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

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Abstract

We study the process of job search in granular detail using a field experiment and tracking young job seekers for six years in urban labor markets in Uganda. We study how labor market interventions impact expectations over own job prospects and underlying search behaviors, and how both map to long run labor market outcomes. The interventions considered are the offer of vocational training, vocational training combined with an offer to match workers to firms, and match offers only. Training is offered in sectors with high quality firms. The matching intervention comprises a light touch offer to match workers for interviews with such firms. At baseline, youth are unskilled yet optimistic about their job prospects, especially over the job offer arrival rate from high quality firms. Relative to controls, those offered vocational training become even more optimistic, searching more intensively and directing their search towards higher quality firms. However, for youth additionally offered matching, expectations are revised downwards as call back rates from firms are far lower than their prior. These differential expectations and search behaviors impact long run outcomes: vocational trainees without match offers have higher employment rates, longer employment spells, and end up in higher quality jobs and firms than youth additionally offered matching. Our analysis highlights how interventions can cause youth to become exuberant or discouraged, and this matters for long run outcomes. We discuss implications for the design and targeting of interventions meant to help young people find good jobs. *JEL: J64, O12.*

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1 Introduction

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 unemployment rates in most African countries being higher today than in 2015. Many countries throughout Sub Saharan Africa thus face the challenge of helping large cohorts of labor market entrants find good jobs.

We study the process by which young workers search for jobs in urban labor markets in a low-income setting: Uganda. We present granular evidence on this process from a field experiment tracking young labor market entrants over six years. The experiment sheds light on how individual expectations, underpinning search behaviors and long run labor market outcomes are all interlinked and 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 matching intervention whereby workers are offered to meet firm owners for interview; (iii) matching only.

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. The sectors we offered training in provide 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 matching component of the experiment comprise 1281 firms operating in 15 urban labor markets. 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).

The field experiment is structured as follows. Using an over subscription design, 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, sector-specific and training has large measurable impacts on worker skills, with the experimentally identified private returns to these certifiable skills to be 20-30% [Alfonsi *et al.* 2020].

At a second stage of randomization, we offer light-touch matching between workers and firms operating in good sectors and tracked as part of the firm-side of the experiment. Workers were asked whether they wanted their details to be passed onto these firms: nearly all agreed. Firms were then presented shortlists of workers that were either: (i) all vocationally trained, or; (ii) all 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 match offer once vocational trainees had completed their courses. This ensures there is no differential compliance with vocational training based on future match offers. As a result we document that sector specific skills accumulation is not statistically different between those offered vocational training and those offered vocational training and matching. Among those not assigned to vocational training, the match offer intervention takes place exactly at the same time as 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 matching (T2); (iii) matching (T3); (iv) controls (C).

Worker expectations over their own job prospects form the foundation of our analysis. 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. Optimistic beliefs have 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 central to understand how workers react to the match offer intervention.

From the worker's perspective, the key outcome generated from the matching intervention is whether the firm they are matched to decides to call them back, 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 match offers 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 matching is made), the median trained worker believes there is a 30% chance in the next month of receiving a job offer from a firm in one of our study sectors – this is far higher than employment rates actually experienced by those only offered vocational training over the same time period.

Among those randomized out of training, they continue to search for work over the next six months, but with little improvement in their labor market outcomes. Employment rates remain flat 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 good sectors we consider. On the eve of match offers 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 match offer 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. Conditional on skills, 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 (e.g. they are both either trained or untrained, and similar on other characteristics). There is little basis on which to prefer one over another.

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

An alternative hypothesis is that 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: (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 matching 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, match offers generate bad news for the average trained worker. Trained workers without match offers 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 is in line with their priors. Hence, even under the alternative hypothesis, there is no reason why they should alter expectations and 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. How this group of youth respond is ultimately an empirical question.

Our first set of results document how these labor market interventions impact the expectations workers hold over their job prospects, a full year after training is completed and/or match offers implemented.

First, comparing workers offered vocational training to controls (T1 vs C), the former group further revise upwards their beliefs over the job offer arrival rate and the distribution of expected earnings conditional on being employed in a study sector firm. Comparing these to actual labor market outcomes for youth, these changes mean they become increasingly optimistic on the job offer arrival rate, while their beliefs over expected earnings move more closely in line with the skills premium offered for trained young workers in these labor markets. Underpinning these changes in expectation along both dimensions, we find workers only offered vocational training search more intensively, and they engage in directed search towards higher quality firms.

Second, workers offered vocational training and matching also have sustained changes in beliefs over their own prospects a full year after training is completed and/or match offers provided. 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). Again, this is underpinned by changes in search behavior: they search less intensively, and search over lower quality firms. These differences in behavior between those offered vocational training with and without match offers runs counter to the null that workers are fully informed of what drives call backs. Their behavior is consistent with them becoming *discouraged* and reacting to the lower than expected call back rate by revising down their beliefs over their own job prospects.

Finally, workers only offered matching – relative to controls – react to the confirmation of their poor job prospects by using credit markets to borrow 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 expectation and search behaviors – translate into long run differences in outcomes for workers, up to five years after training is completed and/or match offers 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 matching do significantly worse that those only offered vocational 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 match offers 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 match offers because those workers are discouraged by the lower than expected rate of call backs.

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 matching, the index increases by less than half the amount ($.051\sigma$), and the two estimates are significantly different ($p = .001$). In short, light touch match offers to those offered vocational training undo half of what is achieved through vocational training alone.

Finally, workers only offered match offers (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 match offers is muted ($.020\sigma$) and not significantly different to controls.

We use mediation analysis to decompose this long run holistic measure of labor market success into parts mediated through skills, expectations and search behaviors. Among workers offered vocational training, 20% of the long run impact is mediated by sector-specific skills. Expectations also play a prominent role: the expected job offer arrival rate explains 8% of the long run impact, and the minimum expected earnings from employment in a study sector explains a further 10%. Once skills and expectations on both margins are accounted for, search behaviors related to search intensity, directed search or credit play relatively muted roles. This reinforces the idea that these search behaviors are driven by changes in expectations, and have little independent impact on long run outcomes.

Among workers offered both vocational training and matching, sector-specific skills play the most important role in mediating long run outcomes. These skills – that do not differ between those only offered vocational training and those additionally offered matching – explain the same increase in our holistic measure of labor market success for both groups of youth. The role of expectations in mediating long run outcomes is however far more prominent for those only offered vocational training. The reason is that workers additionally offered matching are discouraged, and end up with expectations and search behaviors 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 to youth; (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. Finally, we discuss the broad implications our study has for the design and targeting of interventions that provide skills and/or match offers.

Job search is a classic question in labor economics, with fifty years of work since seminal papers by McCall [1970] and Mortensen [1970]. We make three novel contributions to this body of work.

First, we experimentally identify the role that prominent labor market policies – training and matching – play in determining expectations and search behaviors of young workers, and how these map into long run labor market outcomes. We provide a highly granular economic analysis on individual labor market dynamics that combines experimental variation in policies young workers are exposed to, data on beliefs and multiple dimensions of search behavior – such as search intensity and 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. Linking expectations to search behaviors (rather than inferring one from the other) represents an advance for the job search literature [Mueller and Spinnewijn 2021].[1]

Second, we build on a nascent experimental literature evaluating similar labor market programs of training and matching 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, Krueger and Mueller 2016, Conlon *et al.* 2018, Mueller *et al.* 2021, Potter 2021]. We link these literatures by providing the insight that because labor market interventions impact beliefs, light-touch match offers can backfire because workers misinterpret the lack of call backs from potentially good employers. We show this causes them to revise down their short-run expectations and search effort, and this ultimately impacts their long-run labor market success. 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 matching, and marginally improve some labor market outcomes as a result.

Third, understanding the heterogeneous effects of matching 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 matching – 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 job prospects might then a be more effective labor market policy

[1] Two other papers providing detailed 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.

than providing them access to microcredit for example.

This paper is part of a larger project encompassing a cluster of field experiments to study urban labor markets in a low-income setting. Our earlier work contrasted labor market returns to certified vocational training versus non-certified firm-sponsored apprenticeships [Alfonsi *et al.* 2020]. As we make clear throughout, there is some overlap with our results on labor market outcomes (Tables 8 and 9). The focus of our current paper is squarely on the granularities of the job search process, embodied in worker expectations and search behaviors. We study how these margins are impacted by the offer of skills and/or matching, and how shifts in expectations and search behavior then translate into long run labor market success. In our later discussion, we re-examine the results in Alfonsi *et al.* [2020] in light of the findings here on the nature of the job search process for youth.[2]

The results reveal the central role that exuberance and discouragement play in determining whether and how young workers find good jobs in urban labor markets in a developing economy. We do so in a context that shares all the hallmarks of economies throughout Sub Saharan Africa: large cohorts of youth enter the labor market each year, and absent intervention, these youth have low skill levels and face a future of remaining reliant on casual and itinerant work with few prospects of advancing up the job ladder. Ultimately we show how policy intervention can help or hinder their search for good jobs.

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 eve of match offers being announced. Section 4 presents treatment effects on expectations and underpinning job search behaviors. Section 5 shows how the interventions map into persistent differences in labor market outcomes across workers. Section 6 uses mediation analysis to show the relative importance of skills, expectations and search behaviors for long run labor market success. Section 7 discusses re-examining the results in Alfonsi *et al.* [2020] in light of our findings, and then discusses 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 labor markets in Uganda. Multiple frictions affect the job search process, including: (i) skills mismatch – youth enter labor markets with skills not well suited to the needs of firms [Frederiksson *et al.* 2018]; (ii) credit – workers cannot finance human capital investments to correct for skills mismatch even if these generate private returns; (iii) information

[2] The previously considered apprenticeship treatment plays no role in the current study. The treatment arms related to matching were not separately studied in our earlier work.

– 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 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 begin with, we derive the share of young people engaged in casual jobs, and in more regular jobs. 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 where neither worker nor firms are obligated to supply or demand labour on a regular basis.[3]

Panel A of Figure A1 shows that at all ages, young workers remain reliant on casual work, with there only being a slow increase in them accessing regular work as they age. This flat dynamic is in contrast with labor markets in higher-income settings, where the first years after entry are typically characterized by rapid wage growth as young people frequently switch towards better paying jobs [Topel and Ward 1992].

To highlight the inability of workers in our context to invest in their human capital, Panel B shows how skills vary by age, again using the UNHS data. By age 25, fewer than 6% of young workers make any investment in training or higher education post labor market entry. 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, and a mass of talent remains underutilized.

Our treatment offering workers vocational training relaxes credit constraints workers face in acquiring valuable skills that reduce mismatch, and our matching 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.[4]

Workers Individuals were recruited into our experiment from throughout Uganda, using an advertised offer to potentially receive six months of sector-specific vocational training at one of our partner VTIs. The eligibility criteria target disadvantaged youth. The first row of Table A1 shows applicant characteristics: 57% are men, they are aged 20, and almost none have previously

[3] In our context 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, animal rearing, fishing and agricultural day labor.

[4] 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.

received vocational training.[5]

Table 1 shows labor market histories at baseline. 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 are $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).

Panel A of Table 2 provides descriptive evidence from our sample on job characteristics, split by casual and regular jobs. 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.

Firms To draw a sample of potential employers for the matching intervention, we 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 at one of our partner VTIs; (ii) having between one and 15 employees (plus a firm owner). Our sample comprises 1281 small and medium sized enterprises, employing 3735 workers in total at baseline.[6] 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. Firms report currently being size constrained because they are unable to find: (i) skilled workers (67%); (ii) trustworthy workers (57%); (iii) unskilled workers (28%).

Job Search and Recruitment in Urban Labor Markets Panels B to D of Table 2 provide descriptive evidence on how youth in our control group normally search for jobs, and recruitment processes used by firms once the meet potential employers. 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%

[5] 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. Our sample appears well targeted towards disadvantaged youth. To see this, Table A1 compares them to those aged 18-25 in the 2012/3 UNHS data. Our sample workers are 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.

[6] 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).

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. 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.

2.2 Design

Figure 1 shows the oversubscription design of our field experiment. Applicants 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 and short term offers to match with firms in our firm-side survey sample.

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 match offer, 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 matching once vocational trainees had completed their courses. This helps avoid lock-in or threat-effects on search [Black *et al.* 2003], and also ensures match offers 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 match offer. A six month tracker survey fielded just prior to match offers being announced sheds 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.

The pairwise intent to treat comparisons we focus on to study expectations and search behavior are: (i) T1 vs C: the impact of the offer of vocational training; (ii) T2 vs T1: the differential impact of the match offer on those previously offered vocational training; (iii) T3 vs C: the impact of the match offer 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.

Upon graduation, vocational trainees receive a credible certificate verifying their new skills. As we document in Alfonsi *et al.* [2020], there are high returns in employment to having certifiable skills from reputable VTIs in these urban labor markets.

Matching The match offer is a light-touch and one-off intervention. 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 offered vocational training and those randomized out of that offer). Firms were then presented shortlists of workers that were either: (i) all vocationally trained, or; (ii) all 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.

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.[7]

To understand the salience of the matching intervention to workers, we use data from controls on the frequency of job applications made. We only collected this at the final follow up, six years after baseline. 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 period. In short, job seekers make fewer than one application per month. This highlights the high salience to workers of the

[7] 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].

match offer – that provides an opportunity for worker's details to be passed on to established firms in good sectors.

The Appendix describes in more detail how worker-firm match offers were implemented, including the exact scripts used to communicate the process to workers and firms. 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 matching 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.

The entire match offer process – from when workers are first informed of the possibility to when firms might call back a worker for interview – is typically around two weeks. The entire process was set up to ensure workers were fully informed that BRAC was not searching for jobs on their behalf. We measure short run search behavior a year after the match offers are first announced, so these impacts are not driven by any substitution of search effort between workers and BRAC.

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. This is when their prior beliefs over their labor market prospects are measured. 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 match offers being provided. 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 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.[8]

For workers involved in matching 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/matching) – corresponding to Phases 2 and 3 of the timeline shown in Figure 2.

[8] 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 matching and vocational training + matching 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.

This allows us – perhaps uniquely – to track a panel of young labor market entrants over six years, measuring their short run expectations over job offer arrival rates and expected earnings in good jobs, to underlying dimensions of search behavior such as search intensity and directed search, and mapping these to their long run labor market outcomes related to employment, earnings, hours, wages, bargaining, spells, and actual job and firm characteristics. We couple this data with measures of worker characteristics such as their cognitive ability and psychological traits, to shed light on the external validity of our findings to alternative samples along these dimensions.

Balance, Compliance and Attrition Table 1 shows baseline 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 matching, there is near full compliance in that all workers agree for their details to be passed onto potential employers. On compliance 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 matching – this is as expected because match offers are only announced upon training completion, compliance with training is independent of the expected returns from match offers; (iii) women and the more educated are less likely to comply; (iv) the correlates of compliance do not differ between those offered only vocational training and those who later also offered matching.[9]

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 Expectations

Worker expectations over their job prospects are the foundation of our analysis. We first detail expectations among controls by describing: (i) their baseline expectations over the job offer arrival rate from firms in our study sectors (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. We next zoom in to consider the evolution of these beliefs among workers in our treatment arms between baseline and the eve of any announcement of match offers being made. Finally, having

[9] 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 match offers between workers offered vocational training or not (T2 vs T3), but that is not our focus.

documented the evolution of beliefs, we consider the reaction of workers to call backs (or lack thereof) once match offers are actually made.

3.1 Expectations and Reality

Expected Job Offer Arrival Rate The first margin of beliefs relevant for job search is the expected job offer arrival rate from firms in good sectors – defined to be the eight sectors in which we offered vocational training. At baseline we asked controls what was their expected probability of finding a job in our study sectors in the next month, six months and year. The job offer acceptance rate is over 90%, so this essentially corresponds to worker beliefs over the job offer arrival rate of good jobs. The distribution of these beliefs are shown in the first three box-whisker plots in Figure 3A. 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 youth 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 by comparing 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 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.[10]

Do young workers revise their expectations as they naturally engage in job search over those two years? The next three box-whisker plots in Figure 3A show the distribution of revised expectations over job offer arrival rates at first follow-up. These are revised downwards: the median expectation 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.

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 last three box-whisker plots in Figure 3A. These are still far lower than worker expectations over the job offer arrival rate, with the divergence increasing with the time horizon considered: only 7% of workers actually find a job within a month, 10% do so within six months, and 13% do so within a year.[11]

[10] 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.

[11] Examining correlates of beliefs over job offer arrival rates, women tend to be more optimistic over all horizons,

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, Mueller and Spinnewijn 2021, Potter 2021], and in lower-income labor markets including Ethiopia [Abebe *et al.* 2021a] and South Africa [Banerjee and Sequeira 2021].

Expected Earnings The second relevant margin of beliefs is worker's expected earnings conditional on employment in a good sector job. This is central to job search models emphasizing workers learn about the wage offer distribution [Wright 1986, Burdett and Vishwanath 1988]. To establish a benchmark for these beliefs, the first two box-whisker plots in Figure 3B 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).

To measure worker's expected earnings if they were employed in the good sectors that we offered vocational training in, we elicit 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). These beliefs are derived for all controls, irrespective of their search effort or employment status, and hence are not driven by compositional changes.[12]

We asked individuals 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 measure their expected earnings. The next three box-whisker plots in Figure 3B show the distribution of minimum, maximum and expected earnings in these good jobs. We see an intuitive ranking across expectations, with greater dispersion across controls in their expected maximum earnings. Average expected earnings are higher than actual earnings from the kinds of regular work that controls engage 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 these youth recognize jobs in our study sectors are better than the kinds of work they have previously experienced.[13]

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.

[12] 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 (who might already be working in their most preferred study sector), 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.

[13] The expectation questions were introduced to respondents as follows: "For some of these questions I will ask you to estimate the possibility out of 10 that some events would occur. This means that on a scale of 0 to 10, 0 will mean surely not possible, and 10 will mean it will definitely happen. Let's practice this to be sure you have

To assess the accuracy of beliefs, the final batch of box-whisker plots takes earnings data from workers actually employed in the 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 and actual earnings of unskilled and newly hired workers in these sectors. It is as if the distribution of entry level earnings in these good sectors is almost common knowledge among labor market entrants.[14]

Search Intensity How do these expectations translate into the intensity of job search? We recognize that the notion of unemployment is somewhat vague in these urban labor markets given the prevalence of informal/casual work. Hence we define individuals as unemployed if they are not involved in any work activity. Those engaged in casual work or unpaid work in family businesses are considered employed. Panel A of Figure 4 shows that over the fours years from first follow-up, the share of youth unemployed at some point in the year falls from 90% to 70%. However, the share reporting looking for a job never rises above 60%. Panel B shows the intensive margin of search intensity: 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 they actually spend unemployed.

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. The results above showed controls 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. In sharp contrast, the above results above showed that control youth are optimistic over the job offer arrival rate from firms in our study sectors. Such persistent optimism can reduce search intensity and thus contribute to slow exit rates out of non-employment. This is key to our analysis because this margin of belief can be impacted by the match offer intervention.

the idea. On a scale of 0 to 10, what do you think is the possibility that it will rain tomorrow? On a scale of 0 to 10, what do you think is the possibility that it will rain at any time in the next year? The score for the possibility of 'rain tomorrow' should be lower than the score for 'in the next year'. If it is not, review the 0 to 10 point scale until it is clear the respondent understands before proceeding." The exact wording of the questions is as follows: "With your current skill set, what is the possibility out of 10 that you could get a job in <occupation> in the next<time period>?"; "With your current skill set, what do you think is the minimum/maximum monthly amount that you could earn in <occupation>?"; "What do you think is the possibility out of 10 that you could receive <(max-min)/2> monthly with your current skill set?"

[14] 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. 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.

3.2 The Evolution of Expectations Until Match Offers are Announced

We next zoom in on the evolution of beliefs between baseline and the eve of match offers being announced. We contrast the evolution of beliefs among those assigned to vocational training relative to controls. For those assigned to vocational training, we measure their expectations just as they complete their training course, and prior to any match offer being announced. For controls, 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 match offers 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 match offers are announced. A similar exercise could be conducted by interpolating reasonable non-linear monotonic changes in beliefs.

Expected Job Offer Arrival Rate The first set of bars in Figure 5A show beliefs of controls at baseline over the arrival of job offers from good sectors, for each time horizon. The second set of bars show the same beliefs for controls six month later, on the eve of match offers being announced. As described above, we see that although controls hold optimistic beliefs on this margin at baseline, they gradually become more realistic as they naturally search. The third set of bars in Figure 5A show that on the eve of match offers being announced, beliefs of vocational trainees have moved sharply in the *opposite* direction to controls: 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 at all in the interquartile range of beliefs among the two groups of workers. For example, at the point of graduation, the median trainee believes they will receive a job offer in their most preferred good sector with a probability of .9 in the next twelve months; 25% of trainees believe this will occur with probability one.[15]

To formally test differences in mean beliefs between workers in treatment arms over time, Column 1 in Table 3 shows the expected job offer arrival rate, pooling 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 match offers 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 over time (Row 1=Row 3, Row 2=Row 4). Column 1 of Table 3 shows that beliefs over the job offer arrival rate: (i) significantly rise among those assigned to vocational training (Row 1 = Row 3); (ii) significantly fall among those randomized out of vocational training (Row 2 = Row 4). On the eve of match offers being announced, beliefs on job offer arrival rates

[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.

thus significantly differ between workers offered vocational training and those that are not (Row 3 = Row 4).

How realistic are these updated beliefs of newly trained workers on the eve of match offers being announced? We can benchmark them in two ways. 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 only rises to 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 5A 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 can consider these trained workers as remaining overly optimistic or exuberant over the job offer arrival rate from good sectors at the time they graduate, and any match offers announced.

Expected Earnings We next consider the evolution of expectations over the earnings distribution in our study sectors. Figure 5B shows the distribution of beliefs youth hold over the minimum and maximum expected earnings from being employed in their most preferred sector. We show this for: (i) all workers at baseline; (ii) controls on the eve of match offers being announced; (iii) graduating vocational trainees, on the eve of match offers being announced. Comparing the first two sets of bars we see that for controls, beliefs over the earnings distribution 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.

The third set of bars show that 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. How realistic are these upward revisions to expected earnings? Expected mean earnings rise by 41% (with similar percentage increases in expected

[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 because of the eligibility criteria used. On the other hand the kinds of VTIs they attend are higher quality than the average VTI. 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).

minimum and maximum expected earnings). In Alfonsi *et al.* [2020] we show the actual returns to vocational training are between 20 and 30%, so workers are slightly optimistic about these returns.

Columns 2 to 4 in Table 3 formally test differences in means of these distributions between workers in treatment arms or over time. We see that: (i) at baseline there are no significant differences in expected earnings across workers assigned to vocational training or not (Row 1=Row 2); (ii) there are no significant changes in expected earnings over time among workers randomized out of vocational training (Row 2 = Row 4); (iii) there are significant changes in expected earnings over time among workers assigned to vocational training (Row 1 = Row 3);(iv) hence, in line with the patterns shown in Figure 5B, on the eve of match offers being offered, there is a significant bifurcation of beliefs between those offered vocational training and those randomized out of it (Row 3 = Row 4).

To probe how uncertainty over earnings in good sectors changes over time, we construct the coefficient of variation as a measure of the dispersion of expected earnings (again assuming a triangular distribution). This is shown in Column 5 of Table 3. From baseline to the eve of match offers being announced there are relatively minor changes in uncertainty among those assigned to vocational training (Row 1 = Row 3). Hence on the eve of match offers being announced, the precision of beliefs over expected earnings does not differ significantly between those with and without the offer of vocational training (Row 3 = Row 4).

3.3 Call Backs and their Determinants

For workers offered matches to firms in good sectors, the key outcome is whether they receive a call-back, i.e. an invitation to meet the firm owner. The entire process from when match offers are announced until when workers are invited to interview is around two weeks (although workers never called back would obviously only later realize this).

The call back rate tightly relates to the job offer arrival rate. On the eve of match offers being announced, this is a margin of belief over which vocational trainees are increasingly optimistic or exuberant, while those not assigned to vocational training are slowly becoming more realistic.

How do actual call back rates compare to worker's prior beliefs in each treatment arm? As Figure 5A shows, on the eve of match offers 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, in the two weeks from match offers being announced and firms responding, 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. 18% of unskilled workers actually receive a call back, confirming their prior.

To understand how the average worker in each treatment arm might react to these call back rates, we need to be precise on the *actual* 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 matching intervention almost fully removes the possibility that worker characteristics determine call backs.[17]

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 The matching intervention was clearly explained – using fixed scripts – to workers and firms, as detailed in the Appendix. Given the wording, workers were fully aware their details were being handed over to only a few good firms, and those firms would be within a small geographic area of their residence. 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, 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 many 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 expectations and search strategies of workers – irrespective of whether they have earlier been vocationally trained or not (T2 vs T1, T3 vs C) – are unaffected by a single match offer.

An alternative view is that the match offer would be highly salient to workers because: (i) it involves a reputable NGO such as BRAC – perhaps especially so among those workers that were completing BRAC sponsored vocational training; (ii) as described earlier, workers typically submit less than one job application per month and rarely have the chance for their details to be presented to firm owners in good sectors. In these labor markets where job search often involves

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

workers trying to approach firms informally, the match offer 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 seriously consider their credentials.

Our alternative hypothesis is thus that some workers are imperfectly informed and misinterpret what drives call backs in the experiment. 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, and trainees remain optimistic over their prospects as they graduate (Figure 5); (ii) there are no market substitutes for the match offer intervention, and so the intervention, even though light touch, is viewed as a highly salient opportunity for them to find meaningful work. Under this alternative, the low call back rates from match offers generate bad news for the average trained worker.

While we do not have data to micro-found such 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 match offers (T2 vs. T1), under this alternative a key distinction is that trained workers with match offers 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 match offers 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 expectations and 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. How they respond to this is ultimately an empirical question, that we now turn to.

4 Skills, Expectations and Search Behaviors

4.1 Empirical Method

We analyze how the offer of vocational training with and without match offers impact skills, expectations and underlying search behaviors. Expectations and search impacts are measured at first follow-up, 24 months after baseline and a full year after trainees have graduated and any 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; (ii) $\beta_2 - \beta_1$ (T2 vs T1): the differential impact of matching on those offered vocational training relative to those only offered vocational training; (iii) β_3 (T3 vs C): the impact of match offers on those randomized out of the offer of vocational training.[18]

4.2 Preliminaries

Sector Specific Skills Skills acquisition can have direct impacts on outcomes over and above any effect through expectations and search behavior. In our earlier work using data from this project, Alfonsi *et al.* [2020], we showed how the offer of vocational training translates into human capital accumulation. We discuss those results in more detail in the Appendix. Here we briefly reiterate the main findings and extend them to also shows impacts on skills for those offered matching. We measure individual skills using a sector-specific skills test we developed in conjunction with skills assessors and modulators of written and practical occupational tests in Uganda. The test was conducted on all workers (including controls) at second and third follow-up, so measuring persistent skills accumulation. There is no differential attrition by treatment into the test. The main results (reported in Table A6) are: (i) workers offered vocational training significantly increase their measurable skills by 21% (or $.29\sigma$ of test scores); (ii) estimating an ATE on sector specific skills acquired, among those that take-up training, skills accumulation increases by 28% over controls (or $.37\sigma$ of test scores).[19]

The novel findings here shed light on whether match offers have additional impacts on skills. We find that: (i) workers offered vocational training and matching have no different skills accumulation

[18] 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.

[19] 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.

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 match offers.

Two key implications follow. First, the offer of vocational training translates into real changes in human capital accumulation. Our experiment thus allows us to study how the acquisition of valued labor market skills impact expectations and job search. Second, exposure to match offers does not change skills accumulation. Hence, when we later compare long run labor market outcomes between vocational trainees with and without match offers, those results will not be mediated through skills differences between the groups.

Other Dimensions of Human Capital Table A7 shows offers of vocational training or matching 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 matching; (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 matching. This battery of results helps rule out our findings on long run labor market outcomes are mediated through these margins. We later exploit the time invariance of these traits to shed light on the external validity of our findings if they were to be extended to alternative samples of job seekers.

4.3 Expectations

We present findings on how the interventions impact expectations for all workers irrespective of their employment status, ensuring results are not driven by composition effects. Table 4 shows how worker expectations over their own labor market prospects respond to labor market interventions. Starting with beliefs over the job offer arrival rate, Column 1 shows a full year after training is completed, those offered vocational training revise upwards their belief on this margin (by 1.84 on a 0-10 scale). Columns 2 to 4 show treatment effects on the other key margin of expectations: expected earnings if workers were able to transition into their most preferred study sector job. Among 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. Column 5 shows there is no overall change in the dispersion of expectations as measured by the coefficient of variation.

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

The next row shows impacts on the expectations of those offered vocational training but who were, a year earlier, additionally provided match offers. 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 matching. We see that workers additionally offered matching significantly revise down their beliefs over the job offer arrival rate in good sectors, despite them being as skilled as those without match offers ($p = .082$). They also have lower expected earnings from working in these good sectors – this difference is most pronounced at the minimum expected earnings ($p = .095$). Workers additionally offered matching also hold significantly less precise beliefs over earnings relative to those only offered vocational training ($p = .036$).

The following points are of note. First, over the same one year time period, there are no differences in labor market outcomes between workers in these treatment arms. To see this, Table A8 summarizes short run labor market treatment effects at first follow up. We see no short run divergence in outcomes between those offered vocational training with and without match offers. 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, earnings conditional on employment, self-employment, the quality of firms employed at as measured through an index of firm characteristics, or the quality of jobs performed.[20] This evidence suggests there is limited scope for feedback effects from short run labor market outcomes driving differences in expectations and search behavior between vocational trainees with and without match offers.

Second, the evidence thus suggests youth offered vocational training and matching are *discouraged* relative to youth only offered vocational training as measured by the margins of expectation shown in Table 4. On four out of five dimensions of belief, the general significant and downward revisions of beliefs for workers offered matching on top of vocational training, is in line with the alternative hypothesis, that the low call back rates they experience from match offers represent bad news for them relative to their prior expectation at the time they completed vocational training (Figure 5). This is in contrast to those only offered matching. The third row of Table 4 shows ITT estimates on the expectations of this group (relative to controls). Their beliefs over the job offer arrival rate and expected earnings and unaffected. This is in line with the rate of calls backs among this group of (unskilled) workers being in line with their prior expectation.

4.4 Is This Really Misattribution?

We have no direct measure of workers misattributing information from the lack of call backs in the matching intervention. An alternative explanation is that low call back rates might cause workers

[20]We construct the firm 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. 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. Both indices is scaled so treatment effects can be interpreted as effect sizes.

to revise beliefs about the state of labor demand in aggregate. Hence their changed expectations might reflect beliefs over market conditions, not their own prospects. To narrow the interpretation that in response to match offers, we elicited worker beliefs over the following aggregate labor market conditions: (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 also combine these into a single index.

Table 5 shows how the treatments impact each component of the labor market beliefs index. For no treatment group do we find significant changes in beliefs for any dimension of labor market conditions, and there are no significant impacts on the market beliefs index overall either. This reinforces the notion that workers respond to the information generated through match offers by updating their beliefs over their own prospects, not their beliefs over aggregate labor market conditions.

4.5 Search Behaviors

We can underpin these changes in expectations along both margins by examining specific changes in search behavior. We thus build on much of the earlier work on expectations and search by directly measuring expectations and search behavior, rather than inferring one from the other [Mueller and Spinnewijn 2021]. The evidence already strongly hints at underlying search behaviors being impacted by the interventions: for example, in many job search models, the minimum expected wage helps pin down the reservation wage of a worker (because a potential employer would not make an offer she knows will be rejected). The fact that this proxy for the reservation wage shifts upward with the offer of vocational training strongly suggests workers are adjusting actual search behaviors.

A potential explanation for why those offered vocational training and matching revise down their beliefs on the job offer arrival rate from firms in good sectors relative to those only offered vocational training is because that they search for jobs less intensively. We provide direct evidence on this below. Why would the average skilled worker also offered matching revise down their beliefs on wages *conditional* on obtaining a job in a good sector relative to those only offered vocational training? Two potential overlapping explanations are: (i) they direct their search towards lower quality firms and jobs; (ii) they revise down their belief on the returns to their ability or skills in good jobs. Our data allows us to provide direct evidence on (i) below.

4.5.1 Search Intensity

It is natural to suppose the intensity of job search is an important driver of job offer arrival rate. Endogenous search intensity is a key extension of the canonical search model [Pissarides 2000, Shimer 2004]. There are income and substitution effects of higher expectations over the job offer arrival rate onto changes in search intensity: if the income effect dominates then higher expectations should map to higher search intensity. If the substitution effect dominates because workers believe the returns to a given level of search intensity are higher, then higher expectations will map to lower search intensity. Of course, it might be reasonable that among disadvantaged youth entering the labor market where the value of employment is far higher than non-employment, the income effect dominates. At the same time, there is evidence among US job seekers that workers underestimate the returns to search [Spinnewijn 2015].

We examine how our interventions affect various types of search intensity by first considering the extensive margin of search. The result in Column 1 of Table 6 shows that workers offered vocational training are, relative to controls, significantly more likely to report having actively searched for a job. The income effect thus dominates and the magnitude of the effect is of economic significance: these workers increase the likelihood of searching 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 (Column 3).[21] 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.

Along all these margins of search intensity, we do not find any evidence that the substitution effect dominates, consistent with workers not updating that the returns to search have increased – rather the income effect dominates and the expected job offer arrival rate rises because search intensity has increased.

We combine all these margins 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 6 shows this index of search behaviors rises significantly for those offered vocational training by $.089\sigma$.

For the match offer interventions, recall that there are only two weeks from their announcement and most call backs occurring (or not). Hence at first follow up, a year after the interventions are completed, these changes in search intensity are not driven by worker's effort being in any way substituted by BRAC.

Workers additionally offered matching 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

[21] 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].

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 ($p = .053$). This aligns with the earlier result that the expected job offer arrival rate for jobs in good sectors is significantly lower for those offered vocational training and matching relative those only offered vocational training (Table 4, Column 1).

Finally, workers only offered matching do not change search behavior among most margins except reporting spending fewer days actively searching for work.

4.5.2 Directed Search

A natural explanation for why workers revise their beliefs on wages *conditional* on obtaining a job in a good sector are because they direct their search towards particular firms and jobs. Directed search is exactly 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 see if such search behavior is impacted by labor market interventions, we asked workers about characteristics of the *ideal* firm and *ideal* job they were searching for. 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 index is scaled so that treatment effects are interpreted as effect sizes. The treatment effects on the ideal firm index are shown in Column 1 of Table 7: we see evidence that workers offered vocational training significantly 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 A9 shows the firm characteristics driving this: these workers search for firms that can provide training and other material benefits.

Of course the change in directed search towards better firms might also help explain their revised beliefs on the job offer arrival rate, if the rate of job offers is higher from higher quality firms.

Workers additionally offered matching a year earlier search for firms that are no different to those targeted by control workers, and they are borderline significant to firms targeted by those only offered vocational training ($p = .102$). These differences in directed search tie closely to the differences in earnings expectations conditional on employment in a good sector in Table 4.

In contrast we see no differences across treatment arms in the ideal job workers seek. Table A10 confirms that no component of the ideal job searched for index shifts.[22]

[22] 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.

4.5.3 Credit

A final dimension of search behavior we consider builds on the idea that labor and credit markets are interlinked [Lentz and Tranaes 2005, Lise 2013].[23] 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 7, with Table A11 showing the impacts on each component.

We see that for those offered vocational training – with or without match offers – 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 matching: their overall credit index rises significantly (.090σ). 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).

This is another suggestion that the lack of call backs from the matching intervention serves to concretize and crystallize unskilled workers' low expectations of finding a modern wage job of the type vocational training institutes prepare individuals for. We assess below whether their stated intention of borrowing for self-employment – as measured a year after matching is offered – actually translates into higher rates of self-employment in the long run.

5 Labor Market Outcomes

The six-year study period allows us to map out how offers of vocational training and matching translate into labor market outcomes in the long run, and ultimately how these are mediated through changes in expectations and search behavior. 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 match offers. 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 in strata s in survey wave t:

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

[23] 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).

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).[24]

5.1 Employment

We begin in Table 8 by tracking standard measures of employment, and transitions into regular work. The first row shows the long run impacts of the offer of vocational training. 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 matching 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 ($p = .104$).[25]

Linking these results back to those on expectations highlights the plausibility of overoptimism driving the search for good jobs. Specifically, we note the difference in expected job offer arrival rates between those offered vocational training with and without match offers (and accounting for the fact that this is on a 0-10 scale) was (1.84-1.45)/10 = .039 (Table 4, Column 1). Contrasting this with the actual differential likelihood of these two groups of youth finding a good job (Table 8, Column 3) is $.113 - .066 = .047$, which is of the same order of magnitude.

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

[25] 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.

The final row of Table 8 shows outcomes for those only offered matching. Relative to controls, their employment outcomes improve significantly along both extensive and intensive 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 matching.

5.2 Earnings, Bargaining and Spells

Earnings are a second key labor market outcome to consider. Column 1 of Table 9 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 8). Examining next earnings impacts for workers offered vocational training and matching, 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.

At first sight it is slightly puzzling how, among those offered vocational training, the additional match offer has more pronounced impacts on employment outcomes (Table 8) than on earnings, despite the documented differences in expectations and 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 9 shows treatment effects on this bargaining index. Only workers in one treatment arm are impacted: those offered both vocational training and matching, 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.[26]

Why would only those offered vocational training and matching many years earlier bargain 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 (Tables A6 and A7).

Rather, our results offer the novel possibility that the search process itself might influence how

[26] 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.

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 9 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 matching, 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, those offered vocational training and matching end up 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.[27]

5.3 Sorting into Jobs, Firms and Self-Employment

Our final batch of outcomes consider how our interventions impact labor market sorting. 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 7). 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 10.[28]

The first row shows 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 matching up to five years earlier, they end up in jobs not significantly different to those for controls. Their job index 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 matching intervention.[29]

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

[28] Individuals who do not have a job are excluded from Columns 1 and 2. All our indices allow for missing values on some of outcomes, with outcomes being re-weighted to account for this.

[29] Our results complement earlier findings from field experiments in low-income settings that job assistance raises

The last row of Table 10 shows that workers with match offers 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 matching, realized firm quality is significantly lower than those that were only offered vocational training ($p = .035$); (ii) indeed, vocational trainees with matching end up at firms of lower quality than controls; (iii) those only offered matching 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 matching 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 match offers 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. 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].

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 10 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.[30]

For workers only offered matching, 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 match offers on their expectations and search behavior was for them to start borrowing to start up in self-employment.

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

job quality, although most of these have done so on narrower dimensions of job quality and over a shorter horizon [Beam, 2016, Franklin 2018].

[30] 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.

transfers to underemployed youth fade over time [Blattman *et al.* 2020, Abebe *et al.* 2021b]. In contrast, our findings emphasize that initial conditions upon labor market entry have persistent impacts on the outcomes of youth: the skills and expectations workers have when entering the labor market matter at least six years later. Among those offered vocational training and matching, the discouragement caused by a lack of call backs effectively scars these youth. The opposite is the case for workers only offered matching: for them the lack of call backs confirms their labor market prospects and causes them to successfully borrow for self-employment.

6 Linking Outcomes to Expectations and Search Behavior

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 expectations and job search behavior. 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 10. 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 matching ($p = .001$), for whom the index rises by less than half the amount ($.051\sigma$). In short, the impacts of matching 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 matching is not significantly different to controls.

To see how skills, expectations and search behaviors contribute to these impacts, we consider the following set of mediators: the measured sector-specific skills of individuals, the expected job offer arrival rate of a job in their preferred good sector in the next year, the reservation wage as measured by the minimum expected earnings conditional on employment in a good sector job, search intensity as proxied by whether they have actively searched for a job in the last year, directed search in terms of the ideal job and firm indices, 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 10. 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. Expectations also play a prominent role: the expected job offer arrival rate explains 8% of the long run impact, and the minimum expected earnings from employment in a study sector explains a further 10%.

Once skills and expectations on both margins are accounted for, search behaviors related to search intensity, directed search or credit play relatively muted roles. This reinforces the idea that these search behaviors are driven by changes in expectations, and have little independent impact on long run outcomes.

Among workers additionally offered matching, sector-specific skills and expected earnings 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 matching. 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 behaviors play less of a role in determining the long run labor market success of those offered both vocational training and matching – the reason being that these workers are discouraged in a variety of dimensions, and so end up with search behaviors closer to controls overall.

For workers only offered matching, no single mediator is prominent, although borrowing has a positive effect.[31]

Taken together these results provide novel evidence on how expectations and underpinning search behaviors mediate the impacts of labor market interventions related to the provision of skills and/or the offer of matching 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

[31] 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 A8 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 expectations and job search strategies induced across workers, not the inherent quality of first jobs/firms experienced.

on the role that multiple dimensions of expectations and search behaviors play.

7 Discussion

7.1 Reconsidering Alfonsi et al. [2020]

How do the results in this paper inform our earlier work, Alfonsi *et al.* [2020], where we focused on the long run impacts of vocational training on labor market outcomes? In that paper we pooled together workers offered vocational training with and without match offers, and documented the long run private return from the offer of vocational training was around 20-30%. A key mechanism we highlighted driving these gains was certifiability of skills gained through vocational training: this enabled youth to increase their labor market mobility in terms of unemployment to employment transitions, and employment to employment transitions.

The findings from the current analysis buttress those earlier results in reconfirming the importance of the certifiability of skills. To see this note that workers offered vocational training – with and without match offers – both do significantly better than controls (the overall labor market index for those with match offers significantly rises by $.051\sigma$ relative to controls). The certifiability of skills still plays the driving role in this difference to controls. Moreover, the mediation analysis reconfirms that certifiable skills are the most important mediator for long run labor market success (Figure 6), and they play an equally important role for those offered vocational training with and without match offers. The current analysis builds on this insight from Alfonsi *et al.* [2020] by demonstrating the foundational role that expectations play in the process of job search: expectations shift in response to labor market interventions, making workers exuberant or discouraged. In turn these expectations drive search behaviors along multiple margins, and skills, expectations and search behaviors all mediate long run labor market success for youth.[32]

7.2 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.

[32] In our earlier work our focus was on contrasting the returns to vocational training versus firm-sponsored training. In the current analysis we have not considered those workers assigned to firm-sponsored training because their search behaviors will be endogenously determined by their experience as apprentices within firms. It remains an open question to understand how apprenticeships shape expectations and search behaviors of youth once they leave the firm they originally receive training from.

7.2.1 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, expectations, job search behaviors and long run labor market outcomes.

Our match 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. Although unusual, these present opportunities that workers would like and have considered.

Natural alternatives to the kind of match offer we have studied are to provide information directly to workers 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].[33] 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, careers advice or counselling, because what is perceived by young job seekers and how their expectations are shaped, matters as much as what is actually presented to them [Babcock *et al.* 2012].

[33] 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.

7.2.2 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 would apply if 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].[34] 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.[35,36]

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).

[34] 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.

[35] 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].

[36] 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 ($p = .600$). Hence even within treatment arms involving matching offers, we find no evidence that low ability workers respond less than high ability workers ($p = .667$). Across treatment arms and within high and low ability individuals, we continue to find significant differences in the labor market success of those offered vocational training with and wiithout matching ($p = .099, .011$).

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 likely understood the nature of match offers – 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 homogeneous 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. Again, across treatment arms and within high and low self-evaluation individuals, we continue to find significant differences in the labor market success of those offered vocational training with and wiithout matching ($p = .004, .016$). This suggests the response to call backs does not depend on notions of self-evaluation, and that misattribution of information generated from call backs is a phenomena applying to workers irrespective of their underlying appraisal of their own worthiness, effectiveness, and capability.

7.2.3 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 match offers are implemented. An alternative approach to raise call back rates in light-touch matching 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.

7.3 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 match offer 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 four broad implications for the design and targeting of interventions that provide skills and/or match offers.

First, the value of vocational training operates both through giving workers higher skills that are valued in these labor markets, but also by changing their expectations – making them exuberant with regards to their job prospects. This drives them on to be willing to search more intensively, approach firms directly, and target higher quality firms. These changes in expectations and search behavior alongside the skills acquired, drive forward their long run labor market outcomes and aid their transition out of the sea of casual jobs in these urban labor markets, into more regular jobs. That there are positive returns from exuberance is not *ex ante* obvious. Genicot and Ray [2017, 2020] develop a theoretical framework in which raised aspirations can lead to worse outcomes if those raised goals are not reached and lead to frustration.

Second, given labor market entrants have biased beliefs, 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 match offers and discouraged as a result. In the long run, those offered vocational training without match offers progress further up the job ladder than those also provided match offers. Among those randomized out of vocational training – unskilled workers – the opposite is true: match offers that credibly confirm 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.

Third, 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].[37]

8 Conclusion

Labor markets play a critical role in driving the process of economic development. We have studied how young labor market entrants search for jobs in the context of a youthful economy in Sub Saharan Africa. We show how standard labor market interventions impact multiple dimensions of expectations and search behavior, and mediate long run labor market outcomes. Our analysis sheds light on the fundamental mechanics of the process through which young people can transition from the kinds of casual and itinerant work that they are usually reliant on, towards more regular and formal work. This transformation is central to building state capacity and driving forward economic development [Besley and Persson 2014, La Porta and Schleifer 2014, Jensen 2019].

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 expectations and search behavior to the interventions. 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, Mueller and Spinnewijn 2021, Potter 2021]. Our results point to the formation of expectations over one's job market prospects depending critically on the skill level of workers, and how and when those skills were acquired. Doing so will be critical to advancing our understanding of what are likely to be the most effective labor market policies to help large cohorts of young workers find good jobs in urban labor markets in the developing world.

[37] 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.

A Appendix

A.1 Implementation of the Matching Intervention

The match offer 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 Skills

Sector Specific Skills 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). 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 controls) at second and third follow-up, so measuring persistent skills accumulation. There is no differential attrition by treatment into the test.[38]

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 later. 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 test scores). In Alfonsi *et al.* [2020] we estimate the steady state labor market returns to these skills to be 20-30%.[39]

The Table also sheds light on whether match offers have additional impacts on skills. We see that: (i) workers offered vocational training and matching 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 match offers.

There are two key implications of these results. First, the offer of vocational training translates

[38] 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).

[39] 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.

into real changes in human capital accumulation. Our experiment thus allows us to study how the acquisition of valued labor market skills impact expectations and search behavior. Second, exposure to match offers does not change skills accumulation. Hence, when we later compare long run labor market outcomes between vocational trainees with and without match offers, those results will not be mediated through skills differences between the groups.

A.3 Research Ethics

Following Asiedu *et al.* [2021] we discuss research ethics. On policy equipoise, both vocational training and matching 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 matching. 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 matching component of the program. BRAC had been matching workers to firms for apprenticeship programs for some time prior to this study. The matching 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
	(1)	(2)	(3)	(4)	(5)	(6)
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**
N=390	(.032)	(.023)	(.013)	(.029)	(1.26)	(2.80)
	[.985]	[.185]	[.761]	[.263]	[.062]	[.039]
Vocational Training + Matching	.360	.149	.050	.205*	5.25	15.1
	(.034)	(.026)	(.015)	(.030)	(1.20)	(3.01)
N=307	[.694]	[.228]	[.255]	[.065]	[.808]	[.945]
Matching	.367	.127	.057	.251	5.56	15.2
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 me characteristic, where standard errors are derived from an OLS regression of the characteristic of interest on dummy variables for the treatment groups. All regress dummies and a dummy for the implementation round. The comparison group in these regressions are Control workers. Robust standard errors are reported throughou 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 assi 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 drop 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 ca any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching wate 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 v 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 mor deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary 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. 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. 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. 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. 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

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

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 4 and 5 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 matching being announced; (iii) between individuals not assigned to Vocational Training at baseline and on the eve of matching being announced; (iv) between individuals assigned and not assigned to Vocational Training at the eve of matching being announced.

Table 4: Expectations Over Own Job Prospects

OLS regression coefficients, robust standard errors in parentheses

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

	Job Offer Arrival Rate	Expected Earnings Conditional on Employment [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	1.84***	17.7***	31.8***	25.4***	-.002
	(.205)	(3.06)	(4.85)	(4.37)	(.005)
	{.000, .001}	*{.000, .001}*	*{.000, .001}*	*{.000, .001}*	*{.661, 881}*
Vocational Training + Matching	1.45***	12.0***	23.6***	17.9***	.009
	(.217)	(3.28)	(5.37)	(4.67)	(.006)
	{.000, .001}	*{.000, .002}*	*{.000, .001}*	*{.000, .001}*	*{.108, .282}*
Matching	.242	3.21	6.04	3.47	-.000
	(.216)	(3.05)	(4.97)	(4.44)	(.007)
	{.261, .286}	*{.327, .297}*	*{.222, .236}*	*{.414, .449}*	*{.995, .986}*
P-value: VT = VT + Matching	*[.082]*	*[.095]*	*[.129]*	*[.105]*	*[.036]*
Mean in Control Group	4.19	42.9	72.5	57.8	.107
N. of observations	1,171	952	946	801	797

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 2 to 5 refer to the workers' expected earnings in their preferred sector among the eight study sectors. In Columns 4 and 5 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 2 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. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + matching.

Table 5: Expectations Over Labor Market Conditions

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	Market beliefs index
	(1)	(2)	(3)	(4)	(5)
Vocational Training	-.045	.014	-.016	-.038	-.048
	(.037)	(.036)	(.037)	(.036)	(.046)
	{.201, .398}	*{.698, .886}*	*{.690, .883}*	*{.297, .496}*	*{.305, .603}*
Vocational Training + Matching	-.058	.027	-.039	-.031	-.054
	(.041)	(.040)	(.040)	(.040)	(.052)
	{.141, .398}	*{.500, .850}*	*{.313, .665}*	*{.430, .496}*	*{.301, .603}*
Match Offer	-.026	.017	-.004	-.054	-.039
	(.041)	(.041)	(.041)	(.040)	(.053)
	{.505, .539}	*{.673, .886}*	*{.918, .926}*	*{.181, .414}*	*{.441, .603}*
P-value: VT = VT + Matching	*[.749]*	*[.752]*	*[.569]*	*[.873]*	*[.907]*
Mean in Control Group	.581	.592	.441	.438	.028
N. of observations	1,227	1,228	1,229	1,228	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 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. In Column 5 the outcome is an index of these worker's labor market beliefs, 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 + matching.

Table 6: Search Intensity

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 + Matching	.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}*
Matching	-.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 + Matching	*[.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 + matching.

Table 7: Directed Search

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

	Ideal Firm Searched For (1)	Ideal Job Searched For (2)	Credit Index (3)
Vocational Training	.103***	-.054	.040
	(.036)	(.040)	(.049)
	{.004, .013}	*{.169, .313}*	*{.410, .651}*
Vocational Training + Matching	.030	-.022	-.035
	(.039)	(.041)	(.043)
	{.454, .480}	*{.605, .593}*	*{.420, .651}*
Matching	.042	-.064	.090*
	(.039)	(.042)	(.048)
	{.311, .480}	*{.139, .303}*	*{.066,.190}*
P-value: VT = VT + Matching	*[.102]*	*[.465]*	*[.133]*
Mean in Control Group	-.046	.020	-.021
N. of observations	1,215	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. In Column 1 the Ideal Firm Searched For Index has the components in Columns 1 to 5 of Table A9. In Column 2 the Ideal Job Searched For 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 + matching.

Table 8: 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 + Matching	.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}*
Matching	.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 + Matching	*[.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. Regular jobs include all other jobs that are not in the list of casual jobs. In Column 5 the eight study sectors are: motor-mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical wiring and welding. The dependent variables in Columns 3 to 5 exclude casual work. In Column 6 the Employment 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 + matching.

Table 9: 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 + Matching	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}*
Matching	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 + Matching	*[.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 5 and 6, 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 + matching.

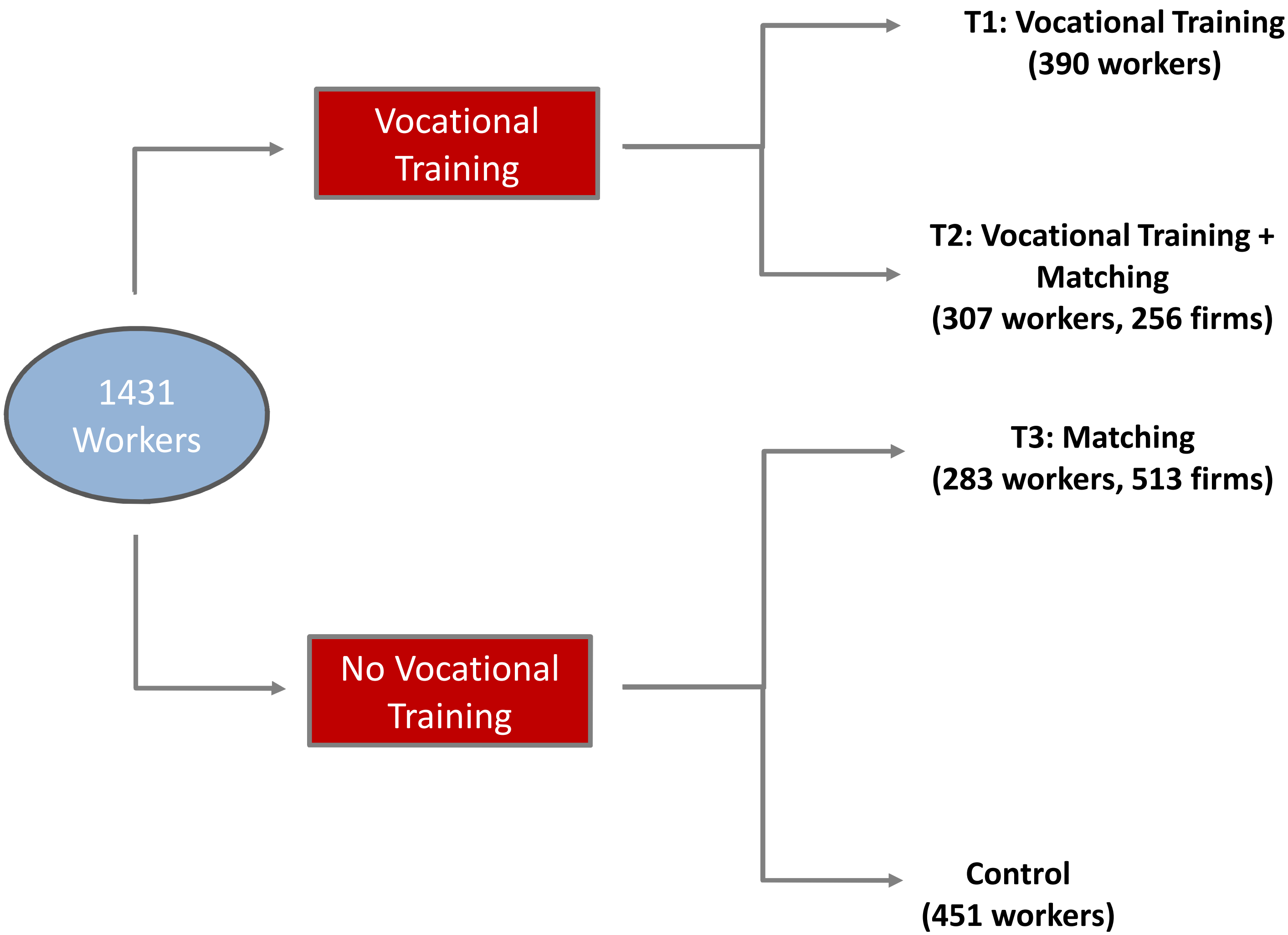
Table 10: Realized Jobs, Realized Firms and Self-Employment

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 + Matching	.042	-.058*	.076***	.051***
	(.032)	(.031)	(.015)	(.020)
	{.202, .349}	*{.069, .106}*	*{.000,.001}*	*{.014, .021}*
Matching	-.013	-.067**	.040***	.020
	(.030)	(.031)	(.013)	(.018)
	{.683, .672}	*{.021, .079}*	*{.004, .002}*	*{.288, .273}*
P-value: VT = VT + Matching	*[.077]*	*[.035]*	*[.100]*	*[.001]*
Mean in Control Group	-.025	.045	.061	-.042
N. of observations	2,429	2,504	3,699	3,725

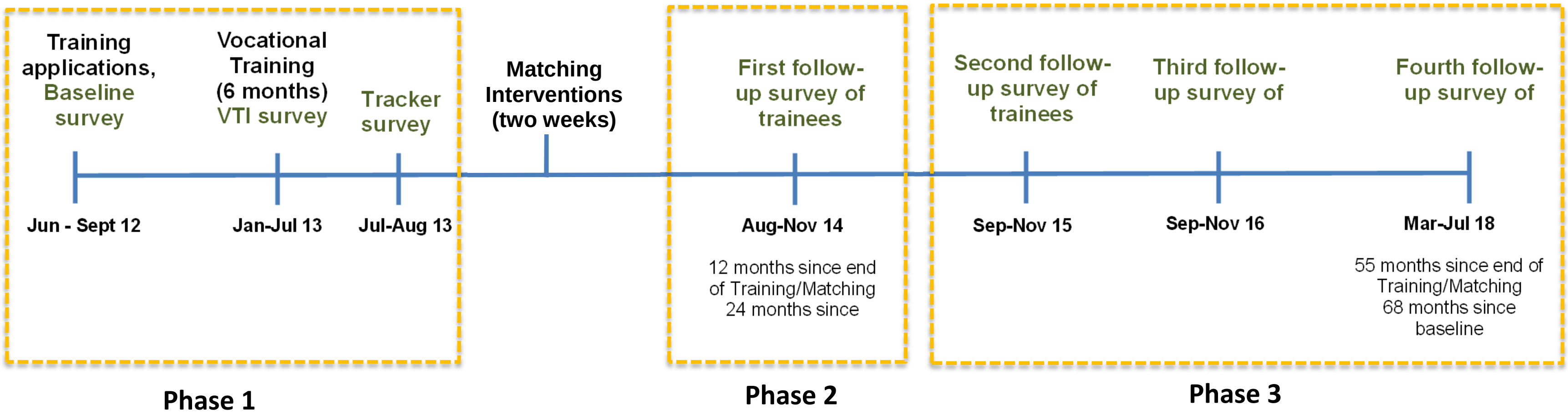
Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the second, third and fourth worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a 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 Labor Outcomes 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 + matching.

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.

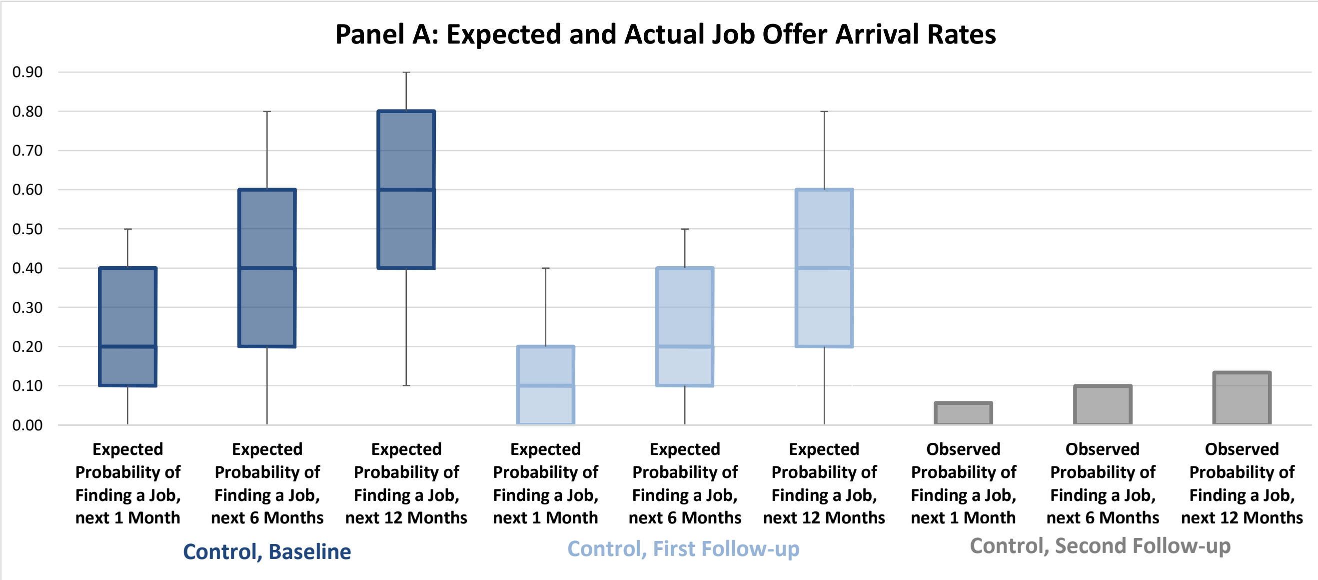
Figure 2: Timeline of Worker Surveys and Interventions

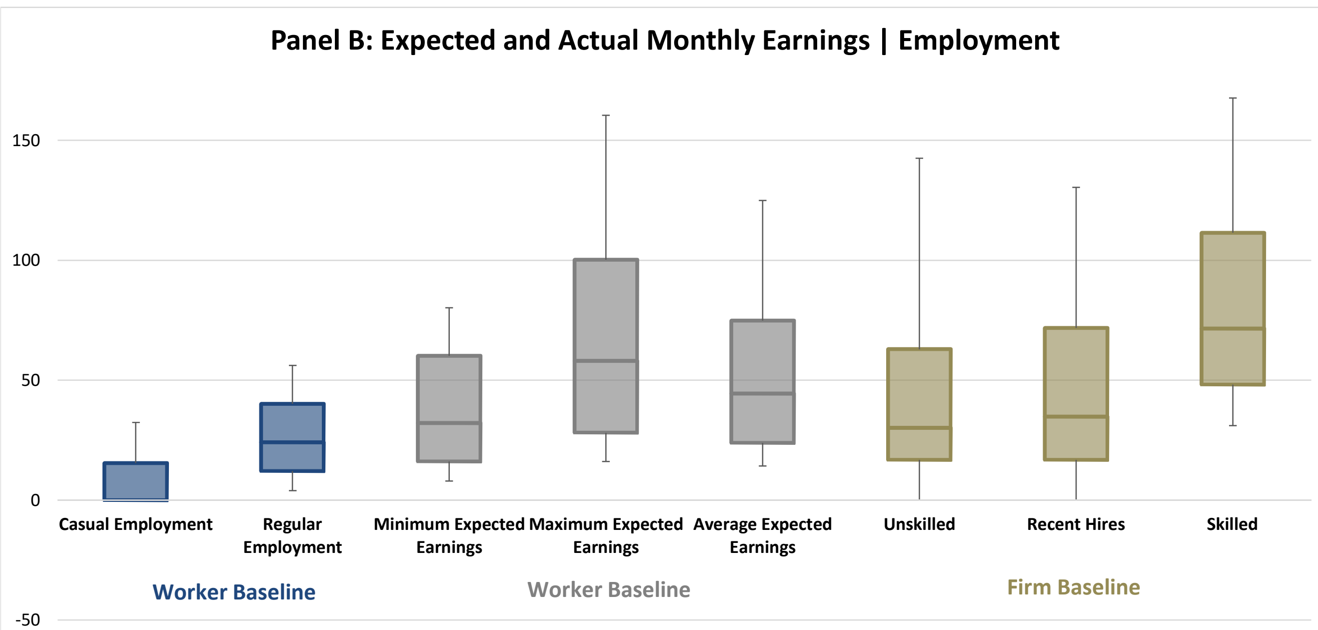


Notes: The timeline highlights the relevant dates for the main batch of workers and worker surveys. A second smaller round of applications and baseline surveys (17% of the overall sample) were conducted in May and June 2013. The majority of trainees from the first round of applicants started training 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, Matching and Vocational Training + Matching interventions, in line with the two batches of first round trainees from the vocational training institutes. The first round of the Untrained, Matching and Vocational training + Matching interventions took place in August-September 2013 (with each Matching intervention taking around two weeks from start to finish for a given worker). The second round took place in December 2013-February 2014.

Figure 3: Expectations Among Controls

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

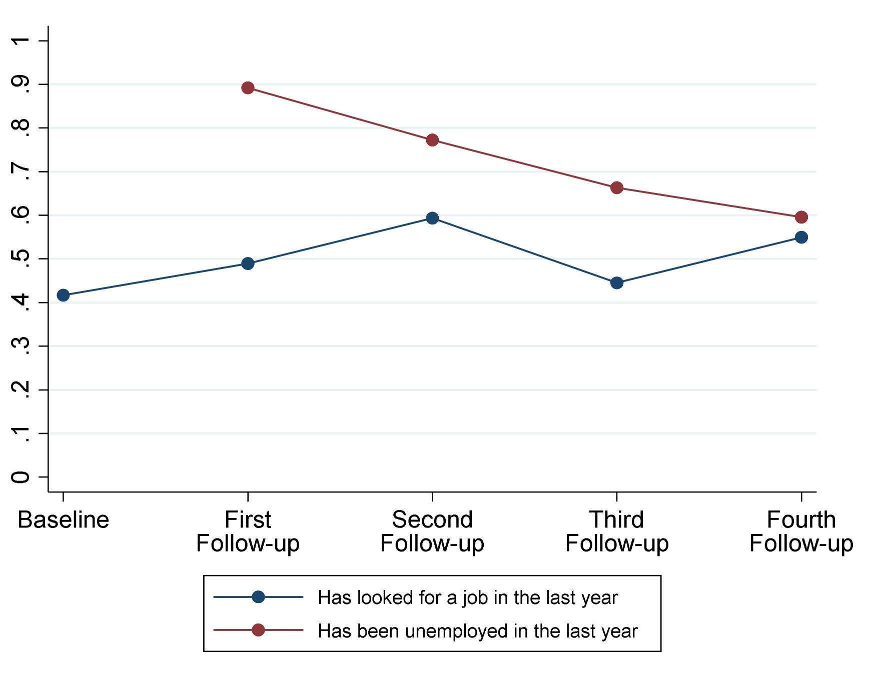




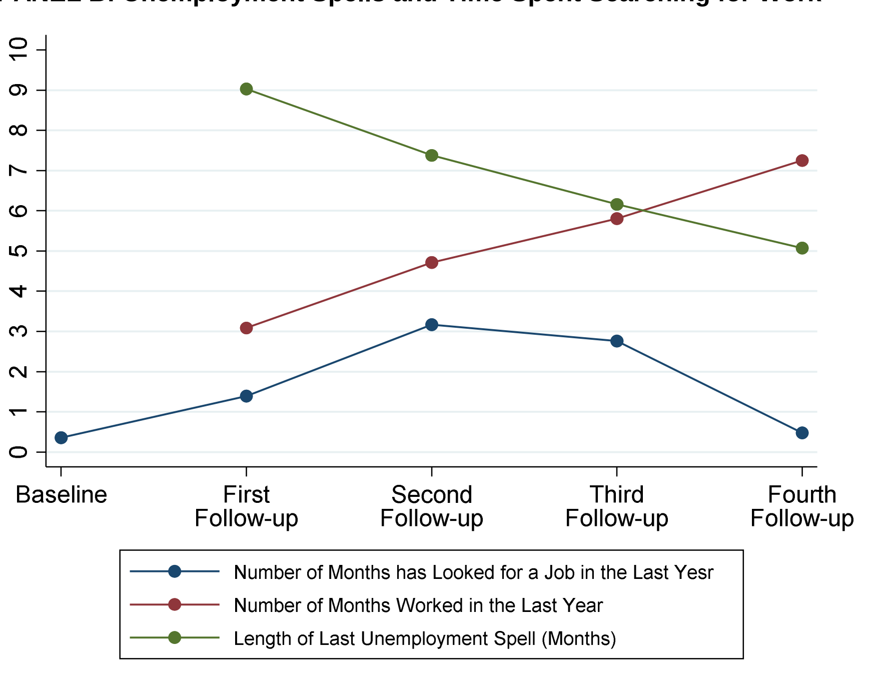
Notes: Panel A shows the distribution of expected probabilities of finding a job at various horizons, at baseline and first follow-up. The third set of bars are for the actual probabilities of finding employment in these good sectors among control workers at second follow-up. The sample used to construct Panel A only includes individuals who were not employed in any of the eight study sectors at first follow-up. Panel B 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 + Matching and Matching treatments were later matched to. We consider the actual distribution of earnings among unskilled, recently hired and skilled workers in these firms.

Figure 4: Labor Market Outcomes and Search Effort, Controls

PANEL A: Unemployment and Job Search



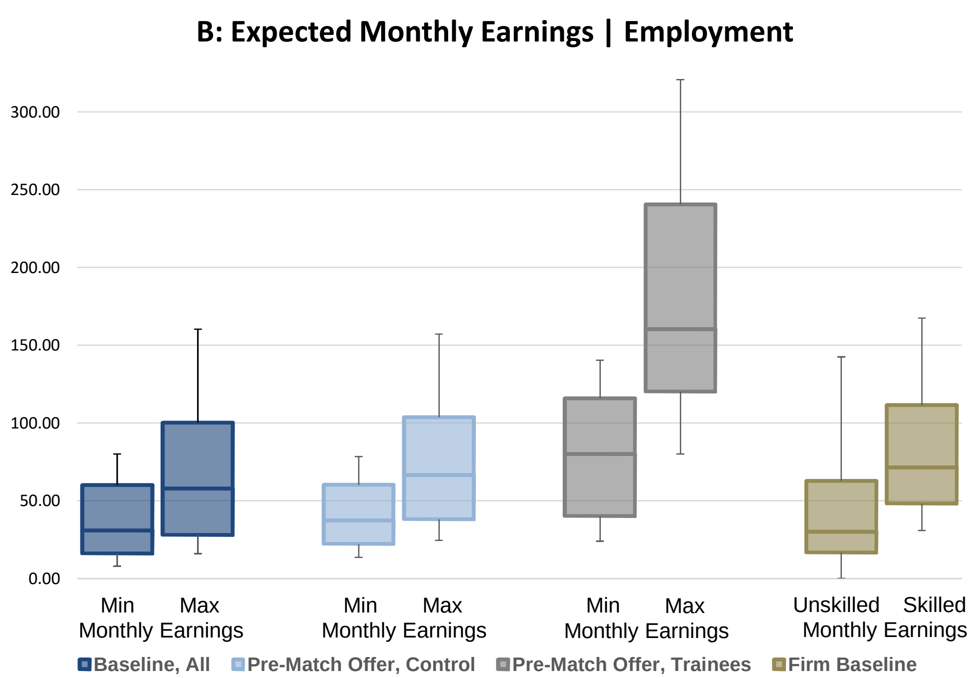
PANEL B: Unemployment Spells and Time Spent Searching for Work



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.

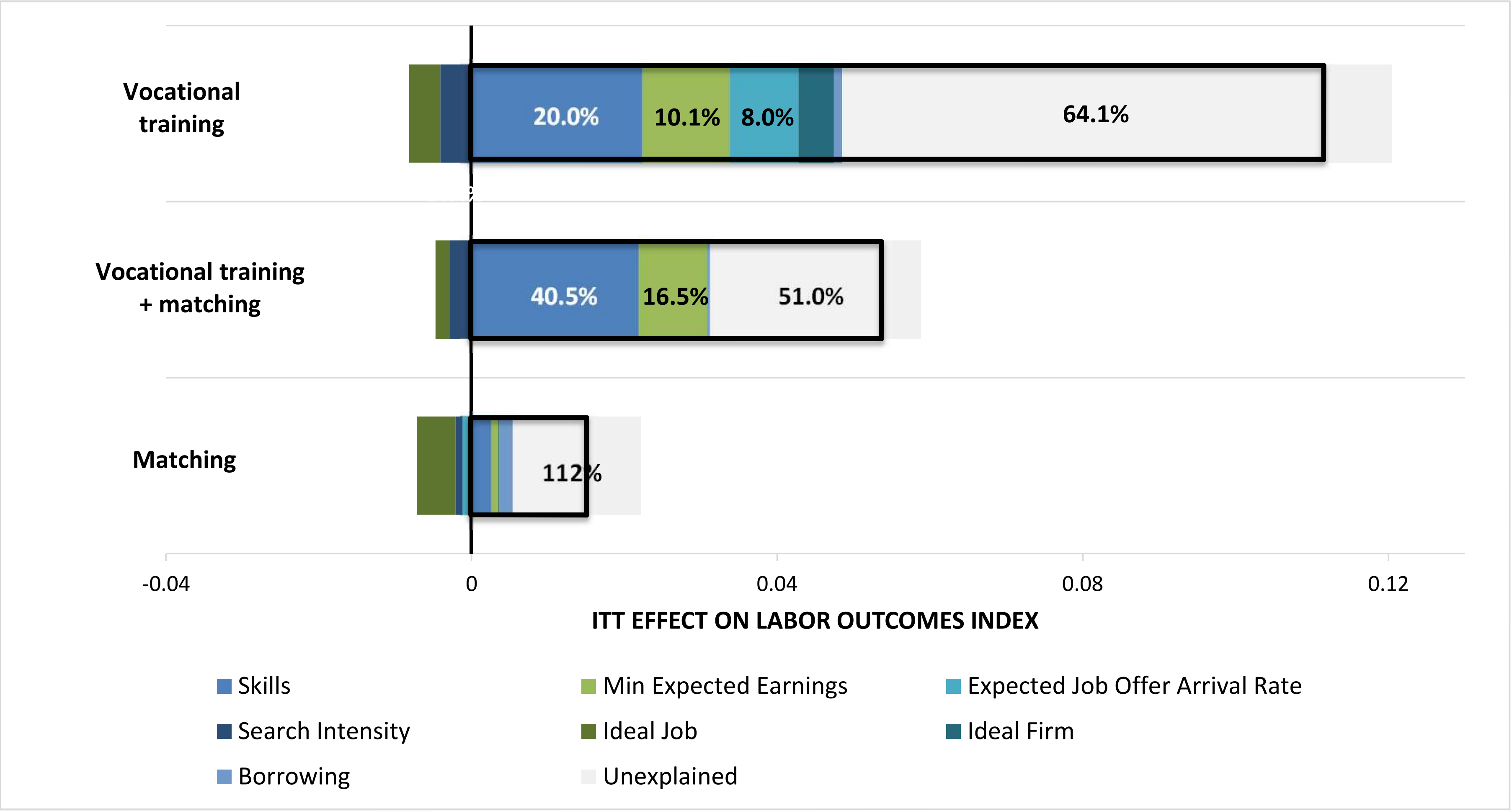
Figure 5: The Evolution of Expectations Until Match Offers are Announced

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



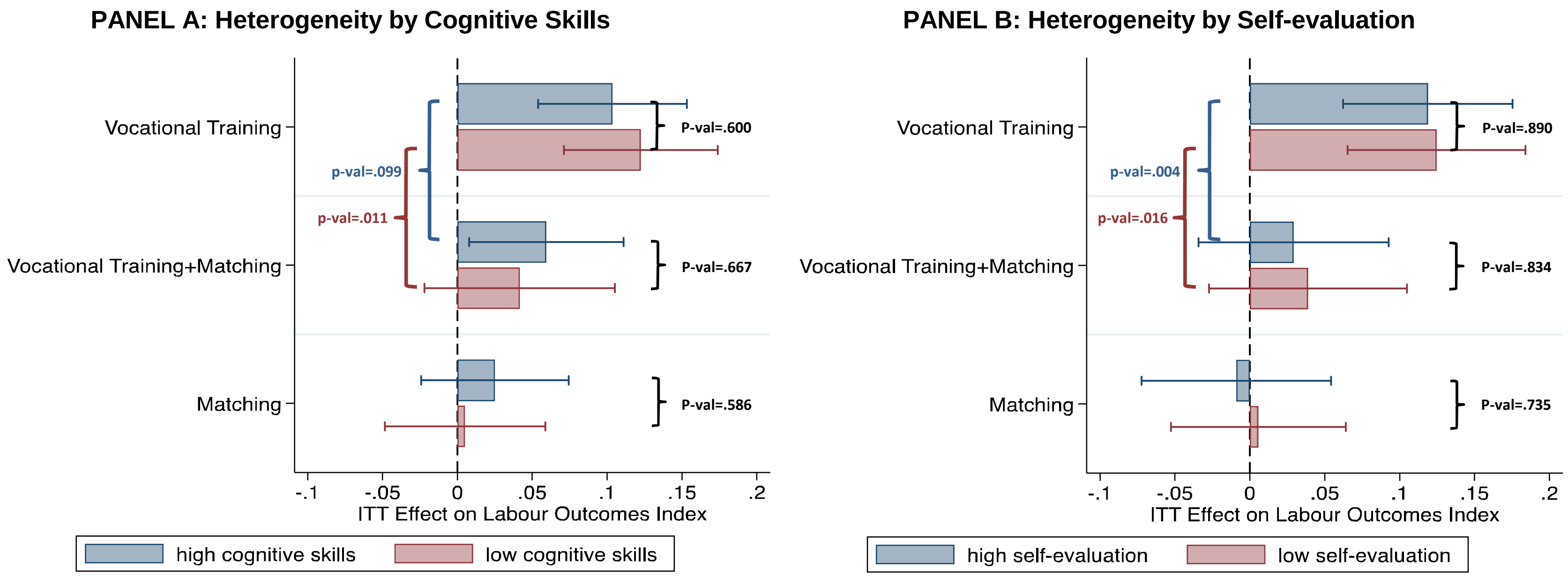
Notes: The data used is from baseline, VTI surveys conducted towards the end of the training period while trainees were still enrolled at the vocational training institutes, and we extrapolate back from the first worker follow-up survey assuming a linear evolution of beliefs, to what would have been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

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 + Matching	20.0	.030	.123*	.029	.038	{.845}
N=307	(.147)	(.012)	(.023)	(.011)	(.015)	
	[.913]	[.163]	[.090]	[.237]	[.830]	
Matching	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 + Matching	-.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 Vocational Training + Matching		-.027
		(.081)
Age X Vocational Training + Matching		-.020
		(.020)
Any Child X Vocational Training + Matching		.085
		(0.152)
Education Level X Vocational Training + Matching		0.028
		(.020)
Has Ever Worked X Vocational Training + Matching		.005
		(.077)
Literacy/Numeracy Test Score X Vocational Training + Matching		-.034
		(.076)
Mean of dependent variable	.653	
P-value: worker covariates	[.000]	[.001]
P-value: worker covariates X Vocational Training + Matching		[.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 + matching 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 + matching 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 + Matching	-.038	-.036	-.386
	(.027)	(.027)	(.246)
Matching	.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 + Match Offer		Match Offer	
	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 matching program). The control variables are measured in the baseline survey of workers and firms, and process reports for treatments involving match offers. 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: 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 + Matching	.252***	7.44***	10.8***
	(.025)	(1.43)	(2.19)
	{.000, .001}	*{.000, .001}*	-
Matching	.014	1.14	.803
	(.029)	(1.41)	(2.01)
	{.643, .610}	*{.428, 417}*	-
P-value: VT = VT + Matching	*[.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 + Matching treatments, and as being called back in the Matching 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 + matching.

Table A7: Personality, 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 + Matching	-.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}*
Matching	.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 + Matching	*[.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 scores 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 + matching.

Table A8: 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]	Earnings in the last month [USD] | Employment	Has done any casual work in the last month | Employment	Self-employed in the last month	Quality of Firm Employed At	Realized Job
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vocational Training	.068*	.173***	1.01***	3.82	3.35	.003	.014	.101	.066
	(.036)	(.030)	(.273)	(2.77)	(5.39)	(.044)	(.022)	(.075)	(.066)
	{.062, .109}	*{.000, .001}*	*{.000, .001}*	*{.171, .292}*	{.518, .881}	{.941, .948}	*{.571, .785}*	*{.178, .393}*	{.306, .475}
Vocational Training + Matching	.093**	.149***	.911***	5.17*	1.30	.018	-.013	.035	.061
	(.039)	(.033)	(.320)	(3.01)	(5.78)	(.048)	(.025)	(.072)	(.072)
	{.017, .047}	*{.000, .001}*	*{.006, .006}*	*{.086, .210}*	{.826, .968}	{.708, .907}	*{.584, .785}*	*{.617, .844}*	{.396, .475}
Matching	.055	.011	-.025	2.63	-.118	.034	.025	.007	-.204**
	(.039)	(.028)	(.277)	(2.90)	(.577)	(.049)	(.025)	(.091)	(.080)
	{.175, .171}	*{.678, .701}*	*{.931, .924}*	*{.373, .364}*	{.979, .989}	{.515, .848}	*{.328, .696}*	*{.931, .950}*	{.011, .027}
P-value: VT = VT + Matching	*[.545]*	*[.533]*	*[.784]*	*[.686]*	*[.726]*	*[.760]*	[.299]	*[.684]*	*[.945]*
Mean in Control Group	.359	.126	1.23	17.7	17.7	.063	.094	.010	-.008
N. of observations	1,225	1,231	1,231	1,172	453	502	1,231	505	504

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 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 5 exclude casual work. In Column 7 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 Columns 8 and 9 the realized firm and realized job indexes are 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 + matching.

Table A9: 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 + Matching	-.245	-.095	.042*	.037
	(.155)	(.063)	(.025)	(.029)
	{.110, .302}	*{.132, .315}*	*{.093, .167}*	*{.209, .334}*
Matching	-.044	-.020	.040*	.022
	(.125)	(.054)	(.024)	(.028)
	{.730, .753}	*{.722, .779}*	*{.099, .167}*	*{.454, .404}*
P-value: VT = VT + Matching	*[.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 + matching.

Table A10: 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 + Matching	-.043	-.020	.036	-.008	.002
	(.039)	(.038)	(.025)	(.018)	(.040)
	{.273, .448}	*{.646, .850}*	*{.130, .339}*	*{.640, .888}*	*{.959,.959}*
Matching	-.085**	-.026	-.032	.005	-.037
	(.039)	(.039)	(.030)	(.017)	(.041)
	{.034, .090}	*{.538, .850}*	*{.283, .464}*	*{.782, .888}*	*{.379, .556}*
P-value: VT = VT + Matching	*[.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 + matching.

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 + Matching	-.018	.027	-.004	-.006
	(.038)	(.038)	(.003)	(.014)
	{.643, .604}	*{.445, .472}*	*{.261, - }*	*{.652, .689}*
Matching	.046	.090**	.003	.034*
	(.039)	(.039)	(.003)	(.019)
	{.242, .372}	*{.018, .054}*	*{.389, - }*	*{.060, .191}*
P-value: VT = VT + Matching	*[.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 + matching.

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 + Matching	.035	.018	.055**	.065***
	(.022)	(.018)	(.022)	(.023)
	{.110, .075}	*{.297, .826}*	*{.012, .058}*	*{.002, .017}*
Matching	-.024	.018	-.031	.013
	(.022)	(.019)	(.022)	(.022)
	{.286, .475}	*{.349, .716}*	*{.149, .255}*	*{.544, .768}*
P-value: VT = VT + Matching	*[.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 + matching.

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 + Matching	-.003	.027	.061**	.058**	-.027
	(.031)	(.028)	(.031)	(.029)	(.030)
	{.920, .929}	*{.336, .556}*	*{.038, .092}*	*{.049, .107}*	*{.360,.724}*
Matching	.030	.010	-.038	-.032	.006
	(.030)	(.028)	(.030)	(.028)	(.029)
	{.314, .519}	*{.750, .748}*	*{.194, .193}*	*{.240, .259}*	*{.819, .974}*
P-value: VT = VT + Matching	*[.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 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. 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 + matching.

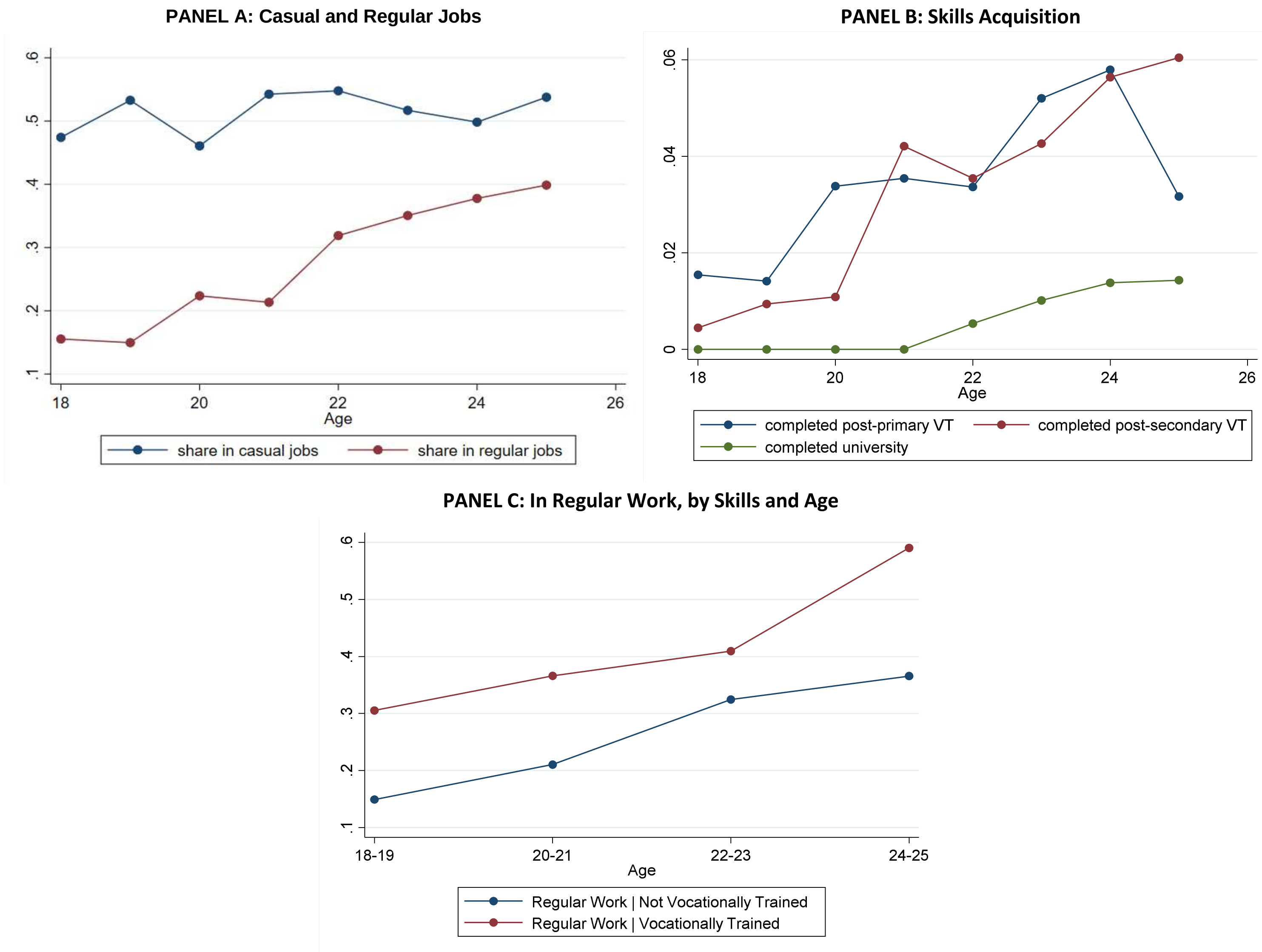
Table A14: Components of the Realized Firm Quality Index

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 + Matching	-.415	-.062**	-.007	-.024	-.037**
	(1.26)	(.031)	(.028)	(.038)	(.017)
	{.756, .938}	*{.053, .100}*	*{.794,.928}*	*{.523, .808}*	*{.032,.065}*
Matching	-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 + Matching	*[.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 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. 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 +matching.

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></td><td></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>