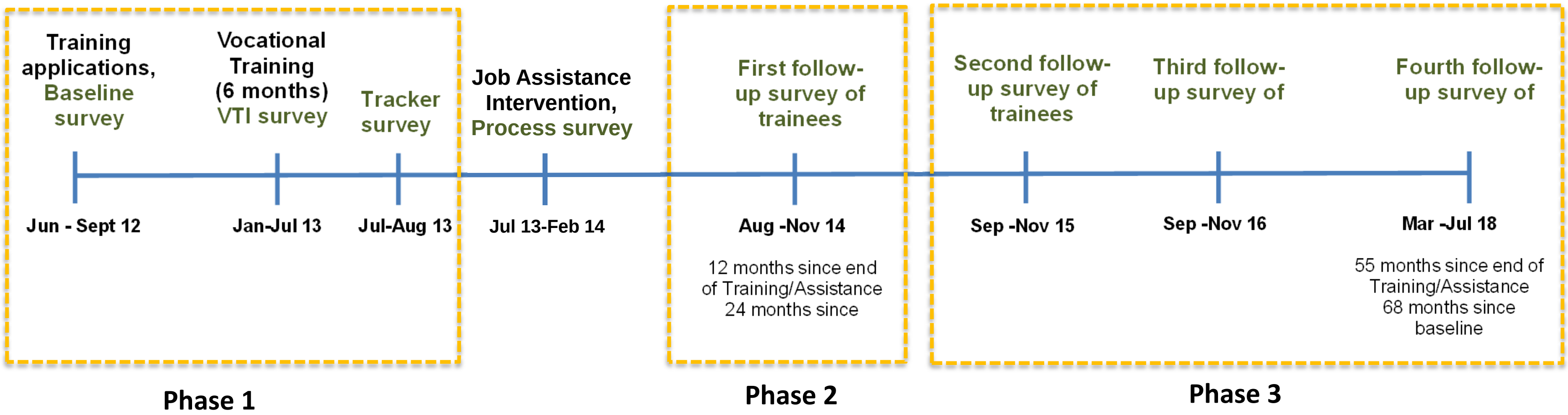
Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the second, third and fourth worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 the Realized Job Index has the components in Columns 1 to 5 of Table A13. In Column 2 the Realized Firm Index has the components in Columns 1 to 5 of Table A14. The components of the Labour Outcomes Index in Column 4 are the components of the Employment Index, the components of the Realized Job and Realized Firm indexes, earnings from regular jobs in the last month and the length of the last employment spell. All indices are constructed following Anderson's [2008] approach. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Figure 1: Experimental Design



Note: The numbers in parentheses refer to the number of eligible applicants originally assigned to each treatment, and the number of firms assigned to each treatment.

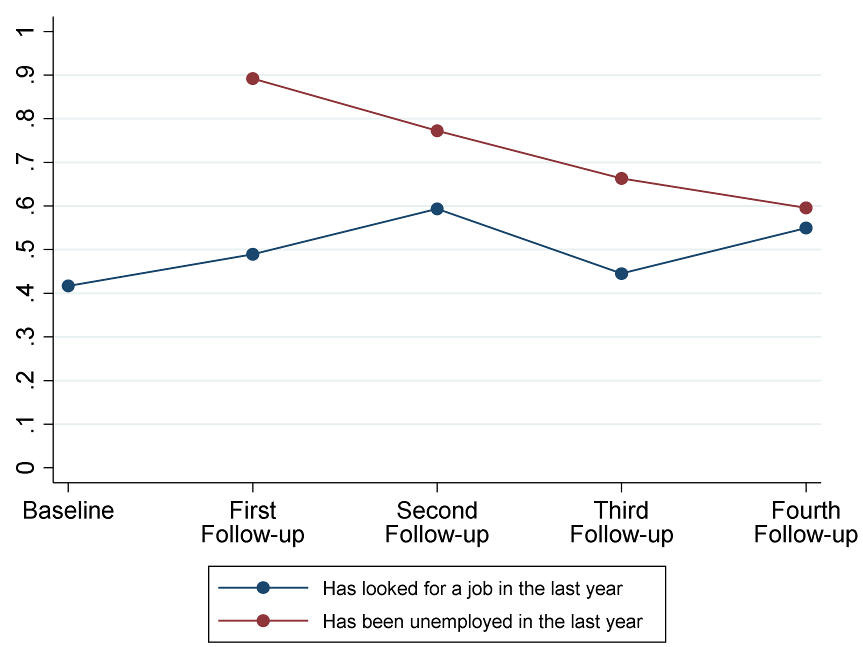
Figure 2: Timeline of Worker Surveys



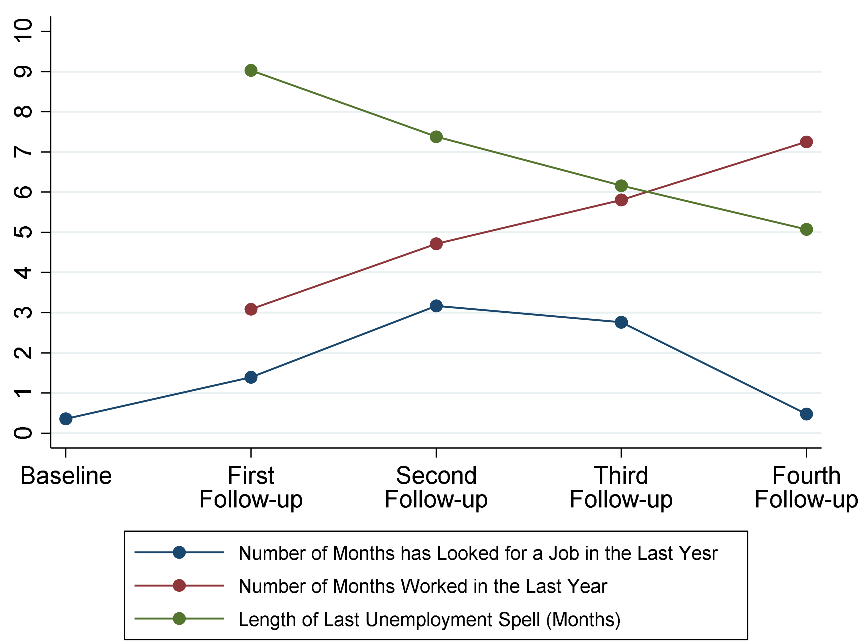
Notes: The timeline highlights the relevant dates for the main batch of workers and worker surveys. A second smaller round of applications and baseline surveys (17% of the overall sample) were conducted in May and June 2013. The majority of trainees from the first round of applicants started training in January 2013, as shown in the timeline. For logistical reasons, a smaller group received training between April and October 2013. The trainees from the second round of applications received vocational training between October 2013 and March 2014. VTI surveys were collected towards the end of the training period while trainees were still enrolled at the VTIs. Workers from the second round of applicants were not included in the Tracker Survey. There were two rounds of Untrained, Job Assistance and Vocational Training + Job Assistance interventions, in line with the two batches of first round trainees from the vocational training institutes. The first round of the Untrained, Job Assistance and Vocational training + Job Assistance interventions took place in August-September 2013. The second round took place in December 2013-February 2014.

Figure 3: Labor Market Outcomes and Search Among Controls

PANEL A: Unemployment and Job Search



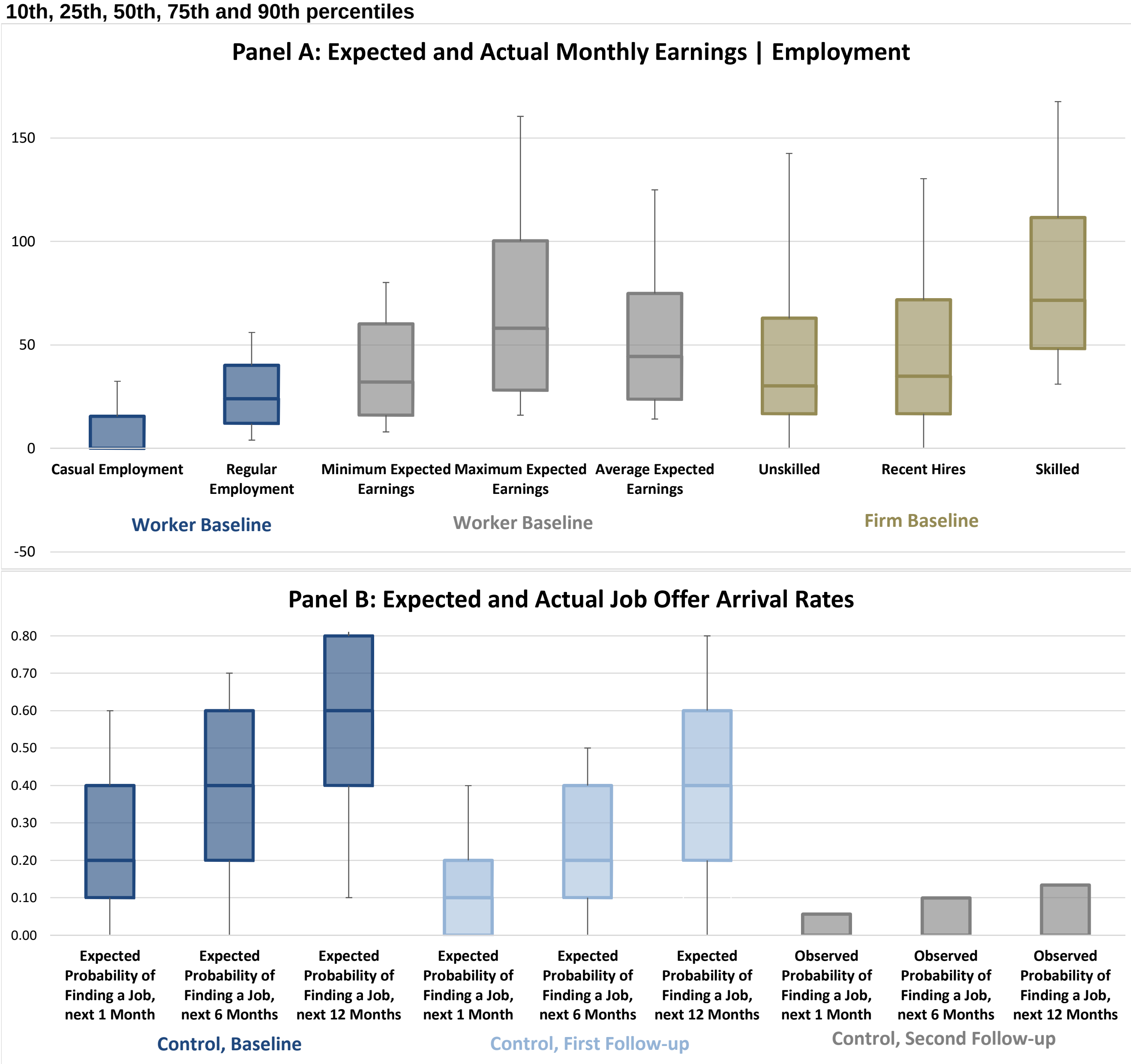
PANEL B: Unemployment Spells and Time Spent Searching for Work



Notes: The sample only includes workers in the Control group. Panel A shows the share of individuals who have been unemployed any time last year, and the share of individuals who have looked for a job in the last year. Panel B shows the number of months the respondent has worked, and has looked for a job in the last year, and the length of the last unemployment spell. All employment outcomes exclude casual jobs or those in agriculture. The length of the last unemployment spell is measured in the 12 months before each follow-up survey and is computed as follows: (i) for individuals who were unemployed at the time of the survey, it is calculated as the number of months between the time of the survey and the end of the last employment spell (if they had any in the 12 months prior the survey); (ii) for individuals who were employed at the time of the survey, it is the number of months not spent in the last employment spell in the 12 months prior the survey (so ignoring previous employment spells). Length of the last unemployment spell and the number of months worked in the last year were not measured at baseline.

Figure 4: Earnings and Employment Expectations

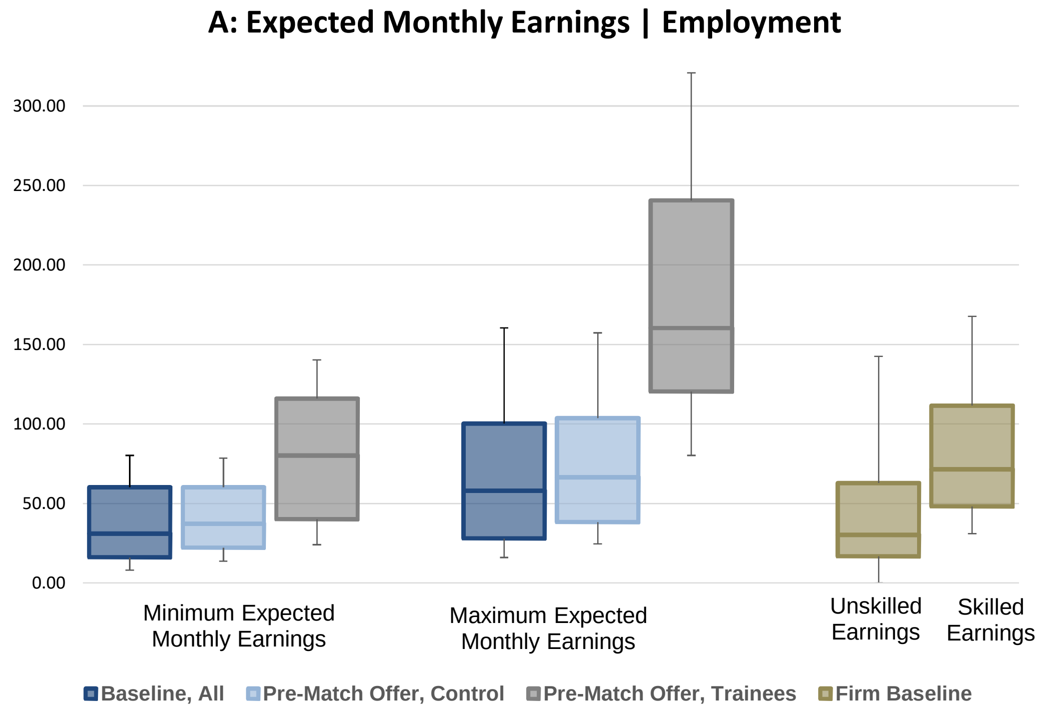
10th, 25th, 50th, 75th and 90th percentiles



Notes: Panel A shows box-and-whisker plots for actual and expected monthly earnings conditional on wage employment from three different samples. Each plot shows the 10th, 25th, 50th, 75th and 90th percentiles of actual/expected earnings distributions The first worker baseline sample shows actual earnings in casual and regular employment at baseline. Casual work includes any of the following jobs where workers are usually hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing compounds. Casual work also includes any type of agricultural labor such as farming, animal rearing, fishing and agricultural day labor. The second worker baseline sample shows minimum, maximum and expected monthly earnings from employment in the respondents' preferred sector among the eight study sectors. The expected earnings are calculated by taking the reported likelihood earnings are above the midpoint of the minimum and maximum, and then fitting a triangular distribution. The third sample - the firm baseline – is taken from firm side baseline survey. This covers individuals employed in the firms that were selected to be part of the experiment at baseline, and to which the workers in the Vocational training + Job Assistance and Job Assistance treatments were later matched to. We consider the actual distribution of earnings among unskilled, recently hired and skilled workers in these firms. Panel B shows the distribution of expected probabilities of finding a job at various horizons, at baseline and first follow-up. The third set of bars are for the actual probabilities of finding employment in these good sectors among control workers at second follow-up. The sample used to construct Panel B only includes individuals who were not employed in any of the eight study sectors at first follow-up.

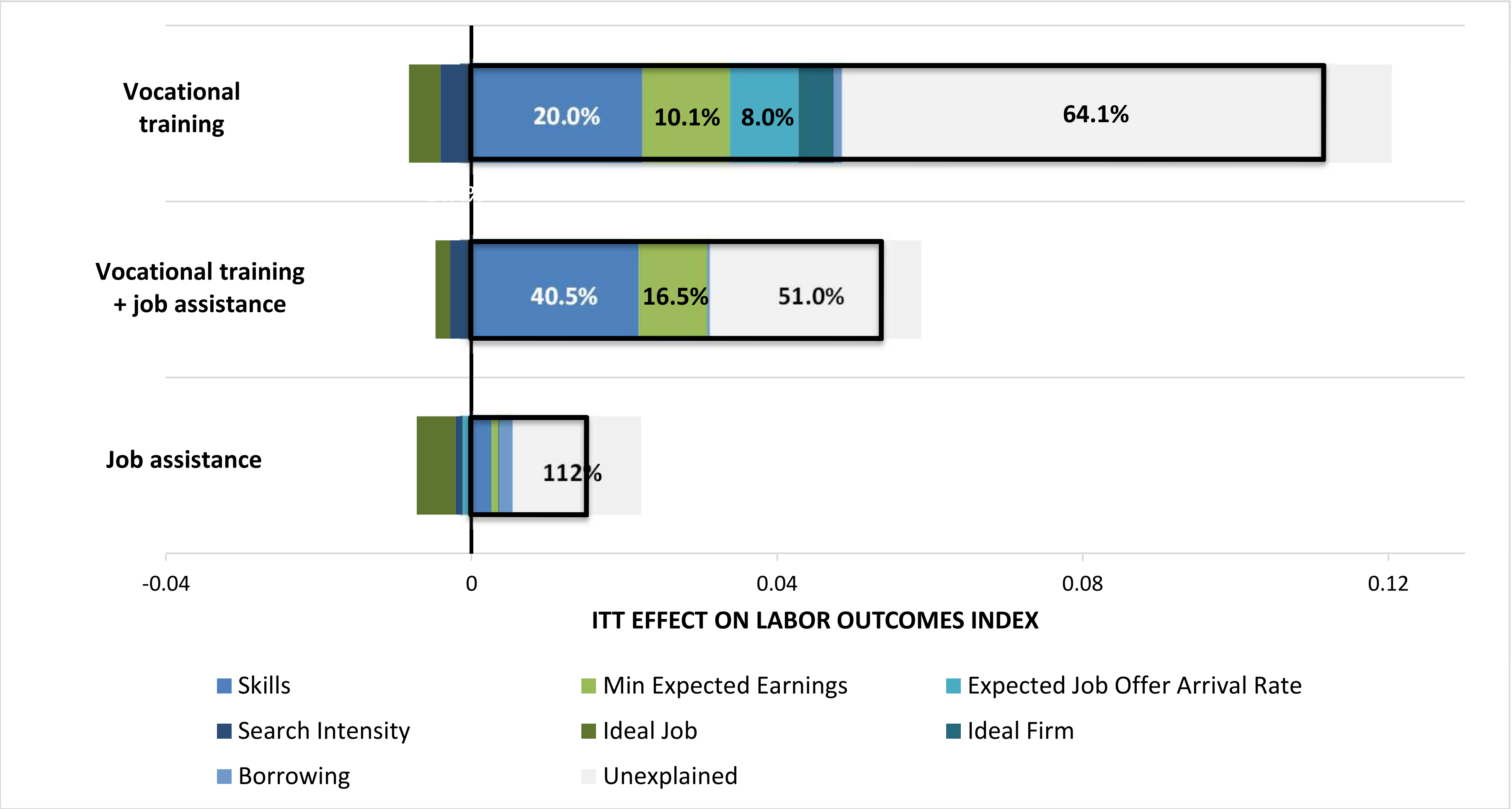
Figure 5: Evolution of Expectations

10th, 25th, 50th, 75th and 90th percentiles



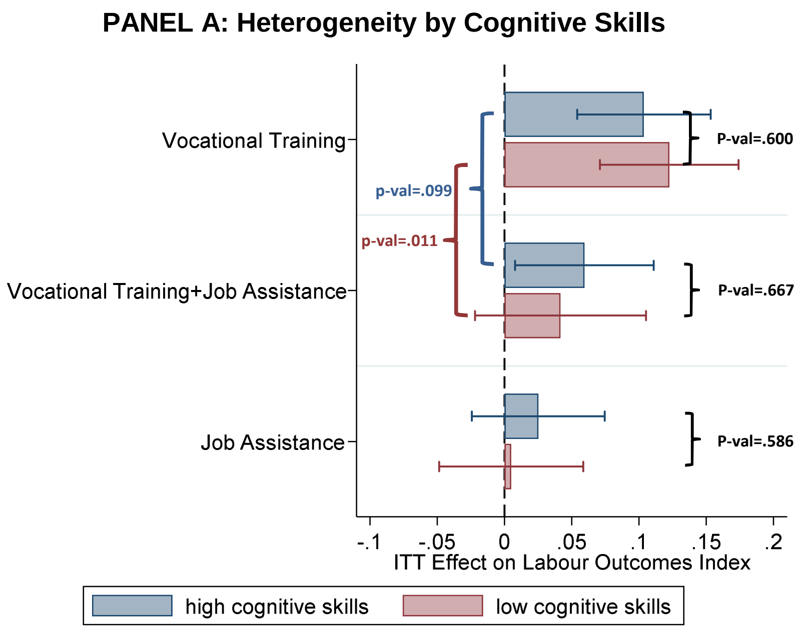
Notes: The data used is from baseline, VTI surveys conducted towards the end of the training period while trainees were still enrolled at the vocational training institutes, and we extrapolate back from the first worker follow-up survey assuming a linear evolution of beliefs, to what would have been beliefs among Controls at the same time as the VTI survey was being fielded. Panel A shows box-and-whisker plots for the minimum and maximum expected monthly earnings conditional on employment in the workers' preferred among the eight study sectors. The plot shows 10th, 25th, 50th, 75th and 90th percentiles of the distribution. Panel B shows box-and-whisker plots for the expected probability of finding a job in one of the eight study sectors in the next one, six and twelve months.

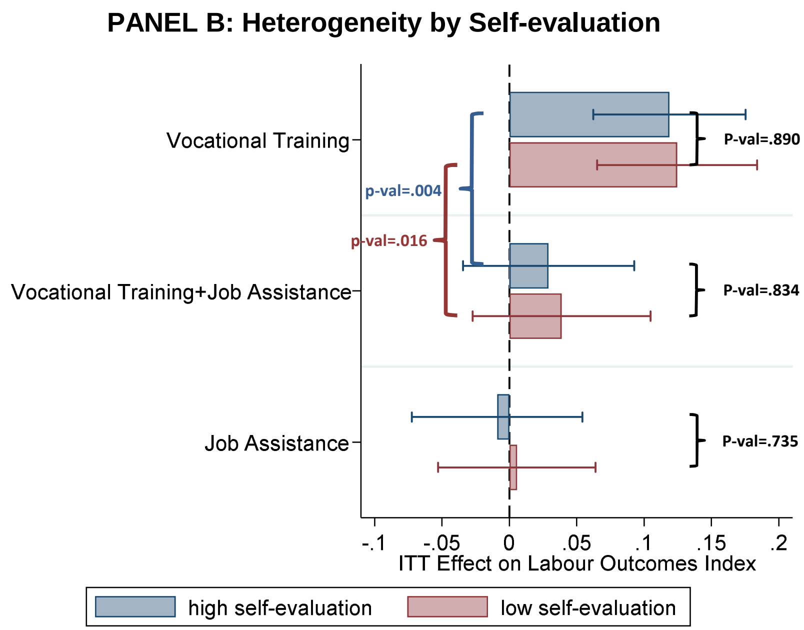
Figure 6: Mediation Analysis



Notes: We show a decomposition of the ITT effect on the labor market index, following the approach of Gelbach [2016]. We show the decomposition of the difference between the ITT effects in the full (with mediators) and restricted (without mediators) models. The black lines show the magnitude of the ITT coefficient from the restricted model. The percentages on the bars show the percentage of the ITT effect in the restricted model that is explained by each mediator. All regressions include strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. The analysis uses the following variables as mediators: the sector specific skills test score, the expected probability of finding a good sector job in the next 12 months, the reservation wage as measured by the minimum expected earnings in a study sector firm, a dummy for whether the individual searched for a job in the previous year, the ideal job index, the ideal firm index and a dummy for whether the individual is borrowing.

Figure 7: External Validity





Notes: We show coefficients and 95% confidence intervals for the ITT effects on the Labour Market Index. In Panel A we split the sample into those of high and low cognitive skills. We measure cognitive ability using the worker score from a short 10-question version of Raven's progressive Matrices test. This is measured at first follow-up, and we split workers into above/below the median in the two panels. In Panel B we split the sample into those of high and low self-evaluation. The self-evaluation index combines measures of self-esteem, locus of control, and neuroticism. The index is built in two steps: (i) among all the items measuring the three personality traits, we select the ones that correlate positively and strongly; (ii) we use principal component analysis to aggregate the items and construct a single index of the underlying trait. An individual is classified as having a high self-evaluation if his self-evaluation score is above the median. Neuroticism is measured at first follow-up, self-esteem and locus of control are measured at third follow-up. All regressions include strata dummies, survey wave dummies and a dummy for the implementation round.

Table A1: External Validity

Means, standard deviations in parentheses

	Age [Years]	Gender [Male=1]	Married	Currently in school	Ever attended vocational training	Has worked in the last week	Has had any wage employment in the last week	Total earnings in the last month
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Baseline, aged 18-25	20.1	.566	.037	.013	.037	.361	.150	6.01
	(1.89)	(.496)	(.188)	(.115)	(.188)	(.480)	(.357)	(17.9)
Uganda National Household Survey 2012/13:								
B. All, aged 18-25	21.1	.465	.395	.309	.062	.681	.293	9.13
	(2.32)	(.499)	(.489)	(.462)	(.241)	(.466)	(.455)	(28.2)
C. Labor Market Active, aged 18-25	21.4	.475	.448	.207	.064	.902	.389	12.2
	(2.33)	(.499)	(.497)	(.405)	(.245)	(.297)	(.489)	(32.0)

Notes: We present characteristics of individuals from three samples: (i) those individuals in our baseline sample aged 18-25; (ii) individuals aged 18-25 and interviewed in the Uganda National Household Survey 2012/13 (UNHS) conducted by the Ugandan Bureau of Statistics; (iii) individuals aged 18-25 and interviewed in the UNHS who self-report being active in the labor market (either because they are engaged in a work activity or are actively seeking employment). The UNHS was fielded between June 2012 and June 2013. Our baseline survey was fielded between June and September 2012. In the UNHS respondents are considered to have attended vocational training if the highest grade completed is post-primary specialized training/diploma/certificate or post-secondary specialized training/diploma/certificate.

Table A2: Baseline Balance on Worker Characteristics

Means, robust standard errors from OLS regressions in parentheses
P-value on t-test of equality of means with control group in brackets
P-value on F-tests in braces

	Age [Years]	Married	Has child(ren)	Currently in school	Ever attended vocational training	F-test of joint significance
	(1)	(2)	(3)	(4)	(5)	(6)
Control	20.1	.027	.102	.011	.042	
N=451	(.230)	(.015)	(.025)	(.010)	(.021)	
Vocational Training	20.0	.056*	.127	.018	.032	{.882}
N=390	(.135)	(.014)	(.022)	(.009)	(.013)	
	[.788]	[.057]	[.342]	[.538]	[.471]	
Vocational Training + Job Assistance	20.0	.030	.123*	.029	.038	{.845}
N=307	(.147)	(.012)	(.023)	(.011)	(.015)	
	[.913]	[.163]	[.090]	[.237]	[.830]	
Job Assistance	20.0	.047*	.122	.007	.027	{.875}
N=283	(.149)	(.015)	(.024)	(.007)	(.014)	
	[.418]	[.092]	[.211]	[.492]	[.332]	

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. All data is from the baseline survey of workers. Columns 1 to 5 report the mean value of each worker characteristic, and standard errors derived from an OLS regression of the characteristic of interest on dummies variable for the treatment groups. All regressions include strata dummies and a dummy for the implementation round. The excluded (comparison) group in these regressions is the Control group. Robust standard errors are reported throughout. Column 6 reports the p-values from F-Tests of joint significance of all the regressors from an OLS regression where the dependent variable is a dummy variable taking value 0 if the worker is assigned to the Control group, and it takes value 1 for workers assigned to the corresponding treatment group and the independent variables are the variables in Columns 1 to 5. Robust standard errors are used in all these regressions.

Table A3: Compliance with Vocational Training

OLS regression coefficients, robust standard errors in parentheses
Dependent Variable: Completed vocational training

	(1)	(2)
Vocational Training + Job Assistance	-.061	.096
	(.04)	(.394)
Female	-.215***	-.200***
	(.040)	(.053)
Age	-.004	.006
	(.010)	(.013)
Any Child	-.050	-.096
	(.063)	(.085)
Education Level	-.018*	-.030***
	(.010)	(.012)
Has Ever Worked	-.018	-.020
	(.038)	(.049)
Literacy/Numeracy Test Score	-.063*	-.047
	(.037)	(.049)
Female X VT+Job Assistance		-.027
		(.081)
Age X VT+Job Assistance		-.020
		(.020)
Any Child X VT+Job Assistance		.085
		(0.152)
Education Level		0.028
		(.020)
Has Ever Worked X VT+Job Assistance		.005
		(.077)
Literacy/Numeracy Test Score X VT+Job Assistance		-.034
		(.076)
Mean of dependent variable	.653	
P-value: worker covariates	[.000]	[.001]
P-value: worker covariates X Job Assistance		[.886]
Observations	636	636

Notes: The sample comprises of all the workers who were offered Vocational Training, so workers in both the Vocational Training and the Vocational Training + Job Assistance treatments. The outcome is a dummy equal to one if the worker completed the 6-months vocational training program offered by BRAC. The explanatory are measured in the baseline survey of workers. We report OLS regression coefficients and robust standard errors in parenthesis. In Column 1 we show that impact of the covariates on vocational training take-up. In Column 2, we interact the covariates with a dummy equal to 1 for individuals in the Vocational Training + Job Assistance treatment. All regressions control for the implementation round

Table A4: Attrition

OLS regression coefficients, robust standard errors in parentheses
Dependent Variable: Worker attrited by Endline (fourth follow up)

	No covariates	With covariates	Heterogeneous
	(1)	(2)	(3)
Vocational Training	.014	.015	-.070
	(.026)	(.026)	(.242)
Vocational Training + Job Assistance	-.038	-.036	-.386
	(.027)	(.027)	(.246)
Job Assistance	.011	.012	-.112
	(.028)	(.028)	(.246)
Age at Baseline		.004	-.003
		(.005)	(.008)
Married at Baseline		-.027	.020
		(.056)	(.113)
Any child at Baseline		-.015	.002
		(.037)	(.060)
Employed at Baseline		.013	.002
		(.022)	(.036)
High Cognitive Skills		.016	.036
		(.020)	(.035)
Mean of outcome in T1 Control group		.145	
F-statistic on Interactions			.967
Number of observations (workers)		1,293	

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. Data is from the fourth worker follow-up survey. Standard errors are adjusted for heteroscedasticity in all regressions. Baseline characteristics include: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, and a dummy for whether the worker was employed at baseline. The variable high cognitive skills at baseline is a dummy equal to 1 if the applicant scored at the median or above on a short 10-question version of Raven's progressive Matrices test at baseline. At the foot of Column 3 we report the F-statistic from an F-Tests of joint significance of all baseline characteristics interacted with a dummy for each of the treatment groups.

Table A5: Correlates of Call Backs

OLS regression coefficients, clustered standard errors in parentheses
Dependent variable: firm called back the worker

	Vocational Training + Job Assistance		Job Assistance	
	Worker and Firm Characteristics	Worker Characteristics and Firm FEs	Worker and Firm Characteristics	Worker Characteristics and Firm FEs
	(1)	(2)	(3)	(4)
PANEL A: Worker Characteristics				
Female	-.056	.031	-.002	-.004
	(.085)	(.059)	(.079)	(.074)
Age	-.011	-.002	.025**	-.005
	(.014)	(.012)	(.012)	(.004)
Any Child	-.046	-.055	-.071	.024
	(.081)	(.079)	(.059)	(.026)
Education Level	.022	.015	-.012	-.009
	(.017)	(.025)	(.011)	(.006)
Has Ever Worked	-.031	-.171*	-.024	.058
	(.086)	(.090)	(.057)	(.040)
Literacy/Numeracy Test Score	-.000	.006	-.007	-.004
	(.014)	(.024)	(.014)	(.004)
PANEL B: Firm Characteristics				
Owner would like to Expand	.182*		.021	
	(.095)		(.064)	
Firm constrained by Lack of Trustworthy Workers	.129*		-.046	
	(.067)		(.077)	
Firm constrained by Inability to Screen Workers	-.114		.073	
	(.073)		(.071)	
Owner Age	-.006		.000	
	(.005)		(.004)	
Owner Education Level	.020**		.001	
	(.009)		(.008)	
Firm Age	.004		.002	
	(.005)		(.011)	
Number of Employees	-.040*		.009	
	(.024)		(.021)	
Log (Monthly Profits)	.058		.021	
	(.039)		(.035)	
Mean of dep. var. in control	.161		.179	
P-value: firm covariates	[.049]	-	[.978]	-
P-value: worker covariates	[.537]	[.614]	[.399]	[.658]
Firm fixed effects	No	Yes	No	Yes
Sector of match dummies	Yes	No	Yes	No
BRAC branch office dummies	Yes	No	Yes	No
Observations	164	164	305	305

Notes: The sample is based on workers and firms involved in match offers. The outcome is a dummy equal to one if the firm expressed interest in meeting with the matched worker (as collected in the process reports as part of the job assistance program). The control variables are measured in the baseline survey of workers and firms, and process reports for treatments involving job assistance. The unit of observation is the match between firm and worker. We report OLS regression coefficients and standard errors clustered at the firm level in parentheses. Regressions in Columns 1 and 3 include sector of match dummies and BRAC branch dummies. Columns 1 and 2 are for match offers made to skilled workers. Columns 3 and 4 refer to match offers made to unskilled workers. The p-values reported at the bottom of each column are from join F-tests of significance of the firm and worker covariates, as indicated in the table.

Table A6: Labor Market Outcomes in the Short Run

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Has done any work in the last month	Any work in one of the eight good sectors in the last year	Number of months worked in one of the eight study sectors in the last year	Total regular earnings in the last month [USD]	Self-employed in the last month	Quality of Firm Employed At
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Vocational Training	.068*	.173***	1.01***	3.82	.014	.101
	(.036)	(.030)	(.273)	(2.77)	(.022)	(.075)
	{.062, .109}	*{.000, .001}*	*{.000, .001}*	*{.171, .292}*	*{.571, .785}*	*{.178, .393}*
Vocational Training + Job Assistance	.093**	.149***	.911***	5.17*	-.013	.035
	(.039)	(.033)	(.320)	(3.01)	(.025)	(.072)
	{.017, .047}	*{.000, .001}*	*{.006, .006}*	*{.086, .210}*	*{.584, .785}*	*{.617, .844}*
Job Assistance	.055	.011	-.025	2.63	.025	.007
	(.039)	(.028)	(.277)	(2.90)	(.025)	(.091)
	{.175, .171}	*{.678, .701}*	*{.931, .924}*	*{.373, .364}*	*{.328, .696}*	*{.931, .950}*
P-value: VT = VT + Job Assistance	*[.545]*	*[.533]*	*[.784]*	*[.686]*	[.299]	*[.684]*
Mean in Control Group	.359	.126	1.23	17.7	.094	.010
N. of observations	1,225	1,231	1,231	1,172	1,231	505

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the second, third and fourth worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 the outcome is a dummy equal to 1 if the respondent has done any work in the month prior the survey, including casual work. Casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing compounds. Casual work also includes any type of agricultural labor such as farming, animal rearing, fishing and agricultural day labor. In Columns 2 and 3 the eight study sectors are: motor-mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical wiring and welding. In Column 3 the dependent variable is total earnings from any regular wage or self-employment in the last month. Individuals reporting no regular wage work or self-employment are assigned a value of zero. The top 1% of earnings values are excluded. The dependent variables in Columns 2 to 6 exclude casual work. In Column 5 the outcome is a dummy equal to 1 if the respondent has been engaged in self-employment in a regular occupation in the month prior the survey. In Column 6 the realized firm index is constructed following Anderson's [2008] approach. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table A7: Treatment Effects on Personality Traits, Cognitive Skills and Psychological Traits

OLS regression coefficients, robust standard errors in parentheses

Randomization inference and Romano-Wolf adjusted p-values in braces

	Extraversion	Agreeableness	Conscientiousness	Neuroticism	Openess	Cognitive skills (Raven's test score)	Locus of control	Control over destiny	Risk-worries	Self-esteem	Self-evaluation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Vocational Training	.002	.043	-.015	-.023	.132*	.123	-.150	.261*	.728	.212	.073
	(.076)	(.079)	(.079)	(.081)	(.078)	(.174)	(.245)	(.157)	(.601)	(.264)	(.078)
	{.989, .991}	*{.582, .893}*	*{.830, .974}*	*{.782, .784}*	*{.087, .513}*	*{.469, .708}*	*{.541, .746}*	*{.118, .567}*	*{.242, .675}*	*{.414, .521}*	*{.345, .732}*
Vocational Training + Job Assistance	-.042	.049	-.015	-.108	.091	-.229	-.476*	.127	.472	-.068	.009
	(.086)	(.086)	(.086)	(.091)	(.087)	(.202)	(.258)	(.170)	(.674)	(.285)	(.087)
	{.641,.949}	*{.555, .893}*	*{.856, .974}*	*{.260,.382}*	*{.293, .693}*	*{.262, .605}*	*{.067,.199}*	*{.477, .785}*	*{.476, .714}*	*{.822,*	*{.913, .855}*
Job Assistance	.013	.055	-.056	-.161*	.139	.092	-.047	.168	-.653	.475	-.082
	(.094)	(.086)	(.084)	(.083)	(.084)	(.189)	(.264)	(.164)	(.687)	(.303)	(.094)
	{.882, .991}	*{.522, .893}*	*{.505, .855}*	*{.056, .141}*	*{.102, .513}*	*{.635, .708}*	*{.862, .849}*	*{.302, .779}*	*{.332, .714}*	*{.114, .286}*	*{.395, .359}*
P-value: VT = VT + Job Assistance	*[.616]*	*[.943]*	*[.998]*	*[.343]*	*[.640]*	*[.087]*	*[.233]*	*[.449]*	*[.712]*	*[.346]*	*[.468]*
Mean in Control Group	.005	-.027	.045	.062	-.078	4.82	11.8	5.80	37.4	30.7	-.040
N. of observations	1,091	1,091	1,091	1,091	1,091	1,091	1,240	1,240	1,239	1,238	991

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline, first, second, third and fourth worker follow-up survey. All regressions control for strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Columns 1 to 5 the outcomes are normalized score for each trait from a short version (10 questions) of the Big Five Inventory test. In Column 6 the outcome is the respondent's score from a short version (10 questions) of Raven's progressive Matrices test. In Column 7 the Locus of Control (LOC) score is calculated using Rotter's (1996) Locus of Control scale. A higher score indicates a more external LOC. In Columns 8 to 10 the outcomes are normalized scores for the respondent's answers to questions related to control over own destiny (Column 8), risk and worries (Column 9) and self-esteem (Column 10). The self-evaluation index in Column 11 combines measures of self-esteem, locus of control, and neuroticism. The index is built in two steps: (i) among all the items measuring the three personality traits, we select the ones that correlate positively and strongly; (ii) we use principal component analysis to aggregate the items and construct a single index of the underlying trait. An individual is classified as having a high self-evaluation if his self-evaluation score is above the median. Neuroticism is measured at first follow-up, self-esteem and locus of control are measured at third follow-up. Outcomes in Columns 1 to 6 are only available at first follow-up, the outcomes in Columns 7 to 10 are only available at third follow-up. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table A8: Components of the Market Beliefs Index

OLS regression coefficients, robust standard errors in parentheses

Randomization inference and Romano-Wolf adjusted p-values in braces

	Lack of firms is a serious problem	Job opportunities not being advertised is a serious problem	Difficulty to show possession practical skills is a serious problem	Difficulty to show possession of soft skills is a serious problem
	(1)	(2)	(3)	(4)
Vocational Training	-.045	.014	-.016	-.038
	(.037)	(.036)	(.037)	(.036)
	{.201, .398}	*{.698, .886}*	*{.690, .883}*	*{.297, .496}*
Vocational Training + Job Assistance	-.058	.027	-.039	-.031
	(.041)	(.040)	(.040)	(.040)
	{.141, .398}	*{.500, .850}*	*{.313, .665}*	*{.430, .496}*
Job Assistance	-.026	.017	-.004	-.054
	(.041)	(.041)	(.041)	(.040)
	{.505, .539}	*{.673, .886}*	*{.918, .926}*	*{.181, .414}*
P-value: VT = VT + Job Assistance	*[.749]*	*[.752]*	*[.569]*	*[.873]*
Mean in Control Group	.581	.592	.441	.438
N. of observations	1,227	1,228	1,229	1,228

Notes: *** denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. For each of the variables in Columns 1 to 4, the respondents were asked whether the issue indicated in the Column heading was (i) not a problem at all, (ii) not a very serious problem, (iii) a somewhat serious problem, (iv) a serious problem, (v) a very serious problem, while looking for jobs. The variables in Columns 1 to 4 were set equal to 1 if the respondents said the issue was either a serious or a very serious problem, and equal to 0 otherwise. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table A9: Components of the Ideal Job Index

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Supervising others	High status	Learning new job-specific skills	Working with others	Flexible schedule
	(1)	(2)	(3)	(4)	(5)
Vocational Training	-.003	-.022	.001	-.020	-.042
	(.036)	(.035)	(.027)	(.017)	(.037)
	{.927, .920}	*{.512, .850}*	*{.973, .960}*	*{.250, .552}*	*{.247, .526}*
Vocational Training + Job Assistance	-.043	-.020	.036	-.008	.002
	(.039)	(.038)	(.025)	(.018)	(.040)
	{.273, .448}	*{.646, .850}*	*{.130, .339}*	*{.640, .888}*	*{.959,.959}*
Job Assistance	-.085**	-.026	-.032	.005	-.037
	(.039)	(.039)	(.030)	(.017)	(.041)
	{.034, .090}	*{.538, .850}*	*{.283, .464}*	*{.782, .888}*	*{.379, .556}*
P-value: VT = VT + Job Assistance	*[.332]*	*[.947]*	*[.168]*	*[.527]*	*[.282]*
Mean in Control Group	.579	.652	.840	.953	.589
N. of observations	1,222	1,219	1,217	1,219	1,222

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. The outcomes in Columns 1, 2 and 5 are constructed from questions asking the respondents to rate, on a scale from 0 to 10, the importance of the ideal job possessing the characteristic described in the respective column. The answers are then recoded as dummies equal to one if the score given by the respondent is greater or equal to the median score for Controls at the same follow-up. The outcome in Column 3 is a dummy equal to one if the respondent reports his/her ideal job would allow him/her to learn new job-specific skills rather than using skills that he/she already possesses. The outcome in Column 4 is a dummy equal to one if the respondent reports his/her ideal job would allow him/her to mostly work with other people rather than alone. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table A10: Components of the Ideal Firm Index

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Firm Size	Firm is Formal	Firm provides training	Firm provides other material employee benefits
	(1)	(2)	(3)	(4)
Vocational Training	.089	.030	.056**	.060**
	(.129)	(.053)	(.022)	(.027)
	{.527, .749}	*{.557, .779}*	*{.007, .033}*	*{.036,.072}*
Vocational Training + Job Assistance	-.245	-.095	.042*	.037
	(.155)	(.063)	(.025)	(.029)
	{.110, .302}	*{.132, .315}*	*{.093, .167}*	*{.209, .334}*
Job Assistance	-.044	-.020	.040*	.022
	(.125)	(.054)	(.024)	(.028)
	{.730, .753}	*{.722, .779}*	*{.099, .167}*	*{.454, .404}*
P-value: VT = VT + Job Assistance	*[.040]*	*[.058]*	*[.586]*	*[.464]*
Mean in Control Group	2.18	.810	.072	.120
N. of observations	378	378	1,213	1,213

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. The sample in Columns 1 and 2 is restricted to individuals who indicate wage employment (rather than self-employment) as being their ideal type of job. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table A11: Components of the Credit Index

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Has any savings	Is borrowing any money	Is borrowing to finance job search	Is borrowing to finance business expenditures
	(1)	(2)	(3)	(4)
Vocational Training	-.047	.049	.004	.017
	(.034)	(.035)	(.005)	(.015)
	{.191, .352}	*{.165, .268}*	*{.592, - }*	*{.314, .449}*
Vocational Training + Job Assistance	-.018	.027	-.004	-.006
	(.038)	(.038)	(.003)	(.014)
	{.643, .604}	*{.445, .472}*	*{.261, - }*	*{.652, .689}*
Job Assistance	.046	.090**	.003	.034*
	(.039)	(.039)	(.003)	(.019)
	{.242, .372}	*{.018, .054}*	*{.389, - }*	*{.060, .191}*
P-value: VT = VT + Job Assistance	*[.446]*	*[.574]*	*[.130]*	*[.147]*
Mean in Control Group	.325	.277	.003	.034
N. of observations	1,231	1,199	1,231	1,231

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. P-values adjusted for multiple testing are not reported for the outcome in Column 3 due to the sparsity of the data. All indexes are constructed following Anderson's [2008] approach. The dependent variables in Columns 3 and 4 are equal to 0 if the respondent is currently not borrowing any money, and equal to 1 if the main purpose for which the respondent is currently borrowing money is to finance job search (Column 3) or finance business expenditures (Column 4). In Column 4 business expenditures include expenses incurred to set up, or register a business, purchasing business assets or inputs, pay wages, etc. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table A12: Components of the Worker-Firm Bargaining Index

OLS regression coefficients, robust standard errors in parentheses

Randomization inference and Romano-Wolf adjusted p-values in braces

If received a job offer, would bargain over:	Wage	Hours	Work Location	Additional Benefits
	(1)	(2)	(3)	(4)
Vocational Training	-.021	.010	.006	.003
	(.021)	(.017)	(.020)	(.021)
	{.346, .475}	*{.570, .826}*	*{.755, .761}*	*{.890, .884}*
Vocational Training + Job Assistance	.035	.018	.055**	.065***
	(.022)	(.018)	(.022)	(.023)
	{.110, .075}	*{.297, .826}*	*{.012, .058}*	*{.002, .017}*
Job Assistance	-.024	.018	-.031	.013
	(.022)	(.019)	(.022)	(.022)
	{.286, .475}	*{.349, .716}*	*{.149, .255}*	*{.544, .768}*
P-value: VT = VT + Job Assistance	*[.013]*	*[.628]*	*[.021]*	*[.006]*
Mean in Control Group	.706	.360	.435	.535
N. of observations	3,440	3,522	3,522	3,522

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table A13: Components of the Realized Job Quality Index

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Supervising others	High status	Learning new job-specific skills	Working with others	Flexible schedule
	(1)	(2)	(3)	(4)	(5)
Vocational Training	.071**	.055**	.084***	.055**	-.004
	(.027)	(.026)	(.028)	(.026)	(.027)
	{.009, .034}	*{.046, .092}*	*{.001, .011}*	*{.037, .107}*	*{.901, .974}*
Vocational Training + Job Assistance	-.003	.027	.061**	.058**	-.027
	(.031)	(.028)	(.031)	(.029)	(.030)
	{.920, .929}	*{.336, .556}*	*{.038, .092}*	*{.049, .107}*	*{.360,.724}*
Job Assistance	.030	.010	-.038	-.032	.006
	(.030)	(.028)	(.030)	(.028)	(.029)
	{.314, .519}	*{.750, .748}*	*{.194, .193}*	*{.240, .259}*	*{.819, .974}*
P-value: VT = VT + Job Assistance	*[.010]*	*[.293]*	*[.422]*	*[.885]*	*[.414]*
Mean in Control Group	.565	.608	.477	.660	.625
N. of observations	2,429	2,430	2,431	2,432	2,433

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. All outcomes are conditional on the respondent reporting having had a job in non-casual occupation in the 12 months prior the survey. The outcomes in Columns 1, 2 and 5 are constructed from questions asking the respondents to rate, on a scale from 0 to 10, the extent to which their last job possessed the characteristic described in the respective column. The answers are recoded as dummies equal to one if the score given by the respondent is greater or equal to the median score for the Control group at the same follow-up. The outcome in Column 3 is a dummy equal to one if the respondent reported his/her last job allowed him/her to learn new job-specific skills rather than using skills that he/she already possesses. The outcome in Column 4 is a dummy equal to one if the respondent reported his/her last job allowed him/her to mostly work with other people rather than alone. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

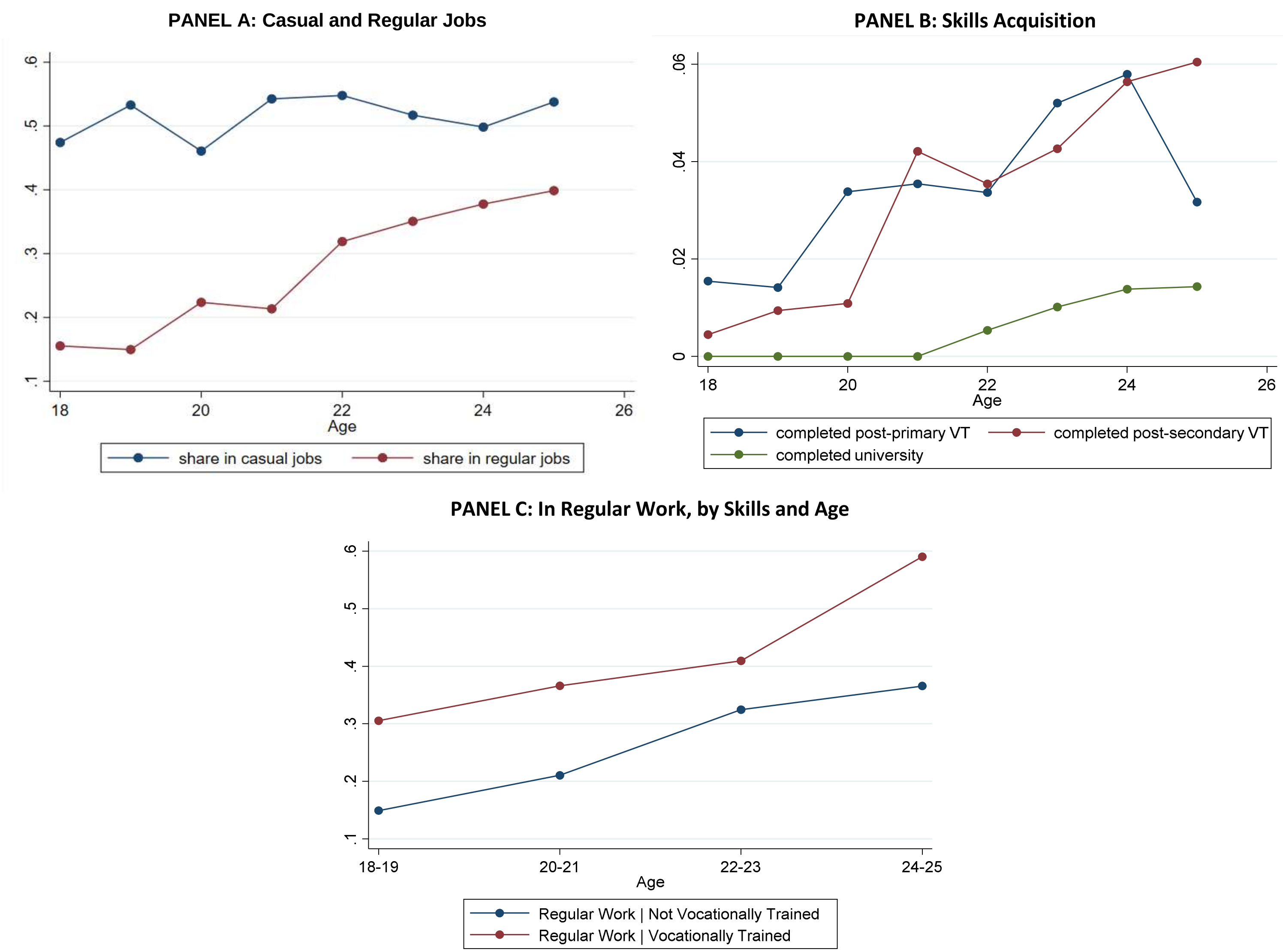
Table A14: Components of the Realized Firm Quality Index

OLS regression coefficients, robust standard errors in parentheses
Randomization inference and Romano-Wolf adjusted p-values in braces

	Number of employees	Registered firm	Had a formal written contract	Was provided training	Had health insurance, pensions or family subsidies
	(1)	(2)	(3)	(4)	(5)
Vocational Training	-.149	-.006	.055**	-.025	.005
	(1.15)	(.028)	(.028)	(.034)	(.018)
	{.893, .938}	*{.836, .843}*	*{.050, .121}*	*{.452, .808}*	*{.794, .781}*
Vocational Training + Job Assistance	-.415	-.062**	-.007	-.024	-.037**
	(1.26)	(.031)	(.028)	(.038)	(.017)
	{.756, .938}	*{.053, .100}*	*{.794,.928}*	*{.523, .808}*	*{.032,.065}*
Job Assistance	-1.74	-.075**	.009	-.027	-.024
	(1.17)	(.030)	(.029)	(.036)	(.019)
	{.140, .314}	*{.015, .032}*	*{.747, .928}*	*{.468, .808}*	*{.208, .337}*
P-value: VT = VT + Job Assistance	*[.818]*	*[.054]*	*[.023]*	*[.977]*	*[.008]*
Mean in Control Group	11.1	.596	.196	.458	.098
N. of observations	2,469	2,328	1,540	1,584	1,768

Notes:***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. All outcomes are conditional on the respondent reporting having had a job in non-casual occupation in the 12 months prior the survey. The sample in Columns 3 to 5 excludes self-employed individuals. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Figure A1: Jobs and Skills by Age



Notes: The data used is from individuals aged 18-25 and interviewed in the Uganda National Household Survey 2012/13 (UNHS) conducted by the Ugandan Bureau of Statistics. Panel A plots the share of individuals in casual and regular jobs by age. Involvement in the two types of jobs is not mutually exclusive. Casual jobs include any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing compounds. Casual jobs also include any type of agricultural labor such as farming, animal rearing, fishing and agricultural day labor. Regular jobs include all other work activities. Panel B plots the share of individuals who completed post-primary vocational training, post-secondary vocational training and university or above by age. Panel C plots the share of individuals in regular work by age, separately for individuals who have not received and have received either post-primary or post-secondary vocational training.

Figure A2: Sector Skills Test for Motor Mechanics

<table>
<tr><th colspan="3">1. MOTOR-MECHANICS</th></tr>
<tr><td colspan="2"></td><td></td></tr>
<tr><td>1</td><td>multiple-choice
What are you advised to do when servicing the engine by changing oil?</td><td>A. Top up lubricating oil
B. Replace oil filter
C. Over hand engine
D. Over hand cylinder head
Correct Answer: B</td></tr>
<tr><td>2</td><td>multiple-choice
What immediate remedy can you give to a vehicle with a problem of excessive tyre wear in the center more than other parts?</td><td>A. Increase tyre pressure
B. Reduce tyre pressure
C. Inflate pressure
D. Remove the vehicle tire
Correct Answer: B</td></tr>
<tr><td>3</td><td>multiple-choice
If a customer reports to you that his/her vehicle charging system works at lower rate, how can you help him?</td><td>A. Replacing the charging system
B. Adjusting the alternator tension
C. Replacing alternator housing
D. Renewing wire insulator
Correct Answer: B</td></tr>
<tr><td>4</td><td>multiple-choice
Which of the following set of systems or component call for mechanical adjustment during general vehicle service?</td><td>A. Tyres, cooling system, master cylinder
B. Break shoes, alternator, and valve clearance
C. Distributor, radiator, propeller shaft
D. Tank, crank shaft, Turbo charger
Correct Answer: B</td></tr>
<tr><td>5</td><td>multiple-choice
What solution would you give a customer with a vehicle engine producing blue smoke?</td><td>A. Top up lubricant
B. Time the engine
C. Replace piston rings
D. Remove carbon deposits
Correct Answer: C</td></tr>
<tr><td>6</td><td>matching
What should you do to stop the following vehicle troubles?</td><td><table>
<tr><td>1</td><td>Battery over charging</td><td>A</td><td>Leaking fuel tank</td></tr>
<tr><td>2</td><td>Engine over heating</td><td>B</td><td>Renew regulator</td></tr>
<tr><td>3</td><td>Lubricant leakage</td><td>C</td><td>Reduce oil to the correct level</td></tr>
<tr><td>4</td><td>Smoke in exhaust</td><td>D</td><td>Renew piston rings</td></tr>
<tr><td>5</td><td>Engine fails to start</td><td>E</td><td>Charge the battery</td></tr>
</table>
Correct Answer: 1B, 2A, 3C, 4D, 5E</td></tr>
<tr><td>7</td><td>order
When changing engine oil, in which order should you perform the following steps?</td><td>A. Drain oil through drain plug
B. Remove oil filter cup
C. Run engine to check leaks
D. Fill new oil through filler cup to level
E. Remove oil filter
F. Warm up the engine
Correct Answer: B, E, A, D, F, C</td></tr>
</table>