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

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There are 420 million young people in Africa today, and only one in three has a regular salaried job. We study how two common labor market interventions—vocational training and matching—affect the job search behavior of young workers. We do so by means of a field experiment tracking young job seekers for 6 years in Uganda's main

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cities. Vocational training amplifies the job seekers' initial optimism, leading them to search more intensively and toward high-quality firms. Adding matching has the opposite effect, plausibly because of low callback rates. These differences affect labor market outcomes in the long run.

I. Introduction

One-third of Africa's 420 million young people have regular jobs (AFDB 2016). Current fertility rates in many parts of the continent mean that ensuring meaningful employment for young labor market entrants will be increasingly challenging (Bandiera et al. 2022). Finding a solution will greatly affect the pace of economic development.

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

We study the issue using a field experiment tracking young labor market entrants over 6 years, shedding light on the link between skills, expectations, search behaviors, and long-run labor market outcomes. We explore these links using two standard labor market interventions (Card, Kluve, and Weber 2017; McKenzie 2017) offering (i) vocational training, (ii) vocational training combined with a light-touch matching intervention that passes worker's details to local firms, and (iii) matching only.

We recruited labor market entrants from across Uganda, offering them the possibility of 6 months sector-specific training in welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring, and catering. These sectors are associated with "good jobs" offering regular employment in high-wage firms. At baseline, 25% of wage-employed Ugandans aged 18–25 work in such sectors. The eligibility criteria targeted disadvantaged youth with limited labor market experience and hence scope to learn about their job prospects through search. We received 1,400 applications: before intervention,

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applicants were unskilled, found work through informal contacts, and mostly held casual jobs. The 1,281 firms in the experiment's matching component operate in the eight sectors in 15 urban labor markets.

Individuals are first randomly assigned to receive an offer of vocational training. Two-thirds take up the offer, and 90% complete their training course. At a second stage of randomization, we offer light-touch matching between workers and firms. Nearly all workers agreed to have their details passed onto these firms. Firms were presented short lists of two workers that were either both vocationally trained or both unskilled. Firms could call back for interview neither, one, or both (and remained free to recruit other workers). Our design thus assigns workers to four groups: (i) the offer of vocational training (T1), (ii) the offer of vocational training and matching (T2), (iii) matching (T3), and (iv) controls (C).

Worker expectations about their own prospects are fundamental for understanding job search. We show that at baseline, although workers have relatively accurate beliefs about the earnings distribution if they could progress into jobs in good sectors, they are optimistic about the job offer arrival rate from employers in these sectors. Optimistic beliefs have been documented among job seekers in the United States (Spinnewijn 2015; Mueller, Spinnewijn, and Topa 2021; Potter 2021), Ethiopia (Abebe et al. 2025), South Africa (Banerjee and Sequeira 2023), and India (Kelley et al. 2024). These beliefs are key to understanding how workers react to the match offer.

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

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

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

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

Our null hypothesis is that workers are perfectly informed and rationally infer there to be no information from one or two callbacks about their job prospects. Under this null hypothesis, the expectations and underpinning search behaviors of workers—irrespective of whether they have been vocationally trained earlier—should be unaffected by the match offer.

The alternative is that workers are imperfectly informed. For trained workers, the lower than expected callback rate causes them to revise down their expectations about their own job prospects. Such misattribution can occur because (i) they are not well informed at baseline, and trainees become even more optimistic relative to their true prospects as they complete vocational training; (ii) there are no market substitutes for the matching intervention, so this offer can be a highly salient and unique opportunity for them to find a good job; or (iii) the intervention is implemented by BRAC, a reputable nongovernmental organization (NGO). Under this alternative, match offers generate bad news for the average trained worker. Trained workers without match offers are insulated from this news and begin their job search with the increasingly optimistic beliefs described above.

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

Our first set of results document how these interventions impact worker expectations about their job prospects, a full year after training is completed and/or match offers made. First, comparing workers offered vocational training with controls, trainees revise upward their expectations about the job offer arrival rate and expected earnings conditional on being employed in a study sector. Compared with actual outcomes, their beliefs about the job offer arrival rate become increasingly optimistic, while their beliefs about expected earnings move more in line with the skills premium offered for trained youth. Workers offered only vocational training search more intensively relative to controls and direct their search toward higher-quality firms.

Second, workers offered vocational training and matching also have sustained changes in beliefs about their own prospects a full year after training is completed and/or match offers provided. However, relative to those only offered training, they revise down their expectations about the job offer arrival rate and distribution of earnings conditional on employment in a job in a good sector. This is consistent with those additionally provided match offers becoming discouraged and reacting to the lower than expected callback rate by revising down their beliefs about their job prospects. Such discouragement is reflected in search behavior: relative to those offered only vocational training, those additionally offered matching search less intensively and search over lower-quality firms. Finally, workers offered only matching—relative to controls—do not adjust their expectations, as their callback rate is in line with their prior expectations.

Our second batch of results examines whether the labor market interventions, through experimentally induced changes in skills, expectations, and search behaviors, translate into differences in outcomes up to 5 years after training is completed and/or match offers provided. We find that relative to controls, those offered vocational training (with or without matching) are more likely to be employed, to transition into regular work, to be employed in good sectors, and end up in better jobs and in higher-quality firms. However, contrasting workers offered vocational training with and without the additional offer of matching, we find that those with match offers do significantly worse on labor market outcomes up to 6 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. In terms of sectoral allocation, they work less time in one of the eight good study sectors. Relative to those offered only vocational training, they end up sorting to lower-quality firms and lower-quality jobs, have lower earnings, experience longer unemployment spells, and experience shorter employment spells.

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

To quantify these long-run differences, we construct an index of labor market success combining information on employment in good jobs, earnings, employment spells, and characteristics of jobs and firms workers end up being employed at. This index significantly increases by 0.115σ for those offered vocational training relative to controls. For those additionally offered matching, the index increases by less than half (0.051σ), and the two estimates are different ($p = .001$). In short, because match offers to those offered vocational training cause youth to become discouraged, this undoes half of what is achieved through vocational training alone. This result quantifies the foundational role expectations play in the long-run job search process.

Finally, workers offered only match offers (that confirm their job market prospects) are significantly more likely to enter self-employment. However, on the overall index of labor market success, we find that, in line with earlier meta-analyses (Card, Kluve, and Weber 2017; McKenzie 2017), the impact of match offers is not significantly different from controls.

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

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

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

Second, we build on a nascent experimental literature evaluating training and matching interventions in low-income countries (Beam 2016; Groh et al. 2016; Acevedo et al. 2020; Abebe et al. 2021, 2025; Carranza et al. 2022; Banerjee and Sequeira 2023). We bridge between this work and a recent literature on behavioral job search that shows that job seekers tend to be optimistic about job-finding rates, which delays exit from unemployment (Spinnewijn 2015; Krueger and Mueller 2016; Conlon et al. 2018; Mueller, Spinnewijn, and Topa 2021; Potter 2021).

Our intent was that combining vocational training with match offers would improve long-run outcomes relative to either intervention alone. This did not occur. The reason is that light-touch match offers can backfire if workers misinterpret the lack of callbacks from potentially good employers and become discouraged. This implication stems beyond matching to a broader set of interventions providing information to job seekers (Abebe et al. 2020; Chakravorty et al. 2024).[1]

[1] For example, Abebe et al. (2020) show that attending a job fair with many potential employers leads optimistic job seekers to revise downward their labor market expectations without creating discouragement, so that search effort and labor market

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

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

Section II describes our context, design, and data. Section III describes the evolution of beliefs and search behavior among controls. Section IV presents treatment effects on expectations and search behaviors. Section V examines long-run differences in labor market outcomes. Section VI examines the relative importance of skills, expectations, and search behaviors for long-run outcomes. Section VII concludes by reexamining Alfonsi et al. (2020), discussing external validity, implications, and future work. Additional design details, results, and research ethics are given in the appendix (available online).

II. Context, Design, and Data

A. Context

Our study covers 15 urban labor markets in Uganda, including Kampala. Multiple imperfections characterize the job search process: (i) youth enter labor markets lacking skills demanded by firms, (ii) workers cannot finance

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outcomes increase as a result. In the case of job fairs and other undirected interventions not tailored to the individual, signals are likely to be more informative of the status of the labor market as whole rather than individual job prospects. Hence, there is less scope for workers to misattribute signals as being informative of their own job prospects. Our contribution is to highlight the risks of directed interventions that provide information tailored to the individual, which can be easily misattributed. Our result on discouragement is consistent with Banerjee and Sequeira (2023), who find that providing subsidies for job search leads to discouragement and worse labor market outcomes as workers expand the geographical scope of their job search but fail to find better jobs.

human capital investments after labor market entry, and (iii) firms lack information on worker histories or skills (Abebe et al. 2020; Alfonsi et al. 2020). The additional imperfections we document are that youth hold optimistic beliefs about their job prospects and can misattribute information generated from matching interventions.

We use the Uganda National Household Survey (UNHS) from 2012–13 to describe features of our context. We first derive the share of young people in casual and regular jobs. We classify casual work as jobs in which workers are typically hired on a daily basis, in line with a standard definition of casual jobs being those where neither workers nor firms are obligated to supply/demand labor regularly. Figure A1*A* (figs. A1–A4 are available online) shows that at all ages of young workers rely on casual work. Figure A1*B* shows how skills vary by age. By age 25, fewer than 6% of youth make any investment in skills after labor market entry. Figure A1*C* shows how skills raise the likelihood of being in regular work, yet the majority of skilled youth still do not find regular work. Hence, the labor market fails to clear even for high-skilled youth, and a mass of talent remains underutilized.

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

Workers.—Individuals were recruited into our experiment using an advertised offer to potentially receive 6 months of sector-specific vocational training at one of our partner VTIs. The eligibility criteria targeted disadvantaged youth. The first row of table A1 (tables A1–A14 are available online) shows applicant characteristics: 57% are men, they are age 20, and almost none have vocational training.[2]

Table 1 shows labor market histories at baseline. Employment rates for controls are 40%, with casual work being the most prevalent activity. Unconditionally, average monthly earnings from regular work are $5, corresponding to around 10% of the Ugandan per capita income. Conditional on work, earnings are $13 per month. Hence, these individuals remain unlikely to be able to self-finance vocational training (that costs more than $400).

[2] The eligibility criteria were (i) being aged 18–25, (ii) having completed at least (most) a P7 (S4) level of education (corresponding to 7–11 years), (iii) not being in full-time schooling, and (iv) having a poverty score, based on family size, assets, type of building lived in, village location, fuel used, number of household members attending school, monthly wage, and education of the household head. Applicants were ranked 1–5 on each dimension, and a total score was computed. A geographic-specific threshold score was used to select eligibles. Table A1 shows that the program is well targeted toward disadvantaged youth by comparing our sample with those aged 18–25 in the 2012–13 UNHS data. This remains so when we compare with youth in the UNHS who report being labor market active.

Table 1
Baseline Balance on Labor Market Histories

	Any Work in Past Month (1)	Any Regular Wage Employment in Past Month (2)	Any Self-Employment in Past Month (3)	Any Casual Work in Past Month (4)	Total Regular Earnings in Past Month (USD) (5)	Total Regular Earnings in Past Month (USD) for Regular Employment (6)	F-Test of Joint Significance (7)
Control							
(N = 451)	.401	.120	.038	.296	5.11	13.0	
	(.052)	(.026)	(.017)	(.051)	(1.29)	(2.41)	
Vocational training							
(N = 390)	.389	.149	.034	.253	7.29*	19.1**	.798
	(.032)	(.023)	(.013)	(.029)	(1.26)	(2.80)	
	[.985]	[.185]	[.761]	[.263]	[.062]	[.039]	
Vocational training + matching							
(N = 307)	.360	.149	.050	.205*	5.25	15.1	.772
	(.034)	(.026)	(.015)	(.030)	(1.20)	(3.01)	
	[.694]	[.228]	[.255]	[.065]	[.808]	[.945]	
Matching							
(N = 283)	.367	.127	.057	.251	5.56	15.2	.995
	(.034)	(.025)	(.016)	(.031)	(1.25)	(2.86)	
	[.373]	[.815]	[.211]	[.204]	[.728]	[.883]	

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

* Significant at the 10% level.
** Significant at the 5% level.

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

Firms.—For the matching intervention, to draw a sample of employers offering good jobs, we conducted a census in the 15 urban labor markets selecting firms that (i) operated in the eight study sectors and (ii) had 1–15 employees (plus a firm owner). Our sample comprises 1,281 firms, employing 3,735 workers in total at baseline.[3] Firms are not selected on the basis of having a vacancy, but at baseline, 92% of them report being willing to expand, with 52% stating that they would do so by hiring workers. Firms report being size constrained because they are unable to find skilled workers (67%), trustworthy workers (57%), or unskilled workers (28%).

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

B. Design

Figure 1 shows our oversubscription design. Applicants were first randomly assigned to receive vocational training. Within those assigned to training, a further random assignment took place. The first group was assigned to 6 months of training at one of our partner VTIs and, upon graduation, transitioned into the labor market to search for jobs unassisted (T1). This is the business-as-usual training model, where VTIs are paid to train workers but not to find them jobs. The second group of trained workers were, upon graduation from the VTI, provided light-touch offers to match with firms in our firm-side survey sample (T2). Workers randomized out of the offer of training were also randomly assigned into two groups. At the same time as vocational trainees were

[3] On average these firms have been in operation for almost 7 years, have monthly profits of $217, and have a capital stock valued at $1,209. Among firm owners, 53% are women, and they are on average age 35 and have 11 years of education (far higher than our worker sample).

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

Table 2
Jobs, Search, and Recruitment

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

NOTE.—The data used are 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 includes only workers and firms in the control groups. Casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing, and slashing compounds. Casual work also includes any type of agricultural labor, such as farming, animal rearing, fishing, and agricultural day labor. For casual work, the list of activities indicated is exhaustive. Regular jobs include all other jobs that are not in the list of casual jobs, so the list is not exhaustive. In panel A, the sample includes all workers for the following outcomes: involved in this activity in the past month, self-employed, and number of months involved in the activity in the past year. The remaining outcomes in panel A are conditional on the worker being involved in casual or regular work. Panel B shows the share of workers who have used the corresponding method to look for work in the year before the survey. The list of methods is not exhaustive, as it excludes self-employed individuals who started their firm from scratch. Panels C and D show the share of employees hired through the corresponding method. The top 1% of earnings values are excluded. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 US dollars.

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

The pairwise intent to treat comparisons we focus on are (i) T1 versus C (the offer of vocational training), (ii) T2 versus T1 (match offers to those previously offered training), and (iii) T3 versus C (match offers to those randomized out of training). Although workers were randomly assigned to each treatment arm with their initial application, they were informed about potential matching

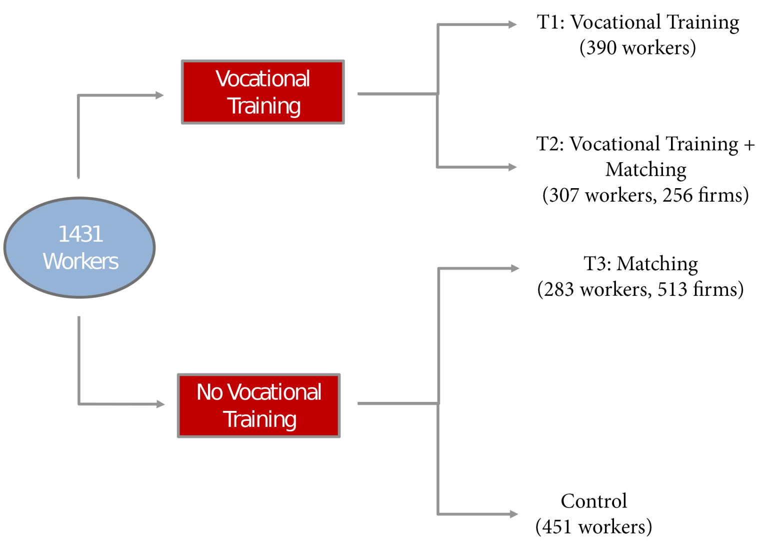

Fig. 1.—Experimental design. The numbers in parentheses refer to the number of eligible applicants originally assigned to each treatment and the number of firms assigned to each treatment.

only after vocational trainees had completed their courses. This ensures that match offers for those randomized into and out of the offer of training take place simultaneously. Those randomized out of the offer of vocational training, however, might have found work before the match offer. A 6-month tracker survey fielded just before match offers being announced sheds light on this: 16% of controls are in some work activity, most remain reliant on casual jobs, and more than 90% remain interested in a matching opportunity.

Vocational training.—The vocational training intervention provides workers 6 months of sector-specific training in one of eight sectors. Our intervention partner, BRAC, covered costs at $470 per trainee. Courses were held Monday to Friday for 6 hours per day; 30% of content was dedicated to theory, and 70% was dedicated to practical work providing sector-specific skills. VTIs signed contracts with BRAC to deliver these standard training courses. They were monitored by regular and unannounced visits by BRAC staff to ensure that workers were present and being trained. For each worker, VTIs were paid half the training fee up front and the remainder when the worker graduated. This staggered timing of payments ensured that workers nearly always completed training conditional on enrollment. Upon graduation, trainees receive a certificate. As documented in Alfonsi et al. (2020), there are high returns to having certifiable skills from reputable VTIs in these urban labor markets.

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

Workers were first asked whether they wanted their details to be passed onto potential employers in our firm-side survey: nearly all agreed (among those offered training and those randomized out of that offer). Firms were presented short lists of workers that were either (i) all vocationally trained or (ii) all unskilled but had demonstrated labor market attachment in the sense they had been willing to undertake 6 months of training. There were a maximum of two workers randomly assigned to firms on each list. In the first case, firms knew what sector the worker had been trained in but not that training had been paid for by BRAC. We presented stylized curriculum vitae of workers to firms (fitting a common template). The firm could call back for interview neither, one, or both workers (and remained free to recruit from outside the evaluation sample). The median worker was matched to a single firm.

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

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

The appendix details how worker-firm match offers were implemented, including the exact (fixed) scripts used to communicate the process to workers and firms. This wording ensured that ex ante workers were aware that their details were being handed over to a few firms. No offer of employment was given—BRAC officers made clear that they were acting only by providing curriculum vitae to firms, and they were not searching for jobs on the worker's behalf. Workers were not told the likelihood of being called back or ex post any reason why firms did not call them back. Firms were not provided worker contact details—they had to come through BRAC officers, so we can rule out our results being due to firms recalling workers or storable offers.

[5] Meta-analyses of job assistance programs (Card, Kluve, and Weber 2017; McKenzie 2017) emphasize that worker-firm matches can be either directed (as in our match offer treatments that are directed toward firms in sectors where workers were originally offered training) or undirected, such as through the use of job fairs (Beam 2016; Abebe et al. 2020).

[6] To further understand the salience of the matching intervention to workers, we use data from controls on the frequency of job applications made. We collected this only at the final follow-up, 6 years after baseline. The average number of job applications made in the preceding year is 4.7, rising to 8.1 applications among those who were nonemployed for that entire period. In short, job seekers make fewer than one application per month.

The matching intervention involves only BRAC officers, with VTIs playing no role. As VTIs do not normally match workers to firms, there are no preexisting ties between VTIs and firms.

The entire match offer process—from when workers are first informed of the possibility to when firms might call back—is typically around 2 weeks. We measure short-run search behavior a year after the match offers are first announced, so impacts are not driven by any substitution of search effort between workers and BRAC.

C. Data

Timeline and surveys.—Figure 2 shows the study timeline. The baseline worker survey took place from June to September 2012, just after applications for vocational training were received. This is when initial beliefs about labor market prospects are measured. Among those taking up the training offer, they were next surveyed at the end of their 6-month course. We use this to measure posterior beliefs about labor market prospects just as workers complete training but before they have knowledge of match offers. Among those randomized out of training, we next surveyed them just as trainees were graduating and measure the opportunity cost of attending the training courses. These two rounds of data collection are under phase 1 of the timeline.[7]

For workers involved in matching treatments, we record key outcomes (callbacks, job offers, offer refusals, etc.). Workers were tracked 24, 36, 48, and 68 months after baseline (12, 24, 36, and 56 months after the end of training/matching)—corresponding to phases 2 and 3 of the timeline.

This allows us—perhaps uniquely—to track a panel of young labor market entrants over 6 years, measuring their short-run expectations about job offer arrival rates and expected earnings in good jobs, linking these to underlying dimensions of search behavior, such as search intensity and directed search, and mapping expectations and search behaviors onto long-run labor market outcomes related to employment, earnings, hours, wages, bargaining, spells, and actual job and firm characteristics.

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

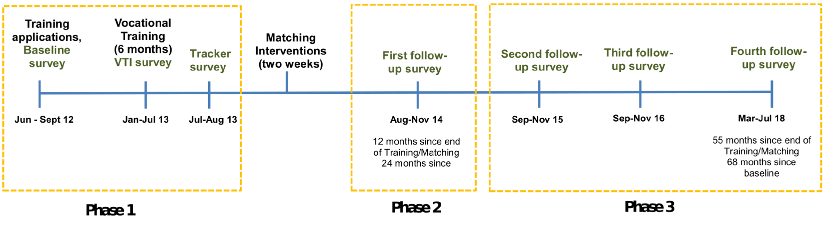

FIG. 2.—Study timeline. The timeline highlights the relevant dates for the main batch of workers and worker surveys. A second, smaller round of applications and baseline surveys (17% of the overall sample) was conducted in May and June 2013. The majority of trainees from the first round of applicants 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 toward the end of the training period while trainees were still enrolled at the VTIs. Workers from the second round of applicants were not included in the tracker survey. There were two rounds of untrained, matching, and vocational training plus matching interventions, in line with the two batches of first-round trainees from the VTIs. The first round of the untrained, matching, and vocational training plus matching interventions took place in August–September 2013 (with each matching intervention taking around 2 weeks from start to finish for a given worker). The second round took place in December 2013–February 2014.

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

On compliance with the offer of training, 68% of individuals take up the offer, with more than 95% of them completing training conditional on enrollment. Table A3 shows correlates of training completion: 65% of individuals comply with vocational training; this is no different between those offered only vocational training and those later also offered matching. This is expected because match offers are only announced later, so compliance with training is independent of the expected returns from match offers; women and the more educated are less likely to comply; and the correlates of compliance do not differ between those offered only vocational training and those later also offered matching.[8]

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

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

III. Expectations

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

A. Expectations and Reality among Controls

Expected job offer arrival rate.—The first margin of beliefs relevant for job search is the expected job offer arrival rate from firms in good sectors—defined to be the eight sectors in which we offered training. At baseline we asked controls their expected probability of finding a job in our study sectors in the next month, 6 months, and year (where job offer acceptance rates are more than 90%). The distribution of beliefs is shown in the three left-hand boxplots in figure 3*A*. Reassuringly, these right shift as we increase the time horizon. The median belief among controls is that they have a 20% chance of receiving a job offer in good sectors within a month, a 40% chance within six months, and a 60% chance within a year.[9]

We assess the accuracy of beliefs by comparing them with actual youth employment rates in regular wage jobs. As figure A1*C* shows using the UNHS data, for unskilled youth, employment rates in regular jobs are 20%, rise by 10% for workers 2 years older, and plateau thereafter. This is far lower than the baseline belief held by the median control worker of a 60% job offer arrival rate from firms in good sectors in the next year.[10]

[9] The expectation questions were introduced to respondents as follows: "For some of these questions I will ask you to estimate the possibility out of 10 that some events would occur. This means that on a scale of 0–10, 0 will mean surely not possible and 10 will mean it will definitely happen. Let's practice this to be sure you have the idea. On a scale of 0–10, what do you think is the possibility that it will rain tomorrow? On a scale of 0–10, what do you think is the possibility that it will rain at any time in the next year? The score for the possibility of 'rain tomorrow' should be lower than the score for 'in the next year.' If it is not, review the 0–10-point scale until it is clear the respondent understands before proceeding." As probabilities were elicited on a 0–10 scale, a concern is that workers might not have been able to express probabilities for rare events. To check for this, we note that at baseline, 22% of youth report having a zero probability of finding a job in the next month, and 57% report a probability less than 20%. Reassuringly, individuals report higher probabilities of finding a job over longer horizons—only 11% and 9% report a zero probability of finding a job in the next 6 and 12 months, respectively.

[10] In making a comparison to the UNHS, we are of course contrasting the stock of young workers in the economy with regular jobs to the flow probability our evaluation sample workers express about entry into regular jobs. The economy-wide flow of young workers into regular jobs might be even lower than the stock measured in the UNHS or potentially higher if the rate of job separations is also very high.

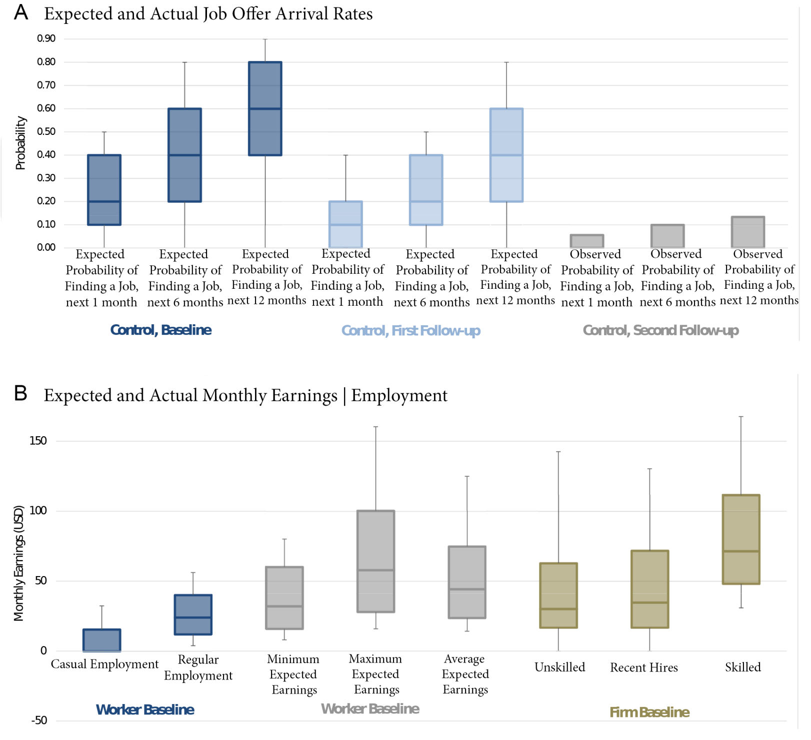

FIG. 3.—Expectations among controls. *A* shows the distribution of expected probabilities of finding a job at various horizons at baseline and first follow-up. The right-hand set of bars are for the actual probabilities of finding employment in these good sectors among control workers at second follow-up. The sample used to construct *A* includes only individuals who were not employed in any of the eight study sectors at first follow-up. *B* shows boxplots 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 above the midpoint of the minimum and maximum and then fitting a triangular distribution. The third sample—the firm baseline—is taken from the 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 plus matching and matching treatments were later matched to. We consider the actual distribution of earnings among unskilled, recently hired, and skilled workers in these firms.

The three middle boxplots in figure 3*A* show the distribution of revised expectations about job offer arrival rates at first follow-up. Beliefs are revised downward: the median expectation among controls is that they have a 10% chance of receiving a job offer from a firm in a good sector within a month, a 20% chance within six months, and a 40% chance within a year. Controls therefore become gradually more realistic as they search.

To see how quickly expectations converge to reality, we calculate the actual likelihood of finding a good job over those horizons using our second survey, fielded a year later. As shown in the three right-hand boxplots in figure 3*A*, these likelihoods are still far lower than worker expectations about the job arrival rate, with the divergence increasing with the time horizon considered: only 7% of workers actually find a job within a month, 10% within six months, and 13% within a year.[11]

These results complement a growing literature on the persistence of optimistic beliefs (Bénabou and Tirole 2002; Compte and Postlewaite 2004; Van den Steen 2004). We add to evidence that displaced workers are optimistic about job offer arrival rates both in the United States (Spinnewijn 2015; Mueller, Spinnewijn, and Topa 2021; Potter 2021; Mueller and Spinnewijn 2023) and in lower-income labor markets (Abebe et al. 2020; Banerjee and Sequeira 2023; Kelley et al. 2024).

Expected earnings.—The second margin of beliefs is worker's expected earnings conditional on employment in a job in a good sector (Wright 1986; Burdett and Vishwanath 1988). The two left-hand boxplots in figure 3*B* show the entire distribution of actual monthly earnings of controls at baseline, from casual and regular work. As expected, the distribution of earnings from regular employment is right shifted relative to earnings in casual employment (where the majority of workers report being unpaid).

To measure worker's expected earnings if they were employed in good sectors, we elicit beliefs for the worker's most preferred sector (for those in T1 and T2, this typically corresponds to the sector in which they receive training). These beliefs are derived for all controls, irrespective of their search effort or employment status and hence are not driven by compositional changes.[12]

[11] The correlation between the expected probability of finding a job in the next 12 months and whether the worker engages in job search is low: for the control group, it is .120 at baseline. If we regress the two against each other at baseline, we obtain a partial coefficient of .021 ($p = .07$). Of course, this cross-sectional correlation is likely attenuated because of omitted-variable bias. For example, worker ability can be positively correlated with job expectations and negatively correlated with search.

[12] Only individuals who report a zero probability of finding a job in their most preferred good sector in the next 12 months are excluded from the sample. For employed workers (who might already be working in their most preferred study sector), we ask them to consider a scenario where their firm shut down and they were to transition to a job in their most preferred study sector. These beliefs are elicited at baseline, pretreatment but after individuals have been recruited into the evaluation sample

We asked individuals their minimum and maximum expected earnings if offered a job in their preferred study sector. We asked them the likelihood that their earnings would lie above the midpoint of the two and fitted a triangular distribution to construct their expected earnings. The three middle boxplots in figure 3*B* show the distribution of minimum, maximum, and average expected earnings. Average expected earnings are higher than actual earnings from the kinds of regular work that controls engage in at baseline—indeed, median earnings in actual regular work lie below the 25th percentile of expected average earnings if the worker could move into their most preferred good job. Hence, these youth recognize that jobs in our study sectors are better than the kinds of work they have previously experienced.[13]

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

Search intensity.—How do these expectations translate into the intensity of job search relative to unemployment spells? We define individuals as unemployed if they are not involved in any work activity. Those engaged in casual work or unpaid work in family businesses are considered employed. Figure A2*A* shows that over the 4 years from first follow-up, the share of youth unemployed at some point in the year falls from 90% to 60%. However, the share reporting looking for a job never reaches 60%. Figure A2*B* shows the intensive margin of search intensity: in the year before baseline, workers spend around 9 months unemployed yet spend less than 1 month looking for work. While the days spent searching rise over time, they never get close to matching the time actually spent unemployed. This apparent misallocation of time can be due to workers either being discouraged—with their poor labor market outcomes being a self-fulfilling prophecy—or as a result of them being optimistic about the returns to search effort. The results above showed that controls have reasonably accurate beliefs about the wage offer distribution in good firms. In contrast, optimism about the job offer arrival rate from good firms can reduce search intensity and slow exit out of nonemployment. This is key to our analysis because this margin of belief can be directly impacted by the match offer intervention.

through the oversubscription design. They might then reflect an element of expecting to be trained.

[13] The questions are as follows: “With your current skill set, what is the possibility out of 10 that you could get a job in <occupation> in the <time period>?” “With your current skill set, what do you think is the minimum/maximum monthly amount that you could earn in <occupation>?” “What do you think is the possibility out of 10 that you could receive <(max – min)/2> monthly with your current skill set?”

B. How Vocational Training Changes Expectations

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

Expected job offer arrival rate.—The left-hand set of bars in figure 4*A* shows beliefs of controls at baseline about the arrival of job offers from good sectors for each time horizon. The middle set of bars shows the same beliefs for controls 6 months later, when match offers are about to be announced. As described earlier, although controls hold optimistic beliefs at baseline, they gradually become more realistic as they search. The right-hand set of bars shows that on the eve of match offers being announced, beliefs of vocational trainees have moved sharply in the opposite direction to controls: they revise upward their belief about the job offer arrival rate at each horizon, with the gap in beliefs between trainees and controls opening up considerably at 6- and 12-month horizons. Over those horizons, there is no overlap in the interquartile range of beliefs among the two groups. At graduation, the median trainee believes that they will receive a job offer in their most preferred good sector with a probability of .9 in the next year; 25% of trainees believe this will occur with a probability of 1.[15]

To test differences in beliefs across treatment arms over time, column 1 in table 3 shows the expected job offer arrival rate, pooling those assigned to vocational training (T1, T2) and those assigned out of training (T3, C). Rows R1 and R2 show baseline expectations, and rows R3 and R4 show expectations on the eve of match offers being announced. At the bottom of the table, we report p-values on tests of equality of expectations, between groups at the same moment in time (R1 = R2, R3 = R4) and within workers in a given treatment over time (R1 = R3, R2 = R4). Column 1 shows that beliefs about the job offer arrival rate (i) significantly rise among those assigned to vocational training (R1 = R3) and (ii) significantly fall among those randomized out of vocational training (R2 = R4). On the eve of match offers being announced, beliefs about job offer arrival rates thus significantly differ between workers offered training and those not offered training (R3 = R4).

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

[15] This upward revision in beliefs is in line with trainees' reported satisfaction with their course: 76% were extremely happy/very happy with the experience, 86% were extremely happy/very happy with the skills gained, 96% reported skills acquisition as being better than or as expected, and 56% reported that 6 months of training was sufficient for them to learn the desired skills.

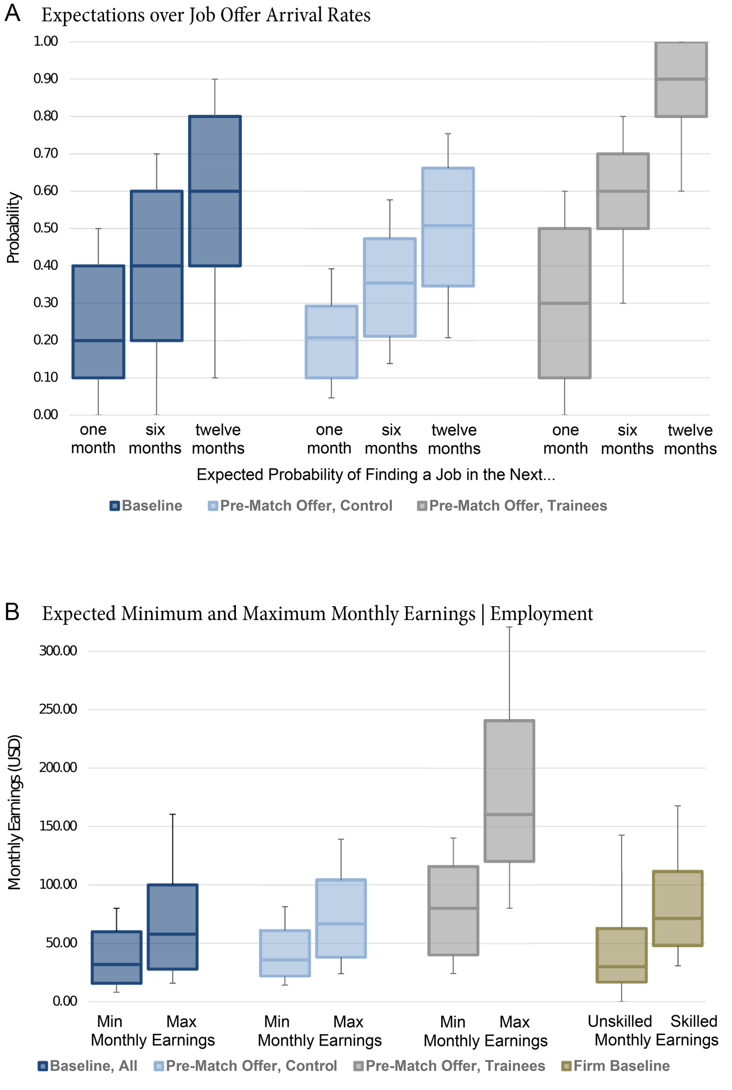

FIG. 4.—Evolution of expectations until match offers are announced. The data used are from baseline VTI surveys conducted toward the end of the training period while trainees were still enrolled at the VTIs, and we extrapolate back from the first worker follow-up survey assuming a linear evolution of beliefs to what would have been beliefs among controls at the same time the VTI survey was being fielded. *A* shows boxplots for the expected probability of finding a job in one of the eight study sectors in the next 1, 6, and 12 months. *B* shows boxplots for the minimum and maximum expected monthly earnings conditional on employment in the workers' preferred sector among the eight study sectors. The plot shows 10th, 25th, 50th, 75th, and 90th percentiles of the distribution.

Table 3
Evolution of Expectations

	Job Offer Arrival Rate (Expected Probability of Finding a Job in the Next Year, 0–10 Scale)	Expected Earnings Conditional on Employment (USD)	
		Minimum	Maximum
	(1)	(2)	(3)
At baseline:			
R1: assigned to vocational training (T1, T2)	5.59	40.0	71.5
	(2.83)	(35.0)	(58.6)
R2: not assigned to vocational training (C, T3)	5.71	42.1	74.6
	(2.90)	(36.8)	(62.1)
On eve of announcement of matching:			
R3: assigned to vocational training (T1, T2)	8.32	82.8	209
	(1.61)	(55.4)	(250)
R4: not assigned to vocational training (C)	5.04	43.0	75.5
	(2.06)	(26.7)	(45.0)
p-value on tests of equality across rows:			
R1 = R2	[.435]	[.307]	[.363]
R1 = R3	[.000]	[.000]	[.000]
R2 = R4	[.000]	[.672]	[.780]
R3 = R4	[.000]	[.000]	[.000]

NOTE.—Shown are means with standard deviations in parentheses. The data used are from baseline VTI surveys conducted toward the end of the training period while trainees were still enrolled at the VTIs, 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 the VTI survey was being fielded. At the bottom of the table, we report *p*-values on the tests of equality of means: (i) between individuals assigned and not assigned to vocational training at baseline, (ii) between individuals assigned to vocational training at baseline and on the eve of matching being announced, (iii) between individuals not assigned to vocational training at baseline and on the eve of matching being announced, and (iv) between individuals assigned and not assigned to vocational training at the eve of matching being announced.

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

Expected earnings.—We next consider the evolution of expectations about the earnings distribution in our study sectors. Figure 4*B* shows the distribution of beliefs youth hold about the minimum and maximum expected earnings

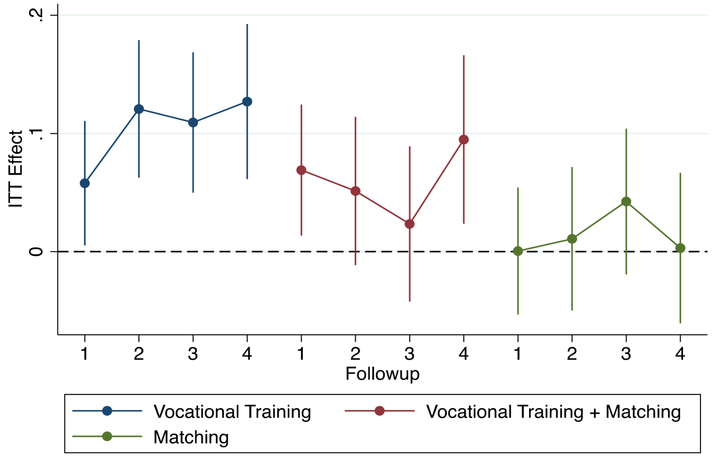

Fig. 5.—Dynamics: labor market index. This graph shows coefficients and 95% confidence intervals for the ITT effect on the labor market index at each follow-up. All coefficients reported in each panel are estimated from the same dynamic treatment effects regression, where the treatment indicators are interacted with dummies for each survey wave, with robust standard errors. All regressions include strata dummies, survey wave dummies, and a dummy for the implementation round. The labor market index takes (i) all components of the employment index, (ii) total earnings, (iii) the length of the last employment spell, and (iv) all components of the indexes of realized jobs and realized firms. The index is constructed following Anderson's (2008) approach.

from being employed in their most preferred sector among: (i) all workers at baseline, (ii) controls on the eve of match offers being announced, and (iii) vocational trainees on the eve of match offers being announced. Comparing the two left-hand sets of bars, we see that for controls, beliefs about the earnings distribution hardly change. This is as expected—controls have relatively accurate beliefs at baseline, and little new information is gained over 6 months of job search.

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

Columns 2 and 3 in table 3 formally test differences in these distributions. We see that (i) at baseline there are no significant differences in expected minimum or maximum earnings across workers assigned to vocational training or not (R1 = R2), (ii) there are no significant changes in expected minimum or maximum earnings over time among workers randomized out of vocational training (R2 = R4), (iii) there are significant changes in expected minimum and maximum earnings over time among workers assigned to vocational training (R1 = R3), and (iv) hence on the eve of match offers being made, there is a significant bifurcation of beliefs between those offered vocational training and those randomized out of it (R3 = R4).

C. Callbacks and Their Determinants

For workers offered matching, the key outcome is whether they receive a callback (i.e., an invitation to interview with the firm owner). When match offers are announced, workers' expected job offer arrival in the next month is our best proxy of what workers might expect callback rates from the match intervention to be. This is a margin of belief over which vocational trainees are increasingly optimistic, while those not assigned to training are slowly becoming more realistic. As figure 4*A* shows, on the eve of match offers being announced, the median trained worker believed that there was a 30% chance they would receive a job offer from a good firm in the next month. In actuality, in the 2 weeks from match offers being announced and firms responding, only 16% of skilled workers in T2 received a callback. Among controls, the median worker had a prior belief of there being a 20% chance they would receive a job offer from a firm in a good sector in the next month. For unskilled workers in T3, 18% actually receive a callback, confirming their prior expectations.

To understand how workers might react, we need to consider the actual correlates of callbacks. 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 callbacks to compliers to the offer of vocational training, and columns 3 and 4 present analogous specifications for callbacks to those randomized out of training. The specifications control for (i) worker and firm characteristics and (ii) worker characteristics and firm fixed effects. At the bottom of each column, we report p-values on the joint significance of worker and firm covariates.

Two results emerge. First, worker characteristics do not predict callbacks. The p-values on the joint test of significance of worker covariates vary from .450 to .631 across specifications. This is unsurprising: firms are presented with two workers that are, by construction, similar on observables. Hence, the design of the matching intervention almost fully removes the possibility that worker characteristics determine callbacks.

Second, callbacks are predicted by firm characteristics. In particular, trained workers are more likely to be called back if they are matched to firms that would like to expand and where owners report being constrained by an inability to find trustworthy workers. Hence, the limiting factor on worker-firm matches actually taking place is firms' willingness to meet workers rather than reservation prestige driving worker refusals to meet firms (Groh et al. 2016).

Reaction to callbacks.—We used fixed scripts to communicate match offers to workers. They were aware that their details were being handed to only a few firms. They were not given ex ante information on the expected callback rate or ex post reasons why they were not called back.

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

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

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

Under this alternative, a key distinction is that trained workers with match offers receive bad news on their own job prospects, just at a time when they are transitioning into the labor market and meeting potential employers. Trained workers without match offers are insulated from this news, so they begin their job search with the increasingly optimistic beliefs shown in figure 4.

For workers randomized out of the offer of training, their prior expectations are in line with callback rates (20% vs. 18%). Hence, even under the alternative hypothesis, there is no reason why they should alter expectations and search behavior. However, because callbacks generated in the experiment are not the kind of signal they receive during regular job search, the low rate of callbacks can provide credible confirmation to them of their true labor market prospects. How they respond to this is an empirical question, which we now turn to.

IV. Skills, Expectations, and Search Behaviors

A. Empirical Method

We analyze how the offer of vocational training with and without match offers impacts skills, expectations, and search behaviors. Expectations and search behaviors are measured at first follow-up, 24 months after baseline, and a year after trainees have graduated and any callbacks have been made (so during phase 2 of the timeline in fig. 3). For worker i assigned to treatment group j in strata s, we estimate intention-to-treat (ITT) effects using the following specification:

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

where y_{is1} is the search behavior of interest at first follow-up ($t = 1$), T_{ij} is a dummy for the treatment arm that worker i is assigned to, y_{i0} is the baseline value of that outcome (where available), and λ_s are strata fixed effects. All regressions control for the implementation round and dummies for month of interview. We present robust standard errors, as randomization is at the individual level, and report p-values adjusted for randomization inference (Young 2019) and multiple hypothesis testing to account for the three treatment effects estimated in (eq. [1]), using the step-down procedure of Romano and Wolf (2016). The ITT coefficients of interest are (i) β_1 (T1 vs. C; the impact of the offer of training), (ii) $\beta_2 - \beta_1$ (T2 vs. T1; the differential impact of matching on those offered training relative to those offered only training), and (iii) β_3 (T3 vs. C; the impact of match offers on those randomized out of training).

B. Preliminaries

Sector-specific skills.—Our earlier work showed how the offer of vocational training translates into human capital accumulation (Alfonsi et al. 2020). Here we briefly reiterate those findings and extend them to show impacts on those offered matching. We measure individual skills using a sector-specific skills test we developed in conjunction with skills assessors and modulators of occupational tests in Uganda. The test was conducted on all workers (including controls) at second and third follow-ups, with no differential attrition by treatment into the test. The main results (reported in table A6) are that (i) workers offered vocational training significantly increase their skills by 21% (or 0.29σ) and (ii) skills accumulation increases by 28% over controls (or 0.37σ) among those that take up training. Table A6 further shows that (i) workers offered training and matching have no skills accumulation different from those offered only training and (ii) there are no differences in skills between those with and without match offers among those randomized out of training. As exposure to match offers does not change skills accumulation, when we later compare long-run labor market outcomes between trainees with and without match offers, those results do not reflect skills differences between treatment arms.

Other dimensions of human capital.—Table A7 shows that offers of vocational training or matching do not impact other dimensions of human capital or worker traits: (i) among youth offered training, there are no differences in the big five personality traits, cognitive ability, and other psychological traits between those with and without matching; (ii) among those randomized out of training, there are also no differences in these outcomes between those with and without matching. This helps rule out our findings on long-run outcomes are mediated through these margins. We later exploit the time invariance of these traits to probe the external validity of our findings if they were to be extended to alternative samples of job seekers.[16]

C. Expectations

We examine how the interventions impact expectations a year later. We do so irrespective of worker's employment status, ensuring that results are not driven by composition effects. Table 4 shows these results. Starting with beliefs about the job offer arrival rate, column 1 shows that a full year after training is completed and workers have been searching for jobs, those offered training retain upward-revised beliefs about this margin (by 1.84 on a 0–10 scale). Columns 2–4 show treatment effects on expected earnings if workers were able to transition into their most preferred study sector job. Among those offered training, their minimum expected earnings from such wage employment are significantly revised upward, their maximum expected earnings are revised upward by a greater extent, and their expected earnings shift forward by \$25.4 per month, corresponding to a 44% rise over controls. Column 5 shows that there is no overall change in the dispersion of expectations of average earnings.

The second row shows impacts on the expectations of those offered vocational training but who were, a year earlier, additionally provided match offers. We report the p-values on the equality of treatment effects on those offered training with and without matching. Workers additionally offered matching significantly revise down their beliefs about the job offer arrival rate in good sectors, despite being as skilled as those without match offers ($p = .082$). They also have lower expected earnings from wage employment in these

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

Table 4
Expectations about Own Job Prospects

	Job Offer Arrival Rate (Expected Probability of Finding a Job in the Next Year, 0–10 scale) (1)	Expected Earnings Conditional on Employment (USD)			
		Minimum (2)	Maximum (3)	Mean (4)	Coefficient of Variation (5)
Vocational training	1.84***	17.7***	31.8***	25.4***	−.002
	(.205)	(3.06)	(4.85)	(4.37)	(.005)
	{.000, .001}	{.000, .001}	{.000, .001}	{.000, .001}	{.652, 645}
Vocational training + matching	1.45***	12.0***	23.6***	17.9***	.009
	(.217)	(3.28)	(5.37)	(4.67)	(.006)
	{.000, .001}	{.000, .002}	{.000, .001}	{.001, .001}	{.095, .104}
Matching	.242	3.21	6.04	3.47	−.000
	(.216)	(3.05)	(4.97)	(4.44)	(.007)
	{.276, .270}	{.317, .276}	{.248, .229}	{.462, .435}	{.993, .990}
p-value: vocational training = vocational training + matching	[.082]	[.095]	[.129]	[.105]	[.036]
Mean in control group	4.19	42.9	72.5	57.8	.107
Number of observations	1,171	952	946	801	797

NOTE.—Shown are ordinary least squares regression coefficients with robust standard errors in parentheses. The data used are 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 inference and Romano-Wolf adjusted p-values are given in braces. Randomization inference p-values are computed following Young (2019), and p-values adjusted for multiple testing within the same regression (i.e., for testing multiple treatments) are computed using the Romano and Wolf (2016) step-down procedure. These are both reported in braces. Minimum, maximum, mean, and coefficient of variation of expected monthly earnings in cols. 2–5 refer to the workers' expected earnings in their preferred sector among the eight study sectors. In cols. 4 and 5, we assume a triangular distribution to calculate average and coefficient of variation of expected monthly earnings. Individuals who report a probability of finding a job in the next 12 months equal to zero are excluded from the sample in cols. 2–5. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 US dollars. We report p-values on the tests of equality of treatment effects between vocational training and vocational training plus matching in brackets.
*** Significant at the 1% level.

good sectors—this difference is most pronounced at the minimum expected earnings ($p = .095$). Workers additionally offered matching also hold significantly less precise beliefs about earnings relative to those offered only vocational training ($p = .036$).

The evidence suggests that youth offered training and matching are discouraged relative to youth offered only training, as measured by these expectations about their own prospects. This is in line with the alternative hypothesis that low callback rates from match offers are misinterpreted as bad news for them relative to their prior expectation at the time they completed training.

This is in contrast to those offered only matching. The third row of table 4 shows ITT estimates on the expectations of this group (relative to controls). Their beliefs about the job offer arrival rate and expected earnings are unaffected. This is in line with the rate of callbacks among this group being in line with their prior expectations.

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

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

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

D. Search Behaviors

1. Search Intensity

Expectations closely link to search behaviors. For example, an explanation for why those offered training revise up their beliefs about the job offer arrival

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

rate is that their expected returns to search have increased. If so, this should map onto changes in search intensity. However, whether greater optimism on the job offer arrival rate leads to more or less search intensity is a priori ambiguous because of countervailing forces.[18] While these issues have been explored among US job seekers (Spinnewijn 2015; Faberman and Kudlyak 2019; Mueller, Spinnewijn, and Topa 2021), we provide among the first evidence for job seekers in a low-income country.

We first consider the extensive margin of search. The result in column 1 of table 5 shows that workers offered vocational training are, relative to controls, significantly more likely to actively search for work. These workers increase the likelihood of searching by 17.5 percentage points, a 36% increase over controls. On the intensive margin, vocational trainees report spending no more days searching for work (consistent with them experiencing shorter unemployment spells, as we later document), but they become significantly more geographically mobile in their search (col. 3). Those offered training are also significantly more likely to report using direct walk-ins to firms (with no crowding out of their reliance on informal information from friends and family). The magnitude of this effect corresponds to a 63% increase in the use of this search channel relative to controls.

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

We combine all of these margins into an index using the approach of Anderson (2008). This uses the data covariance matrix to construct a weighted sum of indicators, giving less weight to items more correlated with each other. The index is standardized to have a mean of 0 and a variance of 1 in the control group at baseline, so estimates are interpretable as effect sizes. Column 6 shows that this index of search behaviors rises significantly for those offered vocational training by 0.089σ. Workers additionally offered matching have more muted responses on these dimensions of search: their index rises by 0.019σ,

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

Table 5
Search Intensity

	Has Actively Looked for a Job in Past Year (1)	Number of Days Has Actively Looked for a Job in Past Year (2)	Has Attempted to Migrate to Find a Job (3)	Main Channel through Which Looked for a Job Is through Family Members/Friends (4)	Main Channel through Which Looked for a Job Is by Walking into Firms and Asking for a Job (5)	Search Index (6)
Vocational training	.175***	.617	.084**	.053	.088***	.089**
	(.036)	(6.04)	(.033)	(.033)	(.028)	(.042)
	{.000, .001}	{.916, .924}	{.015, .010}	{.139, .114}	{.001, .001}	{.026, .036}
Vocational training + matching	.097**	−.713	.060*	−.005	.056*	.019
	(.040)	(6.70)	(.036)	(.036)	(.030)	(.046)
	{.012, .015}	{.908, .898}	{.092, .102}	{.893, .889}	{.074, .061}	{.651, .656}
Matching	−.036	−11.2*	−.036	−.000	−.004	−.003
	(.041)	(6.44)	(.033)	(.036)	(.028)	(.041)
	{.383, .399}	{.072, .074}	{.285, .254}	{.999, .998}	{.886, .895}	{.954, .930}
p-value: vocational training = vocational training + matching	[.053]	[.845]	[.523]	[.125]	[.338]	[.146]
Mean in control group	.490	41.7	.217	.270	.139	−.032
Number of observations	1,231	1,211	1,231	1,231	1,231	1,231

Note.—Shown are ordinary least squares regression coefficients with robust standard errors in parentheses. The data used are 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 inference and Romano-Wolf adjusted p-values are given in braces. Randomization inference p-values are computed following Young (2019), and p-values adjusted for multiple testing within the same regression (i.e., for testing multiple treatments) are computed using the Romano and Wolf (2016) step-down procedure. These are both reported in braces. The variables in cols. 2–5 are set equal to 0 if the worker did not actively look for a job in the past year. Column 6 combines all margins of search intensity and channels from cols. 1–5 into a single index following Anderson's (2008) approach. We report p-values on the tests of equality of treatment effects between vocational training and vocational training plus matching in brackets.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

and this is not different from zero. Hence, the complementarity between search effort and vocational training is weaker for those additionally offered matching. As both groups have the same skills, the lower returns to search are because of discouragement among youth additionally offered matching. The discouragement effect on search is concentrated on one margin: the extensive margin of search intensity ($p = .053$).

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

2. *Directed Search*

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

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

To establish holistic measures of directed search, we asked workers about characteristics of the ideal firm and ideal job they were searching for. We construct the ideal firm index so that higher values correspond to more productive firms because they (i) have more employees, (ii) are formally registered, (iii) provide training, and (iv) provide other material benefits to employees. The index is scaled so that treatment effects are interpreted as effect sizes. The treatment effects on the ideal firm index are shown in column 2 of table 6: workers offered vocational training significantly change the kinds of firm they direct their search toward. Their ideal firm index rises by 0.103σ (a result robust to p-value adjustments). Table A9 shows the firm characteristics driving this: these workers search for firms that can provide training and other material benefits.

Table 6
Desired Sorting and Directed Search

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

Note.—Shown are ordinary least squares regression coefficients with robust standard errors in parentheses. The data used are 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 inference and Romano-Wolf adjusted p-values are given in braces. Randomization inference p-values are computed following Young (2019), and p-values adjusted for multiple testing within the same regression (i.e., for testing multiple treatments) are computed using the Romano and Wolf (2016) step-down procedure. These are both reported in braces. In col. 2 the ideal firm searched for index has the components in cols. 1–5 of table A9. In col. 3 the ideal job searched for index has the components in cols. 1–5 of table A10. All indexes are constructed following Anderson's (2008) approach. We report p-values on the tests of equality of treatment effects between vocational training and vocational training plus matching in brackets.
*** Significant at the 1% level.

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

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

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

3. Credit

In the appendix, we examine a final dimension of search behavior, building on the idea that labor and credit markets are interlinked. The results in table A11 show that workers offered only matching are significantly more likely to borrow than controls. They do so not to finance job search but rather to finance business expenditures, as in starting up in self-employment. The rate of borrowing for self-employment is double that of controls, and the average loan size among this treated group is \$32 (so far below the \$400 value of vocational training offered). This suggests that the lack of callbacks from the BRAC-implemented matching intervention serves to concretize and crystallize unskilled workers' low expectations of finding a wage job of the type VTIs prepare individuals for. As we assess labor market outcomes below, we can examine whether these intentions—measured a year after matching is offered—actually translate into higher rates of self-employment in the long run.[21]

V. Long-Run Outcomes

The 6-year study period allows us to map out how offers of training and matching translate into long-run labor market outcomes. We do so using outcomes measured during phase 3 of the timeline in figure 3 (so 36–55 months after workers graduate and/or are given match offers). We estimate the following ITT specification for worker i assigned to treatment group j in strata s in survey wave t:

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

where y_{ist} is the labor market outcome of interest in survey wave $t = 2, 3, 4$; ϑ_t is a survey wave fixed effect; and all other controls are as previously described. We use robust standard errors as randomization at the worker level and report p-values adjusted for randomization inference and multiple hypothesis testing as before. We later summarize outcomes in an index of overall labor market success. For this index, we show dynamic impacts by survey wave.

[20] We construct the ideal job index so that higher values correspond to jobs higher up the job ladder because they (i) entail supervising others, (ii) have a high social status associated with them, (iii) enable workers to learn new job-specific skills, (iv) entail working with others (as opposed to working alone), or (v) have a flexible schedule. The index is scaled so that treatment effects are interpreted as effect sizes.

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

A. First Job

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

The remaining columns show margins along which first jobs differ between vocational trainees with and without match offers. Those offered only vocational training are significantly more likely to have a formal contract ($p = .022$), and their monthly earnings are significantly higher despite the two groups of worker having identical sector-specific skills ($p = .001$). This is precisely in line with the findings on directed search where these groups of worker diverged in the quality of firms they directed the search toward. For those offered only matching, we see no short-run difference from controls in the timing of their first job, whether it is in a good sector or with a formal firm, or earnings.

B. Employment

Table 8 uses specification (2) to establish long-run impacts on employment and transitions into regular work. Mirroring results in Alfonsi et al. (2020), (i) those offered vocational training are significantly more likely to work, with employment rates rising by 9.4 percentage points or 15% over the long-run average for controls (col. 1); (ii) this is driven by a transition toward regular employment, both on the extensive margin where regular employment rates rise by 11.3 percentage points or 22% (col. 4) and on the intensive margin where these individuals spend 23% more months of the year in regular work (col. 4). In terms of sectoral allocation, they double the months they work in a study sector that offers good jobs (col. 5).

We summarize employment effects by combining outcomes from columns 3–5 into an index, using the Anderson (2008) approach and normalizing the index to zero for controls at baseline so impacts can be interpreted as effect sizes. This index outcome is given in column 6. Relative to controls, for workers offered vocational training, the employment index rises significantly by 0.347σ. Strikingly, in the second row, we see that workers offered vocational training but also offered matching up to 5 years earlier have a significantly

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

Table 7
First Jobs

	Months between Intervention and First Job (1)	First Job in One of Eight Good Sectors (2)	Formal Contract in First Job (3)	Monthly Earnings in First Job (4)
Vocational training	−1.74***	.227***	.059*	8.32**
	(.605)	(.039)	(.034)	(3.88)
	{.004, .006}	{.000, .001}	{.089, .193}	{.036, .028}
Vocational training + matching	−1.61**	.222***	−.020	−4.88
	(.696)	(.044)	(.033)	(3.99)
	{.022, .017}	{.000, .001}	{.543, .553}	{.224, .231}
Matching	−.719	.013	−.030	−3.40
	(.702)	(.043)	(.034)	(3.80)
	{.306, .306}	{.759, .760}	{.376, .553}	{.374, .383}
p-value: vocational training = vocational training + matching	[.847]	[.916]	[.022]	[.002]
Mean in control group	13.6	.312	.118	60.2
Number of observations	1,037	1,051	722	974

NOTE.—Shown are ordinary least squares regression coefficients with robust standard errors in parentheses. The data used are from the baseline and the first, second, third, and fourth worker follow-up surveys. 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 inference and Romano-Wolf adjusted p-values are given in braces. Randomization inference p-values are computed following Young (2019), and p-values adjusted for multiple testing within the same regression (i.e., for testing multiple treatments) are computed using the Romano and Wolf (2016) step-down procedure. These are both reported in braces. Outcomes in cols. 1–4 are conditional on the worker finding a job starting from August 1, 2013, when the training ended, up to the third follow-up survey. In col. 1 the outcome is the number of months between the end of the training intervention on August 1, 2013, and the beginning of the first job. In col. 3 the outcome is conditional on the worker being in wage employment (so workers in self-employment are excluded). In col. 2 the eight study sectors are motor mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical wiring, and welding. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 US dollars. We report p-values on the tests of equality of treatment effects between vocational training and vocational training plus matching in brackets.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

smaller improvement in their employment index of 0.248σ ($p = .030$). The reason why the index is lower relative to those offered only vocational training is that (i) they are less likely to work in regular jobs ($p = .043$); (ii) on the intensive margin, they work significantly fewer months in regular jobs ($p = .011$); and (iii) they work less time in one of the good sectors in which we offered training ($p = .104$).[23]

[23] On other intensive margin measures, we see no difference between skilled workers with and without job assistance in terms of the number of hours they work per day or the number of days they work per week. Including these additional outcomes in the index does not alter the findings: the employment index for matched workers rises by 0.192σ, and for those additionally offered matching, it rises by 0.106σ. These are statistically different ($p = .020$).

Table 8
Employment

	Has Done Any Work in Past Month (1)	Has Done Any Casual Work in Past Month (2)	Has Done Any Regular Work in Past Month (3)	Number of Months of Regular Work in Past Year (4)	Number of Months Worked in One of the Eight Good Sectors in Past Year (5)	Employment Index (6)
Vocational training	.094***	.000	.113***	1.33***	1.94***	.347***
	(.021)	(.015)	(.022)	(.232)	(.207)	(.040)
	{.000, .001}	{.988, .996}	{.000, .001}	{.000, .001}	{.000, .001}	{.000, .001}
Vocational training + matching	.063***	.005	.066***	.690***	1.54***	.248***
	(.023)	(.017)	(.024)	(.257)	(.228)	(.044)
	{.005, .007}	{.748, .767}	{.005, .005}	{.005, .011}	{.000, .001}	{.000, .001}
Matching	.051**	−.003	.054**	.510**	.556***	.117***
	(.022)	(.017)	(.023)	(.246)	(.203)	(.040)
	{.025, .024}	{.840, .826}	{.029, .018}	{.047, .040}	{.009, .007}	{.007, .002}
p-value: vocational training = vocational training + matching	[.152]	[.765]	[.043]	[.011]	[.104]	[.030]
Mean in control group	.623	.169	.524	5.92	1.88	−.167
Number of observations	3,703	3,699	3,700	3,724	3,723	3,725

NOTE.—Shown are ordinary least squares regression coefficients with robust standard errors in parentheses. The data used are from the baseline and the second, third, and fourth worker follow-up surveys. 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 inference and Romano-Wolf adjusted p-values are given in braces. Randomization inference p-values are computed following Young (2019), and p-values adjusted for multiple testing within the same regression (i.e., for testing multiple treatments) are computed using the Romano and Wolf (2016) step-down procedure. These are both reported in braces. In col. 1 the outcome is a dummy equal to 1 if the respondent has done any work in the month before the survey, including casual work. Casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing, and slashing compounds. Casual work also includes any type of agricultural labor, such as farming, animal rearing, fishing, and agricultural day labor. Regular jobs include all other jobs that are not in the list of casual jobs. In col. 5 the eight study sectors are motor mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical wiring, and welding. The dependent variables in cols. 3–5 exclude casual work. In col. 6 the employment index has the components in cols. 3–5 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 US dollars. We report p-values on the tests of equality of treatment effects between vocational training and vocational training plus matching in brackets.

** Significant at the 5% level.
*** Significant at the 1% level.

Linking these results back to those on expectations highlights that optimistic beliefs can drive the search for good jobs. Specifically, we note that the difference in expected job offer arrival rates between those offered training with and without match offers (and accounting for the fact that this is on a 0–10 scale) was $(1.84 - 1.45)/10 = 0.039$ (table 4, col. 1). Contrasting this with the actual differential likelihood of these two groups finding a good job (table 8, col. 3) is $0.113 - 0.066 = 0.047$, which is the same order of magnitude.

The third row of table 8 shows outcomes for those offered only matching. Relative to controls, their employment outcomes improve significantly along both extensive and intensive margins. Naturally, the magnitudes of impact are smaller than for those offered training. Their employment index rises by 0.117σ, so around one-third that of those offered training only.

C. Earnings

Column 1 of table 9 shows that for those offered vocational training, long-run earnings rise by 26% over controls. Columns 2 and 3 show that the bulk of this rise comes from earnings from regular jobs. Examining earnings impacts for workers offered training and matching, we see that (i) total and regular earnings rise significantly over controls and (ii) the point estimates on both are smaller than for workers offered only training, but these differences are only marginally significant.

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

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

Table 9
Earnings

	Earnings in Past Month (USD) (1)	Earnings from Casual Jobs in the Past Month (USD) (2)	Earnings from Regular Jobs in Past Month (USD) (3)	Bargaining Index (4)	Length of Last Unemployment Spell (months) (5)
Vocational training	11.0***	1.12	8.07***	.002	−1.24***
	(2.52)	(.770)	(2.33)	(.023)	(.235)
	{.000, .001}	{.148, .151}	{.000, .001}	{.916, .909}	{.000, .001}
Vocational training + matching	6.11**	−.437	5.74**	.089***	−.667**
	(2.89)	(.870)	(2.69)	(.025)	(.259)
	{.040, .021}	{.625, .647}	{.032, .017}	{.000, .002}	{.010, .010}
Matching	3.27	.610	1.25	−.018	−.411
	(2.71)	(.957)	(2.47)	(.024)	(.250)
	{.255, .252}	{.552, .538}	{.622, .625}	{.453, .465}	{.099, .103}
p-value: vocational training = vocational training + matching	[.099]	[.102]	[.396]	[.001]	[.023]
Mean in control group	43.3	5.15	38.0	−.019	6.20
Number of observations	3,125	3,269	3,541	3,570	3,693

Note.—Shown are ordinary least squares regression coefficients with robust standard errors in parentheses. The data used are from the baseline and the second, third, and fourth worker follow-up surveys. 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 inference and Romano-Wolf adjusted p-values are given in braces. Randomization inference p-values are computed following Young (2019), and p-values adjusted for multiple testing within the same regression (i.e., for testing multiple treatments) are computed using the Romano and Wolf (2016) step-down procedure. These are both reported in braces. In col. 1 the dependent variable is total earnings from any casual and regular wage or self-employment in the past month. The top 1% of earnings values are excluded. The data used in col. 2 are from the second and third worker follow-up surveys because casual earnings were not measured at the fourth follow-up. In col. 4 the wage bargaining index has the components in cols. 1–4 of table A12 and is constructed following Anderson's (2008) approach. In col. 5 the length of last unemployment spells refer to spells in which the respondent has been involved in the past year. The maximum value is 12 months, which corresponds to the respondent having been involved in the same 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 US dollars. We report p-values on the tests of equality of treatment effects between vocational training and vocational training plus matching in brackets.

** Significant at the 5% level.

*** Significant at the 1% level.

can help explain how they close the earnings gap to those offered only training.

D. Realized Sorting

We next consider impacts on sorting, by examining the characteristics of firms and jobs that workers end up at in their last employment spell in each survey wave and the extent to which they engage in self-employment. We collected information on firm and job characteristics to allow a direct comparison to the ideal firm and job characteristics workers expressed directing their search toward (table 6). We construct indexes of realized firm and job quality, where higher indexes correspond to more productive firms or jobs higher up the ladder.

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

Column 2 shows that among those offered training, realized job quality is also significantly lower among those additionally offered matching ($p = .077$). The treatment effects on each component of the job quality index are shown in table A14: this reveals that the key distinction between the two is that those offered only training are significantly more likely to end up in jobs that enable them to supervise others. In contrast, for youth offered both training and matching up to 5 years earlier, they end up in jobs not significantly different from those for controls.

A measure of worker-firm match quality is the length of the employment spell. Column 3 in table 10 reveals that (i) those offered training have significantly longer employment spells than controls (the magnitude of the effect is 1.24 months, corresponding to a 22% increase over controls) and (ii) employment spells are about half the magnitude for trainees additionally offered matching, and this is significantly different from those offered only training ($p = .015$).

The results all point to positive assortative matching between workers, jobs, and firms: those offered training end up higher up the job ladder. This progression is slower for those additionally offered matching. This is despite both groups graduating with identical sector-specific skills. The fact that they sort to different firms, jobs, and sectors represents a misallocation of talent.

[24] Individuals who do not have a job are excluded from cols. 1 and 2. All of our indexes allow for missing values on some outcomes, with outcomes being reweighted to account for this.

Table 10
Realized Sorting

	Realized Firm (1)	Realized Job (2)	Length of Last Employment Spell (months) (3)	Has Done Any Self-Employment in One of the Eight Study Sectors in Past Month (4)	Has Done Any Self-Employment in Other Sectors in Past Month (5)	Labor Outcomes Index (6)
Vocational training	.003	.096***	1.24***	.104***	−.047***	.115***
	(.028)	(.029)	(.234)	(.013)	(.015)	(.018)
	{.891, .910}	{.002, .004}	{.000, .001}	{.000, .001}	{.001, .002}	{.000, .001}
Vocational training + matching	−.058*	.042	.619**	.076***	−.029*	.051***
	(.031)	(.032)	(.258)	(.015)	(.017)	(.020)
	{.069, .049}	{.204, .211}	{.018, .013}	{.000,.001}	{.080, .085}	{.008, .011}
Matching	−.067**	−.013	.452*	.040***	−.002	.020
	(.031)	(.030)	(.248)	(.013)	(.017)	(.018)
	{.027, .024}	{.656, .665}	{.072, .068}	{.003, .003}	{.890, .893}	{.275, .262}
p-value: vocational training = vocational training + matching	[.035]	[.077]	[.015]	[.100]	[.255]	[.001]
Mean in control group	.034	−.025	5.63	.061	.154	−.043
Number of observations	2,504	2,429	3,693	3,699	3,699	3,725

Note.—Shown are ordinary least squares regression coefficients with robust standard errors in parentheses. The data used are from the baseline and the second, third, and fourth worker follow-up surveys. 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 inference and Romano-Wolf adjusted p-values are given in braces. Randomization inference p-values are computed following Young (2019), and p-values adjusted for multiple testing within the same regression (i.e., for testing multiple treatments) are computed using the Romano and Wolf (2016) step-down procedure. These are both reported in braces. In col. 1 the realized firm index has the components in cols. 1–5 of table A12. In col. 2 the realized job index has the components in cols. 1–5 of table A14. In col. 3 the length of last employment spells refer to spells in which the respondent has been involved in the past year. The maximum value is 12 months, which corresponds to the respondent having been involved in the same employment spell for the entire year. The components of the labor outcomes index in col. 6 are the components of the labor outcomes index, the components of the realized job and realized firm indexes, earnings from regular jobs in the past month, and the length of the last employment spell. All indexes are constructed following Anderson's (2008) approach. We report p-values on the tests of equality of treatment effects between vocational training and vocational training plus matching in brackets.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

This misallocation is caused by the revised expectations workers with match offers have, because they misattribute the lack of callbacks and become discouraged in their search, exerting lower search effort and directing it to lower-quality firms. These results represent novel experimental findings on how sorting patterns between workers, firms, and jobs are shaped by labor market interventions in a low-income setting.

Self-employment.—It is natural to consider the extent to which the interventions impact career progression via self-employment. Column 4 of table 10 shows that workers in all treatment arms are more likely to engage in self-employment. Increases in self-employment occur entirely within the study sectors, and we observe significant reductions in workers assigned to vocational training being engaged in self-employment in other sectors (col. 5). We saw earlier that long-run nonemployment rates even for skilled workers remain around 30%, so labor markets do not clear even for them (Banerjee and Sequeira 2023). Hence, the movement into self-employment might represent push factors arising from a lack of labor demand rather than workers preferring self-employment over other jobs. Indeed, we find no short-run treatment effect on those offered vocational training on their stated desire to move into self-employment.

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

E. Dynamics

To summarize long-run impacts and estimate dynamic treatment effects, we construct a holistic index of labor market success combining (i) all components of the employment index, (ii) total earnings, (iii) the length of the last employment spell, and (iv) all components of the indexes of realized firms and realized jobs. The ITT treatment effects on this index are given in column 6 of table 10. On this measure of long-run labor market success, there is a significant increase of 0.115σ for vocational trainees. This increase is significantly larger than for those additionally offered matching ($p = .001$), for whom the index rises by less than half (0.051σ). In short, the impacts of matching on those offered training undo half of what is achieved through training alone. The overall long-run impact of matching is not significantly different from controls.

Figure 5 presents dynamic treatment effect estimates on this index of labor market success by survey wave. This shows the gradual improvement in outcomes for those offered training, diverging away from the slight decline in outcomes for those additionally offered matching. Within each treatment arm, we cannot reject the null hypothesis that impacts are equal across periods. Within survey wave, our overall index implies that vocational trainees have significantly greater labor market success at waves 2 and 3 than those additionally offered matching ($p = .042, .014$). This hints at the possibility that by the final survey wave—some 55 months after training has completed—trainees with match offers finally start to catch up to those offered only training. The cumulative losses to them, in terms of earnings and labor market attachment, however, remain substantial.[25]

Our findings contribute to an ongoing debate about the persistence of intervention impacts in low-income contexts. They emphasize that initial conditions upon labor market entry have persistent impacts on the outcomes of young job seekers: the skills and expectations these individuals have when entering the labor market matter up to 6 years later. Among those offered vocational training and matching, the discouragement caused by a lack of callbacks effectively scars these youth as they transition into the labor market. The opposite is the case for workers offered only matching: for them, the lack of callbacks confirms their labor market prospects and causes them to successfully borrow to start up in self-employment.

VI. Outcomes, Expectations, and Search

We use mediation analysis to link our two sets of results—mapping how labor market interventions translate into long-run labor market outcomes via experimentally induced changes in skills, expectations, and search behavior. Following Gelbach (2016), the treatment effect of intervention T on labor market outcome Y can be decomposed as operating through a set of K mediators each denoted m_k:

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

where R is the part left unexplained. The outcome we focus on is the index of labor market success, and we consider the following as mediators: sector-specific skills, the expected job offer arrival rate of a job in their preferred good sector in the next year, the minimum expected earnings conditional on employment in a job in a good sector, whether they have actively searched for a job in the past year, the ideal job and firm indexes, and whether the individual is borrowing.

The result is shown in figure 6. The x-axis shows the ITT estimate on the labor outcomes index for each treatment arm. The solid black bar shows the same ITT effect (table 10, col. 6), and within each, we show the contribution of each mediator. Among workers offered training, certified sector-specific skills are the most important mediator: 20% of the long-run impact on labor

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

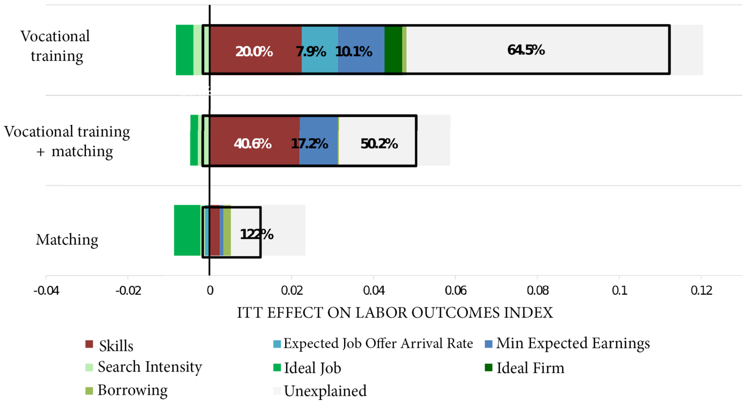

FIG. 6.—Mediation analysis. We show a decomposition of the ITT effects on the labor market index, following the approach of Gelbach (2016). We show the decomposition of the difference between the ITT effects in the full (with mediators) and restricted (without mediators) models. The black lines show the magnitude of the ITT coefficient from the restricted model. The percentages on the bars show the percentage of the ITT effect in the restricted model that is explained by each mediator. All regressions include strata dummies, survey wave dummies, a dummy for the implementation round, and dummies for the month of interview. The analysis uses the following variables as mediators: the sector-specific skills test score, the expected probability of finding a job in a good sector in the next 12 months, the reservation wage measured by the minimum expected earnings in a study sector firm, a dummy for whether the individual searched for a job in the previous year, the ideal job index, the ideal firm index, and a dummy for whether the individual is borrowing.

market outcomes is mediated through skills. This reinforces the findings from Alfonsi et al. (2020). Expectations explain a further 18% of the long-run impact, so they are almost as important as skills. Specifically, the expected job offer arrival rate explains 8% of the long-run impact, and the minimum expected earnings from employment in a study sector explains a further 10%. Other mediators have relatively muted roles.

Among workers additionally offered matching, sector-specific skills and expectations explain 41% and 17% of the labor outcomes index, respectively. However, given that the overall ITT effect to be explained is half the size (0.115σ vs. 0.051σ), the absolute importance of skills is the same for those offered training, with or without matching. This is as expected given that the accumulation of sector-specific skills does not differ between them. Expectations and search behaviors play less of a role in determining the long-run labor market success of those offered both training and matching—because these workers are discouraged, so they end up with expectations and search behaviors closer to controls overall. For workers offered only matching, no single mediator is prominent, although borrowing has a positive effect.

VII. Discussion

A. Revisiting Alfonsi et al. (2020)

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

Our earlier work largely combined the vocational training arms (with and without match offers). The justification for doing so was that the low rate of callbacks suggested that search frictions do not play a large role for firms. What the current analysis brings to the fore is that in the match offer treatments, the lack of callbacks to workers still shapes the expectations and search behavior of young workers, and this in turn determines their long-run labor market outcomes over and above the direct effects of acquiring certified skills that we focused on in Alfonsi et al. (2020). The current analysis shows the near-equal importance of skills and expectations in determining long-run labor market outcomes for youth offered training. Here we find that despite the increased mobility due to certifiable skills, trainees with match offers do significantly worse than those offered only training. The reason is novel: they are imperfectly informed and misattribute the lack of callbacks from match offers, causing them to revise down their expectations about their own prospects and search differently as they transition into the labor market. This leads to differential patterns of sorting for them: they end up at worse firms and in worse jobs, progressing less far on the job ladder from casual work toward good jobs. The two sets of analyses are complementary and together provide a detailed picture of the determinants of labor market outcomes for youth in a low-income context.

B. External Validity

Scalability and alternative informational interventions.—The vocational training courses in our study sectors are normally offered by VTIs throughout

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

Uganda. This treatment thus represents a scalable market-based intervention. Our match offer is relatively light touch and scalable. We highlight that young job seekers can misattribute information provided to assist them in job search. This lesson applies more broadly, emphasizing the need to consider the framing of job assistance, careers advice, or counseling.

Purely informational interventions link back to a long-standing discussion on what exactly individuals learn about during job search—aggregate demand conditions, as captured by learning the wage offer distribution (Wright 1986; Burdett and Vishwanath 1988), or returns to their own abilities (Gonzalez and Shi 2010). One way to therefore distinguish between informational interventions is whether they offer directed or undirected signals to workers. For example, in job fair–style job assistance or assistance that provides aggregate information (Abebe et al. 2020; Chakravorty et al. 2024; Kelley et al. 2024), signals conveyed to workers are relatively more informative of the status of the labor market as whole rather than individual job prospects. Hence, there is less scope for workers to misattribute signals as being informative of their own job prospects. Such interventions reduce overoptimism but do less to discourage workers. At the other extreme are highly directed job assistance interventions that tailor feedback specific to the individual (Altmann et al. 2018; Belot, Kircher, and Mueller 2019). Our matching intervention is closer to this second type. Moreover, in our intervention, workers interact with one BRAC placement officer, who simply tells them that the matched firms are not interested in meeting with them, without providing reasons why. One way to potentially avoid risks of misattribution from directed informational interventions is by using mentors to provide continuous feedback to workers, as in Alfonsi, Namubiru, and Spaziani (2024).

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

Figure A3*A* shows that within each treatment arm, the ITT impact on the long-run labor outcome index is not different between those with high and low cognitive ability. This reconfirms the notion that workers likely understood the nature of match offers. Figure A3*B* shows the analysis split between workers of high and low self-evaluation. A similar pattern of homogeneous results emerge, again suggesting that our results might extend to other samples of young job seeker. This also implies that misattribution of information generated from callbacks is a phenomena applying to workers irrespective of their underlying appraisal of their own worthiness, effectiveness, and capability.

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

C. Policy Implications

Our study has three implications for the design of labor market interventions. First, the value of vocational training operates through giving workers valued and certified skills but also by changing their expectations—making them optimistic with regard to their job prospects. This drives them to search more intensively, approach firms directly, and target higher-quality firms. Such beliefs might be motivated or help overcome biases such as procrastination (DellaVigna and Paserman 2005), but overall, it is not obvious that there are always positive returns in job search from holding optimistic beliefs—it can also lead to frustration (Genicot and Ray 2017; Banerjee and Sequeira 2023). Job search is a difficult and complex process requiring prolonged motivation. As van Hooft (2016) describes, the complexity arises from job search being a nonroutine activity in which individuals have limited experience, it involves utilizing a wide array of strategies and methods, it occurs in an ambiguous and competitive environment, and it can be a lengthy process involving multiple rejections. Throughout, job seekers have to avoid their motivation being undermined. Understanding intervention design features that can aid optimism rather than discouragement remains an issue for future work.

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

Finally, our findings relate to policy discussions about how to incentivize providers of vocational training to train and find workers employment. Our results suggest that incentive provision might not be enough: trying to match workers to firms is hard and requires additional information to be gained on both demand and supply conditions. This complements emerging findings that VTIs face severe information frictions when trying to find their own graduates employment (Banerjee and Chiplunkar 2024).

D. Future Agenda

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

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