Worker Heterogeneity and Job Search: Evidence from a Six-Year Experiment in Uganda*

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Abstract

Developing countries face the challenge of aiding large cohorts of labor market entrants find good jobs. How to do so is complicated by job seekers differing in their skills, information and traits. We present results from a six-year field experiment studying job search behavior among youth in urban labor markets in Uganda, who at baseline, are unskilled yet optimistic over their job prospects. We engineer heterogeneity across workers through the offer of vocational training, and job assistance to meet with potential employers. Vocational training leads to measurable improvements in skills, while job assistance alters information workers have on their prospects, as call back rates from employers are low. Search behavior varies across the skills distribution: relative to controls, skilled youth become even more optimistic, search more intensively, and direct search towards better firms. The additional provision of job assistance to skilled youth causes them to revise down their beliefs, search less intensively and over lower quality firms. These differential search strategies impact long run outcomes: skilled workers without job assistance have higher employment rates and spell durations, and match to higher quality jobs and firms. Fixed traits across workers such as their cognitive ability and self-evaluation determine search strategies and outcomes because they interlink with how youth respond to the low call back rates from job assistance. Overall, our study provides insights on sources of worker heterogeneity driving labor market inequalities and inefficiencies, and on the design and targeting of labor market programs. *JEL: J64, O12.*

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1 Introduction

Labor markets play a critical role in the process of economic development. The efficient matching of workers to firms determines labor productivity, the firm size distribution, the nature of macroeconomic cycles, and aggregate growth. We study the process by which workers search for jobs in urban labor markets in a low-income setting: Uganda. In common with many developing countries, Uganda faces a challenge of having large cohorts of young people transitioning into the labor market each year, in search of meaningful work. Understanding how best to aid job seekers match to productive work is complicated by the fact they differ to each other in many dimensions, and there is unlikely to be one most effective policy.

We present evidence from a field experiment tracking young labor market entrants over six years, to shed light on how search strategies vary across workers, and how these different strategies then impact their long run labor market outcomes. We provide insights into the key sources of worker heterogeneity that drive individual search behavior, inequalities and inefficiencies in labor markets, and how policy can be better designed and targeted in low-income settings.

The experiment documents how individual search strategies vary with exogenous variation engineered along the following dimensions: (i) the vocational skills individuals have when they start searching for work; (ii) information they have over their own labor market prospects, as generated through a standard light-touch job assistance intervention. The study timeline allows us to map how changes in search strategies translate into long run outcomes. Under an efficient markets null, the importance of heterogenous initial conditions fade out because eventually the most efficient worker-firm matches occur, as either workers acquire the necessary skills, or learn their true labor market prospects. The alternative is that in the presence of market inefficiencies, arising say from search frictions, credit constraints or *ex post* bargaining, initial differences across workers can have persistent impacts.

Job search is a classic question in labor economics, with fifty years of work since the seminal papers by McCall [1970] and Mortensen [1970]. These emphasized the role reservation wages play in job search, with the optimal stopping problem being one in which workers continue to search until they receive a wage offer of at least their reservation wage. This workhorse framework has been extended in two fundamental directions: (i) to allow for worker heterogeneity; (ii) to consider search strategies beyond the reservation wage. The first of these motivates our experimental design. The second motivates the measurement tools in our data collection.

In classic job search models workers are assumed homogenous and so wage dispersion arises because workers exogenously receive different draws from the wage offer distribution, worker-firm match specific productivity draws, or posted wages. A fundamental shortcoming is that these channels cannot explain persistent differences across workers. A second generation of models introduce worker heterogeneity to deal with this, with differing assumptions over whether the key source of heterogeneity is productivity/skill [Mortensen and Pissarides 1999, Shimer and Smith

2000], education/skill [Acemoglu 1999], comparative advantage [Moscarini 2001], or psychological traits [DellaVigna and Paserman 2005].[1]

That search models advanced to incorporate heterogeneity around 2000 is no coincidence. A parallel literature using matched employer-employee (MEE) data, starting with the seminal work of Abowd *et al.* [1999], began to highlight the fundamental importance worker heterogeneity plays in empirically understanding key outcomes in labor and macro economics, such as earnings inequality, (un)employment spells, and labor market cyclicality.

The second direction in which job search models have been extended is to consider a richer set of search strategies. The most important advancements have been: (i) endogenous search effort [Pissarides 2000, Shimer 2004]; (ii) workers learning during search, where learning can be over the wage offer distribution [Wright 1986, Burdett and Vishwanath 1988] or the returns to own ability [Falk *et al.* 2006, Gonzalez and Shi 2010]; (iii) directed search, where workers search over specific jobs/firms (or parts of the wage distribution) [Moen 1997, Shimer 1996, Acemoglu and Shimer 1999, Shimer 2005].

We bridge the structural job search and reduced form MEE literatures by experimentally identifying the role that skills and information over own labor market prospects play in determining search strategies used by workers, and how these map into long run outcomes, thus explaining inequality in labor markets. We later contrast how important these experimentally induced sources of heterogeneity are relative to fixed immutable worker traits related to their cognitive ability and psychology. We thus provide one of the few economic analysis on individual labor market dynamics that combines experimental variation in worker's initial conditions, data on multiple dimensions of search strategies they then use – reservation wages, beliefs, search intensity, and the nature of directed search – with long run labor market outcomes including information on individual's actual job offers, employment, wages, hours, spells, and the characteristics of jobs and firms they match to.[2]

Labor market entrants were recruited into our study from across Uganda, through the offer of potentially receiving six months of sector-specific vocational training. In line with many labor market programs, the eligibility criteria targeted disadvantaged youth [Attanasio *et al.* 2011, Card *et al.* 2011]. We received 1400 valid applications from individuals with limited labor market experience and much scope to learn about their job prospects by searching. On the labor demand side

[1] A first generation of extensions to MacCall [1970] and Mortensen [1970] modelled labor markets in general equilibrium, where workers and firms meet through a matching function and wages are set through bargaining [Diamond 1982, Mortensen 1982, Mortensen and Pissarides 1994, Pissarides 2000]. These models largely assumed homogeneous workers.

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

of the experiment, we track 1281 firms operating in 15 urban labor markets, including Kampala. We selected firms: (i) operating in one of the eight manufacturing and service sectors in which we offered sector-specific vocational training: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering; (ii) having between one and 15 employees (plus a firm owner). The first criteria ensures we survey firms in sectors that our sample of young workers are seeking to match to through revealed preference of them having applied to the offer of training in these sectors. The second restriction excludes micro-entrepreneurs and ensures we focus on higher productivity firms in these sectors. These sectors constitute an important source of stable wage employment for youth in Uganda: at baseline, 25% of employed workers aged 18-25 work in them.

At baseline, sample workers have poor labor market histories, rely on informal contacts to find work, and hold casual jobs when they do work. They lack skills and likely face credit constraints to investing in the kinds of vocational training we offered. We view the sectors we offered training in as providing a chance to progress up the job ladder beyond these kinds of itinerant casual work. We document 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 over the job offer arrival rate from employers in these good sectors – such optimism has been documented among US job seekers [Spinnewijn 2015, Mueller *et al.* 2020, Potter 2020], Ethiopia [Abebe *et al.* 2020a] and South Africa [Banerjee and Sequeira 2020].[3]

Individuals are first randomly assigned to receive vocational training or not. In earlier work we documented that such intense and sector-specific training has large measurable impacts on worker skills, and the experimentally identified returns to such skills in urban labor markets are 20-30% [Alfonsi *et al.* 2020]. At a second stage of randomization, we offer light-touch job assistance to workers in the form of passing on their details to an established employer in a good sector. For skilled workers, these employers operate in the same sector as the worker has been trained; for unskilled workers randomized out of vocational training, the offer is for their details to be passed onto a firm in a sector in which they would have liked to have been trained. This job assistance is light touch because it is literally only the offer for the personal details of the worker to be passed onto one such potential employer.

Our design thus has four treatment arms, as Figure 1 summarizes: (i) the offer of vocational training; (ii) the offer of vocational training and job assistance; (iii) job assistance; (iv) controls.[4]

From the worker's perspective, the key outcome generated from such job assistance is whether the firm calls back the worker, inviting them to interview. To understand how workers might

[3] Examples of casual work they engage are animal rearing, fishing, loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing, portering/helping at a construction site.

[4] These job assistance treatments were not considered in Alfonsi *et al.* [2020]. At the same time, our earlier work compared the labor mobility induced by vocational skills training to that induced by offering firms wage subsidies in order to recruit workers and train them in-house via standard apprenticeships. This wage subsidy treatment plays no role in this study.

react to call backs (or a lack thereof), we track the evolution of worker beliefs from baseline to the eve of job assistance to workers being announced. We see a sharp bifurcation in beliefs over this period between those randomized in and out of vocational training. Trainees become gradually more exuberant over their job prospects: at the point of graduating (but before any announcement of job assistance is made), the median skilled worker believes there is a 30% chance in the next month of receiving a job offer from the kinds of good employer we consider in the firm-side of the experiment – this is far higher than a nationally representative survey in Uganda fielded close to our baseline (UNHS 2012/3) suggests could be plausible flow rates into regular employment for skilled workers.

Among those randomized out of training, they continue to search for work using the usual channels over the next six months, but with little improvement in their job prospects. Employment rates remain constant and they remain reliant on casual work. Over these six months of search, they gradually revise down their beliefs over the job offer arrival rate from firms operating in the kinds of high-wage sectors we consider. On the eve of job assistance being announced to unskilled youth, the median individual believes there is a 10% chance in the next month of receiving a job offer from an employer in our study sectors.

Match offers are then implemented to these groups of increasingly exuberant skilled youth, and increasingly realistic unskilled individual.

Among skilled workers the actual call back rate is far lower than their prior belief (16% vs. 30%). We show that call backs are actually determined by a lack of vacancies and other firm characteristics. Worker characteristics do *not* determine call backs. However, workers might misattribute the lack of call back as a negative signal of their job prospects – rather than reflecting a lack of vacancies or other firm-specific factors. In short, job assistance generates on average, bad news for skilled workers just as they complete six-months of intense sector-specific training and are meeting potential employers for the first time.

Among unskilled workers, the rate of call backs is more in line with their prior (18% vs. 15%). For them, call backs essentially provide confirmation/reduced uncertainty of their poor job prospects absent any changes in search strategy.

We show that neither skilled nor unskilled workers interpret low call back rates as signaling anything about labor market conditions more generally: they do not revise their priors on a range of beliefs related to shortages of firms or vacancies, or difficulties in being able to signal their practical or soft skills to employers. Rather, both groups of workers interpret the lack of call backs as implying something about their own job prospects.

Our first set of results document how exogenously engineered changes in worker initial conditions impact search strategies a full year after training is completed and/or job assistance implemented and call backs received (or not).

First, comparing workers offered vocational training to the control group (so ignoring those assigned to job assistance), we find search strategies vary as we move up the skills distribution.

These increasingly exuberant skilled workers further revise upwards their beliefs over the job offer arrival rate and the distribution of expected earnings. On the former dimension they become increasingly optimistic, while on the second dimension their beliefs move in line with the skills premium offered for trained workers in these urban labor markets. These skilled workers also search more intensively along multiple margins (time devoted to job search and channels used), and they engage in directed search towards more productive firms.

Second, the news generated to workers about their job prospects by a lack of call backs causes them to change job search strategies. These responses differ between skilled and unskilled workers. Among skilled workers, relative to skilled workers without job assistance, they revise down their beliefs over the job offer arrival rate and wage offer distribution (especially the left tail of wage offers), search less intensively, and search over lower quality firms. Unskilled workers – relative to unskilled controls without job assistance – react to the confirmation of their poor job prospects by borrowing, not to finance job search but with the stated intention of setting up in self-employment.

Our second batch of results examine whether these experimentally induced changes in search strategy then translate into long run outcomes for workers, up to five years after training is completed and/or job assistance provided.

In line with our earlier work [Alfonsi *et al.* 2020], relative to controls, skilled workers are more likely to be employed, to transition from casual work into regular work, to be employed in good sectors, work longer hours and have higher earnings. In contrast, similarly skilled workers that receive an initial shock of bad news over their own labor market prospects from the job assistance do significantly worse on a range of labor market dimensions up to six years later: on the extensive margin they are less likely to work in regular jobs, on the intensive margin, they work significantly fewer months in regular jobs, and in terms of sectoral allocation, they work less time in one of the eight good sectors in which we offered training.

Taken together the results suggest there are long run impacts of match offers on skilled workers: while skilled workers transition from casual to regular work, this transition is slower for skilled workers provided bad news from job assistance when they first entered the labor market. The mechanisms through which this operates is that skilled workers respond to low call back rates in the match offer treatments by altering various dimensions of their job search strategy.

Digging deeper, we document positive assortative matching between workers, jobs and firms: higher skilled workers end up in better jobs and better firms than controls, but also in better jobs and firms than equally skilled workers that were provided job assistance. As a result, skilled workers without job assistance enjoy significantly longer employment spells and significantly shorter unemployment spells than equally skilled workers subject to bad news just as they were transitioning into the labor market up to six years earlier. Positive assortative matching is important for understanding fundamental sources of earnings inequality and the wider role of firms in the economy. Our granular data allows us to present novel findings on the precise patterns of sorting between workers, jobs and firms [Card *et al.* 2013, 2016, 2018].

Finally, unskilled workers with job assistance (that confirms to them their poor job market prospects), fare significantly better in the long run on a range of labor market outcomes, relative to controls. Most importantly, they are significantly more likely to enter self-employment, in line with their stated intention three years earlier.

Our third and final batch of results contrast the impacts of these exogenously varied sources of heterogeneity with more immutable differences in worker traits. In do so, we bridge to the emerging literature on behavioral search. As Babcock *et al.* [2012] set out in an early discussion of the relevance of behavioral economics for labor market policies, job seeking is a complex informational problem: workers have to understand market conditions, vacancies, application processes, their own skills and how firms might value those skills, and determine the quality of matches with employers. At the same time, job search requires willpower, focus and determination. We thus consider how two time invariant traits explain search behavior and labor market outcomes: cognitive ability and self-evaluation. Self-evaluation is a measure of self-confidence and belief in one's own agency [Judge *et al.* 2002]. Individuals with a high self-evaluation are more able to self-regulate and direct behavior towards certain goals (such as job seeking).

We document an important interlinkage between both traits and the response to job assistance among skilled and unskilled workers.

More precisely, among skilled workers with job assistance, those of high cognitive ability essentially ignore the lack of call backs. Their long run outcomes are in line with skilled workers that are offered no job assistance. In contrast, skilled workers with job assistance that are of low cognitive ability, fare not much better than unskilled workers with job assistance. Indeed, for these low cognitive ability individuals, it is almost as if the impact of misattributing bad news from low call back rates offsets the real gains from having acquired highly valued skills.

We find a similar set of results for self-evaluation (which is uncorrelated with cognitive ability): skilled workers of low self-evaluation appear to misattribute low call back rates.

Taken together the results suggest that individuals with low cognitive ability or low self-evaluation misinterpret low call back rates from the original job assistance intervention up to five years earlier, change search strategies because of this discouragement, and then their long run labor market outcomes worsen as a self-fulfilling prophecy.

Our core contribution is to combine experimental and cross sectional variation across individuals to uncover the key sources of worker heterogeneity that drive job search behavior and labor market outcomes. In doing so, we reconcile structural job search models with reduced form evidence from MEE data that has shown the need to understand the origins of worker heterogeneity for key outcomes in labor and macro. We show the relative importance of heterogeneities arising from skills, information over own job market prospects, and their interaction with traits such as cognitive ability and self-evaluation. We show how these sources of heterogeneity across job seekers impact search strategies, drive inequalities in labor market outcomes, and shed light on fundamental sources of inefficiency in labor markets arising from: (i) credit market imperfections that prevent more workers investing in vocational training; (ii) information frictions that cause persistent impacts of news at the point of labor market entry.

Our findings have implications for the study and design of job assistance programs, many of which have been documented to have weak impacts in high- and low-income settings [Card *et al.* 2017, McKenzie 2017]. Our results highlight that although labor market entrants have biased beliefs, trying to debias them through job assistance can backfire. There are long run gains to be had from enabling skilled workers to search without assistance, using their exuberance to search more effectively. This is especially so if those workers are also of low cognitive ability (when such job assistance can entirely undo the provisions of skills altogether because workers are easily discouraged). The highest returns can be generated by offering job assistance to individuals of high cognitive ability and low sector-specific skills. While such unemployed individuals exist in every economy, there are good reasons to argue they constitute a greater share of the unemployed in lower-income settings where resource and information constraints lead to a great misallocation of talent to begin with.

Section 2 describes our context, experimental design and data. Section 3 makes precise how our treatments induce heterogeneity across workers. Section 4 presents treatment effects on job search strategies. Section 5 shows how initial sources of heterogeneity map into persistent differences in labor market outcomes across workers, using mediation analysis to show the relative importance of skills and search strategies. Section 6 considers how individual traits explain job search behavior, labor market outcomes and how these interlink with the experimentally induced variation across workers. Section 7 discusses the external validity and policy implications of our findings. Section 8 concludes. Additional design details and results are in the Appendix.

2 Context, Design and Data

2.1 Context

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

To get a sense of the existence and severity of market imperfections in our context, Panel A of Figure A1 uses the Uganda National Household Survey (UNHS) from 2012/3, to derive the share of young people engaged in casual work, and in more regular employment, by age. For all ages from 18 to 25: (i) a large share of youth remain unemployed; (ii) workers remain reliant on casual work, with there only being a slow increase in workers accessing regular work as they age.[5]

To get an indication of the inability of workers to invest in their human capital, Panel B shows how skills vary by age, again using the UNHS data. We see that fewer than 6% of young workers make any investment in training or higher education post labor market entry. Finally, Panel C shows how skills raise the likelihood of being in regular work, again by age. We see that: (i) there are returns to skills on this extensive margin at each age; (ii) the majority of skilled youth still do not find regular work. In other words, the labor market fails to clear even for relatively high-skilled youth.

Hence skills mismatch is unlikely to be the only imperfection: our treatment offering workers vocational training relaxes credit constraints workers face in acquiring valuable skills, and our job assistance treatments reduce information frictions that might otherwise prevent some worker-firm matches forming.

Vocational Training Institutes Our study is a collaboration with the NGO BRAC, who implemented all treatments, and five reputable vocational training institutes (VTIs). Each VTI could offer standard six-month training courses in eight sectors: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering.[6]

Workers Individuals were recruited into our experiment throughout Uganda, using an advertised offer for eligible applicants to potentially receive six months of sector-specific vocational training at one of the VTIs we collaborated with. The first row of Table A1 shows applicant characteristics: 57% are men, they are aged 20 on average, and the vast majority have never received vocational training.[7]

Table 1 shows baseline labor market histories for our sample. Focusing on the first row for controls, employment rates at baseline are 40% for these youth, with insecure casual work being the most prevalent labor activity. Unconditionally, average monthly earnings from regular work are $5 (so including zeroes), corresponding to around 10% of the Ugandan per capita income at the time. Conditional on work, earnings are $13 per month. These individuals are thus unlikely to be able to self-finance the kind of investment into vocational training we offer (that costs over

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

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

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

$400). To see the representativeness of our sample, Table A1 compares them to those aged 18-25 in the UNHS data from 2012/3. The intervention appears well targeted towards disadvantaged youth: our sample is similar on age, gender and previous experience of vocational training, but worse off at baseline in terms of wage employment and earnings. This remains so when we compare to youth in the UNHS who report being labor market active.

Firms To draw a sample of potential employers, we first conducted a firm census in 15 urban labor markets throughout Uganda, including Kampala. We selected firms: (i) operating in one of the eight manufacturing and service sectors in which we offered sector-specific vocational training; (ii) having between one and 15 employees (plus a firm owner). Our sample comprises 1281 small and medium sized enterprises, employing 3735 workers in aggregate at baseline.[8] Firms are not selected on the basis of them having a vacancy, but at baseline, 92% of them reported being willing to expand in the near future, with 52% stating they would be willing to do so by hiring workers.

Job Search and Matching Table 2 provides descriptive evidence on how labor market entrants in our control group normally find jobs, and recruitment processes used once they match with potential employers. Our study focuses on whether the kinds of worker heterogeneity we induce change search strategies in a way that enables workers to move up the job ladder into more regular forms of work. It is thus useful to split descriptives related to search and matching into those for causal and regular work.

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

Taken together, the evidence suggests search and information frictions are relevant in these

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

labor markets. Indeed, our sampled firms report being size constrained because of inabilities to find: (i) skilled workers (67%); (ii) trustworthy workers (57%); (iii) unskilled workers (28%). The match offer treatments relax constraints on firm's ability to match with workers with sector-specific skills or an attachment to the labor market.

2.2 Design

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

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

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

The pairwise treatment comparisons we focus on are: (i) T1 vs C: the impact of training on worker search strategies; (ii) T2 vs T1: the impact of job assistance on skilled workers; (iii) T3 vs C: the impact of job assistance on unskilled workers.

Vocational Training The vocational training treatment provides workers six months of sector-specific training in one of eight sectors. Our intervention partner BRAC covered training costs, at $470 per trainee, so this is not the kind of human capital investment disadvantaged youth can typically self-finance. Courses were held from Monday through to Friday, for six hours per day; 30% of course content was dedicated to theory, 70% to practical work covering sector-specific skills and managerial/business skills. VTIs signed contracts with BRAC to deliver these standard training courses to workers. They were monitored by regular and unannounced visits by BRAC staff to ensure workers were present and being trained. For each worker, VTIs were paid half the training fee at the start of training, and half at the end, conditional on them having trained the worker (this staggered timing of payments ensured workers nearly always completed the full course of training conditional on enrolment).

Over 95% of workers assigned to these treatment arms are offered training. Around 68% take-up the offer, with over 95% of them completing training conditional on enrolment. Our design is such that match offers are only made to those that complete training. Hence, imperfect compliance with the offer of training does not affect the primary comparison between T1 and T2. However, this does mean we caveat comparisons of the response to job assistance between skilled and unskilled workers (T2 vs T3), but that is more of a secondary focus for our study.[9]

Job Assistance Our job assistance treatments are light-touch, replicating the kind of job assistance often provided to job seekers. In these treatments, workers were first asked whether they wanted their details to be passed onto firms: nearly all agreed (among both skilled and unskilled groups). Firms were then presented lists of workers that were: (i) trained; (ii) unskilled, but had demonstrated labor market attachment in the sense that they had been willing to undertake six months of intense training. In case (i), firms knew what sector the worker had been trained in, but not that training had been paid for by BRAC. We presented stylized CVs of workers to firms. There were a maximum of two workers presented to firms on each list: both workers were either trained or were both untrained. The firm could choose to hire none, one or both workers (and remained free to hire workers from outside the evaluation sample). The median worker was matched to a single firm. The worker-firm match assignments took place between firms operating in the same sector as the worker had been trained in (or had expressed an initial desire to be trained in), and in the same region as the firm and worker were located.[10]

The Appendix describes precisely how match offers were practically implemented. In particular, firms were not provided contact details of workers – they had to come through BRAC officers. Hence none of our results are due to firms recalling workers or workers using storable offers well after match offers actually took place [Katz 1986, Katz and Meyer 1990]. The job assistance program only involves BRAC officers and workers, with VTI employees playing no role. As VTIs do not normally match workers to firms, there are no pre-existing ties between VTIs and firms.

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

[10] Meta-analyses of job assistance programs [Card *et al.* 2017, McKenzie 2017] emphasize that their typical element involves engineered worker-firm meetings, to help overcome search frictions. These meetings can either be directed (as in our match offer treatments that are directed towards firms in sectors where workers were originally offered training) or undirected, such as through the use of job fairs [Beam 2016, Abebe *et al.* 2020a].

2.3 Data

Timeline and Surveys Figure 2 shows the six-year study timeline from 2012 to 2018. The baseline worker survey took place from June to September 2012 just after applications for vocational training were received. Among those taking-up the offer of training, we next surveyed them at the end of their six month course. We use this to measure their posterior beliefs over their labor market prospects just as they complete training but prior to having knowledge over job assistance being offered. Among those randomized out of training, we next surveyed them just as vocational trainees were completing their courses, and use this to assess the opportunity cost of attending six months of vocational training. These two rounds of data collection are under Phase 1 of the timeline shown in Figure 2.[11]

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

Workers were tracked 24, 36, 48 and 68 months after baseline (12, 24, 36 and 56 months after the end of training/job assistance). The worker surveys were designed to measure key components of a class of job search models. This allows us – almost uniquely in the literature – to measure a rich constellation panel data on individuals over six years, on multiple dimensions of search behavior, such as reservation wages, beliefs, search effort/intensity, desired firm and job characteristics, as well as detailed labor market outcomes on job offers, employment, earnings, hours, wages, job and firm characteristics. We couple this data with measures of time invariant worker traits such as their cognitive ability, personality and psychological traits, in order to understand the role of such traits in determining search behavior and outcomes, and how they interlink with experimentally induced dimensions of worker heterogeneity.

Estimation We assigned workers to treatment arms using a stratified randomization where strata are region of residence, gender and education. We estimate intent-to-treat effects for worker i assigned to treatment group T_i in strata s in survey wave $t = 1, 2, 3, 4$ using the following specification:

$$y_{ist} = \sum_j \beta_j T_i + \gamma y_{i0} + \lambda_s + \vartheta_t + u_{ist}, \tag{1}$$

[11] 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 match offer and vocational training + match offer interventions, in line with the two batches of first round trainees from the vocational training institutes. The first round of the match offer and Vocational training + match offer interventions 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.

where y_{ist} is the outcome of interest, y_{i0} is the baseline value of that outcome (where available), λ_s and ϑ_t are strata and survey wave fixed effects. All regressions control for the implementation round and dummies for month of interview. We present robust standard errors as randomization is at the individual level, but also report p-values adjusted for randomization inference [Young 2019] and multiple hypothesis testing to account for the three treatment effects estimated in (1), using the step-down procedure of Romano and Wolf [2016].

The coefficients of interest are the β_j's: ITT effects relative to controls. We estimate how the treatments impact search behavior in the short run, using the first follow up survey ($t = 1$) that occurs 24 months after baseline (so 12-14 months after vocational training courses are completed and/or match offers made). These estimates are under Phase 2 of the timeline shown in Figure 2. We estimate the long run impacts that changes in search strategy have on the individual's labor market outcomes using data from the second to fourth worker follow up surveys ($t = 2, 3, 4$) that take place 36, 48 and 68 months after baseline. Hence when studying labor market outcomes, β_j is the treatment effect of T_{ij} as *averaged* over the last three post-intervention survey waves, corresponding to Phase 3 of data collection in Figure 2.[12]

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

Only 15% of workers attrit by the 68-month endline. In the Appendix we describe correlates of worker attrition, confirming attrition is uncorrelated to treatment, and nor do we find any evidence of differential attrition across treatments based on worker observables (Table A3).

3 Generating Worker Heterogeneity

Our focus is on understanding how worker heterogeneity determines job search behavior. To precisely document how the offer of training and worker-firm match offers induce heterogeneity in initial conditions across workers, we proceed as follows. For the vocational training treatment, we show how the offer translates into actual skills acquisition. For the job assistance treatments, we describe the evolution of beliefs workers hold about their labor market prospects from baseline to just prior to such assistance being announced. We can then interpret more carefully how workers update their beliefs as a result of call backs received in these treatments.

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

3.1 Vocational Training

We now confirm the offer of vocational training caused: (i) significant improvements in measurable skills, that are rewarded in the labor market; (ii) did not impact other worker traits such as their cognitive ability or psychological traits.

3.1.1 Skills

Our earlier work in Alfonsi *et al.* [2020] discusses how the offer of vocational training translates into human capital accumulation. We reiterate some of those results and provide new results on additional skills margins.

We first consider a sector-specific skills test we developed in conjunction with skills assessors and modulators of written and practical occupational tests in Uganda. Each test comprises seven questions (with a combination of multiple choice and more complex questions being used). Figure A2 shows an example of the skills test for the motor mechanics sector. Workers had 20 minutes to complete the test, and we convert answers into a 0-100 score. If workers answer questions randomly, their expected score is 11. The test was conducted on all workers (including those assigned to as controls) at second and third follow-up, so measuring persistent skills accumulation. There is no differential attrition by treatment into the test.[13]

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 3 is a dummy equal to one if the worker reported having skills for a sector, where we report the β_j estimates from specification (1). Focusing on the first row that shows treatment effects for workers offered vocational training, we see they are significantly more likely than controls to report having sector-relevant skills, as measured two and three years after the vocational training is provided. As reported at the foot of the table, 61% of controls report having skills for some sector, and reassuringly this rises to 87% for those offered vocational training.

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

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

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 started 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%.[14]

The remaining Columns examine other skills margins and show that: (i) those assigned to vocational training are significantly more likely to obtain further vocational training later in their careers; (ii) they do not acquire additional skills from the formal education sector. This highlights that in these labor markets positive assortative matching between workers and firms means that skills beget skills – the experimental heterogeneity we induce in skills becomes magnified over time as it sets workers on a very different trajectory of human capital accumulation than that typically experienced by youth in these urban labor markets.

3.1.2 Other Traits

Finally, we check whether the offer of vocational training impacts other worker characteristics. Table A4 shows this is not the case: we document null impacts of the offer of training on: (i) big-5 personality traits; (ii) cognitive ability (as constructed from a 10-question version of the Raven's progressive matrices test); (iii) other psychological traits. Of marginal significance on these other dimensions are that workers become more open, and are slightly more likely to think they have control over their destiny.

Hence comparing job search behavior between those assigned to vocational training and controls provides a clean comparison between workers with significantly different sector-specific skills, but no differences in personality, cognitive ability or psychological traits. Moreover, the fact that training does not impact these other traits allows us to later exploit such cross sectional variation in traits to study their role as an alternative source of worker heterogeneity driving job search and labour market outcomes.

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

3.2 Job Assistance

3.2.1 Search Behavior of Controls

To understand the worker heterogeneity induced through job assistance, we first detail search behavior among controls. Figure 3 shows how employment and search intensity change over time among controls. Panel A focuses on the extensive margin of employment and job search. Over the fours years from first follow-up, the share of workers reporting being unemployed at some point in the year falls from 90% to 70%. However, the share of workers reporting looking for a job never rises above 60%. Panel B shows the intensive margin: in the year prior to baseline, workers spend around nine months unemployed yet spend less than one month looking for work. While the days spent searching rises over time, it never gets close to matching the time these young workers actually spend without work year on year.

This apparent misallocation of time workers can be due to them 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. To be able to carefully interpret the worker heterogeneity induced through match offers, we dig into this further and present evidence on the beliefs control workers hold over their own job prospects.

Beliefs Over Earnings Motivated by job search models emphasizing workers learn about the wage offer distribution [Wright 1986, Burdett and Vishwanath 1988], we start by examining worker's expected earnings if they were employed in sectors that trainees receive sector-specific skills in. More precisely, we elicit these beliefs in the worker's most preferred study sector (for skilled workers this nearly always corresponds to the sector in which they received training).

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

We next show the baseline beliefs controls have if they were to move up the job ladder and be employed in their most preferred study sector. These beliefs are derived for all controls, irrespective of their search effort or employment status, and hence are not driven by compositional changes.[15] We asked controls their minimum and maximum expected earnings if offered a job in their preferred study sector. We asked them the likelihood their earnings would lie above the midpoint of the two, and fit a triangular distribution to derive their expected earnings. The next three box-whisker

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

plots in Figure 4A show the distribution of minimum, maximum and expected earnings of controls in these good jobs. Reassuringly we see the expected ranking, with greater dispersion in the expected maximum earnings. Average expected earnings are higher than actual earnings from the kinds of regular work that controls are engaged in at baseline – indeed, the median earnings in actual regular work at baseline lies below the 25th percentile of expected average earnings if the worker could move up the job ladder into their most preferred sector. Hence controls appear to recognize that these are better jobs than the kinds of work they have experienced at baseline.

To assess the accuracy of the beliefs, the final batch of box-whisker plots takes earnings data from workers actually employed in these eight study sectors, using the sample of firms tracked in our study. We show this for three types of worker: (i) unskilled workers; (ii) recent hires; (iii) skilled workers. The first two are plausible counterfactuals for controls if they were to immediately transition into good sectors. We observe a fair degree of overlap between the distribution of expected earnings and the actual earnings of unskilled and newly hired workers in these sectors. In short, control workers have reasonably accurate beliefs about the wage offer distribution should they move up the job ladder. Biased beliefs on this margin do not appear to be why they devote too little time to search (Figure 3).[16]

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

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

We assess the accuracy of these beliefs using two approaches. First, we compare them to actual youth employment rates in regular jobs. Panel C of Figure A1 shows this using the UNHS data, that is fielded close in time to our baseline. For unskilled youth, employment rates in regular

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

jobs are around 20%, and only rise by a further 10% for workers two years older, and plateau thereafter. This is far lower than the baseline belief held by the median control worker of a 60% job offer arrival rate from firms in good sectors in the next year.[17]

Second, we examine how controls revise beliefs between baseline and first follow-up. The next three box-whisker plots in Figure 4B show the distribution of revised beliefs over job offer arrival rates at first follow-up, after controls have been searching for work for nearly two years. Beliefs are revised downwards: the median belief held among controls is they have a 10% chance of receiving a job offer from a firm in a good sector within a month, 20% within the next six months, and 40% within the next year. Controls are therefore becoming more realistic over time.

To see the speed of convergence, we calculate the *actual* likelihood of finding a good job over exactly these horizons using data from the second follow-up survey, fielded a year later. These actual likelihoods of finding regular work are shown in the remaining box-whisker plots in Figure 4B. These are still far lower than worker expectations over the job offer arrival rate, with the divergence increasing with the time horizon considered: 7% of workers actually find a job within a month, 10% do so within six months, and 13% do so within a year. Such persistent optimism can potentially explain the lack of search effort described earlier, and thus contribute to slow exit rates out of unemployment.[18]

Most generally, these results complement a growing literature on the *persistence* of optimistic beliefs [Benabou and Tirole 2002, Compte and Postelwaite 2004, Van den Steen 2004]. More specifically, we add to evidence, mostly from the US, that displaced workers are optimistic over job offer arrival rates [Spinnewijn 2015, Mueller *et al.* 2020, Potter 2020]. Such optimism has been recently documented among job-seekers in lower-income labor markets including Ethiopia [Abebe *et al.* 2020a] and South Africa [Banerjee and Sequeira 2020].

3.2.2 The Evolution of Beliefs Until the Announcement of Job Assistance

We can assess how workers beliefs evolve from baseline to the eve of job assistance being offered, as this will be critical for how they react to information generated by call backs in the job assistance treatments. We do so for those assigned to vocational training and for unskilled controls.

For those assigned to vocational training, we measure their beliefs at the end of their training course, but prior to job assistance being announced. For controls, we measure changes in beliefs from baseline to first follow-up. Assuming beliefs evolve linearly over time, on the eve of job assistance being announced, these beliefs would have changed half way from what we documented

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

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

between baseline and first follow up. As seen above, unskilled workers hold relatively accurate beliefs over the earnings distribution in study sectors, and become more realistic over job offer arrival rates from firms in good sectors as they search for work.

To begin with, we consider the evolution of beliefs over the earnings distribution in our study sectors. Figure 5A shows the distribution of beliefs on the minimum and maximum expected earnings from being employed in their most preferred sector among: (i) all workers at baseline; (ii) controls; (iii) trainees. For controls, beliefs over the earnings distribution hardly change over the six months since baseline. This is as expected – controls have relatively accurate beliefs already at baseline, and no new information is gained over the first six months of search. Among workers graduating from vocational training, both distributions of minimum and maximum expected wages shift rightward, with an especially pronounced upward shift in the distribution of maximum earnings. This reflects their self-recognition of high returns to their newly acquired skills.

Figure 5B shows how beliefs over the job offer arrival rate evolve among controls and trainees. For controls, we saw earlier that they start off optimistic, but gradually become more realistic over this margin as they search. The beliefs of trainees move sharply in the *opposite* direction: they revise upwards their belief over the job offer arrival rate at each horizon, with the gap in beliefs between trainees and controls opening up the most at the six month horizon. Indeed, close to graduating, 25% of trainees believe they will receive a job offer in their most preferred good sector with certainty in the next six months.[19]

We thus observe a bifurcation of beliefs from baseline until the eve of job assistance being announced: controls slowly become more realistic over time as they search, while trainees become increasingly exuberant over their job offer prospects as they complete their training. How realistic are these beliefs of these newly skilled workers? We can refer back to the evidence from the UNHS survey in Figure A1. Panel C shows the likelihood skilled workers are in regular jobs, by age. At each age this is higher than for unskilled workers (in proportionate terms these employment rates are near double). However, their levels remain low: around 35% of 20-21 year olds have regular jobs, and this rises to only 40% for those aged 22-23. This is far from the beliefs held by trainees as they complete their training. Again in comparing the stock of young workers in regular work in the UNHS to beliefs skilled workers in our sample have over flow probabilities into these good jobs, we are again likely overestimating the true likelihood skilled workers will receive job offers into these good jobs.[20]

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

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

3.2.3 Call Backs

For workers assigned to job assistance treatments, nearly all (skilled and unskilled) agree for their details to be passed onto potential employers. They key outcome from their perspective is whether they receive a call-back, i.e. an invitation to meet the firm owner.[21]

How do actual call back rates compare to prior beliefs? As Figure 5B shows, on the eve of job assistance being announced, the median trained worker believed there was a 30% chance they would receive a job offer from a good firm in the next month. In actuality, only 16% of skilled workers receive a call back. Among controls, the median worker had a prior belief of a 15% chance they would receive a job offer from a good firm in the next month. In actuality, 18% of unskilled workers receive a call back, thus confirming their prior.

To understand how perfectly informed workers should react to these call back rates, we consider the correlates of call backs. Recall that each firm is paired with two workers, who are either both unskilled or both skilled. Columns 1 and 2 of Table A5 show correlates of call backs to skilled workers, Columns 3 and 4 present analogous specifications for call backs to unskilled workers. The two specifications control for: (i) worker and firm characteristics; (ii) worker characteristics and firm fixed effects (exploiting that each firm is presented with two workers). At the foot of each Column we report p-values on the joint significance of worker and firm covariates.

Two important results emerge. First, worker characteristics do not predict call backs, for either skilled or unskilled workers – the p-value on the joint test of significance of worker covariates vary from .242 to .734 across specifications. This is unsurprising: firms are presented with two workers that are by construction, very similar on observables. Hence there is little basis on which to prefer one over another. Second, firm characteristics predict call backs to skilled workers. In particular, skilled workers are more likely to be called back if they are matched to firms that would like to expand (and so have a vacancy), and where owners report being constrained by an inability to find trustworthy workers. Hence in line with other studies, the key limiting factor on worker-firm matches actually taking place is firms willingness to meet workers, rather than reservation prestige driving worker refusals to meet firms [Groh *et al.* 2016].

Our design and results contrast with a long-standing literature using audit studies to determine which worker characteristics determine call backs – their key premise being that employers use observable information in resumes (demographics, work histories etc.) – to infer worker's quality and hence whether to call them back. In our study, the design of the match offer treatments almost fully removes the possibility that worker characteristics determine call backs. This allows us to provide novel evidence on firm-side determinants of call backs.

[21] The entire process from when assistance is announced until when workers are usually invited to interview is around two weeks (although workers never called back would obviously only later realize this). While this can cause short run postponements of search, we measure impacts on search behavior a year later. The worker details provided to firms were their age, gender, language spoken, education, type of training received (if any), and work experience.

If workers realize this because they are perfectly informed, they should infer there is zero information from any given call back (or lack of). Under this null, search strategies of skilled and unskilled workers should be entirely unaffected by job assistance.

However, if some share of skilled workers are imperfectly informed of what drives call backs in the experiment, then the lower than expected call back rate might cause them to revise down their beliefs about their own job prospects. Such misattribution could be likely because: (i) they are not well informed to begin with, becoming more optimistic over time (Figure 5); (ii) there are no market substitutes for the job assistance program, and so the offer might be seen as a unique opportunity to find meaningful work. If so, job assistance on average generates bad news for skilled workers.[22]

Hence between skilled workers with and without job assistance, a precise form of heterogeneity is induced: skilled workers with job assistance receive bad news on their own job prospects, just at a time when they are meeting potential employers for the first time. Skilled workers without such job assistance are insulated from this news, and so begin their job search with the increasingly exuberant beliefs shown in Figure 5.

For unskilled workers, call back rates are in line with their prior. For them, the job assistance program provides credible confirmation that their job market prospects are poor, unless they take some action. Hence between unskilled workers with and without job assistance, the key form of heterogeneity induced is that those subject to job assistance have confirmation of their poor job market prospects to match to good sector jobs.

There are thus good reasons to expect skilled and unskilled workers to react differently to job assistance, unless all are perfectly informed in which case neither group should alter their search behavior.[23]

4 Heterogeneity and Job Search Strategies

We analyze how heterogeneity across job seekers in their skills and provision of job assistance impact search strategies. These effects are measured at first follow-up, 24 months after baseline and a full year after trainees have graduated, and call backs made. We present findings on search strategies for all workers irrespective of their employment status, ensuring results are not driven

[22] While we do not aim to micro-found misattribution, we note it is consistent with job seekers being subject to the gambler's fallacy, in which they become discouraged as they overinfer their own job prospects from a bad draw [Rabin and Vayanos 2010].

[23] To complete the interpretation of the job assistance treatments, we consider whether they impact skills accumulation or other traits. Table 3 shows that: (i) skilled workers that are given job assistance have no different skills accumulation to those only given vocational training; (ii) among unskilled workers, there are no differences in skills between those with and without job assistance. On other worker traits, the results in Table A5 confirm that: (i) among skilled workers, there are no differences in the big-5 personality traits, cognitive ability and other psychological traits between those with and without job assistance; (ii) among unskilled workers, there are also no differences in the big-5 personality traits, cognitive ability and other psychological traits between those with and without job assistance.

by composition effects. Hence these treatment effects should be interpreted as combining: (i) impacts on search behavior while unemployed; (ii) impacts through on-the-job search. On the second channel, Table A6 summarizes short run labor market treatment effects (measured at first follow up).

We see no short run divergence in outcomes between skilled workers with and without job assistance. Skilled workers are 6 to 9pp more likely than controls to have worked in the last month (Column 1), and work about a month longer in one of the study sectors (Column 2) – so there are small changes on the intensive margin of work. There are muted impacts on earnings, self-employment or the quality of firms employed at, as measured through an index of firm characteristics. This last result is one we return to later when considering the long run impact of search strategies on labor market outcomes because it suggests induced heterogeneity across workers – rather than persistent effects of first employment spells at low quality firms – is the key determinant of worker outcomes.[24]

4.1 Reservation Wages

We begin by examining how worker's reservation wage responds to treatment. This is the key endogenous choice of workers in search models, yet is rarely measured in publicly available data. Conceptually, we aim to measure the lowest wage workers would be willing to accept for *any* job (not necessarily their preferred job). To map this to data, we ask workers what would be the minimum wage they would accept for a job requiring a 10 minute commute (and then the same for a 30 minute or 60 minute commute).

The results are in Table 4. We find little precise evidence that any group of treated workers change their reservation wage. We do not claim these estimates represent precise zeroes – they are clearly somewhat noisy. However, in a setting without unemployment insurance, the lack of impact on reservation wages is in line with us picking up some proxy for the reservation utility inducing workers to be active labor market participants. The key takeaway is that adjustments in reservation wages are not the primary channel through which search strategies are impacted as we change the skills or offer job assistance to youth as they enter the labor market.[25]

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

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

4.2 Beliefs

We move on to consider how the worker heterogeneity induced in our experiment impacts beliefs over own labor market prospects, a full year after training is completed and job assistance offered. These results are in Table 5. Columns 1 to 3 show the treatment effects on the distribution of expected earnings if workers were able to transition into their most preferred study sector job.

Focusing first on skilled workers, we see that: (i) they significantly revise upwards their minimum expected earnings, their maximum expected earnings is revised upwards by a greater extent, and their expected earnings shift forward by \$25.4/month, corresponding to a 44% rise over the beliefs of controls. Column 4 shows they also revise upwards their belief over the job offer arrival rate in the next year (by 1.84 on a 0-10 scale). These ITT estimates are all robust to correcting for randomization inference or multiple hypothesis testing.

The next row shows the same outcomes among equally skilled workers but those who, a year earlier, were provided job assistance (again relative to controls). At the foot of each Column we report the p-value on the equality of treatment effects on skilled workers between those with and without job assistance. We see that skilled workers with job assistance have lower expected earnings from working in these good sectors – this difference is most pronounced at the minimum expected earnings ($p = .095$), although all three point estimates are smaller in magnitude than for skilled workers. Column 4 shows that they also significantly revise down their beliefs over the job offer arrival rate in good sectors, despite them being as skilled as those without any job assistance ($p = .082$). Hence skilled workers offered job assistance appear to be *discouraged* relative to equally skilled workers absent information generated from job assistance.

The third row shows ITT estimates on the beliefs of unskilled workers with job assistance (again relative to controls). Their beliefs over expected earnings and the job offer arrival nudge forward on each dimension. Skilled and unskilled workers have significantly different reactions to job assistance, with beliefs being revised in opposite directions: skilled workers revise down beliefs over their own job market prospects relative to equally skilled workers that do not have the match offer ($\widehat{\beta}_{T2} - \widehat{\beta}_{T1} < 0$), while unskilled workers revise upward their beliefs over their own job market prospects ($\widehat{\beta}_{T3} > 0$).

These differential impacts of match offers are in line with skilled and unskilled workers having different priors when job assistance is announced, over the likelihood of receiving job offers from firms in study sectors (Figure 5B). The low call back rates from this assistance represent bad news for skilled workers, while for unskilled workers they are more akin to credible confirmation of their poor prospects absent any change in circumstances or behavior. Our results thus complement a nascent literature examining the process of workers' learning during job search, and are among the first to do so outside a US context [Krueger and Mueller 2016, Conlon *et al.* 2018, Mueller *et al.* 2020, Potter 2020].[26] A notable exception to this is Abebe *et al.* [2020a] who show that among

[26] Krueger and Mueller [2016] use panel data from unemployed job seekers in New Jersey to study the evolution

Ethiopian job seekers randomly assigned to attend job fairs (where few workers are actually hired), individuals also revise down their beliefs over their own labor market prospects.

We provide three additional pieces of evidence to narrow the interpretation that in response to job assistance, workers update their beliefs over their *own* job market prospects, and so take the lack of call backs personally, rather than revising beliefs over other margins.

First, it is natural to think low call back rates cause workers to revise beliefs about the state of labor demand. We thus elicited their beliefs over the following: (i) whether a lack of firms is a problem for job search; (ii) whether a lack of advertised jobs is a problem (signifying a lack of vacancies); (iii) whether workers have difficulties demonstrating their practical skills to employers; (iv) whether workers have difficulty showing their soft skills to employers. We combine these into one index using the approach of Anderson [2008] – this uses the data covariance matrix to construct a weighted sum of indicators in the group, and so gives less weight to items more correlated with each other. These indices are standardized to have mean zero and variance one in the control group, so estimates are interpreted as effect sizes.

Column 5 of Table 5 shows how the treatments impacts worker beliefs over this labor market index: we see no changes in beliefs over market conditions among workers given match offers – for neither skilled nor unskilled workers. Table A7 shows impacts on each dimension of the labor market beliefs index. For no treatment group do we find any evidence of significant changes in beliefs about any dimension of labor market conditions.

Second, if workers interpret low call back rates as signalling their skills are highly valued by firms *larger* than those involved in the job assistance program, then workers might adjust their job offer acceptance rate. Column 6 shows impacts on behavior along this margin, asking workers whether they have ever turned down a job offer in the last year. In line with most other settings, among controls only 7% of workers report turning down job offers, so there could be a potential ceiling effect. However, we see that for workers involved in job assistance, there is a precisely estimated null effect – both point estimates are smaller than .01 in absolute value with standard error of .022. Hence it is not that workers become optimistic on their market value and so reject more offers [Crepon and van der Berg 2016].

Third, taking seriously that call backs are uncorrelated to worker characteristics (Table A5), we examine impacts on beliefs for workers with and without call backs. This is shown in Table A8, focusing on those in the job assistance treatments. Among skilled workers, those actually receiving a call back significantly revise upwards their beliefs relative to those that did not receive

of reservation wages over the unemployment spell. Conlon *et al.* [2018] document workers learning about the *wage offer function* during job search, again using US data. They document updating patterns that are inconsistent with Bayesian updating and estimate a partial equilibrium job search model with on the job search and learning. Potter [2018] develops and estimates a model of Bayesian learning about the *arrival rate of offers* in a job search model, again using US data. Mueller *et al.* [2020] show job seekers' beliefs are biased and under respond to unemployment spells, and then calibrate a model of job search to show how much they contribute to slower flows out of unemployment.

a call back. The interaction of the treatment dummy with whether they receive a call back is positive and significant for both their expected maximum earnings and job offer arrival rate.

Among unskilled workers subject to match offers, those actually called back revise down their beliefs over expected earnings a little: this perfectly matches the earlier finding that showed unskilled workers held slightly overoptimistic beliefs about the earnings distribution of unskilled workers in good sector jobs (Figure 5A).

4.3 Search Intensity

We next examine how search intensity is impacted by treatments. As Marinescu and Skandalis [2020] describe, the earlier literature has essentially used two approaches to measure search effort: (i) self-reported time spent on search activities; (ii) the number of job applications in online job search platforms. We adapt and extend the former approach, so covering all channels of job search. We start by considering an aggregate index of behaviors related to search intensity, constructed using the methodology of Anderson [2008], where higher values of the index correspond to greater search intensity. The results is in Column 1 of Table 6, while Table A9 shows the impacts along each separate component of the index.

As we move up the skills distribution, workers search more intensively, with the index significantly rising by $.092\sigma$ (a result robust to p-value adjustments). Table A9 shows the components driving this are that skilled workers are more likely to report having actively searched for a job, they become more geographically mobile in their search, and are more likely to report using direct walk-ins to firms (while there is no crowding out of their reliance on informal information from friends and family).[27]

Similarly skilled workers given job assistance a year earlier have more muted responses in search intensity: in Column 1 we see their search intensity index is not statistically different to controls. However, in Table A9 we see that they do significantly change behavior along a number of dimensions of the search intensity index, although these results are less robust to p-value adjustments for multiple hypothesis testing.[28]

Unskilled workers provided job assistance do not change search intensity: the point estimate on their overall index is close to zero, and we find no evidence of a shift in behavior along any component of the search intensity index.

Spinnewijn [2015] documents how US job seekers are bad at knowing that search is effective,

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

[28] The overall search index might not be different from zero even though some of its components are because the Anderson approach weights components, and highly correlated components received less weight. In the search intensity index for example, the first component of having actively looked for a job in the last year is inevitably highly correlated with all the other components because those other components are zero if the individual did not search. As a result, this component has little weight in the overall index.

that is, they underestimate the benefits of search. It is not straightforward to map from the documented changes in search intensity to worker beliefs on the returns to search effort because there are both income and substitution effects. Of course, it might be reasonable that in our study context, among disadvantaged youth just entering the labor market, the income effect dominates. If so, the significantly increased search intensity of skilled workers is consistent with them believing the returns to search effort have risen, while the more muted impact on equally skilled workers but also given job assistance a year earlier, suggests they are more discouraged from exerting search effort – in line with their revised beliefs.

4.4 Directed Search

We next consider whether the heterogeneity induced in our experiment causes workers to direct their search towards particular jobs or firms. To do so we asked workers about characteristics of the *ideal* job and *ideal* firm they were searching for. We construct the ideal job index so that higher values correspond to jobs higher up the job ladder because they: (i) entail supervising others; (ii) have a high social status associated with them; (iii) enable workers to learn new job-specific skills; (iv) entail working with others (as opposed to working alone); (v) have a flexible schedule. The index is scaled so that treatment effects are interpreted as effect sizes. The result on the ideal job index is in Column 2 of Table 6: we see no evidence of any treatment impacts on the ideal job workers are searching for, at least along these dimensions. Table A10 confirms that no treatment impacts the ideal job searched for along any of the components of the index.

We construct the ideal firm index so that higher values correspond to more productive or profitable firms because they: (i) have more employees; (ii) are formally registered; (iii) provide training; (iv) provide other material benefits to employees. The treatment effects on the ideal firm index are shown in Column 3: we see significant evidence that skilled workers 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 A11 shows the firm characteristics driving this: more skilled workers search for firms that can provide training and other material benefits.

In contrast, equally skilled workers with job assistance search for firms that are no different to those being targeted by control workers, and this is borderline significant to skilled workers ($p = .102$).[29]

These differences between skilled workers with and without job assistance square nicely with the earlier differential impacts found between these treatment arms, again suggesting skilled workers with match offers are slightly more discouraged – as measured by their expected earnings distribution if they find employment in good sector firms, and their intensity of search.

[29] Interestingly, Table A10 shows that skilled workers with match offers search for slightly smaller and more informal firms, but they do value training in that they search for firms that can provide training.

4.5 Finance

The final element of job search strategy we consider builds on the idea of interlinked labor and credit markets [Lentz and Tranaes 2005, Lise 2013]. To begin with, Column 4 in Table 5 shows that workers do not run down savings as they search. This is not surprising given there is no social insurance in this context and search is not costly: channels of search are informal and often involve walk-ins to firms, not formal application fees for example. We next assess whether the treatments induce workers to borrow to finance search. The results in Columns 5 to 7 show that for skilled workers – with or without the match offer – there is no response along this margin.[30]

For the first time we observe a margin of adjustment in search strategies used by unskilled workers offered job assistance: they are significantly more likely to borrow (Column 5), but they do not use this to finance job search (Column 6), but rather report borrowing to finance business expenditures (Column 7). Given the exact wording of the question, we interpret as them aiming to set up in self-employment. The rate of borrowing for self-employment is double that of controls.[31]

Recall that for unskilled workers with job assistance, call back rates were close to their prior, so confirming their labor prospects are not good unless they change behavior. We assess below whether their stated intention of borrowing for self-employment – as measured a year after job assistance is offered – actually translates into higher rates of self-employment in the long run.

4.6 Summary

We have documented how experimentally induced heterogeneity across young workers leads to significant changes in job search behavior. Our measurement tools were designed to ensure our results map closely to job search models. We find all these models are relevant to understand search behavior among labor market entrants, but they are differentially relevant across different types of workers. Understanding precisely how different groups of unemployed worker search for jobs is a critical step in designing policy instruments to aid them.

No group of worker shifts their reservation wage: as in data from higher-income settings in the context of benefit exhaustion, adjustments on this margin are not first order – despite reservation wages being central to nearly all search models. Less surprising given our context, no group of treated worker run down their savings to finance search. However, as we move up the skills distribution, workers shift forward their beliefs over expected earnings and the job offer arrival rate, they search more intensively, and direct their search towards higher quality firms. In contrast,

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

[31] In Column 7 business expenditures include expenses incurred to set up, or register a business, purchasing business assets or inputs, pay wages, etc.

equally skilled workers that receive bad news on their own job market prospects from the job assistance program are discouraged: they revise down their expectations over earnings and the job offer arrival rate, they do not search as intensively, and do not direct their search towards better jobs or firms. Finally, unskilled workers who receive confirmation of their poor job market prospects from the low call back rates generated by job assistance, alter strategies by borrowing with the intent to finance self-employment.

5 Heterogeneity and Labor Market Outcomes

The six-year study period allows us to map out how experimentally induced changes in job search strategies across workers translate into heterogeneous labor market outcomes in the long run. We do so by estimating (1) using outcomes averaged over the last three survey waves, so 36 to 55 months after workers graduate from vocational training and/or are given job assistance. Under a null of efficient markets, the heterogenous initial conditions we engineer do not matter in the long run because eventually efficient worker-firm matches take place. The alternative is that in the presence of labor market inefficiencies, initial differences matter. Motivated by the job search and matched employer-employee literatures, we quantify how heterogeneity across workers impacts the level and dispersion of outcomes such as earnings and spell durations. At the end of the Section we present a mediation analysis that indicates the relative importance of skills and search strategies for long run outcomes.

5.1 Employment and Earnings

We begin in Table 7 by tracking standard measures of employment, transitions from casual to regular work, and earnings. The first row shows the long run impacts of skills on these core labor market outcomes. Mirroring results described in Alfonsi *et al.* [2020], we find skilled workers: (i) are significantly more likely to work, with employment rates rising by 9.4pp or 15% (Column 1); (ii) transition towards more regular employment, both on the extensive margin where regular employment rates rise by 11.3pp or 22% (Column 2), and on the intensive margin where skilled workers spend 23% more time in regular work (Column 3). Earnings from such regular work rises by US$8.1 per month (21%) over controls (Column 4), and in terms of sectoral allocation, skilled workers have a 103% increase in months worked in any one of the study sectors (Column 5).

We summarize labor market success by combining outcomes from Columns 2 to 5 into one index, again using the Anderson [2008] approach and normalizing the index to be in effect sizes. This index outcome is shown in Column 6. Moving up the skills distribution, the index of worker outcomes rises significantly by $.310\sigma$.

Strikingly, in the next row we see that equally skilled workers, but who received low call back rates from job assistance up to five years earlier, have a significantly smaller improvement in their labor market index of .231σ ($p = .063$). The reason why the labor market index is lower relative to skilled workers without job assistance is: (i) on the extensive margin they are less likely to work in regular jobs ($p = .043$); (ii) on the intensive margin, they work significantly fewer months in regular jobs ($p = .011$); (iii) in terms of sectoral allocation, they work less time in one of the eight good sectors in which we offered training in ($p = .102$).[32]

The final row of Table 7 shows outcomes for unskilled workers with job assistance. Relative to controls, their labor market outcomes significantly improve through both extensive and intensive margins. Naturally the magnitudes of impact are smaller than for both groups of skilled worker, and their overall labor market index rises by .090σ, so around one third that of skilled workers and two thirds that of skilled workers with job assistance.

In short we find that the long run impact of information generated through job assistance is to worsen outcomes for high skilled workers ($\widehat{\beta}_{T2} - \widehat{\beta}_{T1} < 0$), and to improve outcomes for low skilled workers ($\widehat{\beta}_{T3} > 0$).

Our findings contribute to an ongoing debate about the persistent impacts of interventions in low-income contexts. While a body of work has suggested the combined provision of skills and assets can shift occupational choices and incomes in the long run for rural households [Banerjee *et al.* 2015, Bandiera *et al.* 2017], work in urban labor markets suggests the impacts of one-off high-valued transfers to underemployed youth fade over time [Blattman *et al.* 2019, 2020, Abebe *et al.* 2020b]. We have found persistent impacts of skills and information generated through job assistance, where long run impacts of match offers differ between skilled and unskilled workers.

Finally, we note that our results are not driven by gender: the impacts on the labor market index are not statistically difference between men and women in any treatment.

5.2 Earnings Inequality, Bargaining and Spells

The matched employer-employee literature has highlighted the importance of worker heterogeneity for explaining the dispersion of earnings and (un)employment spells. Our research design allows us to quantify how experimentally induced variation in workers contributes to such outcomes. We do so in Table 8.

It is natural to start with earnings inequality, where we consider total earnings from both casual and regular work. Column 1 of Table 8 shows: (i) the causal impact of skills is to increase earnings by 26%, so skills explain 19% of earnings inequality across workers (as measured by the long run standard deviation of earnings in our sample); (ii) the causal impact of job assistance on skilled workers is to increase earnings by 17% relative to controls, but the impacts between skilled workers with and without job assistance are not statistically different so that earnings inequality among skilled workers is not explained by initial informational differences generated

[32] 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.

by the job assistance; (iii) informational differences generated by job assistance do not explain earnings dispersion among unskilled workers either.[33]

The fact that there are persistent impacts of initial differences in skills on earnings suggests a source of labor market inefficiency over and above any search frictions emphasized throughout, is that there are constraints preventing workers accumulating skills. Credit market frictions are the obvious source of this.

Beyond search and credit market frictions, another leading explanation for labor market inefficiencies is *ex post* bargaining between workers and firms (so workers do not take offers as given). To shed light on this, we asked workers whether they have engaged in *ex post* bargaining with firms they received offers from, where dimensions they could bargain over are: (i) wages; (ii) hours; (iii) location; (iv) additional benefits. We combine these into a bargaining index. Column 2 of Table 8 shows the treatment effects on this bargaining index. Only one group of treated workers is impacted: skilled workers with job assistance are significantly more likely to engage in *ex post* bargaining than either controls or equally skilled workers that received no such assistance ($p = .001$). Table A12 shows ITT effects on each index component and we see that these workers report bargaining over locations and additional benefits.

We also see that 70% of workers in the control group report bargaining over wages (and this is not different among treated workers). Hence the overall pattern of results is quite different to that found in US or German data where more than two thirds of workers report not being in a position to bargain over wages, but take offers as given [Wright *et al.* 2019]. Hence the urban labor markets we study are not well described within a competitive search framework, where wages/employment contracts are posted in advance and not negotiated.

Why do skilled workers with job assistance many years earlier bargain harder when they meet a potential employer? One intuition is that workers bargain as their non-employment outside option improves. We can explicitly rule this out because equally skilled workers do not behave in the same way when they meet potential employers.[34]

Rather, our results offer the possibility that the search process itself might influence how hard workers bargain with firms. The results in Table 7 showed that skilled workers with job assistance make a slower transition from casual work towards regular work, and that on the intensive margin they spend less time engaged in regular jobs. Columns 3 and 4 in Table 8 then show treatment

[33] Equilibrium search models have studied the contribution to wage dispersion of worker heterogeneity, firm heterogeneity and market frictions [Bontemps *et al.* 2000, Postel-Vinay and Robin 2002]. Most allow for observed (i.e. education/occupation type) and unobserved (ability/productivity) worker heterogeneity. In line with our results, Postel-Vinay and Robin [2002] find the contribution of unobserved worker heterogeneity to the variance of wages is 40% for high skilled groups, and zero for low skilled groups.

[34] Jaeger *et al.* [2020] study whether the value of non-employment determines wages of the employed, which as they note, is considered a key link in labor models of wage determination [Pissarides 2000], and for macroeconomic models to generate realistic labor demand fluctuations across the business cycle [Hagedorn and Manovskii 2008, Hall and Milgrom 2008]. In wage posting models, the non-employment value also determines reservation wages of the unemployed, pinning down the equilibrium wage offer distribution [Burdett and Mortensen 1998].

effects on (un)employment spells. We see that: (i) skilled workers have significantly shorter unemployment spells and significantly longer employment spells than controls; (ii) these impacts on spells are about half the magnitude for skilled workers with job assistance, so their unemployment spells are significantly longer than for skilled workers ($p = .023$) and their employment spells are significantly shorter ($p = .015$). In short, skilled workers with job offers meet good employers less often. When they do, they might therefore bargain harder. Hence their *ex post* bargaining might be driven more by the frequency of matches and expected employment spell, not outside offers.[35]

To see how important worker heterogeneity in skills and information are for spells in the long run, the results in Column 3 of Table 8 show that: (i) skills decrease unemployment spells by 20%, explaining 23% of the inequality in unemployment spells (as measured by the long run standard deviation of unemployment spells in our sample); (ii) among skilled workers, information generated by low call back rates in match offers explains 12% of the inequality in unemployment spells; (iii) none of the inequality in unemployment spells is robustly explained by informational differences when workers first enter the labor market.

In conclusion, the evidence suggests bargaining as another source of labor market inefficiency for skilled workers. Our results are among the first to establish using experimental variation, that for skilled workers, heterogeneity in information they have over the own labor market prospects when entering the labor market drives long run inequalities in spell durations.

5.3 Sorting into Jobs, Firms and Self-Employment

Our final batch of outcomes probe how worker heterogeneity leads to labor market sorting. We do so by considering the characteristics of jobs and firms that workers end up at in their last employment spell in each survey wave, and the extent to which they engage in self-employment.

We collected information on job and firm characteristics to allow a direct comparison to the ideal job and firm characteristics workers expressed directing their search towards (Table 6). As before, we construct overall indices of job and firm quality, where higher indices correspond to jobs higher up the ladder and more productive firms. The results are in Table 9.

The first row shows that as we move up the skills distribution, workers end up in significantly higher quality jobs – the job index rises by $.096\sigma$ over controls. The treatment effects on each component of the index are shown in Table A13: skilled workers end up in jobs that enable them to supervise others, have high status, learn new job-specific skills (in line with the earlier results on continuous skills accumulation in Table 3), and to work with others.

In sharp contrast, we see that equally skilled workers subject to job assistance up to five years earlier, end up in jobs not significantly different to those among controls. Their job index rises by $.042\sigma$ but we cannot reject the null. Table A13 reveals their jobs are better than those of controls on some dimensions: providing new skills and allowing work with others, but these individuals do

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

not move up the firm hierarchy in that they are not more likely to be supervising others.

Hence there is positive assortative matching between workers and jobs: higher skilled workers end up higher up the job ladder, but this progression is slower for skilled workers whose search strategies were altered because they were provided bad news from job assistance when first entering the labor market.[36]

The last row of Table 9 shows that unskilled workers with job assistance end up in jobs with characteristics that are no different to controls.

Repeating the analysis for characteristics of firms that workers end up employed at, Column 2 shows that among skilled workers, realized firm quality is significantly lower: (i) among those that received job assistance ($p = .035$); (ii) skilled workers with job assistance end up at firms of lower quality than controls. The treatment effects on each component of the index are shown in Table A14, revealing that firm quality is lower for skilled workers with job assistance because they are significantly more likely to end up in informal firms and firms less likely to provide other benefits to workers. Table A14 reveals that realized firm quality is also lower for unskilled workers with job assistance because they are more likely to end up employed in informal firms.

Taken together the results suggest positive assortative matching between workers, jobs and firms: higher skilled workers end up in better jobs and better firms than controls, but also in better jobs and firms than equally skilled workers subject to job assistance. This pattern of results matches closely with the earlier results that skilled workers with job assistance had shorter employment spells, in line with them being at lower productivity firms. Our results contribute novel findings on the precise patterns of sorting between workers, jobs and firms, shedding light on fundamental sources of earnings inequality and the nature of worker-firm complementarities in the economy [Card *et al.* 2013, 2016, 2018].

Our final set of results consider the extent to which workers move up the job ladder via self-employment in our study sectors. Column 3 of Table 9 shows that all treated workers are more likely than controls to engage in self-employment in these sectors. As we saw earlier in Table 7, the fact that long run unemployment rates even for skilled workers remain around 30% just highlights that labor markets do not clear even for them [Banerjee and Sequeira 2020]. Hence the move into self-employment by skilled workers might still represent push factors arising from a lack of labor demand rather than workers preferring self-employment over other jobs.[37]

For unskilled workers with job assistance, the magnitude of the effect (4pp) corresponds to a near 66% increase in long run rates of self-employment over the controls. This aligns perfectly with the stated intent of these workers in the short run – when the main impact on their search strategy of being treated was to borrow funds to set up in some form of self-employment.

[36] Our results complement earlier findings from field experiments in low-income settings that job assistance raises job quality, although most of these have done so on narrower dimensions of job quality and over a shorter horizon [Beam, 2016, Franklin 2017].

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

5.4 Mediation Analysis

We can use mediation analysis to link together the two sets of results so far: that experimentally induced heterogeneity across workers impacts job search strategies along multiple margins, and has persistent impacts on worker labor market outcomes. Following Gelbach [2016], the basic intuition is that the treatment effect of intervention T on outcome Y can be decomposed as operating through as set of mediators, m_j:

$$\frac{dY}{dT} = \sum_{j=1}^{k} \frac{\partial Y}{\partial m_j} \frac{\partial m_j}{\partial T} + R, \tag{2}$$

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

The outcome we focus on is the labor market index (Column 6 in Table 7), and we consider the following skill and search based mediators: the measured sector-specific skills of individuals, the reservation wage as measured by the minimum monthly wage that they would be willing accept for a job requiring a 10-minute commute, beliefs as captured by the expected probability of finding a job in their preferred good sector in the next year, the search intensity index, the ideal job index, the ideal firm index, and whether the individual is borrowing.

The result is shown in Figure 6. The x-axis shows the ITT estimate for each treatment arm on the worker's labor market index, and the dashed red line shows this total ITT effect. The ranking of impacts is as previously described: the largest impact is on workers offered vocational training (the top bar) for whom the ITT impact on the labor market index is $.31\sigma$, followed by those offered training and job assistance ($.23\sigma$) and then unskilled workers with job assistance ($.09\sigma$). Within each bar we show the mediating impact of each factor, indicating the percentage of the overall ITT impact explained by the most prominent mediators.[38]

Among workers offered vocational training, acquired skills are an important mediator driving outcomes, and this channel operates independently of search behavior. However, 13% of the impact on labor market outcomes is directly mediated by skills, while 23% can be explained by search related mediators. Among search strategies, the most prominent mediator is the belief over the job offer arrival rate, explaining 18% of the ITT on the labor market index for skilled workers (13% for those with job assistance). Mediators such as the reservation wage, and ideal jobs and firms that search is directed towards, play relatively little mediating role for long run outcomes in any group. For unskilled workers with job assistance, no single mediator is prominent, although

[38] The total ITT effect (dashed red line) does not overlap with the ITT bar because some mediators can have a negative correlation with the labor market index (so $\frac{\partial Y}{\partial m_j} < 0$).

borrowing has a positive effect.

A large share of the impact on the labor market index remains unexplained (R). This suggests either (i) in line with most models of job search, there are important interactions between the mediators, that the decomposition in (2) does not allow for; (ii) there are important unmeasured mediators. On (ii), an additional mediator to consider would be quality of the initial job/firm that individuals experience. Namely, how much long run outcomes are driven by the worker heterogeneity induced in skills and information, versus persistent effects of initial bad jobs/firms. The earlier results in Table A6 showed short run treatment effects on labor market outcomes (as measured at first follow-up). Most notably the quality of realized firms in the short run is no different to controls for any treatment arm (Column 5). This reinforces the notion that in our study, long run differences in labor market outcomes are driven by heterogeneity in job search strategies induced across workers, not the inherent quality of first jobs/firms experienced.[39]

6 Heterogeneity in Traits

We have so far focused on sources of experimentally induced worker heterogeneity: in skills and information over own prospects induced by job assistance. However, behavioral models have emphasized the role that time-invariant traits have for job search [DellaVigna and Paserman 2005, Falk *et al.* 2006, Caliendo *et al.* 2015, DellaVigna *et al.* 2017, 2020].[40]

Search models represent an optimal stopping problem, so cognitive ability might determine how well worker behavior lines up with theoretical predictions. Despite this, there is surprisingly little work examining how cognition impacts job search [Dohmen and Landeghem 2019]. 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.

On psychological traits, three widely studied traits are self-esteem, locus of control, and neuroticism. Judge *et al.* [2002, 2003] argue they correlate to the same underlying construct, termed self-evaluation. This is a fundamental appraisal of one's worthiness, effectiveness, and capability. An individual with high self-evaluation is well adjusted, positive, self-confident, and believes in her own agency. Such individuals are more able to self-regulate and direct behavior towards goals

[39] Bonhomme *et al.* [2019] discuss two potential reasons why a previous employer may matter for wages and other outcomes: (i) working for a higher quality firm might make a worker more likely to transition to another high quality firm; (ii) the past firm might have a direct effect on a workers wage even after they move.

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

such as job seeking.[41,42]

As shown earlier, cognitive ability and self-evaluation are not impacted by the treatments (Table A4). We thus take both as time invariant. They are also uncorrelated to each other ($\rho = .06$ for the continuous measures). We classify individuals as high/low ability by splitting their cognitive test scores above/below the median, and similarly divide individuals into high/low self-evaluation types. The most succinct way to explore how these traits interact with experimentally induced forms of worker heterogeneity, job search strategies and long run labor market outcomes is to reconsider the mediation analysis for these sample splits.

6.1 Cognitive Ability

Panels A and B of Figure 7 show the mediation analysis for high and low cognitive ability individuals. Comparing the same treatment effect between the panels, the following results emerge.

Among those offered vocational trainings (the top bar), the long run ITT impacts on the labor market index are almost equal between high and low ability individuals ($.33\sigma$ and $.31\sigma$). In sharp contrast, among those offered vocational training and job assistance up to five years earlier, for high ability individuals the ITT effect is $.32\sigma$ (Panel A) while for low ability individuals it is just an eighth of the magnitude at $.04\sigma$ (Panel B). The earlier finding that skilled workers with job assistance fare worse in the long run that skilled workers without job assistance is driven by individuals of low cognitive ability.

This suggests an interaction between cognitive ability and the response to job assistance, that potentially provide information to workers through low call back rates. Recall that call back rates are determined by firm characteristics (such as whether they have a vacancy), and not by worker characteristics. Perfectly informed workers realize this and understand there is no informational content in any given call back, while imperfectly informed workers can misattribute the low call back rate as signaling something about their own job market prospects. The results split by cognitive ability suggest high ability workers are better informed and essentially ignore low call back rates – searching in similar ways as skilled workers not ever offered job assistance. Panel A

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

[42] 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.

shows the mediating role of skills and beliefs to be quite similar among high ability workers with and without job assistance.

Among low cognitive ability individuals, workers offered vocational training and then job assistance are discouraged to such an extent that their long run outcomes are even lower than for unskilled workers with job assistance – their response to low call backs undoes the effect of being provided intense sector-specific skills training.

This interpretation also seems to hold among unskilled workers with job assistance: among this group, high ability individuals do better in the long run than low ability individuals (their labor market indices increase by $.12\sigma$ and $.07\sigma$ respectively over controls).

6.2 Self-evaluation

Panels C and D show the mediation analysis split between workers of high and low self-evaluation. A similar pattern of results emerge as with the split by cognitive ability. Individual self-evaluation does not interact with the long run outcomes of skilled workers, but there is an important interaction between self-evaluation and being offered job assistance. This interlinkage is especially pronounced for skilled workers.

The most striking impact of individual self-evaluation is for workers offered vocational training and then job assistance: Panel C shows the ITT impact on the labor market index to be $.30\sigma$ for high self-evaluation individuals, falling by around half for low self-evaluation individuals (Panel D). Workers of low self-evaluation appear to misattribute low call back rates from job assistance to a high degree. This is in line with what low self-evaluation measures: those individuals are more likely to have an external locus of control, so believe their effort does not affect success, are less likely to adopt new strategies, and be less able to self-regulate to achieve goals.

Taken together the results suggest that workers with low cognitive ability or low self-evaluation are those that misinterpret low call back rates from match offers up to five years earlier, are discouraged, and then their long run labor market outcomes worsen as a self-fulfilling prophecy. For these groups of individuals, once skilled they do better in the long run without any form of job assistance, and just searching with exuberance, even though they are overly optimistic when they first transition into the labor market.

7 Discussion

7.1 External Validity

Our field experiment has many important components and so it is useful to consider the external validity of each aspect: (i) targeted workers; (ii) the scalability of treatments; (iii) information provided to workers; (iv) firms that workers were matched to.

Workers Individuals drawn into our evaluation are the kind of disadvantaged youth that many job training programs target [Attanasio *et al.* 2011, Card *et al.* 2011]. Given that in most developing countries youth unemployment rates are high and there are large cohorts of young job seekers entering the labor market each year, understanding fundamental sources of heterogeneity across these individuals that drive their search behavior and labour market outcomes is important across settings.

Treatments The vocational training offered is provided by pre-existing vocational training institutes throughout Uganda. They normally offer courses of six-months sector specific training in the eight sectors we have focused on. This treatment represents a scalable market-based intervention. Clearly, our treatment offer relaxes credit constraints that would normally prevent young job seekers making such human capital investments. Our results suggest such constraints are a first order source of inefficiency in the urban labour markets studied, driving variation across workers in job search strategies used, and labor market outcomes such as employment rates, earnings, and (un)employment spells.

Our job assistance offer is light-touch, replicating the kind of assistance often provided to job seekers. As there are no market substitutes for such offers, they relax information frictions preventing some worker-firm matches occurring. However, they might be viewed by job seekers as providing a unique opportunity to find meaningful employment because they: (i) allow them to bypass usual channels of job search (informal contacts or walk-ins) and get to the front of job queues; (ii) ensure potential employers are provided the CV of workers they are matched to, enabling the credentials of the worker to be evaluated.[43] Although unusual, these present opportunities that workers would like and seem to have considered. For example, on some dimensions (such as their beliefs over the earnings distribution should they be hired into these sectors) workers are well informed about outcomes even though they have never experienced them.

Information A natural alternative to our design is to provide information directly to workers without involving offers to match workers to firms, that risks imperfectly informed individual misattributing the outcomes of such offers. This information could be about the state of labor demand, about the job prospects of the average young job seeker, or tailored to the specific circumstances of the individual [Altmann *et al.* 2018, Belot *et al.* 2019].[44] Such purely informational

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

[44] Altmann *et al.* [2018] evaluate a light touch intervention providing unemployed German job seekers information about the job search process and the consequences of unemployment. Tracking workers for a year, they find positive impacts of the intervention on employment and earnings of those with the highest predicted risk of unemployment, while there is no impact for workers with low predicted risk of unemployment. Belot *et al.* [2019] evaluate the impact of providing job seekers in Scotland with tailored job search advice through a web-based tool that makes relevant suggestions to job seekers about occupations relevant for their profile. They find that the job-search tool

approaches can be better targeted by understanding heterogeneity in beliefs and knowledge across job seekers. This links back to the long-standing discussion in the job search literature on what exactly individuals learn about during job search – aggregate demand conditions, as captured by learning the wage offer distribution [Wright 1986, Burdett and Vishwanath 1988] – or returns to their own abilities [Falk *et al.* 2006, Gonzalez and Shi 2010].

However, the second general issue we highlight is that individuals with low cognitive ability or low self-evaluation might misunderstand or misattribute information provided to them. This lesson applies to a broader class of information treatments than those implemented through match offers. This links back to Babcock *et al.* [2012] and their emphasis on the need to consider the framing of job assistance offers, because what is perceived by workers matters as much as what is actually presented to them.

Firm Selection A lack of labor demand is a key constraint in experiments involving matching workers to firms. In our context, low call back rates are driven by a lack of vacancies in firms (almost by construction, our design eliminates the possibility that worker characteristics determine call backs). The constraint is logistical in that from when the firm sample is drawn to when match offers made, there can be changes in demand conditions across sectors, regions and the macroeconomy so that even if firms report hiring constraints as binding at baseline, this might no longer be the case by the time job assistance is actually implemented.

A useful thought experiment is what would occur if we could restrict match offers to always involve firms with a vacancy. To do so, we consider impacts on those that were called back by the firm they were matched to in our job assistance program (exploiting the fact that call backs are orthogonal to worker characteristics). To see how call backs interact with job search and long run outcomes, Figure 8 repeats the mediation analysis split between workers with and without call backs (the top bar for skilled workers without match offers is the same as in Figure 6).

Panel A shows that for skilled workers called back, their long run labor market index rises by $.55\sigma$ (where recall the impact for skilled workers without job assistance is $.31\sigma$ as in Figure 6). Panel B shows that for skilled workers without a call back their index rises by only $.15\sigma$, so around one quarter of workers in the same treatment that received a call back. The earlier results from Table A8 show how more positive beliefs are triggered by call backs among skilled workers.

Two implications follow. First, by improving the selection of firms into match offers, this kind of job assistance can benefit workers. Moreover, the impacts of receiving a call back are higher for skilled workers (quadrupling their long run index from $.15\sigma$ to $.55\sigma$ compared to doubling the index from $.08\sigma$ to $.19\sigma$ for unskilled workers). Second, the impacts shown across Panels A and B are akin to having: (i) a perfect selection of firms with vacancies into the sample (Panel A); (ii) having the worse possible selection of firms where none have vacancies (Panel B). These scenarios

broadens the job search activities of job-seekers (i.e. search across a wider range of occupations), and find that job interviews increase as a result, and this is driven by job seekers who initially search more narrowly.

provide reasonable bounds on how much future study designs involving worker-firm match offers can shift labor market outcomes in the long run, for skilled and unskilled youth.[45]

7.2 Policy Implications

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

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

First, in line with research from other settings, we have documented how labor market entrants have biased beliefs [Spinnewijn 2015, Abebe *et al.* 2020a, Banerjee and Sequeira 2020, Mueller *et al.* 2020, Potter 2020]. A natural question is should policy makers design interventions to debias workers? Our results suggest a subtle answer, that depends on the vocational skills of workers.

Among skilled workers, there are returns to them searching while exuberant: they employ different search strategies than equally skilled workers that were provided job assistance and discouraged as a result. In the long run, skilled workers without match offers progress further up the job ladder than equally skilled workers with job assistance. Among unskilled workers the opposite is true: job assistance that credibly confirms their poor prospects unless they change behavior, causes them to adopt new strategies – borrowing for self-employment– and this enables them to do better than controls in the long run.

Second, and following from the last result, unskilled workers are able to access credit markets to finance self-employment. Providing them credible confirmation of their poor prospects might then be more effective that providing them access to microcredit. This obviously relates to an emerging view that microcredit is itself not transformational in driving occupational choice [Banerjee *et al.* 2015], and that small resource transfers to finance job search might not have long run impacts on outcomes [Abebe *et al.* 2020]. Where our study suggests credit market frictions are severe and have long run impacts, are in financing larger scale investments into human capital – such as the kind of intense sector-specific skills training we offered.

Third, our evidence suggests important interplays between job assistance through match offers and fixed worker traits: trying to assist (skilled and unskilled) workers through the offer of matches to firms can backfire for low ability workers, and for those with low self-evaluation. Our results

[45] An alternative approach to raise call back rates in light-touch job assistance would be to provide more information to firms. A class of papers have engineered matches between firms and job-seekers combined with the revelation of information to firms on workers' ability [Pallais 2014, Groh *et al.* 2016, Bassi and Nansamba 2020]. These find that matching *per se* does not generate high call backs (as in our intervention) but that matching with information positively impacts employment outcomes, with impacts varying across the skills distribution.

imply the returns to match offer interventions are maximized by targeting them towards individuals of high cognitive ability and low sector-specific skills: the results in Panels A and B of Figure 7 suggest this would near double labor market outcomes of workers (from $.07\sigma$ to $.12\sigma$). While such unemployed individuals exist in every economy, there are good reasons to argue they constitute a greater share of the unemployed in lower-income settings where resource and information constraints lead to a great misallocation of talent to begin with.[46]

Finally, our findings relate to wider policy discussions about how best to incentivize providers of vocational training. The default position for VTIs in most countries is they have no incentive to match workers to firms. However, it is often debated that government should provide performance-related pay to VTIs, incentivizing them to train *and* find workers employment. Our results suggest that incentive provision might not be enough: trying to match workers to firms is hard and requires additional information to be gained on both demand and supply conditions, in particular: (i) firm vacancies; (ii) worker traits. This complements emerging findings that VTIs face severe information frictions even when trying to find their graduates employment [Banerjee and Chiplunkhar 2018].[47]

8 Conclusion

Many developing countries face the challenge of helping large cohorts of labor market entrants find good jobs. These large pools of job seekers are not homogeneous, and as research in labor economics has highlighted for decades, worker heterogeneity is central to understanding differences in labor market outcomes. We have presented results from a long term field experiment to shed light on some fundamental sources of heterogeneity, arising from experimentally induced differences in worker skills and information, and fixed worker traits related to their cognitive ability and self-evaluation. The measurement tools in our study were designed to shed light on how search strategies used by job seekers vary across workers in these dimensions, and the time period of study enables us to shed light on how these mechanisms of search translate into labor market outcomes and explain progression up the job ladder, and inequality in employment, earnings, and (un)employment spells across workers.

[46] Abebe *et al.* [2020] present evidence from a field experiment in Ethiopia on selecting high ability individuals into clerical positions. They document how decreasing application costs can improve selection because high quality candidates face on average higher application costs. A dynamic selection mechanism drives this: high-ability individuals who face relatively low application costs find work faster and stop searching for work earlier than individuals who have similar ability, but face higher application costs. Over time this creates a positive correlation between ability and application costs among individuals still searching for work. Hence lowering screening costs may be beneficial for employers.

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

By linking short run search strategies to long run labor market outcomes, we shed light on the underlying sources of inefficiency in these low-income labor markets: constraints preventing workers from making investments in their human capital that would otherwise generate large private returns, information frictions that prevent some worker-firm matches occurring, and *ex post* bargaining between workers and firms that mean workers do not take job offers as given.

In doing so, we add new evidence to a nascent literature studying labor market dynamics in low-income settings [Bick *et al.* 2018, Feng *et al.* 2020] and provide an agenda for key ingredients that need to be incorporated into job search models appropriate for such economies [Rud and Trapeznikova 2019, Donovan *et al.* 2020]. Given the central role labor markets play in determining labor productivity, the firm size distribution, incomes and macroeconomic cycles, doing so will be critical to advancing our understanding of what are likely to be the most effective labor market policies to promote economic development.

A Appendix

A.1 Implementation of Match Offers

The match offer treatments were implemented by job placement officers (JPOs) hired by BRAC specifically for this intervention. 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. 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.

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Table 1: Baseline Balance on Labor Market Histories

Means, robust standard errors from OLS regressions in parentheses

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

P-value on F-tests in braces

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

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. All data is from the baseline worker survey. Columns 1 to 6 report the mean of each worker cl 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 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 joi 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 correspondi 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 mont 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 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 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 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 monetary amounts are then converted into August 2012 USD.

Table 2: Jobs, Search and Matching

	Casual Jobs	Regular Jobs
	Farming, animal rearing, fishing, loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing, portering/helping at a construction site	Motor-mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical wiring, construction, welding, factory work, housemaid, childcare, retail, public sector employee
A. Job Characteristics		
Worked in this activity in the last month	.255	.182
Self-employed	.667	.206
Number of months involved in activity in the last year	3.57	3.59
Hours worked in a typical day | employed	5.08	8.31
Days worked in a typical week | employed	5.12	5.42
Earnings in the last month | employed	9.24	25.0
B. Worker Job Search Methods		
Through friends/family member	.201	.483
Direct walk-in	.068	.252
Immediate family owns the business	.145	.050
Read job ad	.009	.017
C. Firm Recruitment Strategies		
Direct walk-in		.410
Through friends/family member		.407
Worker is a family member		.127
Posted job ad		.013
D. Screening		
Had to interview	.014	.192
Had to provide references	.021	.185
Had to take a skills test	.030	.268

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

Table 3: Skills Acquisition

OLS regression coefficients, robust standard errors in parentheses

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

	Sector-Specific Skills Test			Attended further vocational training	Attended further education
	Any relevant skills	Test score (ITT)	Test score (2SLS)		
	(1)	(2)	(3)	(4)	(5)
Vocational Training	.256***	6.42***	8.41***	.052***	.022
	(.023)	(1.21)	(1.60)	(.012)	(.023)
	{.000, .001}	*{.000, .001}*	-	*{.000, .001}*	*{.338, .657}*
Vocational Training + Job Assistance	.252***	7.44***	11.0***	.043***	-.006
	(.025)	(1.43)	(2.19)	(.013)	(.025)
	{.000, .001}	*{.000, .001}*	-	*{.000, .002}*	*{.812, .804}*
Job Assistance	.014	1.14	.949	.000	.019
	(.029)	(1.41)	(2.01)	(.010)	(.026)
	{.643, .610}	*{.428, 417}*	-	*{.978, .985}*	*{.445, .695}*
P-value: VT = VT + Job Assistance	*[.852]*	*[.488]*	*[.261]*	*[.572]*	*[.272]*
Mean in Control Group	.613	30.1	30.1	.037	.384
N. of observations	2,134	2,134	2,134	2,697	2,433

Notes: ****denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline, second and third worker follow-up surveys. All regressions include strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. In Column 1 we report a linear probability model on whether the respondent reports having any sector specific skills or not. In Columns 2 and 3 the dependent variable is the skills test score, from the test administered to workers in the second and third worker follow-ups. Column 2 reports OLS estimates, while in Column 3 we report 2SLS regressions, where we instrument treatment take-up with the original treatment assignment. In Column 3 standard errors are bootstrapped with 1000 replications. Take-up in is defined as the worker having completed the 6-months Vocational Training for the Vocational Training + Match Offer treatments, and as being called back in the Match Offer treatment. Workers that reported not having any sector specific skills are assigned a test score equal to what they would have got had they answered the test at random. Workers that refused to take the skills test are excluded from the regressions in Columns 2 and 3. In Column 4 the dependent variable is a dummy equal to one if the individual attended any training at a vocational training institute, excluding the six-months vocational training individuals underwent as part of the treatments. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 4: Reservation Wages

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

Minimum wage would accept for a job requiring:	10 minute commute [USD]	30 minute commute [USD]	60 minute commute [USD]
	(1)	(2)	(3)
Vocational Training	5.16*	5.51	8.47
	(3.11)	(4.00)	(5.55)
	{.100, .222}	*{.169, .297}*	*{.136, .310}*
Vocational Training + Job Assistance	.154	1.01	3.54
	(3.39)	(4.23)	(5.80)
	{.961, .961}	*{.822, .812}*	*{.520, .703}*
Job Assistance	-2.87	-6.26	-4.26
	(3.11)	(4.07)	(5.70)
	{.350, .577}	*{.130, .297}*	*{.452, .703}*
P-value: VT = VT + Job Assistance	*[.143]*	*[.297]*	*[.409]*
Mean in Control Group	58.6	87.5	123
N. of observations	1,173	1,162	1,167

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline, as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. To construct the outcomes in Columns 1 to 3, respondents were asked to think of an identical job that could be located in three different places, all of which take a different amount of time to travel to. They were then asked what was the minimum monthly wage that they would be willing to take for a job which required them to commute for 10, 30 and 60 minutes each way respectively. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 5: Beliefs

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

	Good Sector Jobs					
	Min. exp. monthly earnings [USD]	Max. exp. monthly earnings [USD]	Exp monthly earnings [USD]	Exp. prob of finding a job in the next year (0 to 10 scale)	Labor market beliefs index	Turned down any job offer in the last year
	(1)	(2)	(3)	(4)	(5)	(6)
Vocational Training	17.7***	31.8***	25.4***	1.84***	-.048	.024
	(3.06)	(4.85)	(4.37)	(.205)	(.046)	(.020)
	{.000, .001}	*{.000, .001}*	*{.000, .001}*	*{.000, .001}*	*{.305, .603}*	*{.246, .521}*
Vocational Training + Job Assistance	12.0***	23.6***	17.9***	1.45***	-.054	-.001
	(3.28)	(5.37)	(4.67)	(.217)	(.052)	(.022)
	{.000, .002}	*{.000, .001}*	*{.000, .001}*	*{.000, .001}*	*{.301, .603}*	*{.952, .944}*
Job Assistance	3.21	6.04	3.47	.242	-.039	.008
	(3.05)	(4.97)	(4.44)	(.216)	(.053)	(.021)
	{.327, .297}	*{.222, .236}*	*{.436, .414}*	*{.261, .286}*	*{.441, .603}*	*{.676, .901}*
P-value: VT = VT + Job Assistance	*[.095]*	*[.129]*	*[.105]*	*[.082]*	*[.907]*	*[.285]*
Mean in Control Group	42.9	72.5	57.8	4.19	.028	.073
N. of observations	952	946	801	1,171	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, as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. Minimum, Maximum and Expected monthly earnings in Columns 1 to 3 refer to the workers' expected earnings in their preferred sector among the eight study sectors. In Column 3 we assume a triangular distribution to calculate the average expected monthly earnings. Individuals who report a probability of finding a job in the next 12 months equal to zero are excluded from the sample in Columns 1 to 3. In Column 5 the outcome is an index of worker’s labor market beliefs, constructed following Anderson's [2008] approach. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 6: Search Intensity, Directed Search, Savings and Borrowing

OLS regression coefficients, robust standard errors in parentheses

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

	Search	Directed Search: Jobs and Firms		Saving and Borrowing			
	Search Intensity Index	Ideal Job Index	Ideal Firm Index	Has any savings	Is borrowing any money	Is borrowing to finance job search	Is borrowing to finance business expenditures
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Vocational Training	.092**	-.054	.103***	-.047	.049	.004	.017
	(.041)	(.040)	(.036)	(.034)	(.035)	(.005)	(.015)
	{.024, .053}	*{.169, .313}*	*{.004, .013}*	*{.191, .352}*	*{.165, .268}*	*{.592, - }*	*{.314, .449}*
Vocational Training + Job Assistance	.041	-.022	.030	-.018	.027	-.004	-.006
	(.046)	(.041)	(.039)	(.038)	(.038)	(.003)	(.014)
	{.367, .565}	*{.605, .593}*	*{.454, .480}*	*{.643, .604}*	*{.445, .472}*	*{.261,*	*{.652, .689}*
Job Assistance	.003	-.064	.042	.046	.090**	.003	.034*
	(.040)	(.042)	(.039)	(.039)	(.039)	(.003)	(.019)
	{.945, .940}	*{.139, .303}*	*{.311, .480}*	*{.242, .372}*	*{.018, .054}*	*{.389, - }*	*{.060, .191}*
P-value: VT = VT + Job Assistance	*[.290]*	*[.465]*	*[.102]*	*[.446]*	*[.574]*	*[.130]*	*[.147]*
Mean in Control Group	-.038	.020	-.046	.325	.277	.003	.034
N. of observations	1,231	1,231	1,215	1,231	1,199	1,231	1,231

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. P-values adjusted for multiple testing are not reported for the outcome in Column 6 due to the sparsity of the data. All indexes are constructed following Anderson's [2008] approach. The dependent variables in Columns 6 and 7 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 6) or finance business expenditures (Column 7). In Column 7 business expenditures include expenses incurred to set up, or register a business, purchasing business assets or inputs, pay wages, etc. At the foot of each column we report p-values on the tests of equality of treatment effects between vocational training and vocational training + job assistance.

Table 7: Labor Market Outcomes in the Long Run

OLS regression coefficients, robust standard errors in parentheses.

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

	Has done any work in the last month	Has done any regular work in the last month	Number of months of regular work in the last year	Total regular earnings in the last month [USD]	Number of months worked in one of the eight good sectors in the last year	Labor Market Index
	(1)	(2)	(3)	(4)	(5)	(6)
Vocational Training	.094***	.113***	1.33***	8.07***	1.94***	.310***
	(.021)	(.022)	(.232)	(2.33)	(.207)	(.036)
	{.000, .001}	*{.000, .001}*	*{.000, .001}*	*{.000, .003}*	*{.000, .001}*	*{.000, .001}*
Vocational Training + Job Assistance	.063***	.066***	.690***	5.74**	1.54***	.231***
	(.023)	(.024)	(.257)	(2.69)	(.228)	(.041)
	{.011, .010}	*{.009, .013}*	*{.008, .013}*	*{.028, .065}*	*{.000, .001}*	*{.000, .001}*
Job Assistance	.051**	.054**	.510**	1.25	.556***	.090**
	(.022)	(.023)	(.246)	(2.47)	(.203)	(.036)
	{.024, .019}	*{.018, .015}*	*{.037, .034}*	*{.617, .616}*	*{.004, .004}*	*{.010, .013}*
P-value: VT = VT + Job Assistance	*[.152]*	*[.043]*	*[.011]*	*[.396]*	*[.104]*	*[.063]*
Mean in Control Group	.623	.524	5.91	38.0	1.88	-.148
N. of observations	3,703	3,700	3,724	3,541	3,723	3,725

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

Table 8: Total Earnings, Bargaining and Spells

OLS regression coefficients, robust standard errors in parentheses

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

	Total earnings in the last month [USD]	Bargaining index	Length of last unemployment spell (months)	Length of last employment spell (months)
	(1)	(2)	(3)	(4)
Vocational Training	11.4***	.002	-1.24***	1.24***
	(2.60)	(.023)	(.235)	(.234)
	{.000, .001}	*{.904, .917}*	*{.000, .001}*	*{.000, .001}*
Vocational Training + Job Assistance	7.28**	.089***	-.667**	.619**
	(2.96)	(.025)	(.259)	(.258)
	{.015, .021}	*{.000, .001}*	*{.013, .024}*	*{.020, .029}*
Job Assistance	3.26	-.018	-.411	.452*
	(2.75)	(.024)	(.250)	(.248)
	{.249, .246}	*{.460, .668}*	*{.081, .102}*	*{.054, .063}*
P-value: VT = VT + Job Assistance	*[.181]*	*[.001]*	*[.023]*	*[.015]*
Mean in Control Group	43.8	-.019	6.20	5.63
N. of observations	3,145	3,570	3,693	3,693

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

Table 9: Realized Jobs, Realized Firms and Self-Employment

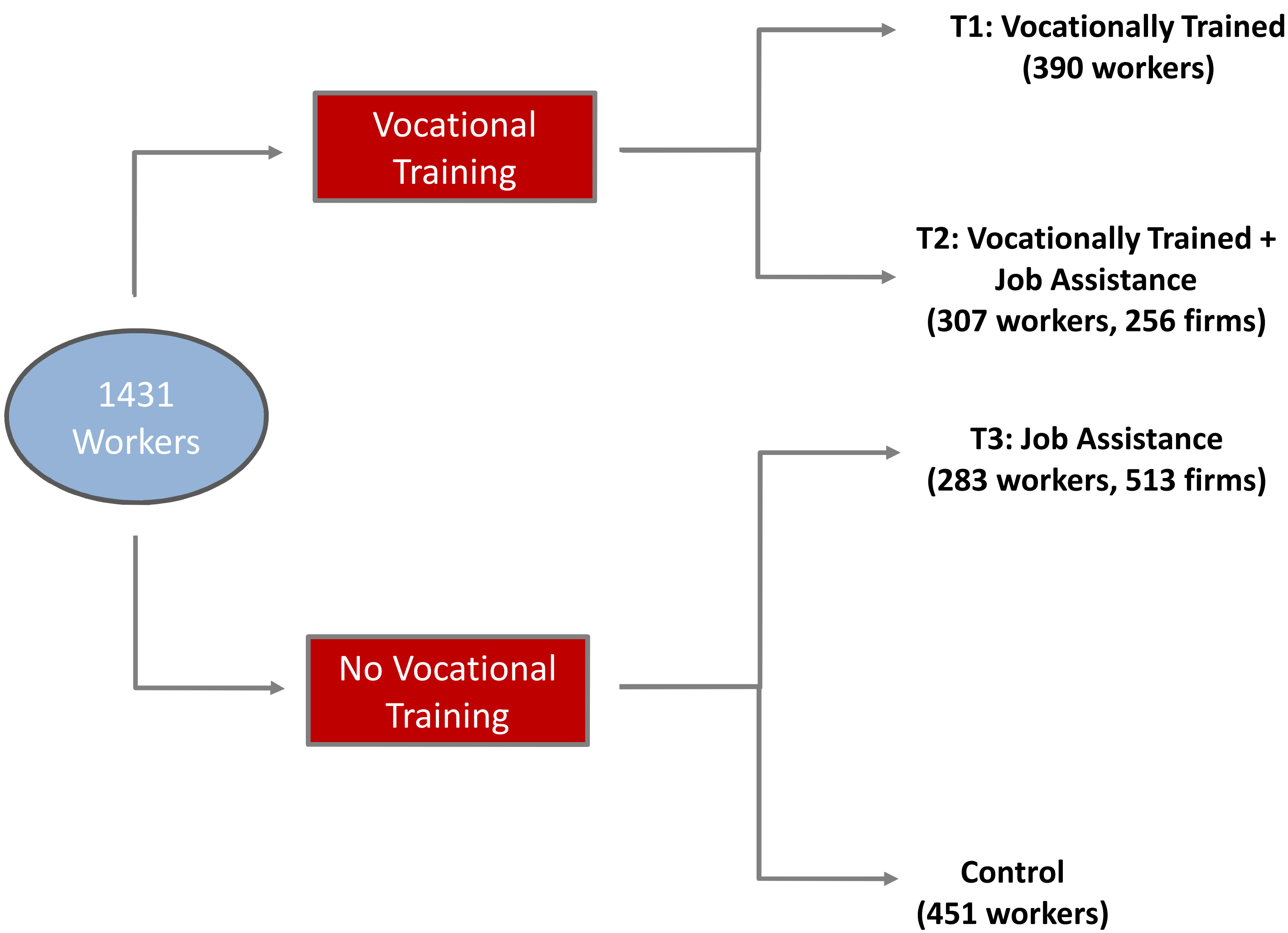
OLS regression coefficients, robust standard errors in parentheses

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

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

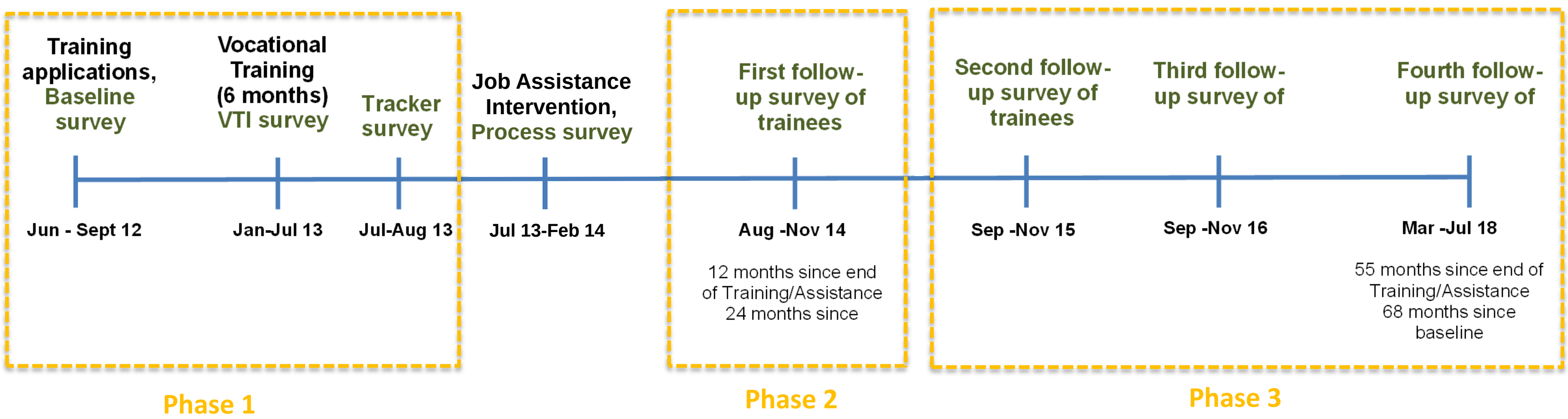
Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the second, third and fourth worker follow-up survey. All regressions control for the value of the outcome at baseline when available, strata dummies, survey wave dummies, a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

Figure 1: Experimental Design



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

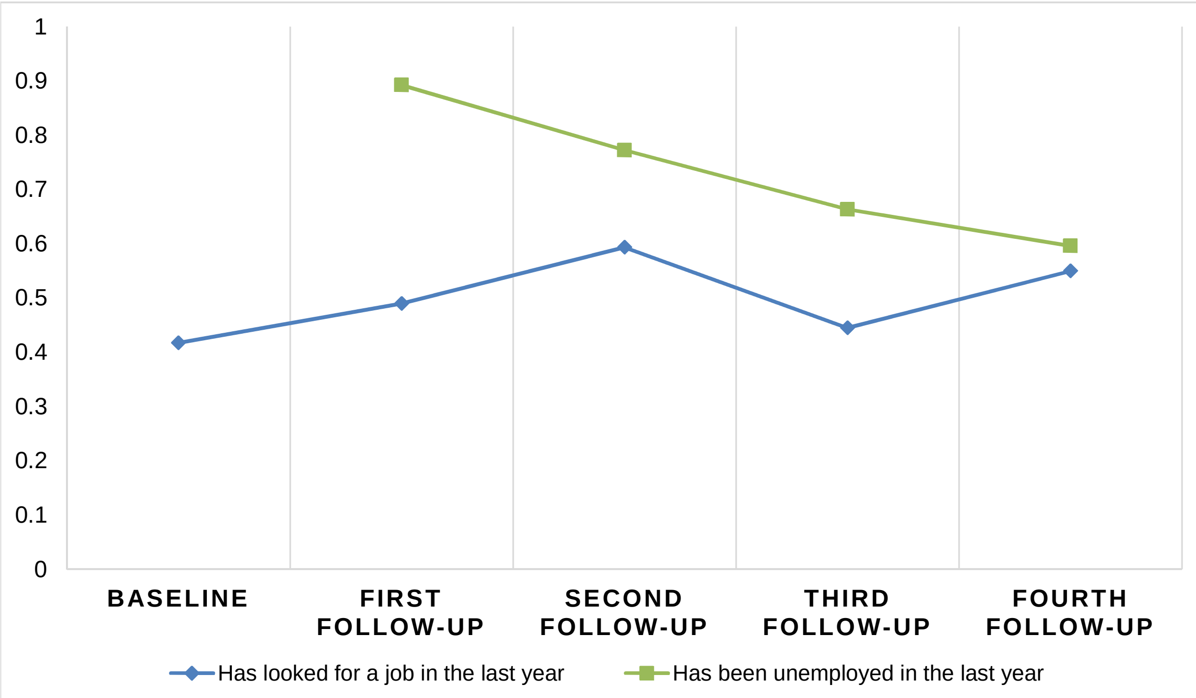
Figure 2: Timeline of Worker Surveys



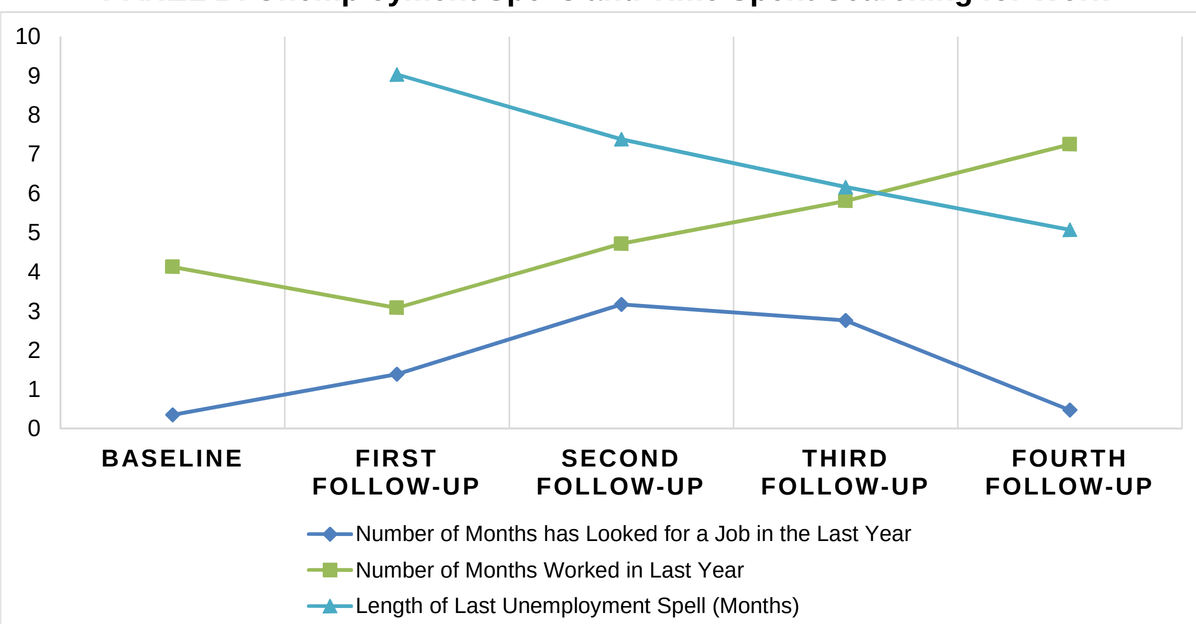
Notes: The timeline highlights the relevant dates for the main batch of workers and worker surveys. A second smaller round of applications and baseline surveys (17% of the overall sample) were conducted in May and June 2013. The majority of trainees from the first round of applicants started training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

Figure 3: Employment Outcomes and Search Among Controls

PANEL A: Unemployment and Job Search



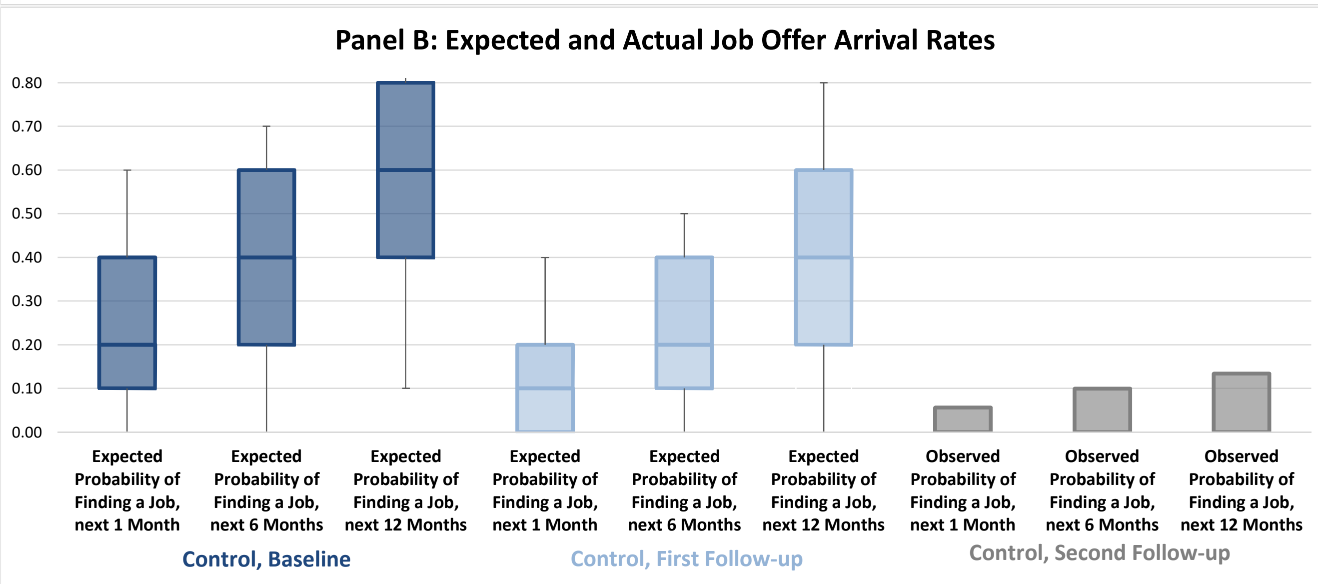
PANEL B: Unemployment Spells and Time Spent Searching for Work



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

Figure 4: Earnings and Employment Expectations

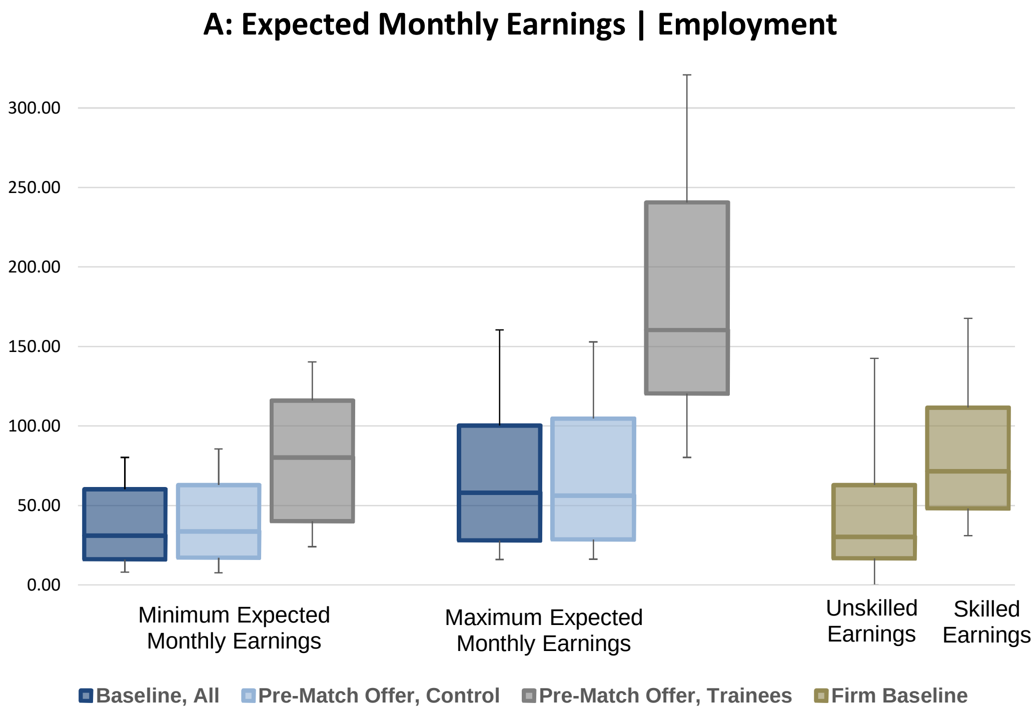
10th, 25th, 50th, 75th and 90th percentiles

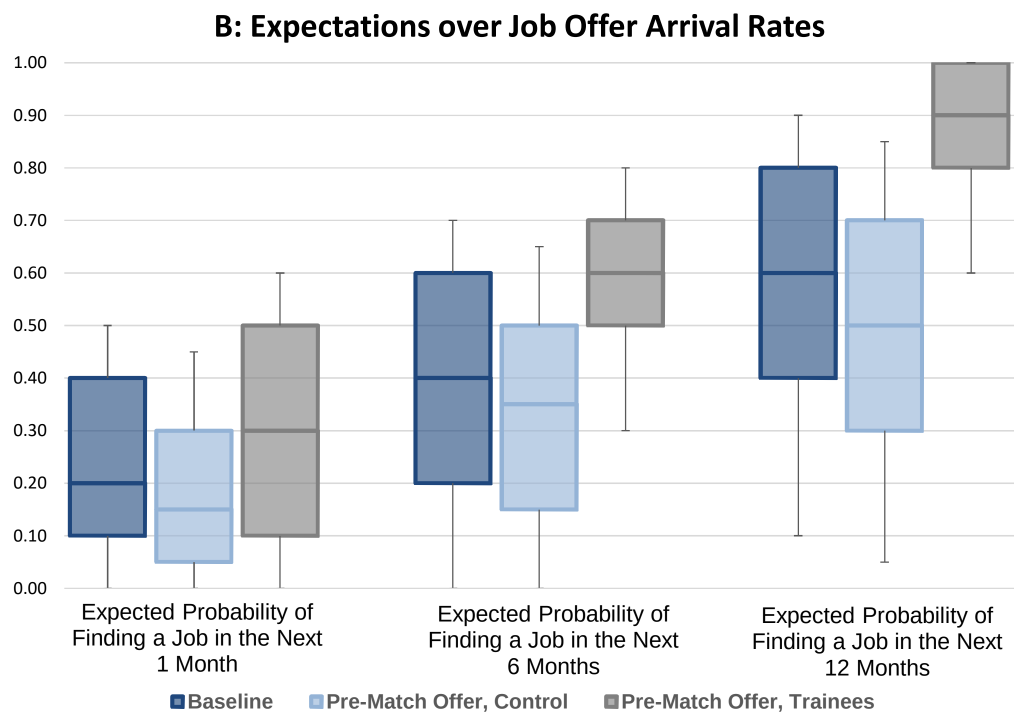


Notes: Panel A shows box-and-whisker plots for actual and expected monthly earnings conditional on wage employment from three different samples. Each plot shows the 10th, 25th, 50th, 75th and 90th percentiles of actual/expected earnings distributions The first worker baseline sample shows actual earnings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

Figure 5: Evolution of Expectations

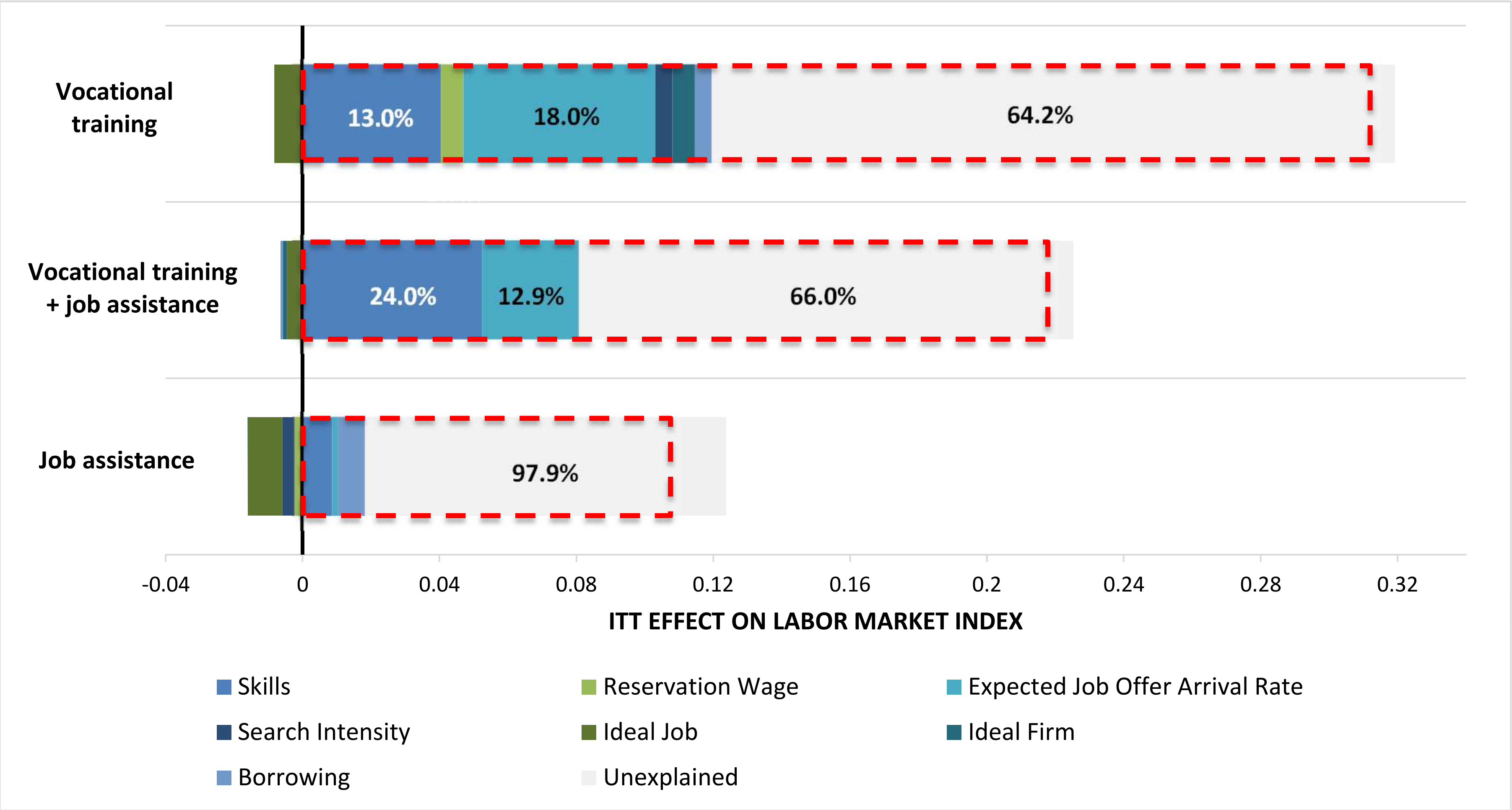
10th, 25th, 50th, 75th and 90th percentiles





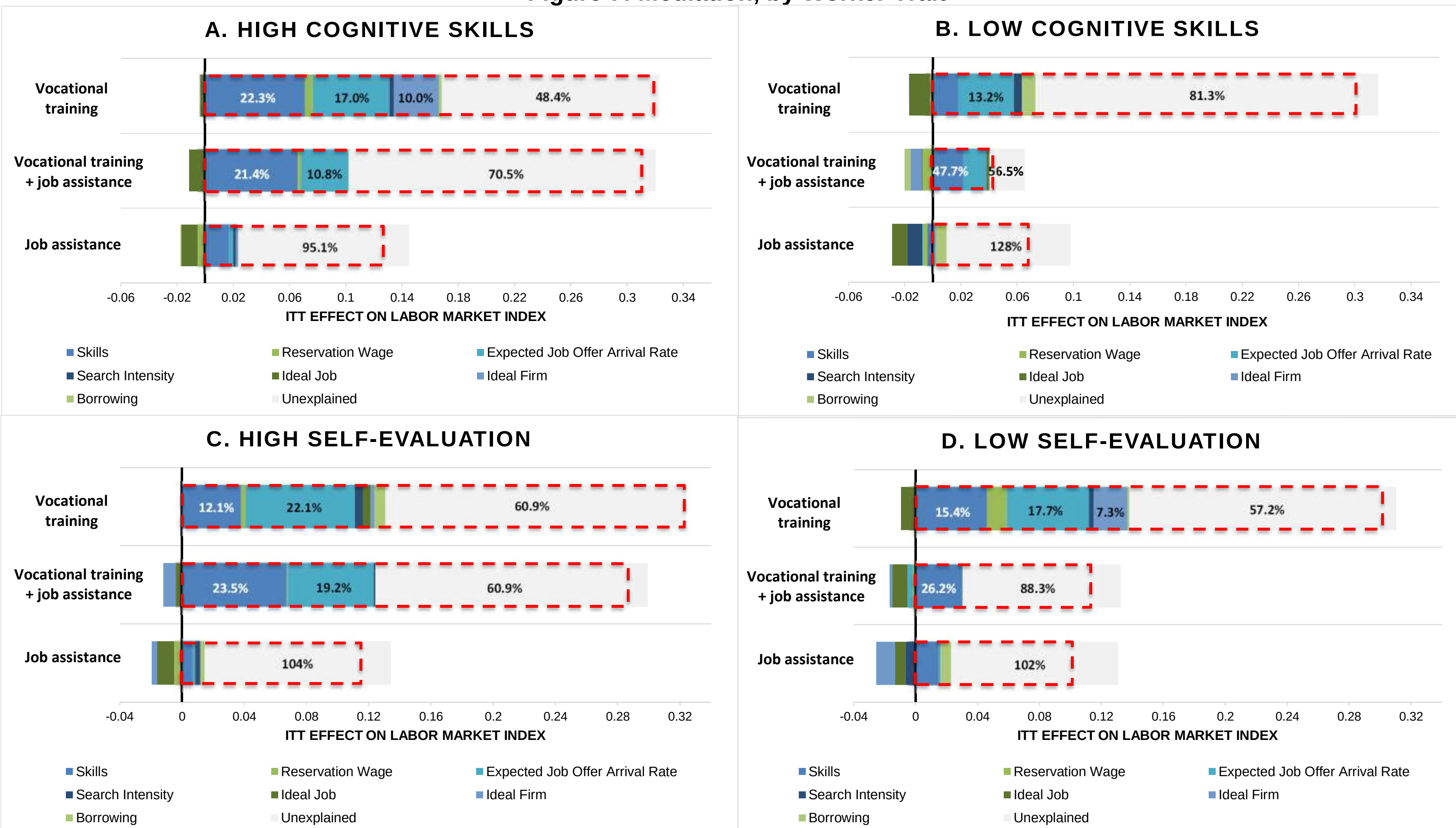
Notes: The data used is from baseline, VTI surveys conducted towards the end of the training period while trainees were still enrolled at the vocational training institutes, and we extrapolate back from the first worker follow-up survey assuming a linear evolution of beliefs, to what would have been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

Figure 6: Mediation Analysis



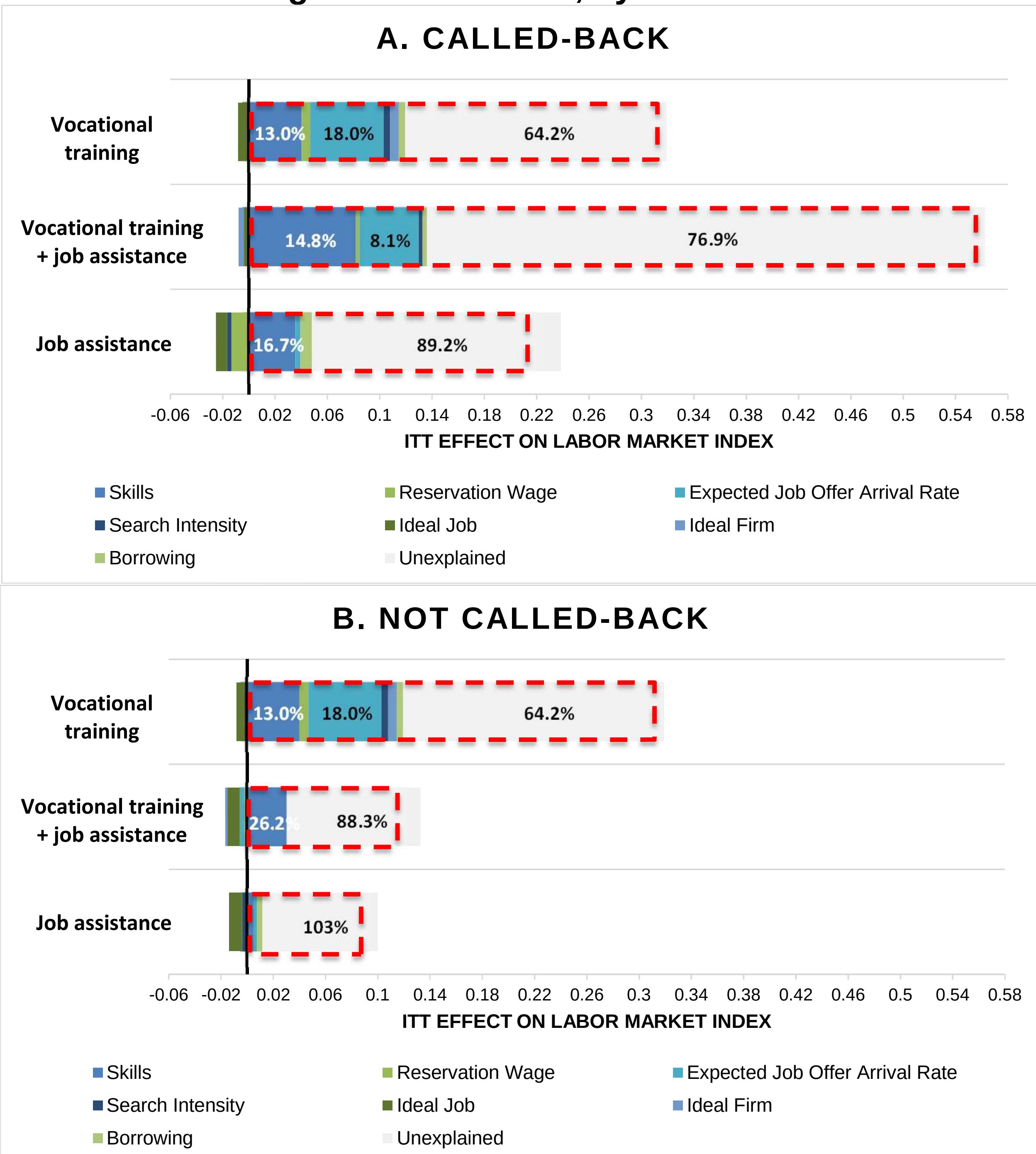
Notes: We show a decomposition of the ITT effect on the labor market index, following the approach of Gelbach [2016]. We show the decomposition of the difference between the ITT effects in the full (with mediators) and restricted (without mediators) models. The dashed (red) lines show the magnitude of 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 monthly wage that they would be willing to take for a job which required them to commute for 10 minutes, the search intensity index, the ideal job index, the ideal firm index and a dummy for whether the individual is borrowing.

Figure 7: Mediation, by Worker Trait



Notes: We show a decomposition of the ITT effect on the labor market index, following the approach of Gelbach [2016]. In Panels A and B 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 Panels C and D 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. In each Panel we show the decomposition of the difference between the ITT effects in the full (with mediators) and restricted (without mediators) models. The dashed (red) 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 monthly wage that they would be willing to take for a job which required them to commute for 10, the search intensity index, the ideal job index, the ideal firm index and a dummy for whether the individual is borrowing.

Figure 8: Mediation, by Call-back



Notes: We show a decomposition of the ITT effect on the labor market index, following the approach of Gelbach [2016]. In Panels A and B we split the sample into those involved in match offer treatments that do receive an actual call back, and those that do not receive a call back. In each Panel we show the decomposition of the difference between the ITT effects in the full (with mediators) and restricted (without mediators) models. The dashed (red) 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 secgtor job in the next 12 months, the reservation wage as measured by the minimum monthly wage that they would be willing to take for a job which required them to commute for 10, the search intensity index, 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 from in the last month
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Baseline, aged 18-25	20.1	.566	.037	.013	.037	.361	.150	6.01
	(1.89)	(.496)	(.188)	(.115)	(.188)	(.480)	(.357)	(17.9)
Uganda National Household Survey 2012/13:								
B. All, aged 18-25	21.1	.465	.395	.309	.062	.681	.293	9.13
	(2.32)	(.499)	(.489)	(.462)	(.241)	(.466)	(.455)	(28.2)
C. Labor Market Active, aged 18-25	21.4	.475	.448	.207	.064	.902	.389	12.2
	(2.33)	(.499)	(.497)	(.405)	(.245)	(.297)	(.489)	(32.0)

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

Table A2: Baseline Balance on Worker Characteristics

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

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

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

Table A3: Attrition

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

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

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

Table A4: Personality Traits, Cognitive Skills and Psychological Traits

OLS regression coefficients, robust standard errors in parentheses

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

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

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

Table A5: Correlates of Call Backs

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

	Vocational Training + Job Assistance		Job Assistance	
	Worker and Firm Characteristics	Worker Characteristics and Firm FEs	Worker and Firm Characteristics	Worker Characteristics and Firm FEs
	(1)	(2)	(3)	(4)
PANEL A: Worker Characteristics				
Female	-.045	-.093	-.007	.003
	(.079)	(.074)	(.082)	(.070)
Age	-.008	-.014	.026**	-.000
	(.013)	(.012)	(.012)	(.004)
Any Child	-.081	-.043	-.019	.019
	(.128)	(.137)	(.065)	(.026)
Education Level	.019	.020	-.017	-.010*
	(.017)	(.017)	(.011)	(.006)
Has Ever Worked	-.024	-.049	-.026	.044
	(.089)	(.084)	(.054)	(.038)
Literacy/Numeracy Test Score	.006	.001	-.010	-.006
	(.015)	(.015)	(.013)	(.005)
PANEL B: Firm Characteristics				
Owner would like to Expand	.200**		.031	
	(.094)		(.070)	
Firm constrained by Lack of Trustworthy Workers	.116*		-.037	
	(.067)		(.095)	
Firm constrained by Inability to Screen Workers	-.106		.100	
	(.077)		(.077)	
Owner Age	-.007		.000	
	(.004)		(.004)	
Owner Education Level	.023**		.001	
	(.009)		(.009)	
Firm Age	.003		.003	
	(.005)		(.011)	
Number of Employees	-.040*		.006	
	(.021)		(.021)	
Log (Monthly Profits)	.047		.025	
	(.037)		(.035)	
Mean of dep. var. in control	.161		.179	
P-value: firm covariates	[.037]	-	[.946]	-
P-value: worker covariates	[.734]	[.689]	[.242]	[.299]
Firm fixed effects	No	Yes	No	Yes
Sector of match dummies	Yes	Yes	Yes	Yes
BRAC branch office dummies	Yes	Yes	Yes	Yes
Observations	162	162	299	299

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

Table A6: Labor Market Outcomes in the Short Run

OLS regression coefficients, robust standard errors in parentheses

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

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

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

Table A7: Components of Labor Market Beliefs Index

OLS regression coefficients, robust standard errors in parentheses

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

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

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

Table A8: Beliefs by Call-back

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

	Min. exp. monthly earnings [USD]	Max. exp. monthly earnings [USD]	Exp monthly earnings [USD]	Exp. prob of finding a job in the next year (0 to 10 scale)
	(1)	(2)	(3)	(4)
Vocational training with job assistance	11.7***	20.8***	15.9***	1.36***
	(3.47)	(5.67)	(4.91)	(.228)
	{.000, .007}	{.000, .003}	{.003,.012}	{.000, .001}
Vocational training with job assistance x Called back	2.17	17.3*	11.6	.706*
	(5.94)	(10.2)	(8.65)	(.421)
	{.735, .718}	{.111, .255}	{.215, .467}	{.127, .264}
Job assistance	4.07	7.36	3.71	.137
	(3.21)	(5.31)	(4.74)	(.228)
	{.201, .501}	{.164, .317}	{.431, .695}	{.566, .561}
Job assistance x Called back	-4.99	-7.55	-1.21	.608
	(6.51)	(9.58)	(8.42)	(.454)
	{.440, .687}	{.450, .441}	{.883, .869}	{.206, .325}
Mean in Control Group	42.9	72.5	57.8	4.19
N. of observations	952	946	801	1,171

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and the first worker follow-up survey. All regressions control for the value of the outcome at baseline, as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. Randomization-t p-values are computed following Young [2019], and p-values adjusted for multiple testing are computed using Romano and Wolf [2016] step-down procedure. These are both reported in braces. Minimum, Maximum and Expected monthly earnings in Columns 1 to 3 refer to the workers' expected earnings in their preferred sector among the eight study sectors. In Column 3 we assume a triangular distribution to calculate the average expected monthly earnings. Individuals who report a probability of finding a job in the next 12 months equal to zero are excluded from the sample in Columns 1 to 3. 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 A9: Components of the Search Intensity Index

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
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Vocational Training	.175***	6.26	.084**	.053	.088***
	(.036)	(4.25)	(.033)	(.033)	(.028)
	{.000, .001}	*{.139, .256}*	*{.012, .026}*	*{.112, .277}*	*{.003, .010}*
Vocational Training + Job Assistance	.097**	10.4**	.060*	-.005	.056*
	(.040)	(5.11)	(.036)	(.036)	(.030)
	{.021, .030}	*{.041, .125}*	*{.101, .167}*	*{.886, .989}*	*{.072, .121}*
Job Assistance	-.036	-3.54	-.036	-.000	-.004
	(.041)	(4.33)	(.033)	(.036)	(.028)
	{.385, .372}	*{.405, .416}*	*{.270, .251}*	*{.996, 1.00}*	*{.899, .889}*
P-value: VT = VT + Job Assistance	*[.053]*	*[.440]*	*[.523]*	*[.125]*	*[.338]*
Mean in Control Group	.490	26.5	.217	.270	.139
N. of observations	1,231	1,173	1,231	1,231	1,231

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

Table A10: Components of the Ideal Job Index

OLS regression coefficients, robust standard errors in parentheses

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

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

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

Table A11: Components of the Ideal Firm Index

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

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

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

Table A12: Components of the Worker-Firm Bargaining Index

OLS regression coefficients, robust standard errors in parentheses

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

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

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

Table A13: Components of the Realized Job Quality Index

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

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

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

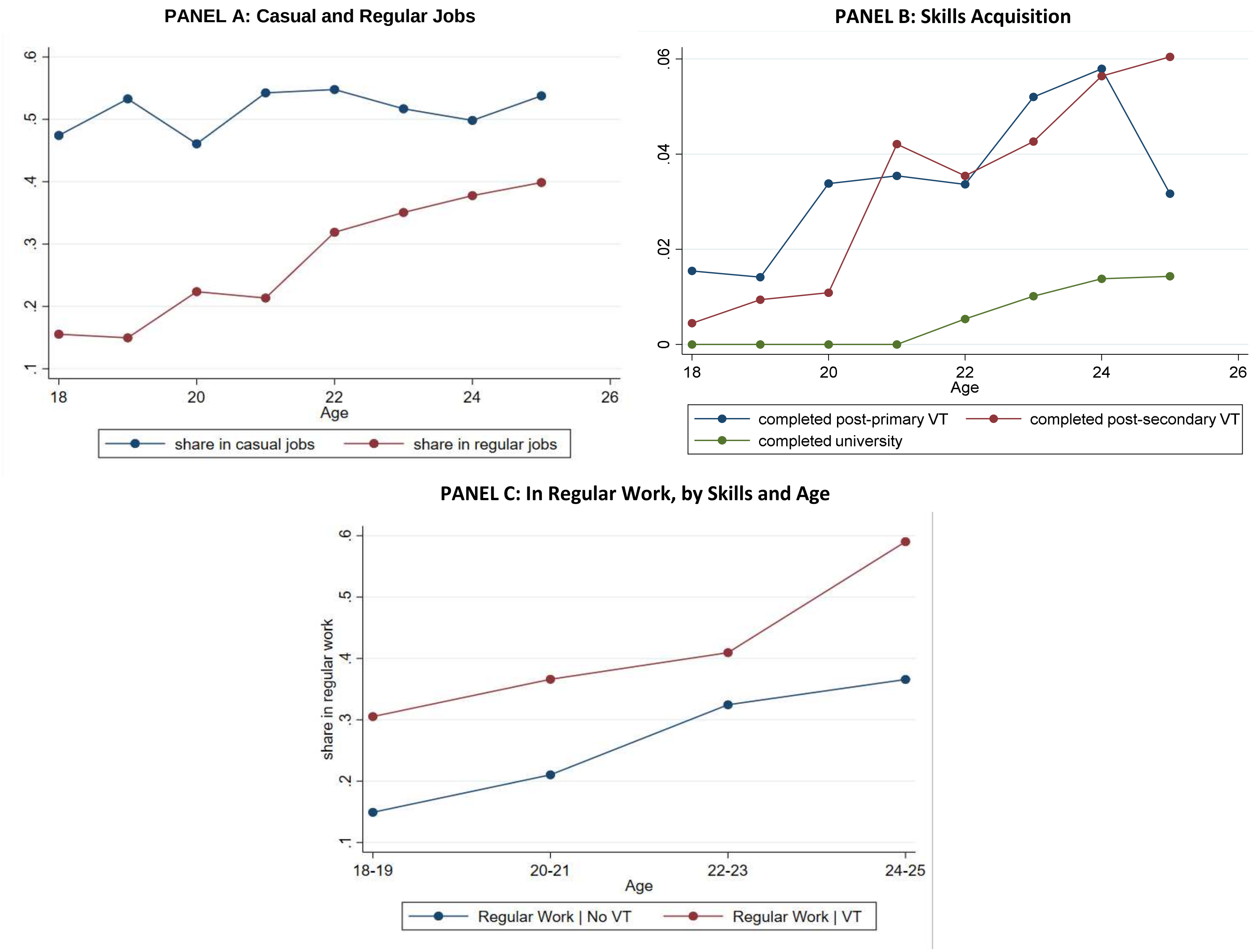
Table A14: Components of the Realized Firm Quality Index

OLS regression coefficients, robust standard errors in parentheses

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

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

Figure A1: Jobs and Skills by Age



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

Figure A2: Sector Skills Test for Motor Mechanics

<table>
<tr><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>
</table>