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

Oriana Bandiera **Vittorio Bassi** **Robin Burgess** **Imran Rasul**
Munshi Sulaiman **Anna Vitali**†

July 2023

Abstract

There are 420 million young people in Africa today. Understanding how youth search for jobs and what affects their ability to find good jobs is of paramount importance. We do so using a field experiment tracking young job seekers for six years in Uganda's main cities. We examine how two standard labor market interventions impact their search for good jobs: vocational training, vocational training combined with matching youth to firms, and matching only. Training is offered in sectors with high quality firms. The matching intervention assigns workers for interviews with such firms. At baseline, unskilled youth are optimistic about their job prospects, especially over the job offer arrival rate from high quality firms. Those offered vocational training become even more optimistic, search more intensively and direct their search towards high quality firms. However, youth additionally offered matching become discouraged because call back rates from firm owners are far lower than their prior. As a result, they search less intensively and direct their search towards lower quality firms. These divergent expectations and search behaviors have persistent impacts: vocational trainees without match offers achieve greater labor market success, largely because they end up employed at higher quality firms than youth additionally offered matching. Our analysis highlights the foundational but separate roles of skills and expectations in job search, how interventions cause youth to become optimistic or discouraged, and how this matters for long run sorting and individual labor market outcomes. *JEL: J64, O12.*

*We gratefully acknowledge financial support from the Mastercard Foundation, PEDL, IGC and an anonymous donor. We thank Daron Acemoglu, Orazio Attanasio, Tim Besley, Gaurav Chiplunkar, Bruno Crepon, Ernesto Dal Bo, Kevin Donovan, Hank Farber, Fred Finan, Johannes Haushofer, Francis Kramarz, David Lagakos, Camille Landais, Thomas Le Barbanchon, Steve Machin, Alan Manning, David McKenzie, Costas Meghir, Andreas Mueller, Karthik Muralidharan, Gerard Padro i Miquel, Rohini Pande, Barbara Petrongolo, Steve Pischke, Fabien Postel-Vinay, Barbara Petrongolo, Jean-Marc Robin, Jesse Rothstein, Yona Rubinstein, Nick Ryan, Johannes Spinnewijn, David Stromberg, Gabriel Ulyssea, John Van Reenen, Chris Woodruff and seminar participants for comments. IRB approval is from UCL (5115/003, 007). The study is registered (AEARCTR-0000698). All errors are our own.

†Bandiera: LSE, o.bandiera@lse.ac.uk; Bassi: USC, vbassi@usc.edu; Burgess: LSE, r.burgess@lse.ac.uk; Rasul: UCL, i.rasul@ucl.ac.uk; Sulaiman: BRAC, munshi.slmn@gmail.com; Vitali: UCL, anna.vitali.16@ucl.ac.uk.

1 Introduction

One third of Africa's 420 million young people have regular, salaried jobs [AfDB 2018]. Current fertility rates in many parts of the continent mean that ensuring meaningful employment for young labor market entrants will be increasingly challenging [Bandiera *et al.* 2022]. Finding a solution will greatly affect the pace of economic development.

This paper studies how young Ugandan workers search for jobs. Jobs for young workers in Uganda and across Africa are primarily unskilled and informal. At baseline, youth in our study rely on informal jobs such as (un)loading trucks, transporting goods on bicycles, fetching water, and agricultural day laboring. This paper addresses how search behavior influences workers' ability to secure good, formal jobs in manufacturing and services. These jobs offer regular employment and wage progression; bad jobs are insecure and have flat earning profiles.

We study the issue using a field experiment tracking young labor market entrants over six years, shedding light on the link between skills, expectations, search behaviors and long run labor market outcomes. We explore these links using two standard labor market interventions [Card *et al.* 2017, McKenzie 2017] offering: (i) vocational training; (ii) vocational training combined with a light touch matching intervention that passes worker's details to local firms; (iii) matching only.

We recruited labor market entrants from across Uganda, offering them the possibility of six months sector-specific training in welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering. These sectors are associated with 'good jobs' offering regular employment in high-wage firms. At baseline, 25% of wage employed Ugandans aged 18-25 work in such sectors. The eligibility criteria targeted disadvantaged youth with limited labor market experience and hence scope to learn about their job prospects through search. We received 1400 applications: pre-intervention, applicants were unskilled, found work through informal contacts, and mostly held casual jobs. The 1281 firms in the experiment's matching component operate in the eight sectors in 15 urban labor markets.

Individuals are first randomly assigned to receive an offer of vocational training. Two thirds take-up the offer, and 90% complete their training course. At a second stage of randomization, we offer light-touch matching between workers and firms. Nearly all workers agreed to have their details passed onto these firms. Firms were presented shortlists of two workers that were either both vocationally trained, or both unskilled. Firms could call back for interview neither, one or both (and remained free to recruit other workers).

Our design thus assigns workers to four groups: (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 prospects are fundamental for understanding job search. 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 optimistic about the job offer arrival rate from employers in these sectors. Optimistic beliefs have been documented among job seekers in the US [Spinnewijn 2015, Mueller *et al.* 2021, Potter 2021], Ethiopia [Abebe *et al.* 2020], South Africa [Banerjee and Sequeira 2022] and India [Kelley *et al.* 2022]. These beliefs are key to understanding how workers react to the match offer.

The key outcome for workers from the matching intervention is whether firms call them back for interview. To understand how they react to call backs (or a lack thereof), we track the evolution of worker beliefs from baseline to the eve of match offers being announced. We see a bifurcation in expectations between those randomized in and out of vocational training. During vocational training, trainees become ever more optimistic over their job prospects: at graduation (but before matching is announced), the median trained worker believes there is a 30% chance of receiving a job offer from a firm in one of our study sectors in the next month. This is far higher than employment rates actually experienced by vocational trainees over the same period.

Among those randomized out of training, they continue to search for work but their employment rates remain flat and they remain reliant on casual work. They gradually revise down their beliefs over the job offer arrival rate from firms in good sectors. On the eve of match offers being announced, the median unskilled individual believes there is a 20% chance in the next month of actually receiving a job offer from an employer in our study sectors.

The match offer intervention is thus implemented to these groups of increasingly optimistic youth offered vocational training, and increasingly realistic youth randomized out of vocational training. Among trainees the actual call back rate is far lower than their prior: only 16% actually receive a call back. Among those randomized out of training, call back rates are in line with prior expectations (18% vs. 20%).

Call backs are determined by vacancies and other firm characteristics. Conditional on skills, worker characteristics do *not* determine call backs – this is unsurprising given our design because firms are presented with two workers that are, by construction, similar on observables.

Our null is that workers are perfectly informed and rationally infer there to be no information from one or two call backs 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.

The alternative is that workers are imperfectly informed. For trained workers, the lower than expected call back rate causes them to revise down their expectations about their own job prospects. Such misattribution can occur because: (i) they are not well informed at baseline, and trainees become even more optimistic relative to their true prospects as they complete vocational training; (ii) there are no market substitutes for the matching intervention, so this offer can be a highly salient and unique opportunity for them to find a good job; (iii) the intervention is implemented by BRAC, a reputable NGO. 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 optimistic beliefs described above.

For workers randomized out of the offer of training, the rate of call backs is in line with their prior belief on the job arrival rate. However, call backs generated in the experiment provide more salient and credible information over their job prospects relative to information received during the regular job search process. The low rate of call backs in the matching intervention might then confirm their labor market prospects. How they respond is ultimately an empirical question.

Our first set of results document how these interventions impact worker expectations over their job prospects, a full year after training is completed and/or match offers made.

First, comparing workers offered vocational training to controls, trainees revise upwards their expectations over the job offer arrival rate and expected earnings conditional on being employed in a study sector. Compared to actual outcomes, they become increasingly optimistic on the job offer arrival rate, while their beliefs over expected earnings move more in line with the skills premium offered for trained youth. Workers only offered vocational training search more intensively relative to controls, and direct their 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 training, they revise down their expectations over the job offer arrival rate and distribution of earnings conditional on employment in a good sector job. This is consistent with those additionally provided match offers becoming *discouraged* and reacting to the lower than expected call back rate by revising down their beliefs over their job prospects. Such discouragement is reflected in search behavior: relative to those only offered vocational training, those additionally offered matching search less intensively, and search over lower quality firms. Finally, workers only offered matching – relative to controls – do not adjust their expectations as their call back rate is in line with their prior.

Our second batch of results examine whether the labor market interventions, through experimentally induced changes in skills, expectations and search behaviors, translate into differences in outcomes up to five years after training is completed and/or match offers provided.

We find that relative to controls, those offered vocational training (with or without matching) are more likely to be employed, to transition into regular work, to be employed in good sectors, and end up in better jobs and in higher quality firms. However, contrasting workers offered vocational training with and without the additional offer of matching, we find those with match offers do significantly worse on labor market outcomes 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 good study sectors. Relative to those only offered vocational training, they end up sorting to lower quality firms and lower quality jobs, have lower earnings, experience longer unemployment spells, and shorter employment spells.

In short, while those only offered vocational training transition up the job ladder from casual to regular work, this transition is significantly slower for those also provided match offers. This is despite both groups of workers graduating from vocational training with identical sector-specific skills: the fact they sort to different firms, jobs and sectors all represent a misallocation of talent. This misallocation is caused by the revised expectations workers with match offers have, because they initially misattribute the lack of calls back from a standard labor market intervention and become discouraged in their search for good jobs.

To quantify these long run differences, we construct an index of labor market success combining information on employment in good jobs, earnings, employment spells, and characteristics of jobs and firms workers end up being employed at. This index 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 ($.051\sigma$), and the two estimates are different ($p = .001$). In short, because match offers to those offered vocational training cause youth to become discouraged, this undoes half of what is achieved through vocational training alone. This result quantifies the foundational role expectations play in the long run job search process.

Finally, workers only offered match offers (that confirm their job market prospects), are significantly more likely to enter self-employment. However, on the overall 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 not significantly different to controls.

We decompose the impact on the long run index of labor market outcomes into parts mediated by skills, expectations and search behaviors. Among workers offered vocational training, certifiable sector-specific skills are the most important mediator: 20% of the long run impact is mediated by them. Expectations explain a further 18%. Among workers offered both vocational training and matching, sector-specific skills play the most important role in mediating long run outcomes. These skills explain the same increase in the index for both groups. The role of expectations in mediating long run outcomes is however more prominent for those only offered vocational training because workers additionally offered matching become discouraged, and end up with expectations closer to controls.

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

First, we shed light on the fundamentals of the job search process for youth by experimentally identifying the role that prominent labor market policies – training and matching – play in determining expectations, search behaviors, and how these map to long run outcomes. We build on existing work by providing a granular analysis on individual labor market trajectories that combines experimental variation in policies young workers are exposed to, data on beliefs and multiple dimensions of search behavior, with a set of long run labor market outcomes shedding light on employment, earnings and sorting.

Second, we build on a nascent experimental literature evaluating training and matching interventions in low-income countries [Beam 2016, Groh *et al.* 2016, Abebe *et al.* 2020, 2021, Acevedo *et al.* 2020, Banerjee and Sequeira 2022, Carranza *et al.* 2022]. We bridge between this work and a recent literature on behavioral job search that shows job-seekers tend to be optimistic about job finding rates and this delays exit from unemployment [Spinnewijn 2015, Krueger and Mueller 2016, Conlon *et al.* 2018, Mueller *et al.* 2021, Potter 2021].

Our intent was that combining vocational training with match offers would improve long run outcomes relative to either intervention alone. This did not occur. The reason is that light-touch match offers can backfire if workers misinterpret the lack of call backs from potentially good employers and become discouraged. This implication stems beyond matching, to a broader set of interventions providing information to job seekers [Abebe *et al.* 2020, Chakravorty *et al.* 2023].[1]

This paper is part of a larger project encompassing multiple field experiments studying urban labor markets in Uganda. Our earlier work focused on the labor market returns to certified vocational training versus non-certified firm-sponsored apprenticeships [Alfonsi *et al.* 2020]. We showed that the returns to vocational training are higher because certified skills aid labor market mobility. The current paper focuses on a different question: how do standard labor market interventions impact expectations and search behavior. Given job search is redundant for firm-sponsored training because workers are assigned to firms from the start, we focus on the job search process among vocational trainees.

This paper reconfirms the main mechanisms identified in our earlier work. We layer on the matching intervention that was not the focus of our earlier work. We study the link between interventions and job search by providing granular evidence on the job search process, utilizing survey modules on expectations and search behaviors that were not previously exploited, and we add an additional survey wave of data to pin down long run effects. We show the *near equal importance* of expectations and skills in determining long run sorting of youth in labor markets and their outcomes, because standard labor market interventions cause them to become optimistic, discouraged or confirm their job prospects.

Section 2 describes our context, design and data. Section 3 describes the evolution of beliefs and search behavior among controls. Section 4 presents treatment effects on expectations and search behaviors. Section 5 examines long run differences in labor market outcomes. Section 6 examines the relative importance of skills, expectations and search behaviors for long run outcomes. Section 7 concludes by re-examining Alfonsi *et al.* [2020], discussing external validity, implications and future work. Additional design details, results and research ethics are in the Appendix.

[1] For example, Abebe *et al.* [2020] show that attending a job fair with many potential employers leads optimistic job seekers to revise downwards their labor market expectations without creating discouragement, so that search effort and labor market outcomes increase as a result. In the case of job fairs and other undirected interventions not tailored to the individual, signals are likely to be more informative of the status of the labor market as whole, rather than individual job prospects. Hence there is less scope for workers to misattribute signals as being informative of their own job prospects. Our contribution is to highlight the risks of directed interventions that provide information tailored to the individual, which can be easily misattributed. Our result on discouragement is consistent with Banerjee and Sequeira [2022], who find that providing subsidies for job search leads to discouragement and worse labor market outcomes as workers expand the geographical scope of their job search but fail to find better jobs.

2 Context, Design and Data

2.1 Context

Our study covers 15 urban labor markets in Uganda, including Kampala. Multiple imperfections characterize the job search process: (i) youth enter labor markets lacking skills demanded by firms; (ii) workers cannot finance human capital investments post labor market entry; (iii) firms lack information on worker histories or skills [Abebe *et al.* 2020, Alfonsi *et al.* 2020]. The additional imperfections we document are that youth hold optimistic beliefs over their job prospects, and can misattribute information generated from matching interventions.

We use the Uganda National Household Survey (UNHS) from 2012/3 to describe features of our context. We first derive the share of young people in casual and regular jobs. We classify casual work as jobs in which workers are typically hired on a daily basis, in line with a standard definition of casual jobs being those where neither worker nor firms are obligated to supply/demand labour regularly. Panel A of Figure A1 shows that at all ages young workers rely on casual work. Panel B shows how skills vary by age. By age 25 fewer than 6% of youth make any investment in skills post labor market entry. Panel C shows how skills raise the likelihood of being in regular work, yet the majority of skilled youth still do not find regular work. Hence the labor market fails to clear even for high-skilled youth, and a mass of talent remains underutilized.

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 our eight sectors: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering.

Workers Individuals were recruited into our experiment using an advertized offer to potentially receive six months of sector-specific vocational training at one of our partner VTIs. The eligibility criteria targeted disadvantaged youth. The first row of Table A1 shows applicant characteristics: 57% are men, they are aged 20, and almost none have vocational training.[2]

Table 1 shows labor market histories at baseline. Employment rates for controls are 40%, with casual work being the most prevalent activity. Unconditionally, average monthly earnings from regular work are $5, corresponding to around 10% of the Ugandan per capita income. Conditional on work, earnings are $13 per month. Hence these individuals remain unlikely to be able to

[2] 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, type of building lived in, village location, fuel used, number of household members attending school, monthly wage, and education 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. Table A1 shows the program is well targeted towards disadvantaged youth by comparing our sample to those aged 18-25 in the 2012/3 UNHS data. This remains so when we compare to youth in the UNHS who report being labor market active.

self-finance vocational training (that costs over $400).

Panel A of Table 2 describes characteristics of casual and regular jobs. The first row reiterates that at baseline workers are reliant on casual work, especially including forms of subsistence self-employment. Regular jobs offer longer hours per day, similar days per week of work, and earnings that are almost three times higher.

Firms For the matching intervention, to draw a sample of employers offering good jobs we conducted a census in the 15 urban labor markets selecting firms: (i) operating in the eight study sectors; (ii) having one to 15 employees (plus a firm owner). Our sample comprises 1281 firms, employing 3735 workers in total at baseline.[3] Firms are not selected on the basis of having a vacancy, but at baseline, 92% of them report being willing to expand, with 52% stating they would do so by hiring workers. Firms report being size constrained because they are unable to find skilled workers (67%), trustworthy workers (57%), or unskilled workers (28%).

Job Search and Recruitment Panels B to D of Table 2 describe how our control group normally searches for jobs, and firm recruitment processes. Panel B shows methods of job search: the majority of youth rely on informal contacts through friends/family, especially for regular jobs. They are more likely to use direct walk-ins to firms when searching for regular jobs. Fewer than 2% of workers report finding work through posted 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. Panel D shows how interviews, references and skills tests are more common for regular jobs, although even there, the minority of workers report being screened using such methods.[4]

2.2 Design

Figure 1 shows our oversubscription design. Applicants were first randomly assigned to receive vocational training. Within those assigned to training, a further random assignment took place. The first group was assigned to six months of training at one of our partner VTIs and upon graduation, transitioned into the labor market to search for jobs unassisted (T1). 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, provided light touch offers to match with firms in our firm-side survey sample (T2). Workers randomized out of the offer of training were also randomly assigned into two groups. At the same time as vocational

[3] 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 worker sample).

[4] Distinguishing regular jobs in the eight study sectors from those in other sectors, jobs in our sectors offer higher hourly wages, are more likely to be found via family members and to require a skills test.

trainees were graduating, these unskilled workers were either: (i) offered the same kind of light touch match offer (T3); (ii) held as controls (C). We assigned workers to each treatment arm using a stratified randomization where strata are region of residence, gender and education.

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

Although workers were randomly assigned to each treatment arm with their initial application, they were only informed about potential matching once vocational trainees had completed their courses. This ensures match offers for those randomized into and out of the offer of training take place simultaneously. Those randomized out of the offer of vocational training however might have found work before the match offer. A six month tracker survey fielded just prior to match offers being announced sheds light on this: 16% of controls are in some work activity, most remain reliant on casual jobs and over 90% remain interested in a matching opportunity.

Vocational Training The vocational training intervention provides workers six months of sector-specific training in one of eight sectors. Our intervention partner BRAC covered costs, at $470 per trainee. Courses were held Monday to Friday, for six hours per day; 30% of content was dedicated to theory, 70% to practical work providing sector-specific skills. VTIs signed contracts with BRAC to deliver these standard training courses. 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 upfront and the remainder when the worker graduated. This staggered timing of payments ensured workers nearly always completed training conditional on enrolment.

Upon graduation, trainees receive a certificate. As documented in Alfonsi *et al.* [2020], there are high returns to having certifiable skills from reputable VTIs in these urban labor markets.

Matching The match offer is a light-touch and one-off intervention replicating common labor market interventions in high and low-income settings. The intervention was designed to help workers and firms overcome search frictions.[5]

Workers were first asked whether they wanted their details to be passed onto potential employers in our firm-side survey: nearly all agreed (among those offered training and those randomized out of that offer). Firms were presented shortlists of workers that were either: (i) all vocationally trained, or; (ii) all unskilled, but had demonstrated labor market attachment in the sense they had been willing to undertake six months of 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

[5] Meta-analyses of job assistance programs [Card *et al.* 2017, McKenzie 2017] emphasize worker-firm matches 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.* 2020].

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 call back for interview neither, one or both workers (and remained free to recruit from outside the evaluation sample). The median worker was matched to a single firm.

Worker-firm match assignments were restricted to take place between firms in the same sector as the worker had been trained in (T2), or had desired to be trained in (T3). Both had to be in the same region to increase match feasibility.

The matching intervention is highly salient to young job seekers: it provides them a unique opportunity to match with good firms, and there are no market substitutes for it. As young workers are transitioning into the labor market, signals of job prospects are likely to receive a high weight. Moreover, the intervention was implemented by BRAC, a reputable NGO, further increasing the salience of the intervention and credibility of signals generated from it.[6]

The Appendix details how worker-firm match offers were implemented, including the exact (fixed) scripts used to communicate the process to workers and firms. This wording ensured that *ex ante* workers were aware their details were being handed over to a few firms. No offer of employment was given – BRAC officers made clear they were only acting by providing CVs to firms, and they were not searching for jobs on the worker's behalf. Workers were not told the likelihood of being called back, nor *ex post* any reason why firms did not call them back. Firms were not provided worker contact details – they had to come through BRAC officers, so we can rule out our results being due to firms recalling workers or storable offers. The matching intervention only involves BRAC officers, with VTIs 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 – is typically around two weeks. We measure short run search behavior a year after the match offers are first announced, so impacts are not driven by any substitution of search effort between workers and BRAC.

2.3 Data

Timeline and Surveys Figure 2 shows the study timeline. The baseline worker survey took place from June to September 2012 just after applications for vocational training were received. This is when initial beliefs over labor market prospects are measured. Among those taking-up the training offer, we next surveyed them at the end of their six month course. We use this to measure posterior beliefs over labor market prospects just as workers complete training but prior to having knowledge of match offers. Among those randomized out of training, we next surveyed them just

[6] To further 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.

as trainees were graduating, and so measure the opportunity cost of attending the training courses. These two rounds of data collection are under Phase 1 of the timeline.[7]

For workers involved in matching treatments, we record key outcomes (call backs, 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.

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, linking these to underlying dimensions of search behavior such as search intensity and directed search, and mapping expectations and search behaviors to long run labor market outcomes related to employment, earnings, hours, wages, bargaining, spells, and actual job and firm characteristics.

Balance, Compliance and Attrition Table 1 shows baseline labor market characteristics of workers by treatment arm. Table A2 shows other characteristics. The samples are well balanced, and normalized differences in observables are small.

On compliance with the offer of training, 68% of individuals take-up the offer, with over 95% of them completing training conditional on enrolment. Table A3 shows correlates of training completion: (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 expected because match offers are only announced later so 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 later also offered matching.[8]

Only 15% of workers attrit by the 68-month endline. Table A4 describes correlates of attrition. It is uncorrelated to treatment, and there is no evidence of differential attrition across treatments based on observables.

[7] 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.

[8] The main reasons for not taking up the training offer were family (35%), followed by distance to the VTI (15%). Only 13% reported not taking up because they had found a job.

3 Expectations

Worker expectations over their job prospects are the foundation of our analysis. We first detail expectations among controls at baseline. We then zoom in on the evolution of beliefs across treatment arms between baseline and the eve of match offers being announced. Finally, we consider workers reaction to call backs (or lack thereof) from the match offers.

3.1 Expectations and Reality Among Controls

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 training. At baseline we asked controls their expected probability of finding a job in our study sectors in the next month, six months and year (where job offer acceptance rates are over 90%). The distribution of beliefs are shown in the first three box-whisker plots in Figure 3A. Reassuringly, these right-shift as we increase the time horizon. The median belief among controls is they have a 20% chance of receiving a job offer in good sectors within a month, 40% within six months, and 60% within a year.[9]

We assess the accuracy of beliefs by comparing them to actual youth employment rates in regular wage jobs. As Panel C of Figure A1 shows using the UNHS data, for unskilled youth, employment rates in regular jobs are 20%, rise by 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]

The next three box-whisker plots in Figure 3A show the distribution of revised expectations over job offer arrival rates at first follow-up. Beliefs 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 six months, and 40% within a year. Controls therefore become gradually more realistic as they search.

To see how quickly expectations converge to reality, we calculate the *actual* likelihood of finding

[9] 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 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." As probabilities were elicited on a 0 to 10 scale, a concern is that workers might not have been able to express probabilities for rare events. To check for this we note that at baseline, 22% of youth report having a zero probability of finding a job in the next month, and 57% report a probability less than 20%. Reassuringly, individuals report higher probabilities of finding a job over longer horizons – only 11% and 9% report a zero probability of finding a job in the next 6 and 12 months respectively.

[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. The economy-wide flow of young workers into regular jobs might be even lower than the stock measured in the UNHS, or potentially higher if the rate of job separations is also very high.

a good job over those horizons using our second survey, fielded a year later. As shown in the last three box-whisker plots in Figure 3A, these are still far lower than worker expectations over the job arrival rate, with the divergence increasing with the time horizon considered: only 7% of workers actually find a job within a month, 10% within six months, and 13% within a year.[11]

These results complement a growing literature on the *persistence* of optimistic beliefs [Benabou and Tirole 2002, Compte and Postelwaite 2004, Van den Steen 2004]. We add to evidence that displaced workers are optimistic over job offer arrival rates both in the US [Spinnewijn 2015, Mueller *et al.* 2021, Potter 2021, Mueller and Spinnewijn 2023], and in lower-income labor markets [Abebe *et al.* 2020, Kelley *et al.* 2020, Banerjee and Sequeira 2022].

Expected Earnings The second margin of beliefs is worker's expected earnings conditional on employment in a good sector job [Wright 1986, Burdett and Vishwanath 1988]. The first two box-whisker plots in Figure 3B show the entire distribution of *actual* monthly earnings of controls at baseline, from casual and regular work. 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 good sectors, we elicit beliefs for the worker's most preferred sector (for those in T1 and T2 this typically 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 construct their expected earnings. The next three box-whisker plots in Figure 3B show the distribution of minimum, maximum and expected earnings. Average expected earnings are higher than actual earnings from the kinds of regular work that controls engage in at baseline – indeed, median earnings in actual regular work lie below the 25th percentile of expected average earnings if the worker could move into their most preferred good job. Hence these youth recognize jobs in our study sectors are better than the kinds of work they have previously experienced.[13]

[11] The correlation between the expected probability of finding a job in the next 12 months and whether the worker engages in job search is low: for the control group, it is .120 at baseline. If we regress the two against each other at baseline, we obtain a partial coefficient of .021 ($p = 0.07$). Of course, this cross-sectional correlation is likely attenuated due to omitted variables bias. For example, worker ability can be positively correlated with job expectations and negatively correlated with 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 wording of the questions is: "With your current skill set, what is the possibility out of 10 that you

To assess the accuracy of beliefs, the final batch of box-whisker plots takes earnings data from workers actually employed in the study sectors, using our firm sample. We show earnings for: (i) unskilled workers; (ii) recent hires; (iii) skilled workers. We observe a high overlap between the distribution of expected and actual earnings of unskilled and newly hired workers in these sectors. The distribution of entry level earnings in these good sectors is almost common knowledge among labor market entrants.

Search Intensity How do these expectations translate into the intensity of job search relative to unemployment spells? 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 A2 shows that over the four years from first follow-up, the share of youth unemployed at some point in the year falls from 90% to 60%. However, the share reporting looking for a job never reaches 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 actually spent 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 in good firms. In contrast, optimism over the job offer arrival rate from good firms can reduce search intensity and slow exit out of non-employment. This is key to our analysis because this margin of belief can be directly impacted by the match offer intervention.

3.2 How Vocational Training Changes Expectations

We next consider the evolution of beliefs until match offers are announced. For those completing training, we measure their expectations just as they graduate but prior to match offers being announced. For controls, we measure beliefs at baseline and first follow-up and assume beliefs evolve linearly over time. Nothing hinges on this assumption, it is only made to interpolate a belief at the time match offers are announced.[14]

Expected Job Offer Arrival Rate The first set of bars in Figure 4A 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, when match offers are about to

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] For example, very similar results are generated if we assume workers update at a decreasing speed, namely they update their beliefs faster at first and then slow down, or if we assume the opposite – that workers update at an increasing speed over time.

be announced. As described earlier, although controls hold optimistic beliefs at baseline, they gradually become more realistic as they search. The third set of bars 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 6 and 12 month horizons. Over those horizons, there is no overlap in the interquartile range of beliefs among the two groups. At 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 year; 25% of trainees believe this will occur with probability one.[15]

To test differences in beliefs across 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 training (T3, C). Rows R1 and R2 show baseline expectations; Rows R3 and R4 show expectations on the eve of match offers being announced. At the foot of the Table 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 shows 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 thus significantly differ between workers offered training and those that are not (Row 3 = Row 4).

To benchmark the realism of these updated beliefs, we consider the actual rate at which vocational trainees work in one of the study sectors in the 12 months from graduation, as measured at second follow up. As discussed in detail later, 30% of vocational trainees end up working in one of the eight study sectors over this time frame. We see from the last set of bars in Figure 5A that this is far below even the 10th percentile of beliefs held by these workers as they completed training. It is because of this wedge between expectations and reality that we consider trained workers as remaining optimistic over the job offer arrival rate from good sectors at graduation.

Expected Earnings We next consider the evolution of expectations over the earnings distribution in our study sectors. Figure 4B shows the distribution of beliefs youth hold over the minimum and maximum expected earnings from being employed in their most preferred sector among: (i) all workers at baseline; (ii) controls on the eve of match offers being announced; (iii) 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

[15] This upward revision in beliefs is in line with trainees reported 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.

months of job search.

The third set of bars show that for training graduates, distributions of minimum and maximum expected earnings shift rightward, with an especially pronounced upward shift in the distribution of maximum earnings. Workers thus recognize the high returns to their acquired skills. How realistic are these upward revisions? Expected mean earnings rise by 41% (with similar percentage increases in expected minimum and maximum expected earnings). In Alfonsi *et al.* [2020] we show the actual returns to certified vocational training are between 20 and 30%, so workers are optimistic about the returns to skills.

Columns 2 and 3 in Table 3 formally test differences in these distributions. We see that: (i) at baseline there are no significant differences in expected minimum or maximum earnings across workers assigned to vocational training or not (Row 1=Row 2); (ii) there are no significant changes in expected minimum or maximum earnings over time among workers randomized out of vocational training (Row 2 = Row 4); (iii) there are significant changes in expected minimum and maximum earnings over time among workers assigned to vocational training (Row 1 = Row 3); (iv) hence 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).

3.3 Call Backs and their Determinants

For workers offered matching, the key outcome is whether they receive a call-back, i.e. an invitation to interview with the firm owner. When match offers are announced, workers' expected job offer arrival in the next month is our best proxy of what workers might expect call back rates from the match intervention to be. This is a margin of belief over which vocational trainees are increasingly optimistic, while those not assigned to training are slowly becoming more realistic. As Figure 4A 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 in T2 actually received 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 in T3 actually receive a call back, confirming their prior.

To understand how workers might react, we need to consider 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 to the offer of vocational training, Columns 3 and 4 present analogous specifications for call backs to those randomized out of training. The specifications control for: (i) worker and firm characteristics; (ii) worker characteristics and firm fixed effects. At the foot of each Column we report p-values on the joint significance of worker and firm covariates.

Two results emerge. First, worker characteristics do not predict call backs. The p-values on the joint test of significance of worker covariates vary from .450 to .631 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.

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 where owners report being constrained by an inability to find trustworthy workers. Hence the 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 We used fixed scripts to communicate match offers to workers. They were aware their details were being handed to only a few firms, they were not given *ex ante* information on the expected call back rate, nor *ex post* reasons why they were not called back.

Our null hypothesis is that workers have perfect information about the returns to skills and labor market conditions. They rationally infer there to be zero information from any given call back (or lack thereof) because: (i) they do not learn anything about their own labor market prospects (as workers characteristics do not determine call-backs), and, (ii) they do not learn anything about the labor market, as this is one or two draws from many potential employers. Under this null, the expectations and search strategies of workers – irrespective of whether they have been trained or not – are unaffected by actual call back rates.

The alternative hypothesis is that workers are imperfectly informed, and misattribute what drives call backs. Such misattribution can occur because: (i) labor market entrants are imperfectly informed about their job prospects to begin with, being optimistic about job offer rates from good firms; (ii) there are no market substitutes for the match offer, and so the intervention is viewed as a highly salient opportunity for them to find good jobs; (iii) it involves a reputable NGO such as BRAC – perhaps especially so among those workers that were completing BRAC sponsored vocational training. Under this alternative, the low call back rates from match offers generate bad news for trained workers, causing them to revise down their beliefs about their job prospects.

While we do not attempt to micro-found such misattribution, it is consistent with job seekers being subject to the gambler's fallacy, in which they become discouraged as they overinfer their job prospects from one bad draw [Rabin and Vayanos 2010], and with a literature studying why individuals hold unrealistically positive views of their own prospects [Carrillo and Mariotti 2000, Benabou and Tirole 2002, Santos-Pinto and Sobel 2005, Koszegi *et al.* 2022].

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 optimistic beliefs shown in Figure 4.

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 can provide credible confirmation to them of their true labor market prospects. How they respond to this is 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 search behaviors. Expectations and search behaviors are measured at first follow-up, 24 months after baseline and a year after trainees have graduated and any call backs made, so during Phase 2 of the timeline in Figure 3. 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, and 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 training; (ii) $\beta_2 - \beta_1$ (T2 vs T1): the differential impact of matching on those offered training relative to those only offered training; (iii) β_3 (T3 vs C): the impact of match offers on those randomized out of training.

4.2 Preliminaries

Sector Specific Skills Our earlier work showed how the offer of vocational training translates into human capital accumulation [Alfonsi *et al.* 2020]. Here we briefly reiterate those findings and extend them to show impacts on those offered matching. We measure individual skills using a sector-specific skills test we developed in conjunction with skills assessors and modulators of occupational tests in Uganda. The test was conducted on all workers (including controls) at second and third follow-up, with no differential attrition by treatment into the test. The main results (reported in Table A6) are: (i) workers offered vocational training significantly increase their skills by 21% (or .29σ); (ii) among those that take-up training, skills accumulation increases by 28% over controls (or $.37\sigma$). Table A6 further shows: (i) workers offered training and matching have no different skills accumulation to those only offered training; (ii) among those randomized out of training, there are no differences in skills between those with and without match offers. As exposure to match offers does not change skills accumulation, when we later compare long run labor market outcomes between trainees with and without match offers, those results do not reflect skills differences between treatment arms.

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 training, there are no differences in the big-5 personality traits, cognitive ability and other psychological traits, between those with and without matching; (ii) among those randomized out of training, there are also no differences in these outcomes between those with and without matching. This helps rule out our findings on long run outcomes are mediated through these margins. We later exploit the time invariance of these traits to probe the external validity of our findings if they were to be extended to alternative samples of job seekers.[16]

4.3 Expectations

We examine how the interventions impact expectations a year later. We do so irrespective of worker's employment status, ensuring results are not driven by composition effects. Table 4 shows these results. Starting with beliefs over the job offer arrival rate, Column 1 shows a full year after training is completed and workers have been searching for jobs, those offered training retain upwards revised beliefs on this margin (by 1.84 on a 0-10 scale). Columns 2 to 4 show treatment effects on expected earnings if workers were able to transition into their most preferred study sector job. Among those offered training, they significantly revise upwards their minimum expected earnings from such wage employment, 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 controls. Column 5 shows there is no overall change in the dispersion of expectations of average earnings.

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 training with and without matching. 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

[16] The interventions do not change workers' perceived locus of control differentially for those assigned to training relative to those also assigned matching ($p = .233$), although for those assigned to training and matching, their locus of control is significantly lower than controls at the 10% level. This is consistent with discouragement effects arising from reduced expectations over their own ability to find good jobs, rather than whether finding good jobs depends on their own effort versus external factors such as the state of the economy.

($p = .082$). They also have lower expected earnings from wage employment 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 evidence suggests youth offered training and matching are *discouraged* relative to youth only offered training as measured by these expectations over their own prospects. This is in line with the alternative hypothesis, that low call back rates from match offers are misinterpreted as bad news for them relative to their prior expectation at the time they completed training.

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 being in line with their prior.

Is This Really Misattribution? While we have no direct way to measure workers misattributing information from the lack of call backs, we can still probe the issue in three ways. First, we consider reported satisfaction over job prospects, also measured for the full sample irrespective of employment status at first follow-up. The result is shown in Column 1 of Table A8: workers offered vocational training and matching are significantly less likely to report being satisfied about their job situation relative to those only assigned vocational training ($p = .004$). This is consistent with them being discouraged by the outcomes of the matching intervention.

Second, we examine the idea that signals from the match intervention cause workers to become more uncertain about the value of skills. At first follow up we asked workers their expected returns from additional training. Column 2 of Table A8 shows the impact on the dispersion of expectations of the average earnings returns to additional training. Relative to controls, workers offered training and matching become significantly more uncertain over returns to additional training, and their uncertainty over future skills acquisition is higher than for those offered training alone ($p = .054$).[17]

Third, we rule out that low call back rates cause workers to revise beliefs about the state of aggregate labor demand, rather than their own prospects. To do so we elicited beliefs over the following aggregate conditions: (i) whether a lack of firms is a problem for job search; (ii) whether a lack of advertised jobs is a problem; (iii) whether workers have difficulties demonstrating their practical skills to employers; (iv) whether workers have difficulty demonstrating their soft skills to employers. The remaining Columns of Table A8 show how the treatments impact each dimension, as well as an overall index of labor market beliefs. For no treatment arm do we find significant changes in beliefs for any dimension of labor market conditions.

[17]The expected returns from additional vocational training are calculated as the percentage difference between the worker's reported expectations with additional vocational training and the worker's expectations with his/her current skills set (where under both scenarios we asked the minimum and maximum expected earnings).

4.4 Search Behaviors

4.4.1 Search Intensity

Expectations closely link to search behaviors. For example, an explanation for why those offered training revise up their beliefs on the job offer arrival rate is because their expected returns to search have increased. If so, this should map into changes in search intensity. However, whether greater optimism on the job offer arrival rate leads to more or less search intensity is *a priori* ambiguous because of countervailing forces.[18] While these issues have been explored among US job seekers [Spinnewijn 2015, Faberman and Kudlyak 2019, Mueller *et al.* 2021], we provide among the first evidence for job seekers in a low-income country.

We first consider the extensive margin of search. The result in Column 1 of Table 5 shows that workers offered vocational training are, relative to controls, significantly more likely to actively search for work. 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), but they become significantly more geographically mobile in their search (Column 3). Those offered 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 this effect corresponds to a 63% increase in the use of this search channel relative to controls.

For all measures of search intensity we find no evidence that workers search less. The results are consistent with the offer of vocational training, and hence acquired skills and increasingly optimistic expectations, being complementary to search effort.

We combine all these margins into an index using the approach of Anderson [2008]. This uses the data covariance matrix to construct a weighted sum of indicators, giving less weight to items more correlated with each other. The index is standardized to have mean zero and variance one in the control group at baseline, so estimates are interpretable as effect sizes. Column 6 shows this index of search behaviors rises significantly for those offered vocational training by $.089\sigma$. Workers additionally offered matching have more muted responses on these dimensions of search: their index rises by $.019\sigma$ and this is not different from zero. Hence the complementarity between search effort and vocational training is weaker for those additionally offered matching. As both groups have the same skills, the lower returns to search are because of discouragement among

[18] Following Faberman and Kudlyak [2019] we present the intuition as follows. With endogenous search effort, the optimal effort (s) equates the marginal costs and benefits of an additional unit of effort. Denote the cost of search as $\phi(s)$, assumed increasing and convex in s. The marginal benefit is the product of the increase in job finding probability with the expected surplus from finding a job. The job finding probability can be denoted $\lambda(s, \theta, T_i)$ which depends on search effort, aggregate labor market conditions (θ) and treatment assignment $T_i \in \{VT, VT + M, M\}$ – that can shift a worker's underlying skills or beliefs. The expected surplus from finding a job is $E[V - U|T_i]$ where V (U) is the value of employment (unemployment) and generally also depends on treatment. Hence the optimal search effort is given by $\phi\prime(s) = \lambda_s(s, \theta, T_i)E[V - U|T_i]$. Whether treated workers exert more search effort than controls then depends on the sign of λ_{sT_i}, namely whether the offer of vocational training and/or matching (through its effects on skills and beliefs) is complementary or substitutable for search effort.

youth additionally offered matching. The discouragement effect on search is concentrated on one margin: the extensive margin of search intensity ($p = .053$).

Finally, workers only offered matching do not change search behavior along most margins except reporting spending fewer days actively searching. This is in line with the earlier results because for these youth, there was no change in the expected job offer arrival rate, suggesting no change in the expected returns to search and hence search intensity.

4.4.2 Directed Search

The other dimension of search behavior relates to directed search: whereby workers focus their search on particular firms or jobs. Expectations again link to directed search and the evidence already hints at such behavior being impacted. For example, in many job search models, the minimum expected wage helps pin down reservation wages (because a potential employer would not make an offer she knows will be rejected). The fact that this shifts upward with the offer of training is consistent with workers searching over higher wage firms/jobs, as is the fact that the average skilled worker revises up their beliefs on earnings *conditional* on obtaining a job in a good sector relative to those offered training and matching.

To explore the issue we first examine whether workers report wages being an important determinant of the firms they search over. The treatment effects on this outcome are shown in Column 1 of Table 6: while 34% of controls report wages being a determining factor, this rises by a further 11pp for those youth offered vocational training. This is significantly different to those offered vocational training and matching ($p = .050$) in line with these two groups of equally skilled workers searching over different parts of the wage offer distribution [Moen 1997, Acemoglu and Shimer 1999, Shimer 2005].

To establish holistic measures of directed search, 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 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 2 of Table 6: 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.

Workers additionally offered matching search for firms that are no different to those targeted by controls. Their ideal firm index is borderline significantly different to firms targeted by those only offered vocational training ($p = .102$). Examining more closely the components of the ideal firm index, we see that relative to workers only offered vocational training, those additionally offered matching search for significantly smaller firms ($p = .040$) and are significantly more likely to search over informal firms ($p = .058$). This is all despite these two groups of worker having identical sector-specific skills.[19]

We see no differences across treatment arms in the ideal job sought. Table A10 shows no component of the ideal job searched for index shifts for workers offered vocational training (with or without matching).[20]

4.4.3 Credit

In the Appendix we examine a final dimension of search behavior, building on the idea that labor and credit markets are interlinked. The results in Table A11 show that workers only offered matching are significantly more likely to borrow than controls. They do so not to finance job search but rather borrow to finance business expenditures, as in starting up in self-employment. 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 suggests the lack of call backs from the BRAC-implemented matching intervention serves to concretize and crystallize unskilled workers' low expectations of finding a wage job of the type vocational training institutes prepare individuals for. As we assess labor market outcomes below, we can examine whether these intentions – as measured a year after matching is offered – actually translate into higher rates of self-employment in the long run.[21]

5 Long Run Outcomes

The six-year study period allows us to map out how offers of training and matching translate into long run labor market outcomes. We do so using outcomes measured during Phase 3 of the timeline in Figure 3, so 36 to 55 months after workers graduate and/or are given match offers. We estimate the following ITT specification for worker i assigned to treatment group j in strata s in survey wave t:

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

[19] If beliefs are a function of the type of firm that workers direct their search towards, these results can help explain why the magnitude of differences in expectations between those assigned to vocational training and those additionally offered matching are relatively small. More precisely, as those assigned to vocational training and matching direct their search towards lower-quality firms, a countervailing effect on expectations, offsetting impacts of discouragement, is that the underlying probability of finding a job in such lower-quality firms could be higher (because individuals trade off higher wages with lower probabilities of finding a job).

[20] 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.

[21] This stated intent to move into self-employment is consistent with characteristics of the ideal job reported for those in the match only arm: in Table A10 we see that they are significantly less likely than controls to report searching for jobs involving supervising others.

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 report p-values adjusted for randomization inference and multiple hypothesis testing as before. We later summarize outcomes in an index of overall labor market success. For this index we show dynamic impacts by survey wave.

5.1 First Job

We start by considering the first job obtained post-intervention. Column 1 of Table 7 shows controls find their first wage job almost 14 months after the interventions have completed – it takes a long time for unskilled youth to get a foot on the job ladder. Vocational trainees (with and without match offers) find their first job one to two months earlier. Both groups are equally likely to find their first job in one of the eight good study sectors (Column 2), that is 22pp higher than for controls.[22]

The remaining Columns show margins along which first jobs differ between vocational trainees with and without match offers. Those offered only vocational training are significantly more likely to have a formal contract ($p = .022$), and their monthly earnings are significantly higher despite the two groups of worker having identical sector specific skills ($p = .001$). This is precisely in line with the findings on directed search where these groups of worker diverged in the quality of firms they directed the search towards.

For those only offered matching, we see no short run difference to controls in the timing of their first job, whether it is in a good sector or with a formal firm, or earnings.

5.2 Employment

Table 8 uses specification (2) to establish long run impacts on employment and transitions into regular work. Mirroring results in Alfonsi *et al.* [2020], 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 driven by a transition 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 in regular work (Column 4). In terms of sectoral allocation, they double the months they work in a study sector that offer good jobs (Column 5).

We summarize employment effects by combining outcomes from Columns 3 to 5 into an index, using the Anderson [2008] approach and normalizing the index to zero for controls at baseline

[22] The results help ameliorate the concern that workers offered vocational training and match offers assume BRAC searches entirely on their behalf. The fact that even those offered training take around a year post-intervention to find their first job also removes the concern that any of the results on expectations and search behaviors are driven by feedback effects from short run labor market outcomes or on-the-job search.

so impacts can be interpreted as effect sizes. This index outcome is in Column 6. 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 $.248\sigma$ ($p = .030$). 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) they work less time in one of the good sectors in which we offered training ($p = .104$).[23]

Linking these results back to those on expectations highlights that optimistic beliefs can drive the search for good jobs. Specifically, we note the difference in expected job offer arrival rates between those offered 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 finding a good job (Table 8, Column 3) is $.113 - .066 = .047$, which is the same order of magnitude.

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 training. Their employment index rises by $.117\sigma$, so around one third that of those offered training only.

5.3 Earnings

Column 1 of Table 9 shows that for those offered vocational training, long run earnings rise by 26% over controls. Columns 2 and 3 show the bulk of this rise comes from earnings from regular jobs. Examining earnings impacts for workers offered 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 training, but these differences are only marginally significant.

To understand why for those offered training, the additional match offer has more negative impacts on employment than earnings we consider the extent to which workers engage in *ex post* bargaining with firms they receive job offers from. We consider bargaining over wages, hours, location and additional benefits, combining these into a bargaining index. Column 4 of Table 9 shows treatment effects on this bargaining index. Only those offered both training and matching are impacted, being significantly more likely to engage in *ex post* bargaining than those offered only vocational training ($p = .001$). Table A12 shows these workers bargain over locations and additional benefits.

Why would only those offered training and matching years earlier bargain harder with poten-

[23] 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. Including these additional outcomes into the index does not alter the findings: the employment index for matched workers rises by $.192\sigma$, for those additionally offered matching it rises by $.106\sigma$ and these are statistically different ($p = .020$).

tial employers? One intuition is that workers bargain as their non-employment outside option improves. Our experiment rules this out because workers only offered training do not bargain in the same way. We also rule out that such workers are differentially skilled to those only offered vocational training (Table A6). Rather, our results offer the novel possibility that the search process itself influences how hard workers bargain *ex post* with firms. In particular, the frequency of job offers from good firms might determine bargaining behavior. To check this, Column 5 shows unemployment spells for those offered only vocational training are half the length than for those additionally given match offers, and this difference is significant ($p = .023$). Hence those offered training and matching meet good employers less often – but when they do match with potential employers, they bargain harder. This can help explain how they close the earnings gap to those only offered training.

5.4 Realized Sorting

We next consider impacts on sorting, by examining the characteristics of firms and jobs 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 firm and job characteristics to allow a direct comparison to the ideal firm and job characteristics workers expressed directing their search towards (Table 6). We construct indices of realized firm and job quality, where higher indices correspond to more productive firms or jobs higher up the ladder.

Column 1 of Table 10 shows that for those offered vocational training, realized firm quality is significantly lower for those also offered matching ($p = .035$). Indeed, vocational trainees with matching end up at firms of lower quality than controls. The effects on each component of the index in Table A13 reveal firm quality is lower for this group because they are significantly more likely to end up in informal firms and firms less likely to provide other benefits.[24]

Among those only offered matching, they also end up in firms of lower quality than controls because they are more likely to end up employed in informal firms.

Column 2 shows that among those offered training, realized job quality is also significantly lower among those additionally offered matching ($p = .077$). The treatment effects on each component of the job quality index are shown in Table A14: this reveals the key distinction between the two is that those offered only training are significantly more likely to end up in jobs that enable them to supervise others. In contrast, for youth offered both training and matching up to five years earlier, they end up in jobs not significantly different to those for controls.

A measure of worker-firm match quality is the length of the employment spell. Column 3 in Table 10 reveals: (i) those offered training have significantly longer employment spells than controls – the magnitude of the effect is 1.24 months, corresponding to a 22% increase over controls; (ii)

[24] 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.

employment spells are about half the magnitude for trainees additionally offered matching and this is significantly different to those only offered training ($p = .015$).

The results all point to positive assortative matching between workers, jobs and firms: those offered training end up higher up the job ladder; this progression is slower for those additionally offered matching. This is despite both groups graduating with identical sector-specific skills. The fact they sort to different firms, jobs and sectors all represent a misallocation of talent. This misallocation is caused by the revised expectations workers with match offers have, because they misattribute the lack of call backs and become discouraged in their search, exerting lower search effort and directing it to lower quality firms. These results represent novel experimental findings on how sorting patterns between workers, firms and jobs are shaped by labor market interventions in a low-income setting.

Self-employment It is natural to consider the extent to which the interventions impact career progression via self-employment. Column 4 of Table 10 shows that workers in all treatment arms are more likely to engage in self-employment. Increases in self-employment occur entirely within the study sectors, and we observe significant reductions in workers assigned to vocational training being engaged in self-employment in other sectors (Column 5). We saw earlier that long run non-employment rates even for skilled workers remain around 30%, so labor markets do not clear even for them [Banerjee and Sequeira 2022]. Hence the movement into self-employment 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.

For workers only offered matching, the impact on self-employment (4pp) corresponds to a near 66% increase over controls. This aligns with their stated intent to borrow to start up in self-employment. Entry into the labor market via self-employment might be key because they are untrained and so it is difficult for them to find wage employment in good sectors.

5.5 Dynamics

To summarize long run impacts and estimate dynamic treatment effects, we construct 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 firms and realized jobs. The ITT treatment effects on this index are in Column 6 of Table 10. On this 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 ($.051\sigma$). In short, the impacts of matching on those offered training undo half of what is achieved through training alone. The overall long run impact of matching is not significantly different to controls.

Figure 5 presents dynamic treatment effect estimates on this index of labor market success by survey wave. This shows the gradual improvement in outcomes for those offered training, diverging away from the slight decline in outcomes for those additionally offered matching. Within each treatment arm, we cannot reject the null that impacts are equal across periods. Within survey wave, our overall index implies vocational trainees have significantly greater labor market success at waves two and three than those additionally offered matching ($p = .042$, $.014$). This hints at the possibility that by the final survey wave – some 55 months after training has completed – trainees with match offers finally start to catch up to those only offered training. The cumulative losses to them, in terms of earnings and labor market attachment, however remain substantial.[25]

Our findings contribute to an ongoing debate about the persistence of intervention impacts in low-income contexts. They emphasize that initial conditions upon labor market entry have persistent impacts on the outcomes of young job seekers: the skills and expectations these individuals have when entering the labor market matter up to six years later. Among those offered vocational training and matching, the discouragement caused by a lack of call backs effectively scars these youth as they transition into the labor market. 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 to start up in self-employment.

6 Outcomes, Expectations and Search

We use mediation analysis to link our two sets of results – mapping how labor market interventions translate into long run labor market outcomes via experimentally induced changes in skills, expectations and search behavior. Following Gelbach [2016], 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 left unexplained. The outcome we focus on is the index of labor market success, and consider as mediators: sector-specific skills, the expected job offer arrival rate of a job in their preferred good sector in the next year, the minimum expected earnings conditional on employment in a good sector job, whether they have actively searched for a job in the last year, 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 (Column 6 in Table 10),

[25] Our results are reminiscent of the scarring literature, that shows persistent effects of graduating in a recession or other differences in initial conditions [von Watcher 2020]. Our results offers potential mechanisms driving such dynamics, through changes in workers expectations and search behaviors.

and within each we show the contribution of each mediator. Among workers offered training, certified sector-specific skills are the most important mediator: 20% of the long run impact on labor market outcomes is mediated through skills. This reinforces the findings from Alfonsi *et al.* [2020]. Expectations explain a further 18% of the long run impact, and so are almost as important as skills. Specifically, 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%. Other mediators have relatively muted roles.

Among workers additionally offered matching, sector-specific skills and expectations explain 41% and 17% of the labor outcomes index respectively. However, given the overall ITT to be explained is half the size (.115σ vs. .051σ), the absolute importance of skills is the same for those offered training, with or without matching. This is as expected given the accumulation of sector-specific skills does not differ between them. Expectations and search behaviors play less of a role in determining the long run labor market success of those offered both training and matching – because these workers are discouraged, and so end up with expectations and search behaviors closer to controls overall. For workers only offered matching, no single mediator is prominent, although borrowing has a positive effect.

7 Discussion

7.1 Revisiting Alfonsi et al. [2020]

It is useful to bridge the insights of this analysis to our earlier work, Alfonsi *et al.* [2020], using data from this project. There we contrasted labor market returns to certified vocational training versus non-certified firm-sponsored apprenticeships. In the comparison between these supply and demand side policies to train workers, we showed the returns to vocational training are higher because certified skills aid labor market mobility. The current analysis reaffirms that certifiable skills still play a driving role in the labor market success of those offered vocational training relative to controls, irrespective of whether they are also offered matching. In contrast to our earlier work, we have not considered firm-sponsored training in the current analysis because job search is not relevant for firm-sponsored apprenticeships.[26]

Our earlier work largely combined the vocational training arms (with and without match offers). The justification for doing so was that the low rate of call backs suggested search frictions do not play a large role for firms. What the current analysis brings to the fore is that in the match offer treatments, the lack of call backs to workers still shapes the expectations and search behavior of young workers, and this in turn determines their long run labor market outcomes over and above

[26] 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.

the direct effects of acquiring certified skills that we focused on in Alfonsi *et al.* [2020]. The current analysis shows the near equal importance of skills and expectations in determining long run labor markets outcomes for youth offered training. Here we find that *despite* the increased mobility due to certifiable skills, trainees with match offers do significantly worse than those only offered training. The reason is novel: they are imperfectly informed and misattribute the lack of call backs from match offers, causing them to revise down their expectations over their own prospects and search differently as they transition into the labor market. This leads to differential patterns of sorting for them: they end up at worse firms and in worse jobs – progressing less far on the job ladder from casual work towards good jobs.

The two sets of analyses are complementary and together provide a detailed picture of the determinants of labor market outcomes for youth in a low-income context.

7.2 External Validity

Scalability and Alternative Informational Interventions The vocational training courses in our study sectors are normally offered by VTIs throughout Uganda. This treatment thus represents a scalable market-based intervention. Our match offer is relatively light-touch and scalable. We highlight young job seekers can misattribute information provided to assist them in job search. This lesson applies more broadly, emphasizing the need to consider the framing of job assistance, careers advice or counselling.

Purely informational interventions 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 [Gonzalez and Shi 2010]. One way to therefore distinguish between informational interventions is whether they offer directed or undirected signals to workers. For example, in job fairs style job assistance or those that provide aggregate information [Abebe *et al.* 2020, Kelley *et al.* 2022, Chakravorty *et al.* 2023], signals conveyed to workers are relatively more informative of the status of the labor market as whole, rather than individual job prospects. Hence there is less scope for workers to misattribute signals as being informative of their own job prospects. Such interventions reduce over-optimism, but do less to discourage workers. At the other extreme are highly directed job assistance interventions that tailor feedback specific to the individual [Altmann *et al.* 2018, Belot *et al.* 2019]. Our matching intervention is closer to this second type. Moreover, in our intervention workers interact with one BRAC placement officer, who simply tells them that the matched firms are not interested in meeting with them, without providing reasons why. One way to potentially avoid risks of misattribution from directed informational interventions is by using mentors to provide continuous feedback to workers, as in Alfonsi *et al.* [2022].

Workers Individuals in our evaluation are the kind of disadvantaged youth that many job training programs target. We consider if our results would apply if other job seekers were targeted. To shed light on this dimension of external validity, we consider heterogeneous treatment responses with regards to cognitive ability and the psychological trait of self-evaluation – an appraisal of one's worthiness, effectiveness, and capability [Judge *et al.* 2002]. We discuss these in more detail in the Appendix and here describe the main findings.

Panel A of Figure A3 shows 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. This reconfirms the notion that workers likely understood the nature of match offers. Panel B shows the analysis split between workers of high and low self-evaluation. A similar pattern of homogeneous results emerge, again suggesting our results might extend to other samples of young job seeker. This also implies 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.

Firms A lack of labor demand is a key constraint in matching workers to firms. Even though firms in our study normally recruit young job seekers, low call back rates are driven by a lack of vacancies. The constraint is logistical in that between when the firm sample is drawn, and when vocational training completed and match offers were made, changes in demand mean that even if firms report binding hiring constraints at baseline, this might no longer be the case by the time match offers are implemented. Alternative approaches to raise call back rates in matching interventions would be to use more sophisticated algorithms to assign workers to firms [Horton 2017] or provide more information to firms [Pallais 2014, Groh *et al.* 2016, Bassi and Nansamba 2022, Carranza *et al.* 2022].

7.3 Policy Implications

Our study has three implications for the design of labor market interventions. First, the value of vocational training operates through giving workers valued and certified skills, but also by changing their expectations – making them optimistic with regards to their job prospects. This drives them on to search more intensively, approach firms directly, and target higher quality firms. Such beliefs might be motivated or help overcome biases such as procrastination [DellaVigna and Paserman 2005], but overall it is not obvious that there are always positive returns in job search from holding optimistic beliefs – it can also lead to frustration [Genicot and Ray 2017, Banerjee and Sequeira 2022]. Job search is a difficult and complex process requiring prolonged motivation. As van Hooft [2016] describes, the complexity arises from job search being a non-routine activity in which individuals have limited experience, it involves utilizing a wide array of strategies and methods, it occurs in an ambiguous and competitive environment, and it can be a lengthy process involving multiple rejections. Throughout job seekers have to avoid their motivation being undermined. Understanding intervention design features that can aid optimism rather than discouragement remains an issue for future work.

Second, an important feature of our study is that match offers are implemented as workers graduate from training. As we show, the process of training leads to a rapid change in worker beliefs – putting them onto trajectories of beliefs diverging from realistic outcomes. It is exactly then when match offers are implemented. It remains an open question as to whether had match offers been implemented after trained workers had searched for good jobs by themselves, their likelihood to discourage workers would have differed.

Finally, our findings relate to policy discussions about how to incentivize providers of vocational training 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 when trying to find their own graduates employment [Banerjee and Chiplunkar 2023].

7.4 Future Agenda

Labor markets play a critical role in the process of economic development. The efficient matching of workers to firms is key for individual welfare, but also has macroeconomic consequences in determining labor productivity, the firm size distribution, the nature of macroeconomic cycles, and aggregate growth. In the context of a low-income economy, we show how individual expectations are critical for understanding how youth search for good jobs, and are able to transition away from a reliance on casual labor towards more regular wage employment. Our analysis points to the need to incorporate the role of skills, worker expectations and multiple margins of search behavior into job search models. Important recent contributions have considered the evolution of expectations with job search [Conlon *et al.* 2018, Mueller *et al.* 2021, Potter 2021, Mueller and Spinnewijn 2023]. Our results point to the expectations formation process depending on the skills of workers, on (misinterpreted) signals about job prospects, and endogenous search effort. Incorporating such features would advance our understanding of what are likely to be the most effective labor market policies to help youth find good jobs in urban labor markets in the developing world.

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 A4 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.[27]

[27] 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

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%.

A.3 Credit

The link between labor and credit markets has been recognized in the literature. 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. Table A11 shows treatment effects the impacts on each component as well as the overall index.

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 ($.090\sigma$). The channels for this are that 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

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).

form of self-employment (Column 4).

A.4 External Validity: Workers

To shed light on the external validity of our findings to alternative samples of youth, we consider heterogeneous treatment responses with regards to two individual characteristics: cognitive ability and psychological traits.

As search models represent an optimal stopping problem, 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 – such as patience, self-confidence and internal locus of control – have for job search [DellaVigna and Paserman 2005, Caliendo *et al.* 2015, DellaVigna *et al.* 2022]. Three widely studied traits are self-esteem, locus of control, and neuroticism. Judge *et al.* [2002] 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.[28,29]

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. Cognitive ability and self-evaluation are not impacted by the treatments (Table A7), so we take both as time invariant. They are also uncorrelated ($\rho = .06$ for the continuous measures).

Cognitive Ability Panel A of Figure A1 shows treatment effects on the labour outcomes index for high and low cognitive ability individuals. Within each treatment arm, the ITT impact on the 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

[28] 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].

[29] 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.

ability individuals, we continue to find significant differences in the labor market success of those offered vocational training with and without matching ($p = .099, .011$).

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. 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 without matching ($p = .004, .016$).

A.5 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.

Study findings have been presented in multiple meetings with 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 our findings.

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Table 1: Baseline Balance on Labor Market Histories

Means, robust standard errors from OLS regressions in parentheses

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

P-value on F-tests in braces

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

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

Table 2: Jobs, Search and Recruitment

	Casual Jobs	Regular Jobs
A. Job Characteristics		
Worked in this activity in the last month	.256	.179
Self-employed	.661	.202
Number of months involved in activity in the last year	1.95	1.57
Hours worked in a typical day | employed	5.08	8.32
Days worked in a typical week | employed	5.13	5.43
Earnings in the last month | employed	9.76	24.5
B. Worker Job Search Methods		
Through friends/family member	.193	.472
Direct walk-in	.067	.250
Immediate family owns the business	.161	.060
Read job ad	.008	.016
C. Firm Recruitment Strategies		
Direct walk-in		.424
Through friends/family member		.396
Worker is a family member		.135
Posted job ad		.013
D. Screening		
Had to interview	.013	.188
Had to provide references	.020	.185
Had to take a skills test	.028	.261

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. The list of methods is not exhaustive, as it excludes self-employed individuals who started their firm from scratch. Panels C and D show the share of employees hired through the corresponding method. The top 1% of earnings values are excluded. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 3: Evolution of Expectations

Means, standard deviations in parentheses

	Row		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
			(1)	(2)	(3)
At Baseline	**R1**	**Assigned to Vocational Training (T1, T2)**	5.59	40.0	71.5
			(2.83)	(35.0)	(58.6)
	R2	**Not Assigned to Vocational Training (C, T3)**	5.71	42.1	74.6
			(2.90)	(36.8)	(62.1)
On Eve of Announcement of Matching	**R3**	**Assigned to Vocational Training (T1, T2)**	8.32	82.8	209
			(1.61)	(55.4)	(250)
	R4	**Not Assigned to Vocational Training (C)**	5.04	43.0	75.5
			(2.06)	(26.7)	(45.0)
		p-value on tests of equality across rows: R1 = R2	*[.435]*	*[.307]*	*[.363]*
		R1 = R3	*[.000]*	*[.000]*	*[.000]*
		R2 = R4	*[.000]*	*[.672]*	*[.780]*
		R3 = R4	*[.000]*	*[.000]*	*[.000]*

Notes: The data used is from baseline, VTI surveys conducted towards the end of the training period while trainees were still enrolled at the vocational training institutes, and we extrapolate back from the first worker follow-up survey assuming a linear evolution of beliefs, what would have been beliefs among Controls at the same time as the VTI survey was being fielded. 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}*	*{.652, 645}*
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}*	*{.001, .001}*	*{.095, .104}*
Matching	.242	3.21	6.04	3.47	-.000
	(.216)	(3.05)	(4.97)	(4.44)	(.007)
	{.276, .270}	*{.317, .276}*	*{.248, .229}*	*{.462, .435}*	*{.993, .990}*
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 within the same regression (i.e. for testing multiple treatments) 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: 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}	*{.916, .924}*	*{.015, .010}*	*{.139, .114}*	*{.001, .001}*	*{.026, .036}*
Vocational Training + Matching	.097**	-.713	.060*	-.005	.056*	.019
	(.040)	(6.70)	(.036)	(.036)	(.030)	(.046)
	{.012, .015}	*{.908, .898}*	*{.092, .102}*	*{.893, .889}*	*{.074, .061}*	*{.651, .656}*
Matching	-.036	-11.2*	-.036	-.000	-.004	-.003
	(.041)	(6.44)	(.033)	(.036)	(.028)	(.041)
	{.383, .399}	*{.072, .074}*	*{.285, .254}*	*{.999, .998}*	*{.886, .895}*	*{.954, .930}*
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 within the same regression (i.e. for testing multiple treatments) 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 6: Desired Sorting and Directed Search

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

	Firm Wages	Ideal Firm Searched For	Ideal Job Searched For
	(1)	(2)	(3)
Vocational Training	.110***	.103***	-.051
	(.036)	(.036)	(.040)
	{.001, .002}	*{.007, .012}*	*{.204, .199}*
Vocational Training + Matching	.030	.030	-.010
	(.039)	(.039)	(.041)
	{.417, .444}	*{.449, .411}*	*{.809, .791}*
Matching	-.048	.042	-.065
	(.037)	(.039)	(.042)
	{.212, .217}	*{.279, .274}*	*{.122, .124}*
P-value: VT = VT + Matching	*[.050]*	*[.102]*	*[.351]*
Mean in Control Group	.338	-.047	.017
N. of observations	1,213	1,215	1,231

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing within the same regression (i.e. for testing multiple treatments) are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 2 the Ideal Firm Searched For Index has the components in Columns 1 to 5 of Table A9. In Column 3 the Ideal Job Searched For Index has the components in Columns 1 to 5 of Table A10. 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 7: First Jobs

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

	Months between intervention and first job	First job in one of eight good sectors	Formal contract in first job	Monthly earnings in first job
	(1)	(2)	(3)	(4)
Vocational Training	-1.74***	.227***	.059*	8.32**
	(.605)	(.039)	(.034)	(3.88)
	{.004, .006}	*{.000, .001}*	*{.089, .193}*	*{.036, .028}*
Vocational Training + Matching	-1.61**	.222***	-.020	-4.88
	(.696)	(.044)	(.033)	(3.99)
	{.022, .017}	*{.000, .001}*	*{.543, .553}*	*{.224, .231}*
Matching	-.719	.013	-.030	-3.40
	(.702)	(.043)	(.034)	(3.80)
	{.306, .306}	*{.759, .760}*	*{.376, .553}*	*{.374, .383}*
P-value: VT = VT + Matching	*[.847]*	*[.916]*	*[.022]*	*[.002]*
Mean in Control Group	13.6	.312	.118	60.2
N. of observations	1,037	1,051	722	974

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, 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 within the same regression (i.e. for testing multiple treatments) are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. Oucomes in Columns 1 to 4 are conditional on the worker finding a job starting from August 1st 2013, when the training ended, up to the third follow-up survey. In Column 1, the outcome is the number of months between the end of the training intervention on August 1st 2013 and the beginning of the first job. In Column 3, the outcome is conditional on the worker being in wage employment (so, workers in self-employment are excluded). In Column 2 the eight study sectors are: motor-mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical wiring and welding. 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 8: Employment

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}	*{.988, .996}*	*{.000, .001}*	*{.000, .001}*	*{.000, .001}*	*{.000, .001}*
Vocational Training + Matching	.063***	.005	.066***	.690***	1.54***	.248***
	(.023)	(.017)	(.024)	(.257)	(.228)	(.044)
	{.005, .007}	*{.748, .767}*	*{.005, .005}*	*{.005, .011}*	*{.000, .001}*	*{.000, .001}*
Matching	.051**	-.003	.054**	.510**	.556***	.117***
	(.022)	(.017)	(.023)	(.246)	(.203)	(.040)
	{.025, .024}	*{.840, .826}*	*{.029, .018}*	*{.047, .040}*	*{.009, .007}*	*{.007, .002}*
P-value: VT = VT + Matching	*[.152]*	*[.765]*	*[.043]*	*[.011]*	*[.104]*	*[.030]*
Mean in Control Group	.623	.169	.524	5.92	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 within the same regression (i.e. for testing multiple treatments) 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: Earnings

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)
	(1)	(2)	(3)	(4)	(5)
Vocational Training	11.0***	1.12	8.07***	.002	-1.24***
	(2.52)	(.770)	(2.33)	(.023)	(.235)
	{.000, .001}	*{.148, .151}*	*{.000, .001}*	*{.916, .909}*	*{.000, .001}*
Vocational Training + Matching	6.11**	-.437	5.74**	.089***	-.667**
	(2.89)	(.870)	(2.69)	(.025)	(.259)
	{.040, .021}	*{.625, .647}*	*{.032, .017}*	*{.000, .002}*	*{.010, .010}*
Matching	3.27	.610	1.25	-.018	-.411
	(2.71)	(.957)	(2.47)	(.024)	(.250)
	{.255, .252}	*{.552, .538}*	*{.622, .625}*	*{.453, .465}*	*{.099, .103}*
P-value: VT = VT + Matching	*[.099]*	*[.102]*	*[.396]*	*[.001]*	*[.023]*
Mean in Control Group	43.3	5.15	38.0	-.019	6.20
N. of observations	3,125	3,269	3,541	3,570	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 within the same regression (i.e. for testing multiple treatments) 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 Column 5 , the length of Last Unemployment spells refer to spells in which the respondent has been involved in the last year. The maximum value is 12 months, which correspond to the respondent having been involved in the same unemployment spell for the entire year. TAll 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.

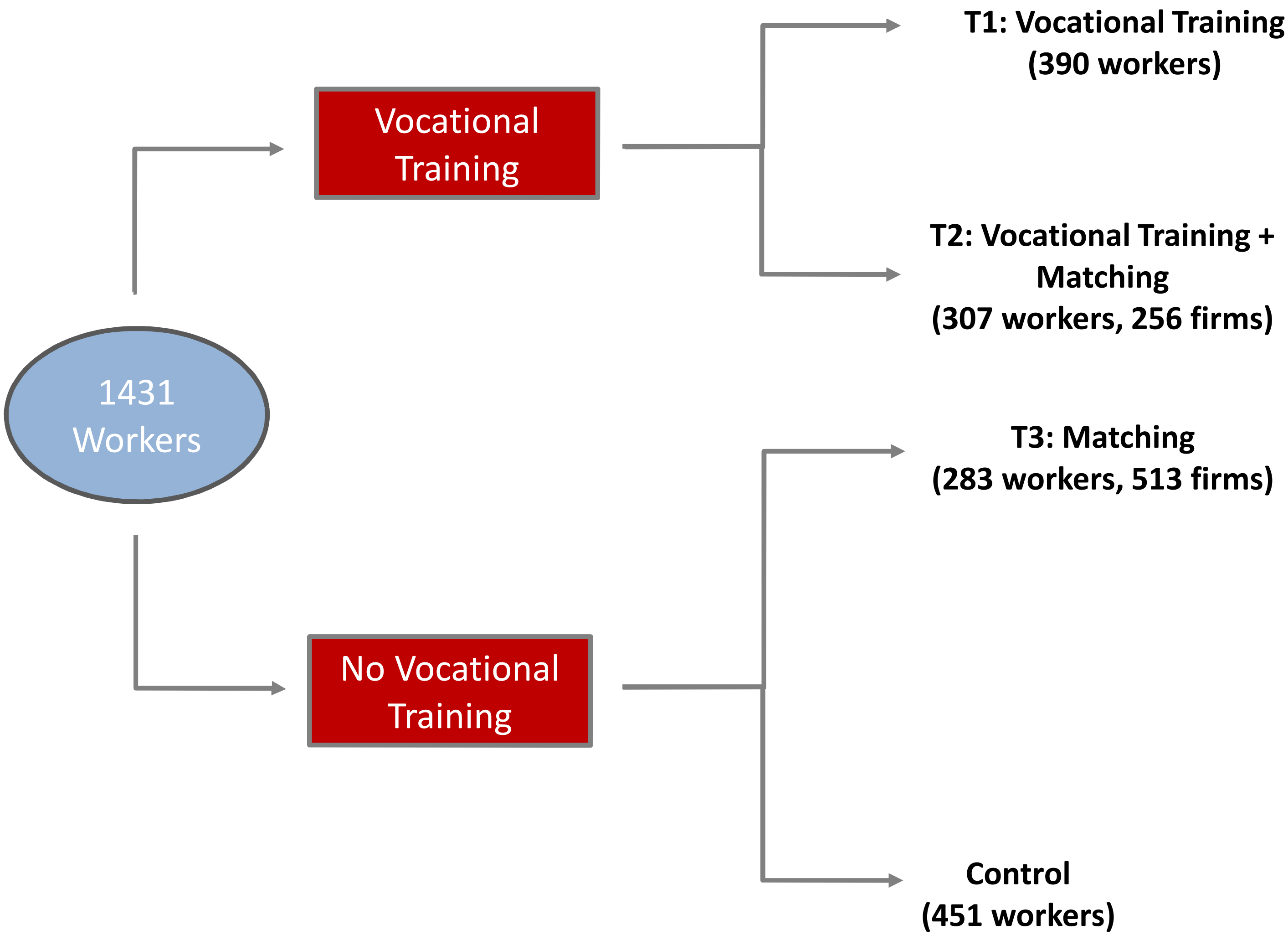
Table 10: Realized Sorting

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

	Realized Firm	Realized Job	Length of last employment spell (months)	Has done any self-employment in one of the eight study sectors in the last month	Has done any self-employment in other sectors in the last month	Labor Outcomes Index
	(1)	(2)	(3)	(4)	(5)	(6)
Vocational Training	.003	.096***	1.24***	.104***	-.047***	.115***
	(.028)	(.029)	(.234)	(.013)	(.015)	(.018)
	{.891, .910}	*{.002, .004}*	*{.000, .001}*	*{.000, .001}*	*{.001, .002}*	*{.000, .001}*
Vocational Training + Matching	-.058*	.042	.619**	.076***	-.029*	.051***
	(.031)	(.032)	(.258)	(.015)	(.017)	(.020)
	{.069, .049}	*{.204, .211}*	*{.018, .013}*	*{.000,.001}*	*{.080, .085}*	*{.008, .011}*
Matching	-.067**	-.013	.452*	.040***	-.002	.020
	(.031)	(.030)	(.248)	(.013)	(.017)	(.018)
	{.027, .024}	*{.656, .665}*	*{.072, .068}*	*{.003, .003}*	*{.890, .893}*	*{.275, .262}*
P-value: VT = VT + Matching	*[.035]*	*[.077]*	*[.015]*	*[.100]*	*[.255]*	*[.001]*
Mean in Control Group	.034	-.025	5.63	.061	.154	-.043
N. of observations	2,504	2,429	3,693	3,699	3,699	3,725

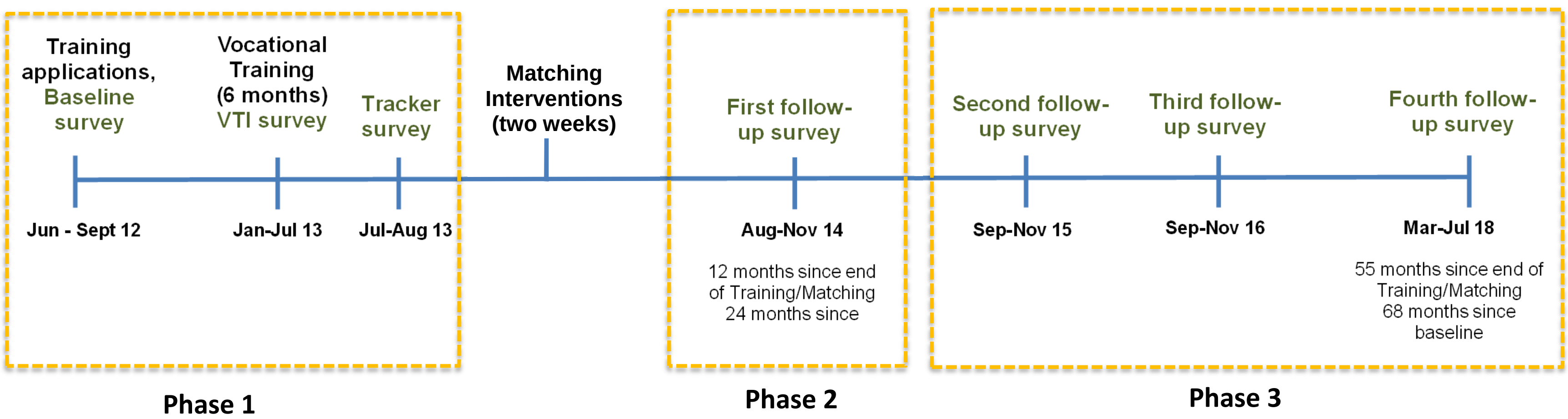
Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the second, third and fourth worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a 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 within the same regression (i.e. for testing multiple treatments) are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 the Realized Firm Index has the components in Columns 1 to 5 of Table A12. In Column 2 the Realized Job Index has the components in Columns 1 to 5 of Table A14. In Column 3 , the length of Last Employment spells refer to spells in which the respondent has been involved in the last year. The maximum value is 12 months, which correspond to the respondent having been involved in the same employment spell for the entire year. The components of the Labour Outcomes Index in Column 6 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.

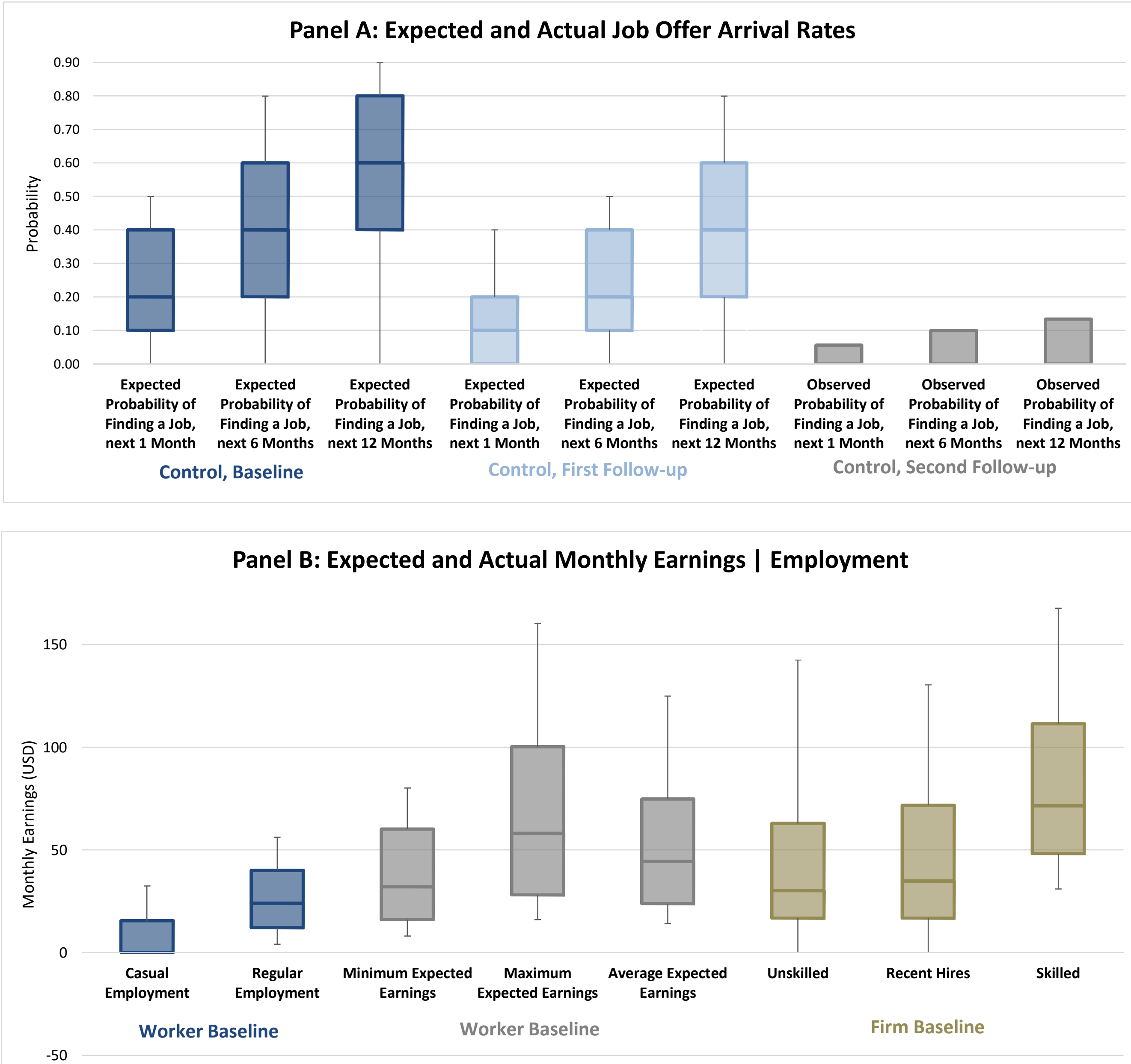
Figure 2: Study Timeline



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

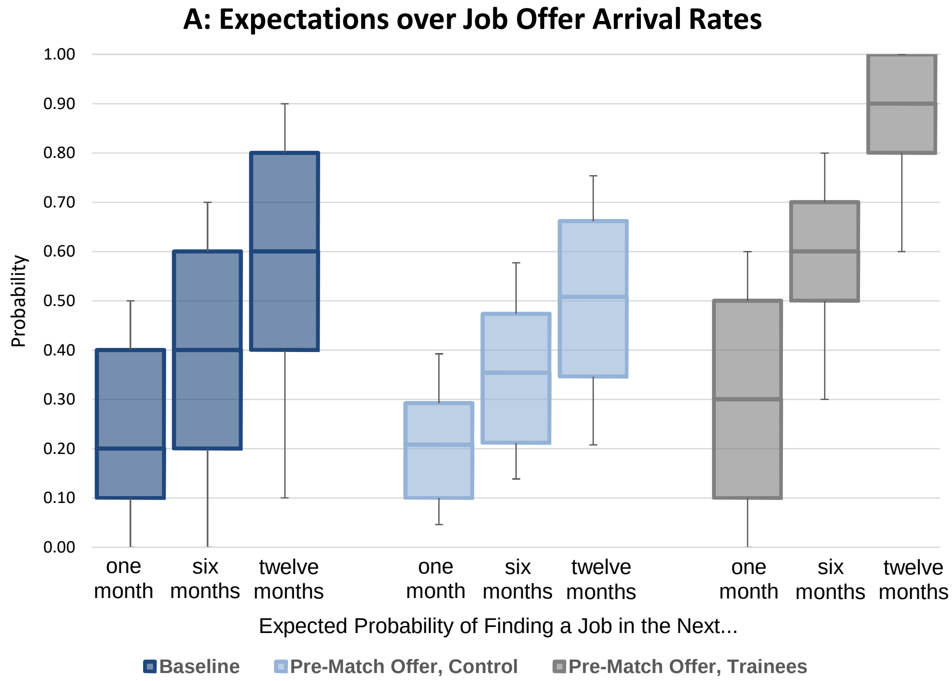
10th, 25th, 50th, 75th and 90th percentiles

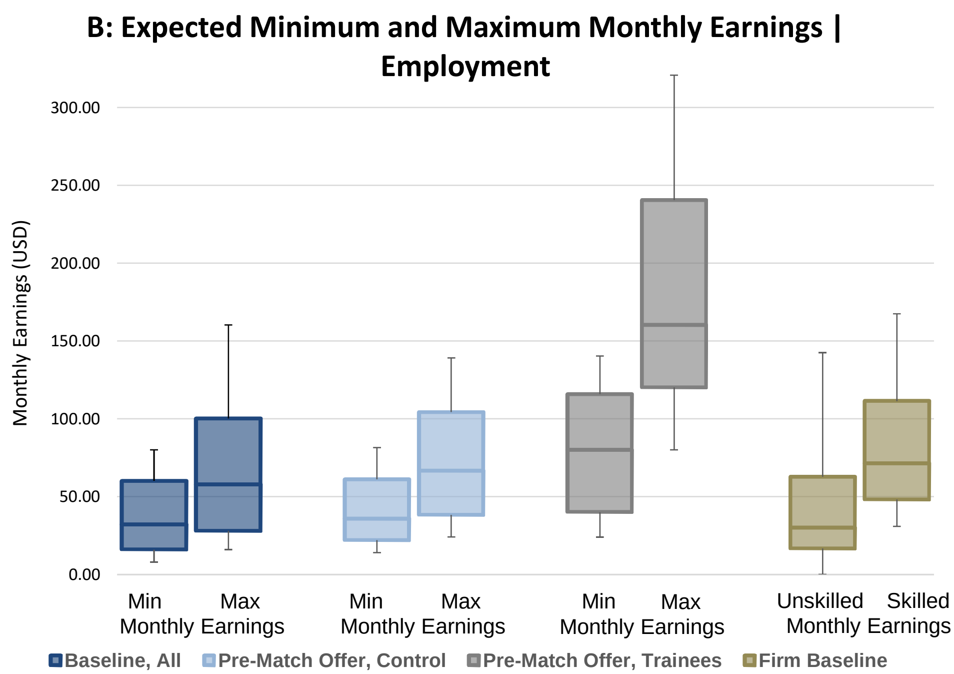


Notes: Panel A shows the distribution of expected probabilities of finding a job at various horizons, at baseline and first follow-up. The third set of bars are for the actual probabilities of finding employment in these good sectors among control workers at second follow-up. The sample used to construct 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: The Evolution of Expectations Until Match Offers are Announced

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

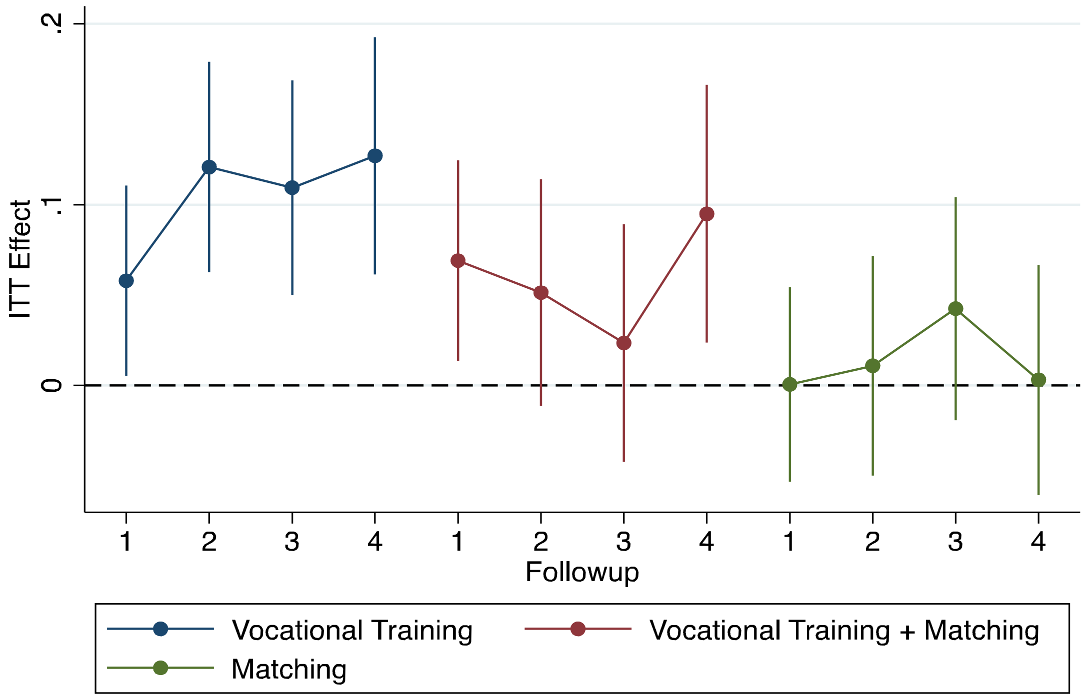




Notes: The data used is from baseline, VTI surveys conducted towards the end of the training period while trainees were still enrolled at the vocational training institutes, and we extrapolate back from the first worker follow-up survey assuming a linear evolution of beliefs, to what would have been 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.

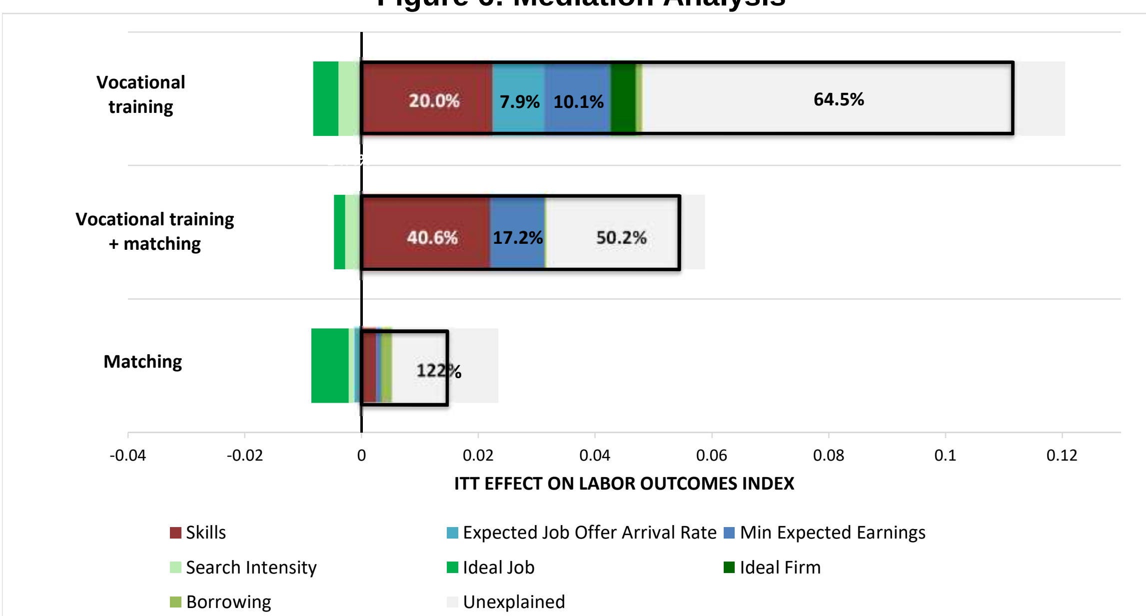
Figure 5: Dynamics

Labor Market Index



Notes: The graph shows coefficients and 95% confidence intervals for the ITT effects on the Labour Market Index at each follow up. All coefficients reported in each panel are estimated from the same dynamic treatment effects regression, where the treatment indicators are interacted with dummies for each survey wave, with robust standard errors. All regressions include strata dummies, survey wave dummies and a dummy for the implementation round. The Labor Market Index takes: (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 index is constructed following Anderson's [2008] approach.

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.

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		0.026
		(.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]	[.000]
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	-.054	.031	-.011	-.004
	(.082)	(.059)	(.075)	(.076)
Age	-.017	-.002	.022**	-.006
	(.014)	(.012)	(.011)	(.004)
Any Child	-.031	-.055	-.049	.027
	(.082)	(.079)	(.048)	(.028)
Education Level	.026	.015	-.006	-.011*
	(.017)	(.025)	(.011)	(.006)
Has Ever Worked	-.007	-.171*	-.033	.063
	(.089)	(.090)	(.056)	(.041)
Literacy/Numeracy Test Score	-.004	.006	-.009	-.004
	(.014)	(.024)	(.013)	(.004)
PANEL B: Firm Characteristics				
Owner would like to Expand	.197**		-.007	
	(.092)		(.056)	
Firm constrained by Lack of Trustworthy Workers	.117*		-.045	
	(.066)		(.077)	
Firm constrained by Inability to Screen Workers	-.131*		.079	
	(.077)		(.071)	
Owner Age	-.008*		.000	
	(.005)		(.004)	
Owner Education Level	.024**		.002	
	(.009)		(.008)	
Firm Age	.002		.002	
	(.005)		(.011)	
Number of Employees	-.029*		.027	
	(.015)		(.018)	
Log (Monthly Profits)	.046		.004	
	(.041)		(.035)	
Mean of dep. var. in control	.162		.178	
P-value: firm covariates	[.040]	-	[.873]	-
P-value: worker covariates	[.450]	[.614]	[.465]	[.631]
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	302	302

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)	(2.59)
	{.625, .626}	*{.387, 410}*	-
P-value: VT = VT + Matching	*[.852]*	*[.488]*	*[.267]*
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 within the same regression (i.e. for testing multiple treatments) 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)
	{.983, .984}	*{.564, .574}*	*{.843, .849}*	*{.771, .761}*	*{.099, .089}*	*{.469, .467}*	*{.557, .555}*	*{.101, .092}*	*{.236, .218}*	*{.456, .444}*	*{.342, .354}*
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)
	{.606,.617}	*{.578, .574}*	*{.865, .854}*	*{.227,.209}*	*{.311, .286}*	*{.221, .255}*	*{.067,.069}*	*{.457, .443}*	*{.465, .482}*	*{.806, .793}*	*{.918, .939}*
Matching	.013	.055	-.056	-.161*	.139*	.092	-.047	.168	-.653	.475	-.082
	(.094)	(.086)	(.084)	(.083)	(.084)	(.189)	(.264)	(.164)	(.687)	(.303)	(.094)
	{.898, .892}	*{.526, .523}*	*{.499, .505}*	*{.060, .050}*	*{.099, .092}*	*{.626, .596}*	*{.855, .862}*	*{.309, .302}*	*{.346, .358}*	*{.123, .118}*	*{.386, .406}*
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 within the same regression (i.e. for testing multiple treatments) 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: Further Evidence on Misattribution and Discouragement

OLS regression coefficients, robust standard errors in parentheses

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

	Satisfied about job situation	Returns from Additional Training: Coefficient of Variation	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)	(6)	(7)
Vocational Training	.049**	.052	-.045	.014	-.016	-.038	-.048
	(.025)	(.060)	(.037)	(.036)	(.037)	(.036)	(.046)
	{.048, .048}	*{.416, .387}*	*{.225, .213}*	*{.697, .690}*	*{.657, .657}*	*{.335, .293}*	*{.299, .266}*
Vocational Training + Matching	-.025	.166***	-.058	.027	-.039	-.031	-.054
	(.023)	(.058)	(.041)	(.040)	(.040)	(.040)	(.052)
	{.249, .264}	*{.004, .006}*	*{.196, .137}*	*{.520, .468}*	*{.331, .333}*	*{.439, .436}*	*{.329, .284}*
Match Offer	-.017	.064	-.026	.017	-.004	-.054	-.039
	(.023)	(.072)	(.041)	(.041)	(.041)	(.040)	(.053)
	{.438, .459}	*{.387, .397}*	*{.518, .535}*	*{.660, .660}*	*{.919, .930}*	*{.189, .186}*	*{.473, .490}*
P-value: VT = VT + Matching	*[.004]*	*[.054]*	*[.749]*	*[.752]*	*[.569]*	*[.873]*	*[.907]*
Mean in Control Group	.097	.006	.581	.592	.441	.438	.028
N. of observations	1,210	695	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 within the same regression (i.e. for testing multiple treatments) are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1, the outcome is a dummy variable equal to one if the respondent reported being satisfied or very satisfied about the aspect indicated in the header (on a 1-5 scale). In Column 2, the expected returns from vocational training are calculated as the percentage difference between the worker's reported expectations with a formal vocational training program (or with additional vocational training for workers that have already received some) and the worker's expectations with his/her current skills set. For each of the variables in Columns 3 to 6, 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 outcomes in Columns 3 to 6 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 7 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 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)
	{.464, .463}	*{.571, .558}*	*{.017, .020}*	*{.026,.024}*
Vocational Training + Matching	-.245	-.095	.042*	.037
	(.155)	(.063)	(.025)	(.029)
	{.104, .128}	*{.167, .141}*	*{.086, .094}*	*{.200, .202}*
Matching	-.044	-.020	.040*	.022
	(.125)	(.054)	(.024)	(.028)
	{.728, .693}	*{.695, .724}*	*{.101, .080}*	*{.405, .472}*
P-value: VT = VT + Matching	*[.040]*	*[.058]*	*[.586]*	*[.463]*
Mean in Control Group	2.18	.810	.072	.122
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 within the same regression (i.e. for testing multiple treatments) 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)
	{.920, .931}	*{.500, .515}*	*{.965, .958}*	*{.245, .240}*	*{.241, .247}*
Vocational Training + Matching	-.043	-.020	.036	-.008	.002
	(.039)	(.038)	(.025)	(.018)	(.040)
	{.304, .271}	*{.645, .613}*	*{.145, .149}*	*{.659, .652}*	*{.966,.957}*
Matching	-.085**	-.026	-.032	.005	-.037
	(.039)	(.039)	(.030)	(.017)	(.041)
	{.032, .021}	*{.492, .484}*	*{.296, .291}*	*{.777, .787}*	*{.391, .375}*
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 within the same regression (i.e. for testing multiple treatments) 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: Credit

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	Credit Index
	(1)	(2)	(3)	(4)	(5)
Vocational Training	-.047	.049	.004	.017	.040
	(.034)	(.035)	(.005)	(.015)	(.049)
	{.148, .142}	*{.167, .159}*	*{.601, .417}*	*{.295, .264}*	*{.427, .390}*
Vocational Training + Matching	-.018	.027	-.004	-.006	-.035
	(.038)	(.038)	(.003)	(.014)	(.043)
	{.628, .626}	*{.490, .447}*	*{.260, .225}*	*{.660, .656}*	*{.450, .426}*
Matching	.046	.090**	-.003	.034*	.090*
	(.039)	(.039)	(.003)	(.019)	(.048)
	{.211, .226}	*{.025, .022}*	*{.386, .345}*	*{.080, .071}*	*{.072,.066}*
P-value: VT = VT + Matching	*[.446]*	*[.574]*	*[.130]*	*[.147]*	*[.132]*
Mean in Control Group	.325	.277	.003	.034	-.021
N. of observations	1,231	1,199	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 within the same regression (i.e. for testing multiple treatments) are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. 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. In Column 5 the Credit Index has the components in Columns 1 to 4, and 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 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)
	{.334, .316}	*{.564, .550}*	*{.760, .774}*	*{.900, .899}*
Vocational Training + Matching	.035	.018	.055**	.065***
	(.022)	(.018)	(.022)	(.023)
	{.110, .111}	*{.294, .295}*	*{.010, .010}*	*{.006, .004}*
Matching	-.024	.018	-.031	.013
	(.022)	(.019)	(.022)	(.022)
	{.294, .293}	*{.328, .369}*	*{.152, .156}*	*{.582, .574}*
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 within the same regression (i.e. for testing multiple treatments) 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 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)
	{.879, .903}	*{.844, .842}*	*{.048, .053}*	*{.463, .445}*	*{.790, .806}*
Vocational Training + Matching	-.415	-.062**	-.007	-.024	-.037**
	(1.26)	(.031)	(.028)	(.038)	(.017)
	{.744, .758}	*{.050, .042}*	*{.815, .826}*	*{.515, .536}*	*{.024,.030}*
Matching	-1.74	-.075**	.009	-.027	-.024
	(1.17)	(.030)	(.029)	(.036)	(.019)
	{.125, .147}	*{.016, .015}*	*{.760, .785}*	*{.444, .479}*	*{.217, .219}*
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 within the same regression (i.e. for testing multiple treatments) 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.

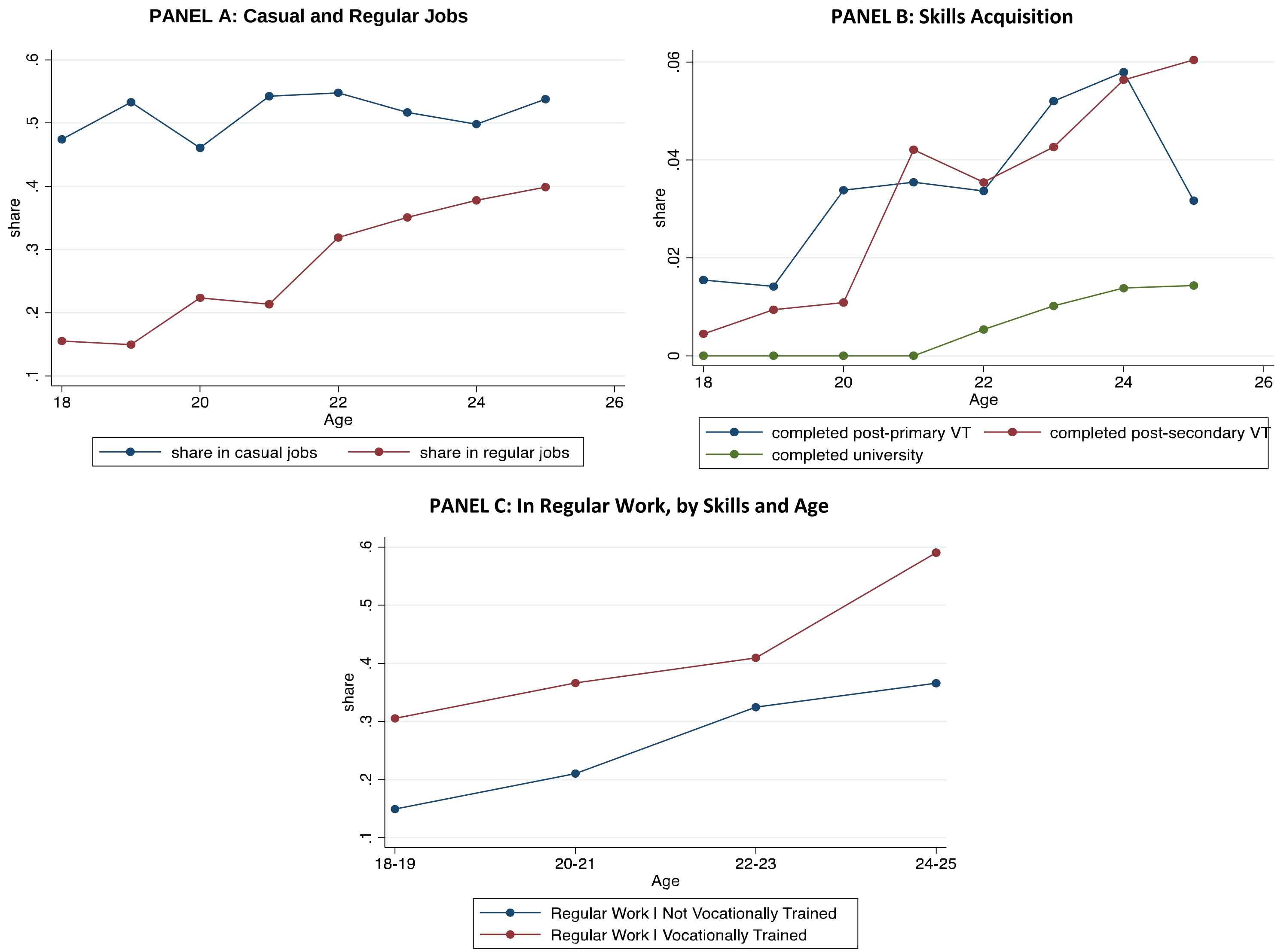
Table A14: Components of the Realized Job Quality Index

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, .021}	*{.038, .048}*	*{.001, .002}*	*{.034, .040}*	*{.871, .891}*
Vocational Training + Matching	-.003	.027	.061**	.058**	-.027
	(.031)	(.028)	(.031)	(.029)	(.030)
	{.944, .936}	*{.337, .330}*	*{.055, .044}*	*{.051, .045}*	*{.364, .359}*
Matching	.030	.010	-.038	-.032	.006
	(.030)	(.028)	(.030)	(.028)	(.029)
	{.296, .324}	*{.736, .704}*	*{.208, .186}*	*{.257, .262}*	*{.839, .820}*
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 within the same regression (i.e. for testing multiple treatments) 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.

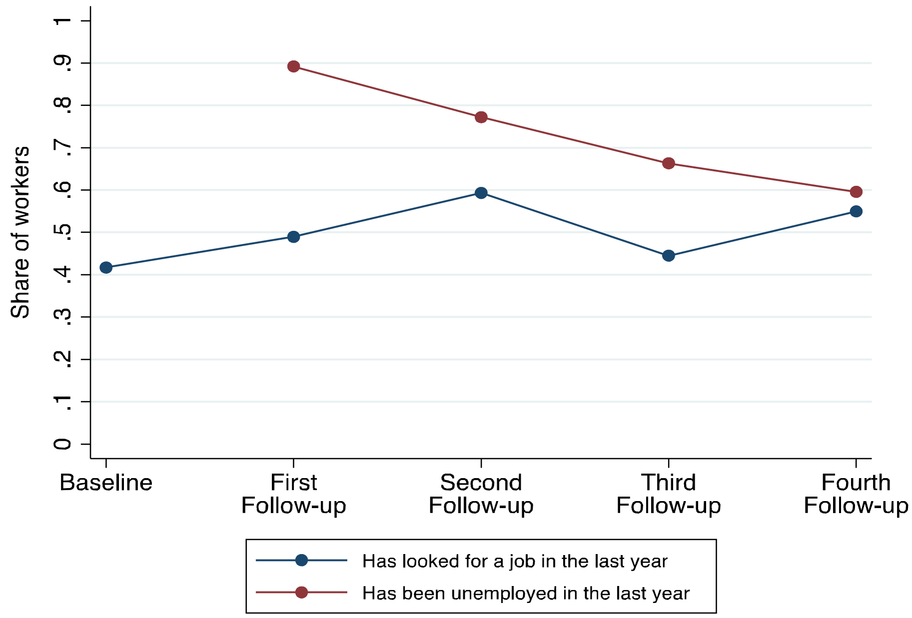
Figure A1: Jobs and Skills by Age



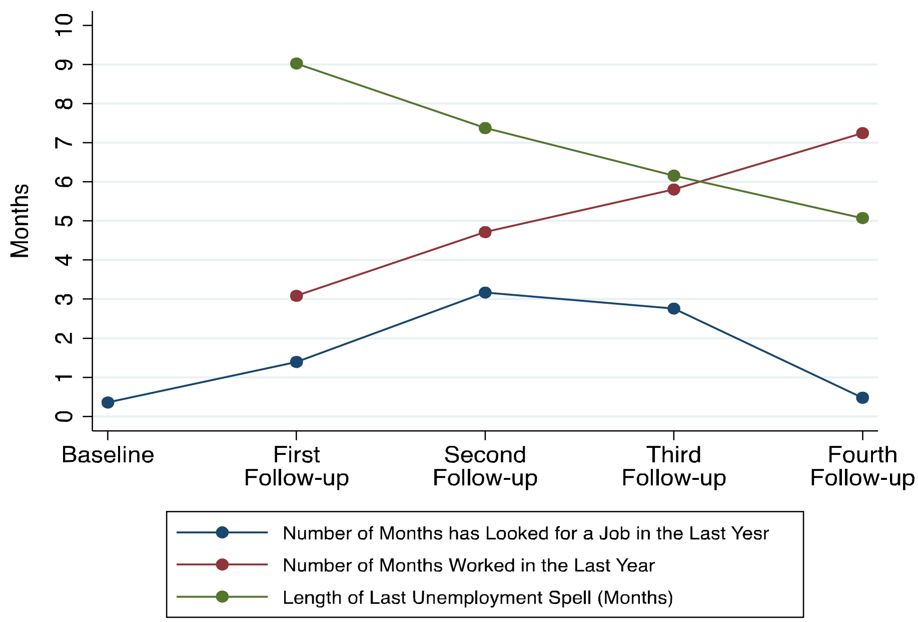
Notes: The data used is from individuals aged 18-25 and interviewed in the Uganda National Household Survey 2012/13 (UNHS) conducted by the Ugandan Bureau of Statistics. Panel A plots the share of individuals in casual and regular jobs by age. Involvement in the two types of jobs is not mutually exclusive. 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: Labor Market Outcomes and Search Effort Among Controls

PANEL A: Unemployment and Job Search



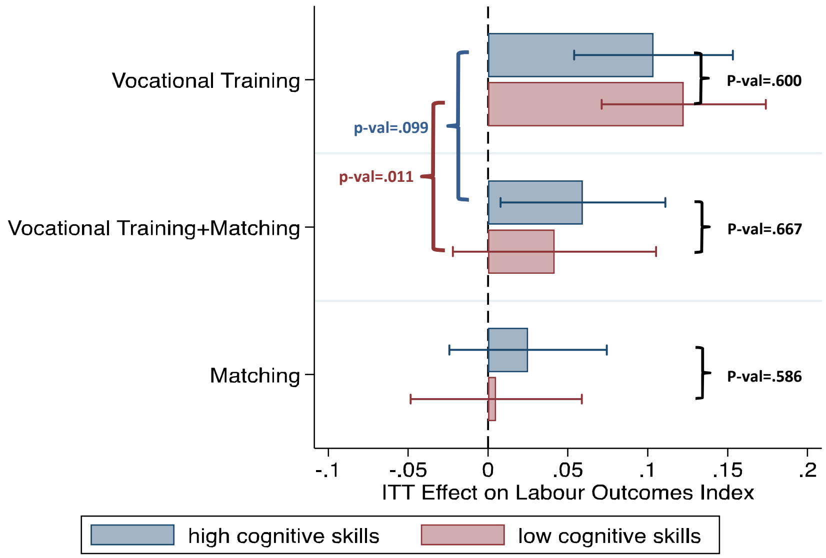
PANEL B: Unemployment Spells and Time Spent Searching for Work



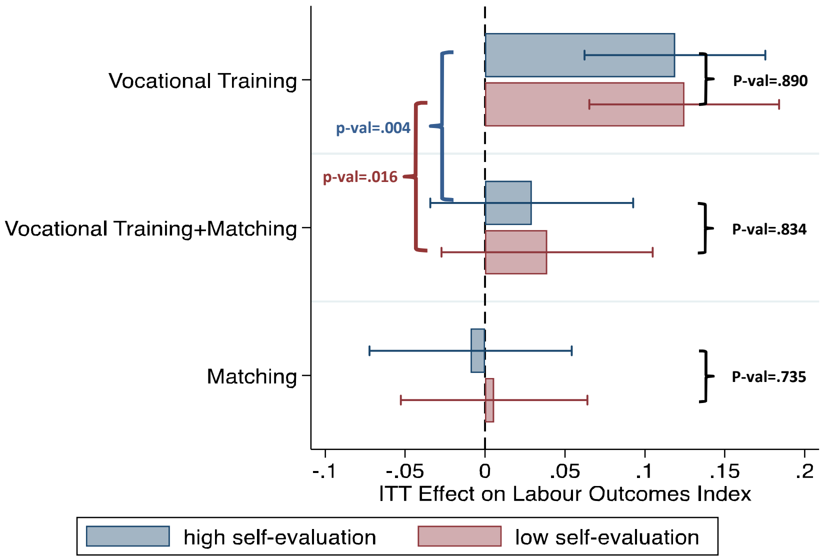
Notes: The sample only includes workers in the Control group. Panel A shows the share of individuals who have been unemployed any time last year, and the share of individuals who have looked for a job in the last year. Panel B shows the number of months the respondent has worked, and has looked for a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

Figure A3: External Validity

PANEL A: Heterogeneity by Cognitive Skills



PANEL B: Heterogeneity by Self-evaluation



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.

Figure A4: Sector Skills Test for Motor Mechanics

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