Tackling Youth Unemployment: Evidence from a Labor Market Experiment in Uganda*

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September 2017

Abstract

Youth unemployment and underemployment are key policy challenges in the developing world. We present evidence from a two-sided labor market experiment, involving treatment and control workers *and* firms, both tracked over four years. The experiment examines two explanations for youth unemployment: that young workers lack skills, or because they face labor market entry barriers. To do so, we measure impacts on workers and firms of experimentally varying: (i) the offer of vocational training to workers before they enter the labor market; (ii) incentivizing firms using wage subsidies, to train workers on-the-job. Relative to control workers, vocationally trained (VT) and firm-trained (FT) workers increase employment rates by 24% and 15% respectively, and total earnings returns for VT workers are near double those for FT workers (40% vs. 22%). Structurally estimating a job ladder model of worker search reveals the mechanisms driving this: VT workers receive higher rates of job offers when unemployed, and higher rates of job-to-job offers. This greater labor market mobility causes the wage profiles of VT workers to diverge away from FT workers. The firm-side of the experiment reveals that some of the higher returns to VT workers are driven by them matching to higher productivity firms, the net employment effects of both forms of training are zero, and under wage subsidies, there is a degree of rent sharing between firms and workers. The results highlight that although policies raising worker skills or relaxing firm constraints can both tackle youth unemployment in low-income labor markets, the former are more effective from a worker's perspective. *JEL Classification: J2, M5.*

*We gratefully acknowledge financial support from the Mastercard Foundation and PEDL. We thank Jerome Adda, Orazio Attanasio, Chris Blattman, Richard Blundell, Dave Donaldson, Marc Gurgand, John Hardman-Moore, Eliana La Ferrara, Thibaud Lamadon, Thomas Le Barbanchon, Marco Manacorda, Karen Mancours, David McKenzie, Costas Meghir, Dilip Mookherjee, Suresh Naidu, Derek Neal, Fabien Postel-Vinay, Barbara Petrongolo, Mark Rosenzweig, Uta Schoenberg, Johannes Schmieder, Miguel Urquiola, Eric Verhoogen, Alessandra Voena, Chris Woodruff and numerous seminar participants for valuable comments. All errors remain our own.

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1 Introduction

Youth unemployment and underemployment are key policy challenges in the developing world. Nowhere is this more poignant than in East Africa where the majority of the population is aged below 25, and youth represent 60% of the unemployed. We study issues driving youth unemployment in urban labor markets in Uganda, the country with the second lowest median age of all countries in the world with 60% of the population being aged below 20, and where formal sector youth employment rates are below 30% [UN 2017]. Our analysis is based on a novel two-sided experimental design that tracks treatment and control workers, *and* treatment and control firms. The experiment is designed to provide insights on two prominent explanations for why youth unemployment rates are typically higher than those for older workers: that young workers lack skills, or because they face barriers to labor market entry.

Under the former explanation, young workers might face binding credit constraints to acquiring skills, or have imperfect knowledge over the types of skill to invest in, or the return to skills [Jensen 2010]. Under the latter explanation, firms might be uncertain over the abilities of inexperienced workers [Farber and Gibbons 1996, Altonji and Pierret 2001, Pallais 2014]. Even under perfect information, firms might be credit constrained and face up front costs to hire workers, or train them before they become productive [de Mel *et al.* 2016, Hardy and McCasland 2017].

To understand the importance of each explanation, our experiment tracks 1700 workers and 1500 firms over four years, measuring the causal impacts on workers and firms of experimentally varying: (i) the offer of six months of sector-specific in-class vocational training to workers before they enter the labor market; (ii) incentives to firms (via wage subsidies) to hire and train workers on-the-job for six months, replicating a classic in-firm apprenticeship structure; (iii) the worker-firm matching process. Such a two-sided design allows us to study transitions into the labor market from both worker and firm perspectives, and thus provide a comprehensive analysis of the returns to vocational and firm-provided training in the same context.[1]

On the worker side of the labor market, our evaluation is based on disadvantaged youth transitioning into the labor market. Relative to youth in the more developed nations these individuals: (i) have typically acquired lower levels of human capital from the formal schooling system (both

[1]Earlier studies have often evaluated a combination of in-class vocational and on-the-job training, e.g. JTPA in the US and the YTS in the UK. In low-income settings, Card *et al.* [2011] and Attanasio *et al.* [2011] both evaluate the impacts of combining three months of vocational training followed by three month apprenticeships, in the Dominican Republic and Columbia respectively. Lalonde [1995] and Heckman *et al.* [1999] survey the earlier literature on job training programs. On-the-job training, internships and wage subsidies are all common policy approaches that have been used to target disadvantaged groups in the labor market [Layard and Nickell 1980, Katz 1998]. The justification for such approaches are typically twofold [Ham and Lalonde 1996, Katz 1998, Bell *et al.* 1999, Blundell 2001]: (i) to reduce employer screening costs; (ii) to provide workers some labor market experience that can have persistent impacts. On the first channel, Autor [2001] and Hardy and McCasland [2017] present evidence on the use of apprenticeships as screening technologies. On the second channel, Pallais [2014] shows via an experiment on an online jobs platform that providing employment to an inexperienced worker helps improve their later employment outcomes, emphasizing that early labor market experiences convey information on the workers skills rather than raising productivity.

because they have fewer years of schooling and a lower quality of schooling); (ii) have fewer options through which to raise their human capital before they seek employment, say through colleges or tertiary education; (iii) face stiff labor market competition from other young entrants because of the demographic structure in Uganda. On the demand side, the firms in our evaluation are small and medium size enterprises (SMEs) in both manufacturing and service sectors. These SMEs represent a core segment of firms in Uganda. As Figure A1 shows, Uganda has a highly skewed firm size distribution relative to the US, in common with other developing countries. In Uganda, the majority of firms have fewer than 10 employees, and the majority of workers are employed in such firms. Hence these firms are key drivers of youth employment in this developing economy.

We sequence our analysis of vocational versus firm-provided training into three parts: (i) presenting reduced form returns to both forms of training from the worker's perspective; (ii) estimating a structural dynamic job ladder model of worker search to pin down mechanisms driving differences in the returns to vocational and firm-provided training; (iii) using reduced form and structural evidence from the firm's perspective to understand how the kinds of firms that workers match to might also drive some of the returns, and how firms are impacted by both forms of training.

The first part of the analysis considers, from the worker's perspective, how the treatment impacts of VT and FT differ over the three post-intervention years workers have been tracked for. We present novel evidence on the task composition and verifiable skills of workers that have transitioned into the labor market through each training route. We then present treatment impacts on worker's employment probability, earnings, wages, and estimate productivity bounds [Attanasio *et al.* 2011]. We contribute to the literature by providing treatment effect estimates of the productivity impacts of on-the-job training, something that has long been debated [Ham and Lalonde 1996, Katz 1998, Blundell *et al.* 1999], and compare them to productivity impacts of vocational training in the same setting.

In the second part of our analysis we assume that by endline (three years post-intervention), workers have reached their steady state wage trajectory. Combined with auxiliary assumptions, we structurally estimate a job ladder model of worker search. This pins down mechanisms driving the treatment effects on labor market outcomes. The key channels investigated are: (i) unemployment-to-job offer arrival rates (UJ); (ii) job-to-job offer arrival rates (JJ). These channels are important because VT workers have certifiable skills from their training in vocational training institutes (VTIs). All else equal, this greater ability to signal their skills to potential employers should make it easier for them to get back on the job ladder if they become unemployed (UJ transitions), and to make job-to-job transitions as they receive outside wage offers (JJ transitions).

The third part of our analysis completes our analysis of both training routes by exploiting the two-sided experimental design to measure how training routes differ in terms of: (i) the kind of firm individuals end up being employed at: this allows us to understand whether the differential returns to workers of vocational and firm-provided training are partly due to the fact that workers

end up being attached to differentially productive (and so better paying) firms; (ii) within-firm employment displacement: this allows us to address a key challenge training evaluations have faced, in measuring whether training workers has impacts on net employment, or just reallocates the same number of jobs to different workers; (iii) impacts on firm profits: combining this with the estimated earning treatment impacts on workers allows us to derive how the surplus generated through training is split between workers and firms. This is important because if workers are more mobile under vocational training, then FT workers should not capture the full social surplus to their skills because of their lack of labor market mobility.

Our key results are as follows. On workers human capital, we find that: (i) within sector, the task composition of employed workers at endline differs between VT and FT workers; (ii) VT workers are not more likely than control workers to be trained on-the-job; (iii) they score higher than FT workers on a sector-specific skills test, while the latter report having fewer skills that are transferable across firms. These results are in line with the predictions of a long literature examining how firm incentives to train workers in general or specific skills depends on labor market frictions [Becker 1964, Acemoglu and Pischke 1998, 1999]: these findings are consistent with VT workers being more mobile because of their certifiable skills, and so their skill set being tilted towards sector-specific rather than firm-specific human capital.

On labor market outcomes, VT and FT workers significantly increase employment rates by 24% and 15% over the control group of workers respectively (this increase is driven by wage employment). Hence both training routes have substantial impacts in pulling youth out of unemployment, into wage employment. This increased labor market attachment of youth is also observed on the intensive margin: VT and FT workers work significantly more months of the year relative to control workers, although the impact is larger for VT workers. Combined with the findings on the extensive margin, the picture that emerges is that pre-intervention these young labor market entrants are underemployed: they are willing to increase their overall labor supply and use their idle labor if given the skills to do so.

All this results in increased monthly earnings: those of FT workers are 22% higher than the control group, while VT worker earnings are significantly greater than for FT workers, being 40% higher than the control group. This is because total labor supply increases for VT workers, while both experience similar increases in hourly wages over the control group of 55%. Productivity bounds analysis suggests both sets of workers become significantly more productive. However, and most remarkably, we find the majority of hired apprentices remain employed at the same firm they were originally assigned to even after the wage subsidy expires. This hints at the key dynamic difference between training routes: relative to young workers that enter the labor market via vocational training, those that transition through apprentices do not move up the job ladder.[2]

[2]In relation to earlier studies, Card *et al.* [2011] find no evidence of employment impacts; Attanasio *et al.* [2011] find a 7% increase in employment rates for women and a 20% earnings increase, with impacts being sustained in the long run [Attanasio *et al.* 2015]. Galasso *et al.* [2004], Levinsohn *et al.* [2014] and Groh *et al.* [2016] evaluate wage

The structural model builds on this insight, to reveal the following: (i) VT workers have higher steady state rates of UJ transitions than FT workers: if they fall off the job ladder into unemployment, they are more likely to get back on it; (ii) VT workers are also more likely to make JJ transitions in steady state: such poaching is in line with them having certifiable skills and having relatively more sector- rather than firm-specific human capital than FT workers. Workers only make JJ transitions if the new wage offer is at least as high as the current wage. Hence over time, the wage profile of VT workers diverges away from FT workers. Indeed, in steady state the earning returns are 34% for VT workers (relative to the control group), and 12% for FT workers. The estimated returns to FT are far lower from the structural model than the reduced form evidence because the structural estimates account for the dynamic mechanisms driving outcomes, the lower mobility, that feeds into lower returns in steady state.

Finally, both training routes significantly reduce steady state unemployment rates for treated workers. Given the transition dynamics above, the supply-side policy of vocational training does so to a far greater extent than the demand-side policy of wage subsidies (14% versus 2.3%). These differential impacts on steady state youth unemployment rates are, from a worker's perspective, they key reason why the supply-side policy of skilling youth pre-labor market entry, so clearly dominates a demand-side policy reducing barriers to labor market entry that youth face, by reducing up-front hiring/screening costs to firms via the wage subsidy.

The third part of our analysis completes the comparison of these supply- and demand-side interventions by examining outcomes from firms' perspective. We find: (i) firms that apprentices *initially* match to are of lower productivity than firms the control group of workers match to: this is shown using reduced form evidence that such firms have significantly lower profits per worker, and extending the structural model to allow for firm heterogeneity and so estimate the productivity of firms matched to *in steady state* under each treatment arm; (ii) among firms hiring apprentices through wage subsidies, there is evidence of a share of them being retained well after the wage subsidy has expired; (iii) there is however, full employment displacement of other workers that otherwise would have been hired into these firms; (iv) firms offered wage subsidies experience a sustained increase in monthly firm profits of 13% [$p = .015$]: this reflects the fact that they have more productive workers, but the additional surplus is not so large as to generate additional employment. Using this increase in profits in conjunction with LATE impacts on wages, we derive that apprentices split the surplus generated with firms, with workers capturing 35% of the surplus.

We combine program accounting costs with the estimated steady state benefits to derive the internal rate of return from each training route assuming a social discount rate of 5%. If we assume gains last over the working life, the IRR to vocational training is 24.7%, while those those to firm training are 9.7%. Assuming the gains last 20 years, these fall to 24% and 8% respectively. Both training routes are found to pay for themselves in a decade. What prevents workers and

subsidy interventions. McKenzie [2017] reviews the evidence on training and wage subsidy programs in low-income settings. We later discuss our findings in relation to this literature.

firms investing in these forms of training given such high rates of return? A key constraint is credit: the cost of vocational training, or for self-financing an apprenticeship in our firms, are both orders of magnitude higher than young workers average annual earnings at baseline. For firms, the effectiveness of wage subsidies suggests that they are also credit constrained in taking on young workers: in the long term such hires increase firm profits. However, our evidence suggests it is lower profitability/productivity firms that are most induced to take-on (and retain) workers hired through this route.

In relation to the large literature evaluating training programs, our key contributions are as follows. First, we cleanly compare the returns to in-class vocational and on-the-job training in the same labor market context. Second, we base our findings on a relatively long run experiment that allows us to contrast experimental treatment effect estimates of the returns to each form of training, to structural estimates derived from a job ladder model of worker search. These structural estimates pinpoints the dynamic mechanisms driving the reduced form impacts. Our evidence supports the basic intuitions of theory that the mechanisms behind training routes differ: vocationally trained workers have certifiable skills from VTIs and so are more mobile in labor markets that FT workers. Third, the two-sided experimental design allows us to provide a comprehensive analysis contrasting the training routes from the dual perspectives of workers *and* firms. The firm-side of the experiment allows us to shed light on: (i) the extent to which the differential returns to workers are driven by their matching to different kinds of firms; (ii) to measure within-firm employment displacement effects and so understand whether training routes differ in terms of net employment creation; (iii) to measure how the surplus generated by skilling workers is split between workers and firms. Ultimately, by using the two-sided experimental design to provide a complete economic comparison of a supply-side (vocational training) versus a demand-side intervention (wage subsidies to firms to hire and train workers on-the-job), we provide policy relevant insights on the underlying causes of youth unemployment in low-income labor markets.[3]

Training programs are popular in both low- and high-income settings: the World Bank has invested $9bn in 93 such programs between 2002 and 2012 [Blattman and Ralston 2015], and expanded training programs were the most common type of labor market policy implemented globally in response to the 2008 financial crisis [McKenzie and Robalino 2010]. Yet, the evidence base for such programs is thin: the meta-analyses of Card *et al.* [2015], Blattman and Ralston [2015] and McKenzie [2017] show relatively weak or short-lived impacts of training programs in low-income settings. Given this, we close our analysis by highlighting potential explanations for the large impacts we document: the experimental design, the selection of workers, and the quality of the vocational training institutes worked with. On each dimension, we make suggestions for future research to take, and discuss the wider implications of our findings for the study of youth

[3]Comparing experimental and structural estimates of the returns to training remains rare in the literature. Notable exceptions are Card and Hyslop [2005] and Hoffman and Burks [2017], although the latter compares to quasi-experimental estimates, not those based on random assignment.

unemployment in low-income labor markets.[4]

The paper is organized as follows. Section 2 describes the setting, experimental design and data. Section 3 presents treatment effect results on worker skills and labor market outcomes by training route. Section 4 develops and structurally estimates the job ladder model to pin down the mechanisms along which training routes differ. Section 5 presents firm side responses, focusing on firm selection, employment and profit impacts by training route. Section 6 concludes by presenting the IRR estimates and discussing external validity. Further robustness checks are in the Appendix.

2 Setting, Experimental Design and Data

Our study is a collaboration with the NGO BRAC, who implemented all treatments, and five vocational training institutes (VTIs). The VTI sector is established in Uganda, with hundreds in operation. We worked with five of the most reputable: each could offer standard six month training courses in the eight sectors we focus on: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering. These sectors constitute an important source of stable wage employment for young workers in Uganda: around a quarter of employed workers aged 18-25 work in one of these sectors.[5]

2.0.1 Workers

The experiment is based on an oversubscription design, where individuals first applied to an offer of potentially receiving six months of sector-specific vocational training at one of the five VTIs we collaborated with. As in other training interventions, disadvantaged youth were targeted [Attanasio *et al.* 2011, Card *et al.* 2011]. We received 1714 eligible applicants, 44% of them from women.[6] Table A1 describes labor market characteristics of workers in our evaluation: on average they are 20 years old, and the vast majority report being out of school and never having been vocationally trained. The first row of Table 1 shows relevant labor market outcomes at baseline for

[4]Card *et al.* [2015] discuss over 800 estimates from 200 studies documenting impacts of active labor market programs. They find in contrast to training or wage subsidies, job search assistance (matching) has comparatively large short and long run impacts and are more pronounced for disadvantaged workers. Search costs have been shown to be relevant for workers and firms in labor markets [Abebe *et al.* 2016, Franklin 2016, Hardy and Macasland 2017, Pallais 2014, Bassi and Nansamba 2017].

[5]The VTIs we worked with were: (i) founded decades earlier; (ii) were mostly for-profit organizations; (iii) training hundreds of workers with an average student-teacher ratio of 10; (iv) in four VTIs, our worker sample shared classes with regular trainees. We derive the share of employed workers aged 18-25 working in these eight sectors using the Uganda National Household Survey from 2012/13.

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

workers: unemployment rates are over 60% for these youth (Columns 2 and 3) with insecure casual work being a prevalent form of labor activity. Average monthly earnings are $6, corresponding to less than 10% of the Ugandan average. Moreover, these are not individuals that could self-finance vocational training (that costs over $400) or apprenticeships in firms.[7]

The oversubscription design is informative of the impact of marginally expanding vocational training programs. Given Ugandan demographics, there is no shortage of the kind of marginal young labor market entrant that our study is based on.

2.0.2 Firms

To draw a sample of firms for the two-sided experimental design, we first conducted a firm census in each of 17 urban labor markets. We selected firms: (i) operating in one of the eight sectors of interest; (ii) having between one and 15 employees (plus a firm owner). The first criteria restricts to manufacturing and service sectors in which we offered sector-specific vocational training, thus limiting skills mismatch in our experiment. The second restriction excludes micro-entrepreneurs and ensures we focus on SMEs that, as Figure A1 highlights, are central to employment in Uganda. We end up with a sample of 1538 SMEs, that in aggregate employ 4551 workers at baseline.

2.0.3 Vocational Training

Table 2 provides data on the supply of, and returns to, vocationally training in this setting. It shows: (i) the share of workers employed at baseline in these firms that self-report having ever received vocational training from a VTI; (ii) the coefficient on a dummy for this self-report in an otherwise standard Mincerian wage regression of log wages. The first row pools across all sectors and shows: (i) 31% of all workers in SMEs at baseline have vocational training from some VTI: this is therefore a common route through which workers acquire skills in Ugandan labor markets; (ii) the Mincerian wage returns to vocational training are over 50%. These results hold by sector: in each there is demand for these skills and there are potentially high returns to them. Of course, the Mincerian returns are not causal: they are likely upwards biased due to worker selection into employment. Our experimental results shed light on the causal impact of vocational training and quantify the selection bias in these Mincerian returns. This evidence just shows the potential for there to be very high returns to vocational training in this setting. This is in contrast to high-income settings where many training programs have been found to have weak or short-lived impacts [Card *et al.* 2015].

[7]Table A2 compares our sample to those aged 18-25 in the Uganda National Household Survey from 2012/3. The program appears well targeted: our sample is worse off in terms of labor market outcomes at baseline, and that remains true when we compare to youth in the UNHS that report being active in the labor market. Irrespective of the precise sample, Table A2 highlights the seriousness of youth unemployment and underemployment in Uganda.

2.0.4 Apprenticeships

Firm-sponsored training is another central means by which workers accumulate human capital [Acemoglu and Pischke 1998, 1999, Autor 2001]. Apprenticeships are a common labor contract throughout Sub-Saharan Africa, including Uganda [Hardy and McCasland 2017]. Table 3 provides evidence on such contracts from our sample of SMEs at baseline. Panel A shows that half of workers employed in these SMEs at baseline report having received on-the-job training in their current firm, with an average training duration of 10 months. Panel B shows a variety of payment structures for apprentices: the majority are unpaid, while others are paid, and some pay for their training. For those 20% of workers that are paid, apprentices report earning an average monthly wage of \$39.[8] Panel C shows the main opportunity cost to SMEs taking on apprentices is the firm owner's time: they are predominantly tasked to train apprentices. Hence it is costly for SMEs to continually try and experiment with new-hires. If SMEs are credit constrained, it is these kinds of up-front costs that are reduced in our wage subsidy treatment.

2.1 Experiment

2.1.1 Design

Panel A of Figure 1 presents the experiment design from a worker's perspective. The oversubscription design is such that 1714 workers initially applied to be trained: we had funding for 500 training places, so individuals were randomly assigned to receive vocational training. Among workers assigned to training, we further randomly assigned them into one of two treatments (the top branch of Figure 1A): the first group completed their six months of training and then transitioned into the labor market. This is the business-as-usual vocational training model, where VTIs are paid to train workers, but not to find them jobs. The second group of trained workers, upon training completion were matched to firms operating in the same sector as the worker had been trained in, and in the same region. Among workers not assigned to vocational training (the lower branch in Figure 1A), workers were randomly assigned to one of three treatments: (i) some were matched to firms; (ii) some were matched to firms and those firms offered a wage subsidy in order to take on the worker and train them on-the-job for six months; (iii) other were held as a control group. Pairwise comparisons across treatment arms are informative of the returns to vocational training (VT), firm-provided training (FT), and labor market search frictions.[9]

[8] To get a sense of the cost of an apprenticeship we note that: (i) for 51% of all apprentices their main cost is the opportunity cost of labor market opportunities during the apprenticeship as well as fixed costs of work (e.g. travel, tools). For 29% of workers that pay for their apprenticeship, on average the total payment is over \$500. Whichever way we might calculate it, the expected cost of an apprenticeship is high and above the annual earnings of our sample of workers at baseline.

[9] As Figure 1A shows, the control group was purposively larger than the other groups in anticipation of higher attrition rates. At the point of application, workers provided a preference ranking over their top three sectors to be trained in. For those assigned to vocational training, 91% of them were trained in one of their top-3 sectors.

Panel B of Figure 1 shows the design from a firms's perspective: firms were randomly assigned to either be matched with vocationally trained workers, matched with untrained workers (among those randomized out of vocational training), matched with untrained workers and given a wage subsidy to hire them. Most importantly, we have a control group of SMEs, that are observationally equivalent at baseline to those firms randomized into a matching treatment. Pairwise comparisons are informative of search frictions firms face in hiring workers, and the employment displacement and profit impacts of hiring workers.[10]

2.1.2 Treatments

Vocational training provides workers six months of sector-specific training in one of eight sectors. In those treatment arms involving vocational training (T3 and T4), BRAC entirely covered training costs, at \$470 per trainee.[11] Vocational training lessons were held Monday-Friday, for six hours per day; 30% of the training content was dedicated to theory, 70% to practical work covering sector-specific skills and managerial/business skills.[12]

For the wage subsidy treatment (T2), firms were offered to meet untrained workers and offered a payment of \$50 a month for six months, to hire one such untrained worker. This was designed as an inflexible wage subsidy where \$12.5/month was to be retained by the owner, and \$38/month was to be paid to the worker. To assess whether the wage subsidy amount is reasonable in our context, we relate to two anchors: (i) Table 3 showed that during apprenticeships in the sample of SMEs, if workers were paid their mean wage was \$39/month; (ii) using the wages of all unskilled workers employed in our SMEs at baseline, our wage subsidy treatment had a subsidy rate (wage subsidy/average wage) of 63% (Figure A2a shows the entire distribution of unskilled wages at baseline among those employed in our SMEs). This is high: for example, de Mel *et al.* [2010, 2016] evaluate a wage subsidy program with a 50% subsidy rate. The wage subsidy lasted six months, conditional on the trainee remaining employed in the firm. Monitoring firms that took

[10] In a companion paper, Bandiera *et al.* [2017] we provide a comprehensive analysis of the firm side impacts of these treatments (and other treatments) in the short and long run, and what light they shed on constraints to expansion that SMEs face. Of relevance for the current analysis is that: (i) firms are balanced on observables across treatments, including on monthly profits, employee numbers, the value of the capital stock, age and owner characteristics; (ii) we find firms assigned to the wage subsidy treatment are more likely to attrit by first follow up, and we account for this by weighting observations using IPWs.

[11] The cost per trainee breaks down as the cost to the VTI (\$400), plus the worker's out-of-pocket costs during training, such as those related to travel and accommodation (\$70). Each VTI received 50% of the payment one week after training began, and the remaining 50% four months later (for trainees still enrolled). Hence VTIs were incentivized to retain trainees, not to find them jobs (as is the norm in Uganda). This simple incentive contract solved drop out problems associated with training programs in low-income settings [Blattman and Ralston 2015]. It also highlights these drop out problems are not driven by worker behavior but by the VTIs themselves. There was no additional stipend paid to trainees during training, and no child care offered either (recall that around 10% of our worker sample have at least one child).

[12] At end of vocational training, workers were asked about their satisfaction with the training: 75% reported being extremely happy/very happy with the VTI experience; 80% were extremely happy/very happy with skills gained; 56% reported that six-months of training was enough time for them to learn the skills they had wanted to, and 36% reported skills acquisition as being as expected (60% reported better than expected).

up the offer, we found the apprenticeship program to be implemented as intended.[13]

In the matching treatments (T4, T5) firms were presented lists of workers that were: (i) willing to work and vocationally trained (T4); (ii) willing to work but untrained (T2, T5). In case (i), the firms knew what the workers had been trained in, where they had been trained, but not that training had been paid for by BRAC. Hence firms might expect workers presented to them to be similar to those able to self-finance such training. In all treatments involving worker-firm matching (T2, T4, T5), there were a maximum of two workers presented to firms on a list, and the randomly assigned matches took place with firms operating in the same sector as the worker had been trained in (or had expressed an initial desire to be trained in), and both worker and firm were located in the same region in Uganda (Central, North, East and West).

2.2 Data

2.2.1 Timeline

Figure 2 shows the study timeline: the baseline worker survey took place from June to September 2012 when workers applied to the offer of vocational training. 1714 eligible workers have been tracked over three follow-up surveys that were fielded 24, 36 and 48 months after baseline (12, 24 and 36 months after the end of vocational training/apprenticeship placements).[14] The lower part of Figure 2 shows the timeline of firm surveys, extending to four post-intervention waves.

Workers are observationally equivalent at the point of application, when assigned to treatment. Subsequent to treatment assignment there is selective non-compliance by workers, but as in any two-sided matching problem, for the treatments involving worker-firm matches, there is also non-compliance by firms. In particular, worker-firm matches only occur if both a worker and the firm express a willingness to meet. Table A3 provides evidence on worker and firm take-up by treatment. The key points of note are that: (i) over 80% of workers take-up the offer of vocational training, over 95% of them complete training conditional on enrolment; (ii) there is variation in firm interest in meeting workers (45% of workers end up being offered a match in the wage subsidy treatment, 12% in the vocational training and match treatment, and 17% in the match treatment).

[13] We monitored the use of wage subsidies via spot checks by BRAC staff to ensure the designated subsidy split was being adhered to. We found that both workers and firms reported the correct subsidy split being made. Figure A2b shows worker and firm reports on the wage subsidy being received by the worker, with a clear spike at $39 /month as intended. Also, we found most apprentices that started working at the matched firm completed the full six months of subsidized on the job training.

[14] We also conducted a tracker survey to those randomized out of vocational training: this was fielded just as vocational trainees were transitioning into the labor market. The purpose of this survey was to construct accurate measures of the opportunity cost of attending the six months vocational training, that is used for the later IRR calculations. The tracker survey had a 19% attrition rate. The work status of respondents were as follows: 15% were currently involved in some work activity, 12% had been involved in a work activity in the last six months (but not on survey date), and 72% had not worked in the last six months. For treatments involving worker-firm matches, we conducted process surveys at the time of the match: these record details of the interview that took place, issues discussed and bargained over.

Hence in common with earlier studies, firm interest is one limiting factor for worker-firm matches to actually occur [Groh *et al.* 2016]. This might be because of stigma effects where firms perceive workers with attached subsidies as being of low quality [Bell *et al.* 1999]. The likelihood of a meeting conditional on the worker-firm match offer is 80% in the wage subsidy treatment; in the other matching treatments (T4, T5) the rates of actual meetings conditional on match offers were lower than for the apprenticeship arm (47-32%). In the wage subsidy treatment almost three quarters of the job offers were accepted, while this percentage is around one quarter for the matching treatments. Hence, this highlights that worker preferences can also be an important source of matches not translating into hires (as also discussed in Groh *et al.* 2016).[15]

As workers are observationally equivalent only at the point of application to vocational training, we mostly present ITT estimates for worker outcomes based on random assignment to treatment at the point of application. We address worker attrition by weighting ITT estimates using inverse probability weights, and also presenting Lee bounds estimates of the main treatment impacts [Lee 2009]. On firm-side selection, in Section 5 we present reduced form evidence on the kinds of firms willing to meet matched workers, and also extend the job ladder model to allow for firm side heterogeneity: we use this to structurally estimate the productivity of firms workers in each treatment arm end up being employed at.

Two other timing-related points are relevant. First, although workers were randomly assigned to treatment at the point of application, they were only informed about any match that might be offered once vocational trainees had completed their courses. This helps avoid lock-in or threat-effects on worker search [Black *et al.* 2003, Sianesi 2004]. Second, the design is such that vocational trainees and apprentices both come into contact with firms at the same time: this is in line with the underlying motivation for our study, to understand labor market transitions of youth. However, inevitably this means that vocational trainees receive their training before apprentices do. This six month divergence in training received is unlikely to bias estimates based on the three years of subsequent follow up data.[16]

2.2.2 Balance, Attrition and Estimation

We use a stratified randomization where strata are region of residence, gender and education. Table 1 shows the labor market characteristics of workers in each treatment, and Table A1 shows demographic and other background characteristics. In both cases, the samples are well balanced

[15]For the treatments involving vocational training (T3, T4), Table A3 shows that: (i) 96% of applicants were offered vocational training; (ii) worker take-up is 77% and 85% in T3 and T4 respectively. In the meta-analysis of McKenzie [2017], most studies have completion rates between 70 and 85%. Among workers that did not take-up the offer: (i) 25% reported not doing so because they had found a job, 8% were in education, 4% were in another form of training, 35% did not take-up for family reasons (they had a child, illness, or family emergency), 15% reported distance as being the main constraint, and 24% reported other reasons.

[16]We show evidence in support of this by exploiting a small second batch of vocational trainees that received their training between October 2013 and April 2014, so when apprenticeships were implemented. The primary worker outcomes do not differ between the main and second batch of vocational trainees.

on observables across treatments, and normalized differences in observables are small.

Attrition is low: 13% of workers attrit by the 48-month endline.[17] The Appendix describes correlates of attrition in more detail, confirming attrition between baseline and endline is uncorrelated to treatment. To estimate reduced form intent-to-treat (ITT) impacts on workers we use the following ANCOVA specification for worker i in strata s in survey wave $t = 1, 2, 3$,

$$y_{ist} = \sum\nolimits_j \beta_j T_i + \gamma y_{i0} + \delta \mathbf{x}_{i0} + \lambda_s + \vartheta_t + u_{ist}, \tag{1}$$

where y_{ist} is the labor market outcome of interest, worker i is assigned to treatment T_i (and there are j treatments), y_{i0} is the outcome at baseline, $\mathbf{x}_{i0}$ are the worker's baseline covariates. λ_s and ϑ_t are strata and survey wave fixed effects respectively. As randomization is at the worker level (i), we use robust standard errors, and we weight ITT estimates using inverse probability weights.[18]

The coefficient of interest is β_j: the ITT impact estimate for treatment T_i as *averaged* over the three post-intervention survey waves. We therefore leave all discussion of dynamics to the structural model. β_j has a causal interpretation under the twin assumptions of random assignment and no treatment spillovers. On spillovers we note that at baseline, workers are geographically and sectorally mobile: the majority are willing to travel to other labor markets to find work, and many are willing to consider working in different sectors.[19] Defining a labor market as a sector-region, our firm census shows that on average, there are 156 employed workers and 40 firms in each market. We match an average of 8 workers per market, corresponding to just 5% of all workers or 7% of new hires (those starting employment in the prior three months). Hence we do not expect the control group to be contaminated by treated workers in the same labor market as they are unlikely to be competing for the same exact job. However, there might be spillover effects on workers *not* in our evaluation sample. In this paper we use the firm side experiment to measure these displacement effects *within* firms that hire and retain treated workers.[20]

[17] This attrition rate compares favorably to other studies such as Attanasio *et al.* [2011] (18%), and Card *et al.* [2011] (38%). Indeed, in the meta-analysis of McKenzie [2017], all but one study have attrition rates above 18%.

[18] The baseline worker characteristics $\mathbf{x}_{i0}$ controlled for are age, a dummy for whether the worker was married, a dummy for whether the worker had any children, a dummy for whether the worker was employed, and a dummy for whether the worker scored at the median or above on a cognitive test administered at baseline. We also control for the vocational training implementation round and month of interview. The weights for the IPW estimates are computed separately for attrition at first, second and third follow-up. The instruments for the IPW estimates are whether the worker was an orphan at baseline, a dummy if anyone in the household of the worker reported having a phone at baseline, a dummy for whether the worker reported being willing to work in more than one sector at the time of their original application to the VTIs and dummies for the survey team the worker's interview was assigned to in each of the three follow-up survey rounds.

[19] At baseline, 33% of workers reported that they had previously attempted to find a job in a different town than the one they come from. Of the ones that had attempted to find a job in another town, 27% had succeeded. Of the ones that did not try to find a job in a different town, 92% said they would like to find a job in a different town than the one of origin. On workers sectoral mobility, at baseline 97% of workers reported being willing to work in more than one sector. Moreover, only 14% of all main job spells of workers in the control group at follow-up are in the same sector as the ideal sector mentioned at baseline.

[20] Crepon *et al.* [2013] provide experimental estimates of the equilibrium impacts of labor market policies in France using a design than randomizes the fraction of treated workers across labor markets, and individual treatment

3 Treatment Effects

Our core focus is on comparing supply- and demand-side training interventions for young workers: providing sector-specific vocational training to workers before they enter the labor market (a supply-side policy), versus incentivizing firms via wage subsidies to take on and train workers on-the-job (a demand-side policy). Evaluating the supply- and demand-side policies in the same context enables us to understand whether high rates of youth unemployment are driven by inexperienced workers lack of skills, and/or because they face barriers to labor market entry driven by firm's hiring/training costs.

Incentives for firms to train workers depend on labor market imperfections [Acemoglu and Pischke 1998, 1999]. A key distinction between these training routes is that vocationally trained workers have more certifiable skills, because VTIs provide graduates with certificates. Certification makes it more likely that vocationally trained workers can be poached, and hence relative to apprentices, this can tilt the balance towards the latter having relatively more firm-specific skills.[21] As a consequence, firms can appropriate more of the returns to worker skills due to labor market imperfections from apprentices. This gives firms monopsony power over apprentices and so enables them to hold their wages below their marginal product.

The friction wage subsidies help overcome relates to firm's credit constraints: apprenticeships subsidize the firm's ability to screen and/or train workers.[22]

In this Section we begin to explore these channels by presenting descriptive and treatment effect estimates on the task composition and skills of workers by training route. We then present treatment effect estimates of each training route on worker's employment, earnings, wages, and bounds analysis on worker productivity. In the next Section, the job ladder model sheds light on whether the labor market mobility of workers differs by training route, so pinpointing mechanisms driving the reduced form impacts. In Section 5 we exploit the two-sided nature of the experiment to present evidence on whether the differential returns to VT and FT are partly due to the fact that workers end up being attached to differentially productive (and so better paying) firms, within-firm employment displacement, and impacts on firm profits. This allows to provide a complete analysis of the impacts of alternative training routes in low-income labor markets from the perspective of workers and firms.

3.1 Tasks

We first present descriptive evidence on whether the composition of tasks performed by employed workers at endline differs between VT workers (pooling treatments T3 and T4) and FT workers

assignment within labor markets.

[21] Evidence of the value of certification in labor markets is provided by Pallais [2014], MacLeod *et al.* [2015] and Bassi and Nansamba [2017].

[22] There is evidence of employer learning [Farber and Gibbons 1996, Altonji and Pierret 2001] and the use of apprenticeships as screening technologies [Autor 2001, Hardy and McCasland 2015].

(T2). For each sector, we construct a list of 30 to 40 tasks performed by workers (based on modifying the O*NET task list).[23] For any given task j in sector k, we construct the share of workers reporting to perform task j, separately for those that have been vocationally trained and trained on-the-job. Figure 3A graphs the difference in these shares for each task j, color coding the Figure by sector. Panels A and B show separately the differential task composition over training types for the manufacturing and service sectors in our evaluation.[24] In each sector we see a stark divergence away from the zero line in the differences in these shares: within a sector, there are some tasks performed relatively more often by VT workers (at the left hand side of the Figures), and other tasks performed relatively more often by FT workers (at the right hand side of the Figures). In every sector, a Chi-squared test rejects the null that the task composition of workers is the same between VT and FT workers. In short, the two types of worker do not conduct the same tasks within firms, despite these SMEs being in the same sector and relatively homogenous at baseline. This might suggest these workers possess different skills, that we now measure.

3.2 Skills

Table 4 provides three results related to treatment impacts on worker skills. First, we assess the extent to which workers view themselves as having been trained by a firm in their first employment spell. To do so we define two dummies: (i) whether the worker reports having received on-the-job training at her first employer; (ii) whether the worker reported being a 'trainee' in her first employment spell. Columns 1 and 2 show that for both outcomes, the ITT estimate of workers assigned to the wage subsidy treatment arm is significant: unsurprisingly, they are between 14 and 22pp more likely than the control group to report being a firm trainee.[25] More surprisingly: (i) vocationally trained workers are no more likely than the control group to have been trainees in their first employment spell; (ii) apprentices are significantly more likely to view themselves as trainees than vocational trained workers ($p = .004, .000$). This suggests firms are less willing to invest in training workers that have been vocationally trained in sector-specific skills before they transitioned into the labor market. This is consistent with firms anticipating these workers to be more mobile than others, because their skills are certifiable to other employers.

The second piece of evidence relates to a sector-specific skills test that we developed in conjunction with skills assessors and modulators of written and practical occupational tests in Uganda.

[23] The Occupational Information Network (O*NET) database contains occupation-specific descriptors for occupations in the US economy. These are designed to reflect the key features of an occupation through a standardized, measurable set of tasks. Further details are here: https://www.onetonline.org/

[24] The data refers to all main job spells reported at endline (so there is one job spell per worker and only employed individuals are included to construct the task composition figures). Workers were asked to report which tasks they performed in each employment spell they had in the year prior to the survey.

[25] This is over a baseline of 40% of workers in the control group reporting to have received training in their first employment spell (Column 1), a magnitude that matches up well with the descriptive evidence in Table 3 where 50% of workers employed in the SMEs at baseline reported having been apprentices in the firm.

This kind of skills test has not been conducted much in the training literature.[26] Each sector-specific test comprises seven questions (five multiple choice, one pairwise-matching questions, and one question requiring tasks to be correctly ordered): Figure A5 shows an example of the skills test for the motor mechanics sector. Workers had 20 minutes to complete the test, and we convert answers into a 0-100 score. If workers answer questions randomly, their expected score is 25. The test was conducted on all workers (including those assigned to the control group) at second and third follow-up. There was no differential attrition by treatment into the test.[27]

Before administering the test, we asked a filtering question to workers on whether they had *any* skills relevant for any of the eight sectors in our study. The dependent variable in Column 3 of Table 4 is a dummy equal to one if the worker reported having skills for a sector. The ITT estimates show that VT workers and FT workers all report being significantly more likely to have relevant skills than those in the control group. As reported at the foot of the Table, 60% of control group workers report having skills for some sector, and this rises by 11pp for FT workers, and by 28pp for VT workers. This increase is significantly greater for VT than FT workers ($p = .000$), suggesting some share of apprentices might be left untrained by firms in receipt of a wage subsidy.

Only workers that reported having skills took the skills test: others were assigned a score of 25 assuming they would answer the skills test at random. Column 4 shows that only VT workers significantly increase their measurable sector-specific skills. Relative to the control group (that on average score just above the random answer baseline), VT workers increase sector-specific skills by 23% (or .3 of a standard deviation in the test score distribution). Strikingly, there is no increase in sector-specific skills among FT workers, and the skills impacts are significantly larger for VT workers than for FT workers ($p = .003$).

One concern might be that ITT impacts among FT workers are hard to detect given the lower rate of worker-firm matches that occur in this treatment arm. We therefore estimate a LATE specification using 2SLS for those treatment arms involving worker-firm matches (T2, T4, T5) relative to the control group, where we instrument whether the offer of a match taking place

[26] Berniell and de la Mata [2016] present evidence from a 12-month apprenticeship program in Argentina. In comparison to the control group, they find little evidence that the cognitive and non-cognitive skills of apprentices are impacted, but that relative to the control group they are able to certify their skills to a great extent and this drives some of the higher employment probabilities for treated workers. They do not set out to measure the task composition of workers, or develop specific tests to measure sector- or firm-specific skills. Adhvaryu *et al.* [2016] present more detailed evidence on worker knowledge, preferences and task assignment of workers in the context of an evaluation of a soft-skills training program for garment workers in India.

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

was made with the original treatment assignment. Under the assumption that assignment to a matching treatment does not impact outcomes for those that do not actually match with a firm, and that there are no individuals that would match with the firm only if they are in the control group, this yields the LATE: the impact of matching with a firm for individuals who match with a firm when assigned to a match treatment, and who do not match otherwise. The LATE specification in Column 5 confirms the earlier result: matched FT workers do not have significantly higher sector-specific skills than the control group. In contrast, VT workers have significantly higher sector-specific skills, and the magnitude of the effect is impressive: their skills test score rises by 60 points over the control group (that score 30/100) and so score near to full-marks on the test.

The final piece of evidence relates directly to firm-specific skills. As Column 6 shows, relative to control workers, VT workers are significantly more likely to report their skills being transferable across firms, while FT workers are no different to the control group in the skills transferability.

Taken together, this evidence suggests VT workers end up with relatively more sector-specific skills than FT workers. This is consistent with the mechanisms laid out above.

As a secondary point we note that for the match treatments (T4 and T5): (i) skills-related outcomes for vocational trainees that could have matched with a firm (T4) do not differ much from outcomes for (unmatched) vocational trainees in T3; (ii) matched workers in T5 have few differences in skills outcomes relative to the control group. This latter result emphasizes that the skills impacts on FT workers come through the wage subsidy component of the treatment, not from the mere fact that the subsidized worker gets to meet a firm *per se*.

Taking stock of this evidence we see that: (i) young workers transitioning into the labor market through VT and FT training routes perform very different tasks in firms; (ii) the balance of evidence suggests VT workers have measurably higher sector-specific skills and indeed report their skills to be transferable across firms; (iii) FT workers report having more firm specific skills. This all fits with the broad differences between the supply- and demand-side training routes as expected. We next examine how these differences in human capital translate into hard labor market outcomes.

3.3 Employment

3.3.1 Extensive Margin

Table 5 presents ITT estimates for extensive margin outcomes. Column 1 shows that, averaged over the three post-intervention survey waves, all treatments positively impact employment probabilities. The two forms of worker training increase employment probabilities by between 7pp and 11pp, corresponding to between 15% and 24% impacts over the control group, whose unemployment rate remains over 55%. Hence, the treatment impacts on youth unemployment rates are of economic significance, and this is so for both supply- and demand-side training policies.

Columns 2 to 4 show these increases in employment are predominantly driven by increases in

wage employment. For example, VT workers (T3) are 7.1pp more likely to be in wage employment, a 26% increase over the control group, and the magnitude of the effect on FT workers is very similar. Interestingly, we see that only vocational trainees (T3) see any significant increase in the likelihood of self-employment: consistent with them having acquired sector-specific skills. At the same time, we do not see any decrease in casual wage employment in any treatment group, so treated workers do not appear to substitute away from insecure forms of causal work towards wage employment. Rather, their overall attachment to the labor market increases, and this pattern of results is reinforced when examining the total effects margin below.[28]

We asked workers involved in any matching treatment, whether they were employed at the same firm that they were originally matched to. Remarkably, Column 5 shows high rates of retention for on-the-job trainees as measured up to 36 months post-placement: on average over this period, 11.5% of all workers assigned to this treatment arm are at the same firm as they were initially matched to. Recall from Table A3 that in this treatment arm: (i) 45% of workers were offered a match; (ii) 80% of workers offered a match actually met with a firm; (iii) 71% of workers accepted such offers. Hence the highest feasible percentage of workers that could have been in retained is $.45 \times .80 \times .71 = 26\%$. In contrast to other wage subsidy studies, we find over the post intervention period, an average of a third of these workers are actually still employed, up to 30 months after the wage subsidy has expired.[29]

Figure 4 plots the survival function for employment in the firm matched to for workers in the wage subsidy treatment, among those hired by the firm: (i) the share of FT workers who were employed at the matched firm for at least 6 months, conditional on starting is 57%; (ii) the average duration of employment at the matched firm, conditional on being higher than six months is 8.9 months. This starts to highlight the dynamic difference in labor market profiles between VT and FT workers: all find employment but the latter get stuck in the same job, while the former move up the job ladder as they make job-to-job transitions. They are able to do so because their skills are certifiable and so can be signalled to potentially new employers. The later model structurally estimates these transition rates.

Finally, the matching treatments (T4, T5) shed light on the nature of search frictions in these labor markets. Extensive margin outcomes for vocational trainees with and without the match are never statistically different: this suggests firms do not face search frictions in finding trained workers. In contrast, there are significant impacts on the extensive margin of a worker having matched with a firm when they first transition into the labor market: these impacts are non-

[28] The notion that training youth in low-income contexts increases their attachments to labor markets is further reinforced if we consider the sectors that workers end up being employed at. Conditional on employment, we find the likelihood of a VT or FT workers being employed in one of the eight sectors we focus on to be around double that for workers in the control group.

[29] For apprentices retained for the duration of the wage subsidy, those with above (below) the median employment spell duration with the firm have an average earning of \$47.6/month (\$40.9/month), so that earnings of retained apprentices do rise slightly over time from the wage subsidy of \$39/month (the top 1% earnings are excluded from the analysis).

trivial as employment probabilities increase by 6.2pp or 14% over the control group. These results highlight that search frictions firms face relate more to them being able to match with workers with some attachment to the labor market (T5 versus C) as signalled by their willingness to apply for vocational training in the first place, rather than meeting suitable skilled workers *per se* (T4 versus T3). An alternatively interpretation, in line with what has been documented in the literature on wage subsidies in low-income settings [Galasso *et al.* 2004, Levinsohn *et al.* 2014, Beam 2016, Groh *et al.* 2016], is that this captures a 'placebo impact' on workers [McKenzie 2017], in terms of workers exerting more effort in job search and becoming more confident in approaching firms.[30]

3.3.2 Total Effects

Table 6 presents ITT estimates for total effect margin outcomes. Column 1 shows that in terms of months worked in the year, FT and VT workers significantly increase their labor market attachment: the magnitude of the effect is 1.1 months for VT workers, significantly larger and over double that for FT workers ($p = .031$), and corresponding to a 25% increase over the months worked by the control group. Column 2 shows that in terms of hours worked per week, VT workers also increase their labor market attachment relative to the control group. Combined with the findings on the extensive margin in Table 5, the picture that emerges is that pre-intervention these young labor market entrants are underemployed: they are willing to increase their overall labor supply and use their idle labor if given the opportunity to do so (rather than reallocating hours across activities).

Column 3 then focuses on hourly wage rates (set to zero for the unemployed). Given the skewed demographics of Uganda with a large supply of young workers, and a large mass of SMEs in the sectors we study, then absent any search frictions, we might reasonably expect labor markets to be competitive and workers to be paid close to their marginal product. Hence examining wage impacts can be informative of productivity impacts. Building on the earlier results on worker skills in Table 4, we indeed see further evidence of there being wage/productivity impacts of both training routes. For FT workers, hourly wages rise by 61% relative to the control group, and for VT workers they rise by 52%. In line with the earlier skills results, both types of trainee appear to be more skilled than the control group, albeit with a different balance between sector- and firm-specific skills.

Column 4 combines multiple total effect margins to derive ITT impacts on total monthly earnings. These rise across all treatments: FT worker earnings rise by 22% over the control group, but VT workers experience a rise of 40% – double that of FT workers, driven by increases in hours worked as well as wages per hour. The final Column presents LATEs for earnings, as these will be useful for the later analysis of rent sharing. We see that among workers that match with a firm,

[30] Other studies, such as Franklin [2016], find large employment impacts from small subsidies enabling workers to extend their geographical search: this again is counter to the view that credit constraints are the sole factor limiting job search and holding back employment outcomes.

the earnings impact on FT workers is 48% and the impact for vocational trainees is 185%.

Two further points are of note. First, these are large experimental returns in earnings to both forms of training, as measured over a three year post-intervention horizon. Why don't workers self-invest in either vocational or apprenticeships given such returns? One explanation is that workers are credit constrained: as documented earlier, worker monthly earnings at baseline are \$5, while the vocational training costs over \$400, and taking the evidence from Table 3, if workers were to self-finance an apprenticeship it might cost upwards of \$500. On the firm side, they might face hiring/screening costs: the match results (T5) suggest SMEs do face costs of finding workers with labor market attachment for example, or that there are placebo effects on workers from meeting with firms *per se*, or just even the offer of a match, even if they are not ever hired by the firm.

An alternative explanation is that workers have incorrect beliefs about the returns to training [Jensen 2010]. We assess thus using information collected from workers at baseline over what they expected their earnings to be if they received vocational training (and conditional on employment). This evidence is shown in Table A6: Column 1 report worker beliefs at baseline, over the average monthly earnings given their current skill set (assuming they were employed). These correspond to just under \$60. We then asked workers what they expected their maximum and minimum monthly earnings to be if they received vocational training (and the likelihood they would be able the midpoint of these two). Fitting a triangular distribution to their beliefs we can derive an expected earnings from vocational training. This is shown in Column 2: on average, workers report earnings would more than double, so a greater than 100% return. This is higher than the experimental (or structural) returns (indeed, it is higher than the Mincerian returns shown in Table 2). Hence, workers are optimistic with regards to the returns to training, and this is not a valid explanation for their lack of investment.

Second, we emphasize that the overall impact on earnings combines: (i) an extensive margin employment effect (Table 5, Column 1); (ii) a composition effect, namely those that are employed are a select group of workers; (iii) a productivity effect, namely the causal change in earnings of those employed. We thus follow Angrist *et al.* [2006] and Attanasio *et al.* [2011] in estimating bounds for the treatment effect on this last component, on productivity. Table A7 gives a detailed breakdown of the bounds, and they are graphically summarized in Figure 5 by treatment arm. This shows that all the treatments have productivity bounds strictly above zero, with these bounds being slightly higher for vocational trainees. These results reinforce the notion that both training routes have substantive impacts on the human capital of workers.

A contribution we make to the training literature is to quantify the causal productivity impacts on workers of on-the-job training. Much of the earlier evidence has been based on observational data and there has been a long-standing debate over whether there are substantive human capital impacts of such training [Blundell *et al.* 1999], especially once the endogenous selection of workers into training is corrected for [Leuven and Oosterbeek 2008]. Our treatment effect estimates are not subject to this concern. Moreover, our two-sided experimental design allows us to later shed

light on the potential selection of firms that VT and FT workers end up being employed at, that partly drive the treatment effects documented above.

3.3.3 Robustness

The Appendix presents robustness checks on our findings by first combining multiple labor market outcomes from Tables 4 and 5 into one labor market index. In Table A8: (i) we show treatment impacts on this labor market index by gender, by sector, and by geography; (ii) we exploit the fact that there is a small batch of vocationally trained workers that are trained later in time and so closer chronologically to when apprenticeships were being implemented, to show that there is little impact on the exact timing of entry into the labor market on this labor market index as measured three years post-intervention. For these checks, we also present Lee bounds treatment effect estimates and address multiple hypothesis testing concerns by showing the core results to be robust to comparing critical values to unadjusted and adjusted Romano-Wolf p-values.

4 Job Ladder Model

The treatment effect estimates document impacts averaged across the three post-intervention waves. We now examine whether and how the dynamic impacts of the supply- and demand-side training interventions differ. As motivation, Figure 6 shows the outcome of estimating a specification that allows treatment effects to vary by survey wave. For outcomes related to employment and earnings (Panels A and B), as well as an overall index of labor market outcomes (Panel C), there are dynamic treatment effects. In particular, it is worth contrasting the steady improvement in outcomes among vocational trainees, with the more static outcomes of youth entering the labor market through on-the-job training. The job ladder model of worker search we now develop and estimate helps pinpoint the exact mechanisms driving these dynamics. The labor market features the model estimates are steady state unemployment-to-job transition rates (UJ) and job-to-job transition rates (JJ). If VT workers have more certifiable skills than FT workers, and their balance of skills is more tilted towards sector- rather than firm-specific skills, then they should make such transitions more frequently, all else equal. As a result they will more quickly move up the job ladder and their wage profiles will diverge away from FT workers, as suggested in Figure 6.

4.1 Set-up

We develop a standard job ladder model of worker search. Workers are assumed risk neutral, their treatment (training) status is denoted by T, and for any given T (where $T = VT$, FT etc.), workers are homogeneous except for their employment status in any period: a worker can be unemployed (u) or employed (e). We assume workers have reached their steady state labor market trajectories/transition rates by November 2015, so more than two years since the end of

vocational training. Firms post a wage w and make take-it-or-leave-it offers, where the offer is a commitment to pay wage w to the worker until the worker is laid off or quits. Employed and unemployed workers sample wages from the same distribution $f(w|T)$, with CDF $F(w|T)$.[31]

Unemployed workers choose search intensity c each period, and their value function is:

$$V^n(T) = -\varphi(c) + \beta \left[\lambda_0(c,T) \max \left\{ \int V(w,T) dF(w|T), V^n(T) \right\} + (1 - \lambda_0(c,T)) V^n(T) \right]. \quad (2)$$

We assume unemployed workers earn zero every period, so the first term is the cost of search effort, $-\varphi(c)$. β is the discount rate. In the next period the worker receives a job offer with probability $\lambda_0(c,T)$, that depends positively on her search effort c and training status T. The worker takes up this job offer if the expected value of the job is higher than the value of remaining unemployed. With probability $(1 - \lambda_0(c,T))$ no job offer arrives and the worker remains unemployed.

Employed workers choose search intensity c and whether to accept or reject wage offers (their reservation wage), and their value function if employed at wage w is:

$$V(w,T) = w - \varphi(c) + \beta \left[\delta V^n(T) + \lambda_1(c,T) \max \left\{ \int V(w,T) dF(w|T), V(w,T) \right\} + (1 - \delta - \lambda_1(c,T)) V(w,T) \right] \quad (3)$$

where such workers are assumed to be able to search on-the-job at the same cost $-\varphi(c)$ as unemployed workers. In the next period, the worker's employment terminates with probability δ: this job destruction rate captures both the quality of jobs and the expected duration of the employment relation. With probability $\lambda_1(c,T)$ the worker receives an outside job offer that is also increasing in c and T. She takes up this opportunity if the expected value of the job offer exceeds the current job value: this is the notion of a job ladder. With probability $(1 - \delta - \lambda_1(c,T))$ the worker's job neither destructs, nor does she receive an outside offer and so she remains in her current job.[32]

The model makes precise that training affects behavior through two mechanisms: (i) the probabilities of receiving job offers: $(\lambda_0(c,T), \lambda_1(c,T))$; (ii) the distribution of offered wages $(F(w|T))$. Through these mechanisms training impacts the worker's endogenous choices over search effort (c) and whether to accept or reject wage offers (reservation wage).

[31] It might be reasonable to expect that $F(w|T=1)$ FOSD $F(w|T=0)$ so that trained workers are more likely to receive higher offers: we let the estimation inform us on this and do not assume any ranking of these distributions. There is an established literature on job ladder search models [Bontemps *et al.* 2000].

[32] There is no wage bargaining in this set-up. Our empirical setting is not well suited to such a version of the model: unionization rates are less than 1% in Uganda, and the demographic structure ensures there is no shortage of potential labor hires available. Both factors dampen workers bargaining power [Rud and Trapeznikova 2016]. The model also assumes a stationary environment so that there is no accumulation of human capital or assets over time, or directed search/memory. Such extensions lie outside the scope of the current paper.

4.1.1 Supportive Evidence

We provide three pieces of evidence supporting the basic structure of this simplified job ladder model in our context, relating to: (i) wage growth; (ii) expectations over job offers; (iii) search behavior. First, the model predicts wage growth occurs between, not within, job spells because a worker's wage only increases when making JJ or UJ transitions. Within a spell, the wage is fixed at w. To examine this prediction we decompose workers' wage growth into that occurring within and between job spells. When doing so over a reference period of a year, we find the average wage growth of job movers is at least twice as high as that of job stayers, irrespective of the exact reference period used.[33]

We next present reduced form ITT estimates for outcomes related to the mechanisms in the structural model. The results are in Table 7. In Column 1 we examine how each treatment impacts workers stated belief that they will find a job in the next six months (on a 0-10 scale), relating to $\lambda_i(c, T)$. We see that FT and VT workers are both significantly more optimistic than the control group. Moreover, VT workers are significantly more optimistic than apprentices in this regard ($p = .000$). Columns 2 to 4 show how this translates into expected wage offers, relating to $F(w|T = 1)$ in the model. FT workers do not expect their offered wages to be any higher than the control group. In sharp contrast, VT workers expect a rightward shift of the distribution of $F(w|T = 1)$ so their minimum and maximum wage offers are significantly higher. Using information on their report of how likely their wage is to be above the midpoint of the two and fitting a triangular distribution, Column 4 shows VT workers' expected offer wages to rise by around 35% relative to the control group. Finally, the last two Columns in Table 7 examine worker search effort, relating to c in the model. In line with offered wages, VT workers are significantly more likely to report actively searching for work, and also to switch towards using more formal search channels. Strikingly, for five out of six outcomes in Table 7 relating to mechanisms in the model, the response of VT workers is significantly higher than that of FT workers.

However, because these reduced form ITT impacts average over unemployed and employed workers, and workers with different employment histories, it is hard to precisely interpret the findings. While all the evidence is suggestive of the main assumptions and mechanisms of the job search model being at play in our setting, we need to structurally estimate the model to properly quantify the mechanisms for each treatment.

[33] To decompose worker's wage growth, we first exploit the fact that for each job spell we have information on the wage in the first month and the last month of the spell. We then choose some reference date and linearly interpolate wages from the first and last month of the spell ongoing at the reference date. We then calculate the wage growth between two reference dates (e.g. between April 2015 and April 2016) for: (i) workers employed in the same job throughout the reference period (job stayers); (ii) workers who change job at least once in the reference period (job movers). To avoid sensitivity to outliers, the top 1% of wages are excluded. Self-employed workers and workers with at least one unemployment spell in the reference period are excluded. We then take the ratio of the average wage growth of the job movers to job stayers. Using the reference period of April 2015 to April 2016 this ratio is 2.06 (the ratio of medians is 2.31).

4.2 Steady State

To close the model we derive steady state conditions. We make the simplifying assumption that $c = \phi(c) = 0$. The implication is that the UJ and JJ transition probabilities, $\{\lambda_0(T),\ \lambda_1(T)\}$, combine worker and firm search effort, leading to job offer arrivals. As unemployed workers are homogeneous, firms have no incentive to make a wage offer to an unemployed worker that she would refuse, so we assume unemployed workers always accept any offer received in equilibrium. In steady state the following condition must then hold for unemployed workers, where N is the total size of the labor force, and u is the unemployment rate:

$$Nu\lambda_0 = N(1-u)\delta, \tag{4}$$

The left hand side is the outflow from unemployment into work, and the right hand side is the inflow into unemployment as jobs destruct. Hence the steady state unemployment rate is:

$$u = \frac{\delta}{\delta + \lambda_0}. \tag{5}$$

For employed workers with wages $\leq w$, the following flow condition defines the steady state:

$$[\delta + \lambda_1(1 - F(w))]N(1-u)G(w) = \lambda_0 F(w)Nu, \tag{6}$$

where the left hand side is the outflow of workers employed at wage w (layoffs plus quits due to better wage offers), and the right hand side is the inflow into employment from unemployment. The cross sectional CDF of accepted job values among the employed, $G(.)$, differs from the offer sampling CDF $F(w)$. $G(w)$ is observed in the data, while $F(w)$ is not. However, given (5), we can derive the following steady state relationship between $F(w)$ and $G(w)$:

$$F(w) = \frac{(\delta + \lambda_1)\, G(w)}{\delta + \lambda_1 G(w)}, \tag{7}$$

$$\frac{F(w) - G(w)}{(1 - F(w))G(w)} = \frac{\lambda_1}{\delta} = \kappa_1. \tag{8}$$

We see that $G(w)$ FOSD $F(w)$ unless there are no JJ transitions ($\lambda_1 = 0$), i.e. because on-the-job search leads to outside offers, there exists a wedge between offered and accepted wages. Finally, κ_1 measures the intensity of interfirm competition for workers (labor market tightness): it corresponds to the number of outside offers a worker receives before being laid off.

4.3 Estimation, Data and Descriptives

In the Appendix we detail the construction of the likelihood function. We then structurally estimate the model using maximum likelihood following the two-step procedure in Bontemps *et al.* [2000]. For each treatment arm T and the control group, the model estimates a separate set of parameters: δ, λ_0 and λ_1 (and their asymptotic standard errors). We can then straightforwardly derive steady state unemployment rates and interfirm competition (u, κ_1). To increase precision, we combine the treatments involving vocational training (T3 and T4). To be clear, as the model is estimated for each treatment arm and control group separately, we do not model the coexistence of differentially treated workers in the same labor market. In the next Section we examine within-firm employment displacement effects more directly.

To estimate the model we convert our panel data into a job spells format: for each worker i we construct a complete monthly history of their employment status $e_i \in \{0, 1\}$ from August 2014, when the matching interventions took place, to November 2016, our endline survey date. We assume workers have reached their steady state trajectories by November 2015. Consistent with the model, we set one wage per employment spell, w_i, and then estimate transition probabilities (τ_{UJ}, τ_{JJ}) using a maximum of two spells since the steady state has been reached.[34]

Table A9 provides descriptive evidence on steady state employment spells, pooling the data across treatments. We see that the steady state unemployment rate is 52%: this is lower than among our workers at baseline, reflecting the fact that some young workers transition into employment. Initial unemployment spells (those being experienced in November 2015) are more likely to be right censored than initial employment spells. The average duration of employment spell is 16 months, and the duration of unemployment spells is 21 months: these are long periods out of the labor force, and initial spells of unemployment when first transitioning into the labor market might have lasting impacts on later labor market outcomes. Hence any impact either training route has on this margin can have substantial impacts on the lifetime welfare of young workers.

4.4 Results

Table 8 presents the structural estimates. Panel A shows the parameter estimates (δ, λ_0, λ_1, κ_1, u) for the control group and each treatment arm. On job destruction rates δ in steady state: (i) vocational trainees end up being employed in jobs with slightly lower job destruction rates

[34] August 2014 coincided with the start of the recall period for job spells reported in the second follow-up worker survey. Hence we use this starting point to balance the trade-off between sufficiently far from the intervention period, and using a wide enough span of worker employment history data to precisely estimate the model. Figure A3 illustrates some of the possible cases how the worker spells data is constructed for worker i, where the spell duration is d_i and transition indicators between spells are τ_{JU_i}, τ_{JJ_i}, τ_{UJ_i}. In the top panel, we consider a scenario in which worker i is unemployed in November 2015, the unemployment spell is not left censored, and the worker transitions into employment (so τ_{UJ_i} is recorded) after d_i months of unemployment. The bottom panel considers a scenario in which worker i is employed in November 2015, the employment spell is left censored, and the worker transitions into a new state after d_i months of employment.

than do FT workers; (ii) both groups of trained workers have job destruction rates lower than the control group of workers, hinting that trained workers have better jobs or greater employment spell durations.[35]

On transition rates, we see that for UJ transitions (λ_0): VT workers have transition rates that are 25% higher than FT workers, and also higher than the control group and matched workers. For JJ transitions (λ_1) VT workers transition rates that are 15% higher than FT workers. Indeed, they again have the highest transition rate, while FT workers have the lowest transition rates of any group of workers. We thus see the stark difference in labor market mobility of vocationally trained workers relative to those that transition into the labor market through apprenticeships. FT workers receive fewer job offers when employed, and as suggested by the reduced form evidence in Table 5, they get stuck at their initial firm placement and do not move up the job ladder. In contrast, vocational trainees are far more mobile: when unemployed they get back onto the job ladder more quickly. When employed, they are more likely to receive outside offers, that they only accept if the value of the offered job is greater than their current one. As a result of these key dynamics, vocational trainees pull away from FT workers in terms of their overall labor market performance: this is all consistent with the earlier evidence from Figure 6, except the structural model precisely quantifies the mechanisms driving this difference. These dynamics are all in line with VT workers having more certifiable skills than FT workers, and their balance of skills being more tilted towards sector- rather than firm-specific skills.

This all feeds through into the inter-firm competition for VT workers: $\kappa_1(VT) > \kappa_1(FT)$ so that in steady state they receive 1.79 job offers per employment spell, while FT workers receive 1.46 job offers per employment spell. This is driven by vocational trainees receiving more outside job offers when employed ($\lambda_1(VT) > \lambda_1(FT)$), and also because their job destruction rates are lower ($\delta(VT) < \delta(FT)$).

On steady state unemployment rates, all treated groups are considerably better off than the control group. In other words, relative to the counterfactual of no intervention, both demand- and supply-side policies effectively reduce youth unemployment for treated workers. The impact on FT workers is to lower their unemployment rates in steady state by 1.35pp (corresponding to a 2.3% reduction); (ii) for VT workers the reduction is 8.51pp (14.4%); (iii) for matched workers, the reduction is 7.50pp (12.7%). All of these impacts are of economic significance, assuming there are no displacement effects. This is an issue we tackle in the next Section.

Panel B focuses on offered and accepted wage CDFs, $F(.)$ and $G(.)$. When unemployed, the offered wage distribution for VT workers, FT workers and the control group are similar. This is in line with our modelling assumption that unemployed workers take any job. The steady state gap between treatment arms opens up in relation to the distribution of *accepted* wages, $G(.)$. Indeed,

[35] The estimated destruction rates match closely other literature estimates. In particular, Rud and Trapeznikova [2016] estimate a different structural model of job search suing UNHS 2010-11 data for Uganda and find a very similar implied annual job destruction rate of 32% as we find for the control group.

$G(VT)$ FOSD $G(FT)$ and $G(C)$. This is as expected: vocational trainees have moved further up the job ladder, and so are only willing to accept jobs with sufficiently high wage offers.

The implied annual earnings impact of having received on-the-job training in steady state relative to the control group is \$37 (corresponding to an 11% increase), the implied annual earnings impact of vocational training is \$107 (34%) and the implied earnings impact of matching is \$89 (29%). These are all far larger than the percentage impacts on steady state unemployment rates, that reinforces the fact that each training route not only reduces unemployment risk for workers, but also leads to higher wages when employed, consistent with the earlier reduced form evidence on the human capital impacts and productivity bounds results of each training route.

Finally, contrasting the experimental and structural returns to training, we saw in Table 6 the treatment effect on earnings from vocational training were 40%. The steady state returns estimated are just slightly lower at 34%. In contrast, the treatment effect on earnings from firm-training was 18%, and the structural model estimates are just over half that at 12%. This contrast arises because the steady state calculations account for the lower UJ and JJ transition rates of FT workers: in steady state they do not move up the job ladder as fast as VT workers, and the control group of workers slowly catches up with them. This dynamic is masked by the reduced form impacts that measure average effects in the three post-intervention survey waves.[36]

5 Firm Responses

We now examine labor demand side responses to each training intervention. This exploits the two-sided experimental design and allows us to measure: (i) the kind of firm workers end up being employed at: this allows us to understand whether the differential returns to workers of vocational and firm-provided training are partly due to the fact that workers end up being attached to differentially productive (and so better paying) firms in each training route; (ii) employment displacement impacts: this allows us to address a key challenge training evaluations have faced, in measuring whether training workers has impacts on net employment, or just reallocates the same number of jobs to different workers; (iii) impacts on firm profits: this is important because if workers are more mobile under vocational training (as suggested by the job ladder model), then FT workers should not capture the full social surplus to their skills because of their lack of labor market mobility.

Figure 1B summarizes the experimental design from firms' perspective. Recall that we drew a sample of 1538 SMEs, operating in one of the eight sectors of interest, and having between one

[36] An alternative hypothesis for these dynamics is that the training routes differ in how they enable workers to learn-how-to-learn, rather than enhancing their productive capacity *per se* [Neal 2017]. Dynamic impacts are then driven by intertemporal complementarity in worker's capacity to learn. Although it is difficult to find skills that impact learning capacity but not productivity, we partially explore this hypothesis by estimating whether workers cognitive abilities, and other preference parameters, change over time and differentially by training route. We do not find any evidence of such mechanisms in our setting.

and 15 employees (plus a firm owner). Firms are assigned to either be held in a control group (T0) or to be matched with: (i) an untrained worker and offered a wage subsidy for six months to hire the worker (T2); (ii) a vocationally trained worker (T4); (iii) an untrained worker (T5).

5.1 Selection

5.1.1 Treatment Effects

To understand whether and how firm-side selection drives returns to training routes, we first consider the characteristics of firms that initially express an interest to meet a worker they are matched to. Taking firms in treatments T2, T4 and T5, Table A10 shows the firm characteristics that predict firm's expressing an interest to meet at least one worker they are matched to. Firms assigned to the match-only treatment T5 are the omitted category. Column 1 shows that firms incentivized with wage subsidies are significantly more likely to express an interest in meeting the worker relative to firms in the pure matching treatments. The magnitude of the effect, at 30%, is in line with the descriptive evidence on firm interest shown in Table A3. Column 2 shows this to be robust to controlling for firm characteristics, and these characteristics (firm size, profits per worker, capital per worker) do not predict firm interest. However, Column 3 shows that firms interested in the wage subsidy treatment have significantly lower profits per worker than firms interested in the other matching treatments. Hence, those firms interested in meeting workers when given a wage subsidy incentive appear to be negatively selected relative to firms offered to meet with similar workers but absent any financial inducement. The lower returns to FT workers relative to VT workers might in part reflect this *initial* match with less profitable firms.

5.1.2 Heterogeneous Firms in the Job Ladder Model

We can extend the job ladder model to estimate the productivity of firms that workers in each training route end up being employed at *in steady state*. To add firms to the job ladder model we first assume they only use labor inputs with a CRTS technology. For a given treatment/training route T, workers are homogenous and assumed to supply one unit of labor per period, but firms are heterogenous in their productivity p, distributed with CDF $P(.)$. A firm is just a collection of jobs with equal wage w. Let M denote the total number of firms, and recall that $\kappa_1 = \frac{\lambda_1}{\delta}$ measures inter-firm competition for workers. The average firm size for those offering wage w is given by:

$$n(w) = \frac{(1-u)N}{M}\frac{g(w)}{\gamma(w)} = \frac{(1-u)N}{M}\frac{1+\kappa_1}{[1+\kappa_1(1-F(w))]^2}\frac{f(w)}{\gamma(w)}, \tag{9}$$

where $g(w) = G'(w)$, obtained by differentiating (7), and $\gamma(w)$ denotes the density of job offers at wage w in the population of firms, and $\frac{f(w)}{\gamma(w)}$ is the sampling weight of type-w firms. With random search, $\frac{f(w)}{\gamma(w)} = 1$ and the size of firms offering wage w is, $n(w) = \frac{(1-u)N}{M}\frac{1+\kappa_1}{[1+\kappa_1(1-F(w))]^2}$. Profits for

firms of size $n(w)$ are,

$$\pi(p,w) = (p-w)n(w) \propto \frac{(p-w)(1+\kappa_1)}{[1+\kappa_1(1-F(w))]^2}, \tag{10}$$

The wage offer function solves the firm's maximization problem,[37]

$$w(p) = \arg\max_w \frac{(p-w)(1+\kappa_1)}{[1+\kappa_1(1-F(w))]^2}, \tag{11}$$

The wage offer function can be inverted to give:

$$p(w) = w + \frac{1+\kappa_1 G(w)}{2\kappa_1 g(w)}. \tag{12}$$

This is used to retrieve the underlying $P(.)$ needed to rationalize the observed distribution of wages. Hence in this extended version of the job ladder model, the firm productivity distribution is a non-trivial function of the CDF of accepted wages $G(w)$ (and hence of offered wages, $F(w)$).

The result is in Panel C of Table 8. Focusing on the average productivity of firms workers are employed at in steady state, we see that relative to firms that the control group of workers end up employed at: (i) FT workers end up in firms with 30% *lower* productivity; (ii) VT workers end up in firms with 55% *higher* productivity (albeit with a high dispersion of productivity); (iii) matched workers end up in firms that are 46% lower productivity.

Pulling together the above evidence on firms with the earlier evidence from workers suggests the differential returns to training route are reflective of at least three channels: (i) the fact that FT workers have less certifiable skills than VT workers; (ii) the differential forms of human capital impacted to workers through each training route, as evidenced in Section 3.1 and 3.2; (iii) the productivity of firms that VT and FT workers end up initially matched with, and in steady state, as evidence above.[38]

5.2 Employment Displacement

The job ladder model implies large reductions in steady state youth unemployment rates from both supply- and demand-side training policies for beneficiary youth. We now consider the wider impacts of the treatments, exploiting the firm-side experiment to measure whether within-firm

[37] We consider the Nash equilibrium of the wage posting game, played by a large number of firms, assuming firms maximize their steady state profit flow subject to the workers' reservation wage policy. We focus on pure strategy equilibria, where all type-p firms post a unique wage $w(p)$. It can be shown that $w(p)$ is increasing in p, so that more productive firms pay higher wages.

[38] To fully disentangle the relative importance of these three channels would require a substantively more complex model. For example, the job ladder model has no wage bargaining so does not allow for differential monopsony power of firms across treatments. Differences in monopsony power could show up in the $F(.)$ and $G(.)$ functions faced by FT workers, that translates into productivity estimates in the extension.

employment displacement effects exist. Given the low frequency of actual employment matches, we estimate the firm-side impacts of being offered to meet a worker, where we instrument the offer with treatment assignment. We measure this impact as averaged over the four post-intervention survey waves of firm side data (that run until July 2017 as shown in the lower panel of the timeline in Figure 2). This is long after any wage subsidy has expired.[39] We thus estimate the following specification for firm f in randomization strata s:

$$y_{fst} = \sum\nolimits_j \beta_j Offer_f + \gamma y_{f0} + \delta \mathbf{x}_{f0} + \lambda_s + \vartheta_t + u_{fst}, \tag{13}$$

where y_{fst} is the firm outcome of interest in post-intervention survey wave t ($t = 1...4$), $Offer_f$ is a dummy equal to one if the firm is offered to meet with a worker, that is instrumented by the treatment firm f is assigned to, T_f. y_{f0} is the same firm outcome at baseline, $\mathbf{x}_{f0}$ are the firm's baseline covariates and λ_s and ϑ_t are strata and survey wave fixed effects respectively. We cluster standard errors by sector-branch, and we weight observations using inverse probability weights. The coefficient of interest is β_j: the LATE impact of being offered a worker as averaged over the four post-intervention survey waves.[40]

The results are shown in Table 9. Column 1 shows that in wage subsidy treatment, matched workers are significantly more likely to be retained within the firm four years post-baseline and well after wage subsidies have expired. However, Column 2 reveals that there is no impact on overall employment in firms that were offered wage subsidies, so that other workers are fully displaced by hired apprentices. The treatment therefore does lead to a change in the allocation of jobs to workers by allowing some to queue jump, but there is full employment displacement of other workers not part of our evaluation sample.[41]

For the other treatments involving matching, workers are retained within firms too infrequently to say anything on employment displacement impacts on firms (and this is true even if we restrict ourselves to the short run as measured in the first follow-up firm survey).[42]

[39] As Figure 2 shows, the fourth follow-up takes place 48 months after baseline, being fielded between March and July 2017. On-the-job training and wage subsidies are paid between January and August 2014.

[40] The baseline firm characteristics $\mathbf{x}_{f0}$ controlled for are owner's gender, firm size at baseline and owner's years of education. The strata are BRAC branch and sector fixed effects. The instruments for the IPW estimates are a dummy for whether the owner reported at baseline an intention to relocate in the future, and the number of firms reported to be economically or socially connected to the firm owner at baseline.

[41] Our two-sided experimental design adds to a nascent literature examining impacts of wage subsidy programmes on firms (above and beyond the impacts on workers). Mel *et al.* [2016] conduct a field experiment in Sri Lanka that provides wage subsidies to SMEs. They find firms increased employment during the subsidy period, but there was no lasting impact on employment, profitability, or sales. McKenzie *et al.* [2016] also find positive (short run) employment impacts of a wage subsidy and matching intervention with firms in Yemen. Hardy and McCasland [2017] evaluate an apprenticeship program with small firms in Ghana, and find firms retain this extra labor for at least six months, and earn higher profits in doing so.

[42] In results not shown: (i) these findings on retention and net employment in firms also hold for manufacturing and service sector firms; (ii) the findings are robust to estimating a LATE specification that replaces the offer of a match with a worker in (13) with a dummy for whether a worker-firm meeting actually took place.

5.3 Rent Sharing

Given the majority of workers are retained even after the wage subsidy has expired, our two-sided experiments allows us to measure whether their (past) presence impacts firm profits. The result is shown in Column 3 of Table 9. Firms offered a wage subsidy have 13.2% higher monthly profits on average over the four-year period than control firms [$p = .015$]. Column 4 shows no capital stock adjustments take place in these firms, and so given no change in net employment, this all suggests the additional profitability arises from the higher productivity of workers hired under wage subsidies than would have been hired.

We can use the estimate on firm profits from Column 3 in conjunction with the wage impacts to assess the degree of rent sharing between firms and workers over the post-intervention period: the firm-side impact of being offered a wage subsidy is .132 x 191 = \$25.2/month. On the worker-side, we can estimate the total effect on monthly wages from having entered the labor market as an apprentice using the analogous LATE estimate shown in Table 6: \$13.6/month. Hence, in terms of rent sharing, 35% of the surplus generated by apprenticeships goes to the worker. The fact that the majority of the surplus is captured by firms highlights that there are labor market frictions that prevent FT moving costlessly to other firms: these likely relate to the non-certifiable and firm-specific nature of their skills, that increases firms monopsony power over the worker.

Finally, we note that intriguingly, Table 9 also shows that firms that matched with untrained workers (T5) also have significantly higher profits than control firms. In our companion paper, Bandiera *et al.* [2017] we provide a more detailed analysis of the longer term impacts on firms of these treatments (and other treatments implemented) on the firm-side of our experiment.

6 Discussion and Directions for Future Work

Youth unemployment and underemployment are key policy challenges in the developing world. The transition into the labor market marks a key stage in the life cycle, and a body of evidence documents how initial experiences and first job opportunities during this transition have persistent impacts on later employment trajectories and welfare [Becker 1994, Pissarides 1994]. This paper provides experimental and structural evidence on this transition from a novel two-sided experiment in the context of urban labor markets in a low-income country: Uganda.

Workers in our evaluation are disadvantaged youth transitioning into the labor market. Relative to youth in the more developed nations these individuals: (i) have acquired lower levels of human capital from the formal schooling system; (ii) have fewer options through which to raise their human capital before they seek employment, say through colleges or tertiary education; (iii) face stiff labor market competition from other youth entrants because of Uganda's highly skewed population pyramid. Firms in our evaluation are small and medium size enterprises (SMEs). Again, given the highly skewed firm size distribution in Uganda relative to the US, the majority

of employment occurs in such SMEs and as such, these firms are key drivers of youth employment in this developing economy.

The experiment is designed to shed light on the two most prominent explanations for high youth unemployment rates: that young workers lack skills relevant for the labor market, or because they face barriers to labor market entry. It does so by contrasting a supply-side (vocational training) versus a demand-side intervention (wage subsidies to firms to hire and train workers on-the-job) to tackle youth unemployment. Relative to control workers, we find high labor market returns for both vocationally-trained (VT) and firm-trained (FT) workers: hence both policies have a role to play in helping young workers acquire human capital and transition into the labor market. However, from the worker's perspective, the returns to VT are near double those to FT, and the main mechanism for the divergence in wage profiles between VT and FT workers is the greater labor market mobility of VT workers: they receive higher rates of job offers when unemployed, and higher rates of job-to-job offers.

We conclude by: (i) estimating the internal rate of return (IRR) to each training intervention; (ii) discussing the external validity of our findings, linking this precisely to policy implication and directions for future research.

6.1 IRR

The supply- and demand-side interventions we evaluate are costly big-push style policies. Hence, it is important to establish whether the returns are sufficiently high to warrant a social planner implementing either policy. Table 10 presents the IRR calculations for each treatment arm, where our benchmark case assumes a social discount rate of 5%.

Panel A shows the breakdown of costs in each treatment. The first line shows the total cost, made up as follows: (i) training costs: the cost per individual of vocational training was $470, while the wage subsidy amounted to $302 per trainee ($50.2/month for six months); (ii) program overhead costs: these vary by treatment depending on whether worker-firm matches needed to be organized, the firm monitored etc.; (iii) the opportunity cost to workers of attending the vocational training: these turn out to be relatively small (comprising less than 10% of the total cost) because levels of youth unemployment and underemployment are so high.[43]

Panel B shows the NPV of the lifetime earning gains, as derived from the job ladder model. We first assume the remaining expected productive life of beneficiaries is 38 years (calculated as the average life expectancy in Uganda in 2012 minus the average age of workers at baseline). The lifetime gains to FT workers are around a third of those accruing to VT workers. The benefit-cost ratio is above one for both interventions: it is 1.69 for FT workers and over 4 for VT workers.

[43] The tracker survey administered to workers randomized out of VT and interviewed just as vocational trainees were completing their courses confirms this is so: workers that are randomized out of vocational training find few employment opportunities during the six months when other workers are being trained.

The ratio falls to one for FT if the social discount rate doubles to 10%. Finally, the IRR to each treatment arm is 9.7% for FT, and almost 25% for VT.[44]

Panel C shows the sensitivity of these findings to alternative assumptions on: (i) the remaining productive life of beneficiaries; (ii) varying the foregone earnings from having to attend vocational training. As productive life shortens, the IRR to FT via wage subsidies drops off quickly, but not for VT. This is as expected given the different wage profiles to the interventions. however it remains the case that the FT intervention still pays for itself in 10 years (over that time frame the IRR = 0). On foregone earnings, only under very extreme assumptions does the IRR for VT ever fall below 10%.

These calculations are based on the cost structure of the NGO BRAC that we collaborated with. This is an established NGO in Uganda. Hence the program overhead costs represent the *marginal* costs to them of extending their activities to the kind of training program evaluated. To get a more accurate sense of the social return of starting up such programs from scratch, Panel D shows what the *total* cost per individual would have to be in order for the IRR to equal the social discount rate, 5%. We see total costs per beneficiary would have to increase from $368 to $621 for the wage subsidy intervention to break even, and total costs would have to rise fourfold for vocational training to break even. The final row performs the same calculation assuming a 10% social discount rate. In this case the wage subsidy intervention would just break even and the costs for vocational training would still need to more than double.[45]

6.2 External Validity

In meta-analyses of training interventions in low-income settings, Blattman and Ralston [2015] and McKenzie [2017] document most interventions have a very low IRR. Figure 7 compares our treatment impacts relative to the other experimental studies discussed in McKenzie [2017] (that all focus on low- and middle-income settings). We do so for two outcomes: (i) employment; (ii) earnings. Our effect sizes are large relative to earlier studies, although the ranking across treatment types is in line with earlier work (with for example, most studies finding that vocational training has higher returns than matching). We speculate over four reasons why our returns are high relative to other studies, each of which opens up avenues for future work.

First, there are design issues: our two-sided experiment has a precise sectoral focus limited to eight sectors. All workers receive vocational training in one of these sectors, and all sampled

[44] Two further points are of note. First, these IRR figures match up closely with the short and long run IRRs for the combined in-class vocational and on-the-job training intervention evaluated in Colombia by Attanasio *et al.* [2011, 2017]. Second, they likely underestimate the utility gains from each intervention as we measure benefits only through earnings, and take no account of reduced earnings risk, or how such human capital investments can reduce worker vulnerability to macroeconomic and other shocks over the longer term.

[45] Two other points are of note: (i) these IRRs do not change much even if we account for the profit impacts on firms that hire apprentices: that is because given employment spells in this economy, these represent relatively small increases in the NPV of the social surplus generated; (ii) it is clear that the matching intervention has a very high social return driven by the fact that the implementation costs are so low relative to the other treatments.

firms operate in one of these sectors (and have at least one employee plus a owner at baseline). This reduces the possibility of worker-firm mismatch. Moreover, our treatments are intensive. Specifically, we separate out in-class vocational training from a wage subsidy program, both last six months, and the wage subsidy treatment had a subsidy rate higher than some other studies.

Second, only 13% of workers attrit over our four year evaluation. This attrition rate compares favorably to other studies such as Attanasio *et al.* [2011] (18%), and Card *et al.* [2011] (38%). Indeed, in the meta-analysis of McKenzie [2017], all but one study has attrition rates above 18%. As Figure 7 shows, some other studies have notably more imprecise treatment effect estimates, that might in part arise from attrition. Moreover, our payment structures to VTIs ensured that the vast majority of workers completed training conditional on starting it, mitigating drop-out problems that earlier studies have faced.

Third, given our oversubscription design, workers that select into our sample are those willing to undergo six months of vocational training. On the one hand, such individuals might be negatively selected in that they are willing to bear the opportunity cost of lost labor market opportunities during the vocational training period. On the other hand, such individuals might be positively selected in that they are more patient than eligible youth in general (as our intervention ensures any benefits are back-loaded). A natural contrast for future work would be to recruit workers using the offer of a potential wage subsidy, that front loads benefits to workers.

Fourth, we worked with a limited set of VTIs in Uganda, pre-selected to be of high quality based on their reputation. There is no shortage of VTIs in Uganda, and as in other low-income contexts, there are concerns over a long tail of low-quality training providers existing in equilibrium. Hence, although our treatments essentially relax credit constraints for workers, it is not obvious the results would be replicated through an unconditional cash transfer: this would rely on workers having knowledge over training providers. Rather a conditional cash transfer (conditioned on having to attend one of these VTIs) is likely to have higher returns, all else equal. This might explain why similar programs providing vouchers to workers that are redeemable at VTIs for training have met with more limited success [Galasso *et al.* 2004, Groh *et al.* 2016].

In addition, the fact that workers can credibly certify the vocational skills obtained from a VTI is a key contrast between VT and FT workers, and is a key factor driving the differential wage profiles of the two forms of training: this serves as an important reminder of the value of certification in these developing labor markets. These avenues open up a rich set of research possibilities studying the market for VTIs as a whole.

A Appendix

A.1 Attrition

Table A4 presents evidence on the correlates of worker attrition. Attrition is generally low, with only 13% of workers attriting by the 48-month endline. Focusing on attrition between baseline and endline, Column 1 shows that: (i) attrition is uncorrelated to treatment assignment; (ii) worker characteristics do not predict attrition in general but workers that score higher on a cognitive ability test at baseline are more likely to attrit. Column 2 shows there to be little evidence of heterogeneous attrition across treatments by baseline cognitive scores at baseline. Any bias that might arise from selective attrition on unobservables cannot be signed *a priori*. Tracked workers would be negatively selected if attriters are more likely to find employment themselves, or they would be positively selected if attriters are least motivated to find work and remain attached to the labor market. To account for attrition, we weight our ITT estimates using inverse probability weights (IPWs). We also show the robustness of the main treatment impacts when using conditional Lee bounds [Lee 2009].

On the IPWs, we proceed as follows. At each survey wave t we define a dummy s_{it} such that we observe (y_{it}, x_{it}) for observations for which $s_{it} = 1$. We then first estimate a probit of s_{it} on z_{it} for each post-intervention survey wave separately, where z_{it} includes: (i) $\mathbf{x}_{i0}$: the vector of baseline covariates used as controls throughout in (1); (ii) strata and implementation round dummies; (iii) $\mathbf{z}_{i0}$, baseline measures excluded from regression analysis: dummies for orphan, anyone in household has a phone, willing to work in multiple sectors, and; (iv) the survey team the respondent was assigned to in each survey round ($Team_{it}$). The underlying assumption is that conditional on z_{it}, y_{it} is independent of s_{it}. $\hat{p}_{it}$ are fitted probabilities from this regression using survey wave t, and so at a second stage, we weight our OLS ITT estimates with weights $1/\hat{p}_{i1}$, $1/\hat{p}_{i2}$, $1/\hat{p}_{i3}$.

A.2 Robustness Checks

To conduct robustness checks we first combine the labor market outcomes considered in Tables 4 and 5 into one index. The index is computed using the following variables for which a baseline value is available: any paid work in the last month, any wage employment in the last month, any casual work in the last month, hours worked in wage employment last week, the hourly wage rate, and total earnings in the last month. Hourly earnings and total earnings are set to zero for unemployed workers. The index is constructed by converting each component into a z-score, averaging these and taking the z-score of the average. z-scores are computed using means and standard deviations from the control group at baseline. Column 1 of Table A8 shows the ITT estimates on this labor market index: all treatment effects are statistically significant and quantitatively large, with the impact of the vocational training treatment (T2) being significantly larger than for the firm-training treatment (T2) ($p = .057$).

In addition to the ITT estimates, we also report: (i) conditional Lee bounds on the treatment effects (where we use the convention that the bound is underlined if it is statistically different from zero); (ii) unadjusted and adjusted Romano-Wolf p-values to account for multiple hypothesis testing. For each ITT estimate we see that: (i) the Lee bounds are nearly always significantly different from zero (the exceptions are the lower bounds on the on-the-job training (T2) and pure match (T5) treatments); (ii) the effect is significant against the Romano-Wolf p-values.[46]

Columns 2 and 3 split the labor market index by gender. Women have been found to benefit more from some training interventions [Friedlander 1997, Attanasio *et al.* 2011], although this finding is far from universal [McKenzie 2017]. We generally find larger impacts on men but for the match treatment we only find impacts for women. Columns 4 and 5 split treatment effects by sector: we generally find larger productivity impacts in manufacturing, again except for the match treatment. Given the correlation between gender and sector (manufacturing sectors tend to be male dominated), it is hard to definitively separate out whether the impacts are driven by gender or sector. Fourth, we consider impacts in labor markets outside of Kampala, where 81% of workers reside: the result in Column 6 largely replicates the main findings.

Finally, we examine the sensitivity of the treatment effects to the timing of labor market entry. To do so, we exploit the fact that we have two batches of vocationally trained workers: the majority of trainees from the first round of applicants started training in January 2013, as shown in Figure 2. For logistical reasons, a second round of randomized-in applicants received vocational training between October 2013 and April 2014 (and so receive their training at the same time as when the apprenticeships are being implemented). In Column 7 we allow the impacts of vocational training (T3) to differ by the first and second batch of trainees: we see no evidence that workers in the second batch have different outcomes as measured by the labor market index.

Throughout Columns 2 to 7, in most cases the Lee bounds remain significantly different from zero, and the treatment effect estimates remain significant against Romano-Wolf p-values.

A.3 Likelihood

We first assume all random events $(\lambda_0, \lambda_1, \delta)$ are realizations of Poisson processes, so that the residual durations are exponentially distributed. As unemployed workers are assumed to be made offers by firms that they would accept, the unemployment spell hazard is λ_0. The hazard rate of job spells with wage w is $\delta + \lambda_1(1 - F(w))$. Hence for a given e_i and initial wages w_{i1} the individual

[46] We bound the treatment effect estimates using the trimming procedure proposed by Lee [2009]. The procedure trims observations from above (below) in the group with lower attrition, to equalize the number of observations in treatment and control groups. It then re-estimates the program impact in the trimmed sample to deliver the lower (upper) bounds for the true treatment effect. The bounding procedure relies on the assumptions that treatment is assigned randomly and that treatment affects attrition in only one direction so there are no heterogeneous effects of the treatment on attrition/selection, in line with the evidence in Table A4. As Lee [2009] discusses, using covariates to trim the samples yields tighter bounds. The covariates we use are the strata dummies. To adjust the individual p-values, we implement the Romano-Wolf [2016] step-down procedure based on re-sampling bootstrap methods, and using the publicly provided code (http://www.econ.uzh.ch/en/faculty/wolf/publications.html9).

likelihood contributions are as follows. The likelihood contribution for $e_i = 1$ is:

$$\begin{aligned} L(\mathbf{x}_i|e_i \;\; = \;\; 1) &= g(w_{i1}) \times (\delta + \lambda_1(1 - F(w)))^{(1-c_i)} e^{-(\delta+\lambda_1(1-F(w_{i1})))d_i} \\ &\times \left(\frac{\delta}{\delta + \lambda_1(1 - F(w_{i1}))}\right)^{\tau_{JU_i}} \left(\frac{\lambda_1(1 - F(w_{i1}))}{\delta + \lambda_1(1 - F(w_{i1}))}\right)^{\tau_{JJ_i}} \end{aligned} \tag{14}$$

where $g(.)$ is the density of $G(.)$, and c_i is an indicator for censoring, and the spell duration is d_i. The transition indicators between spells are τ_{JU_i}, τ_{JJ_i}. The likelihood contribution for $e_i = 0$ is:

$$L(\mathbf{x}_i|e_i = 0) = \lambda_0^{(1-c_i)} e^{-\lambda_0 d_i} . f(w_{i0})^{(1-c_i)}, \tag{15}$$

where $f(.)$ is the density of $F(.)$. Given steady state, $p(e_i = 0) = u = \frac{\delta}{\delta+\lambda_0}$, and $p(e_i = 1) = 1 - u = \frac{\lambda_0}{\delta+\lambda_0}$. Hence the generic likelihood contribution of observation $\mathbf{x}_i$ is:

$$L(\mathbf{x}_i) = \left(\frac{\lambda_0}{\delta + \lambda_0} L(\mathbf{x}_i|e_i = 1)\right)^{e_i} . \left(\frac{\delta}{\delta + \lambda_0} L(\mathbf{x}_i|e_i = 0)\right)^{1-e_i} . \tag{16}$$

The model is then estimated following the two-step procedure in Bontemps *et al.* [2000] for each treatment arm and the control group. In Step 1, $G(.)$ is non-parametrically estimated from the CDF of observed wages among those employed. In Step 2, we substitute $G(.)$ into $L(\mathbf{x}_i)$ using the relationship between $G(.)$ and $F(.)$ in (7). λ_0, λ_1,δ are then estimated, and their asymptotic standard errors calculated using maximum likelihood.

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Table 1: Baseline Balance on Worker Labor Market Outcomes

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

	Number of workers	Currently working	Has worked in the last month	Has done any wage employment in the last month	Any self employment in the last month	Has done any casual work in the last month	Total earnings in the last month [USD]	F-test of joint significance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Workers	1714	.360	.383	.191	.055	.199	5.93	
		(.045)	(.044)	(.026)	(.019)	(.044)	(1.11)	
T1: Control	451	.381	.401	.192	.047	.225	5.11	
		(.049)	(.048)	(.029)	(.020)	(.046)	(1.27)	
T2: Firm Trained	283	.369	.387	.142*	.074	.223	6.44	{.955}
		(.035)	(.035)	(.027)	(.019)	(.030)	(1.35)	
		[.979]	[.985]	[.064]	[.100]	[.771]	[.239]	
T3: Vocationally Trained	390	.358	.389	.224	.041	.196	7.29*	{.831}
		(.032)	(.032)	(.027)	(.014)	(.027)	(1.26)	
		[.763]	[.990]	[.191]	[.750]	[.489]	[.063]	
T4: Vocationally Trained + Matched	307	.320	.360	.215	.056	.158	5.25	{.954}
		(.033)	(.034)	(.030)	(.017)	(.027)	(1.20)	
		[.316]	[.747]	[.527]	[.325]	[.140]	[.758]	
T5: Untrained, Matched	283	.364	.367	.166	.067	.180	5.58	{.996}
		(.033)	(.034)	(.028)	(.018)	(.028)	(1.25)	
		[.707]	[.386]	[.294]	[.213]	[.158]	[.713]	
F-test of joint significance		{.882}	{.908}	{.021}**	{.225}	{.341}	{.379}	

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. All data is from the baseline survey to workers. Column 1 reports the number of workers assigned to each treatment. Columns 2 to 7 report the mean value of each worker characteristic, derived from an OLS regression of the characteristic of interest on a series of dummy variables for each treatment group. All regressions include strata dummies and a dummy for the implementation round. The excluded (comparison) group in these regressions is the Control group. Robust standard errors are reported throughout. Column 8 reports the p-values from F-Tests of joint significance of all the regressors from an OLS regression where the dependent variable is a dummy variable taking value 0 if the worker is assigned to the Control group, and it takes value 1 for workers assigned to treatment group j (with j going from 2 to 5) and the independent variables are the variables in Columns 2 to 6. Robust standard errors are also calculated in these regressions. The p-values reported in the last row are from the F-test of joint significance of the treatment dummies in each Column regression where the sample includes all workers. In Column 6 casual work includes any work conducted in the following tasks where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing the compound. Casual work also include any type of agricultural labor such as farming, animal rearing, fishing and agricultural day labor. In Column 7 workers who report doing no work in the past month (or only did unpaid work in the last month) have a value of zero for total earnings. The top 1% of earnings values are excluded. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 2: The Mincerian Returns to Vocational Training, by Sector

			Worker is skilled: self-reported VTI attendance	
	Share of firms in sector	% workers skilled in sector	Coefficient and SE from worker wage regressions [USD]	Coefficient and SE from worker log(wage) regressions [USD]
	(1)	(2)	(3)	(4)
All Sectors		**31.0%**	**26.2*** **	**.515*** **
			(3.15)	(.045)
Manufacturing				
Welding	14.57%	24.9%	34.5***	.381***
			(6.40)	(.084)
Motor-mechanics	9.80%	23.5%	16.1*	.294*
			(9.41)	(.153)
Electrical wiring	6.37%	41.9%	27.3***	.486**
			(7.60)	(.189)
Construction	4.38%	28.8%	11.5	.289*
			(9.39)	(.170)
Plumbing	3.08%	49.1%	60.9***	.719**
			(19.0)	(.281)
Services				
Hairdressing	39.64%	29.2%	22.9***	.444***
			(5.97)	(.069)
Tailoring	14.96%	41.6%	15.9	.898***
			(9.76)	(.182)
Catering	7.20%	40.2%	26.8**	.330***
			(11.6)	(.109)

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the Census of firms, which includes 2309 firms and 6306 workers. A worker is defined as skilled if he/she was reported as having attended formal vocational training at any point in the past. Coefficients and standard errors in Columns 3 and 4 are from a regression of workers' total earnings in the last month (or the logarithm of workers' total earnings in the last month) on a dummy for being a skilled worker (as defined above). Control variables in these regressions include: employee's age and age squared, gender, tenure and tenure squared, firm size, BRAC branch dummies and firm sector dummies. Robust standard errors are reported. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. The top 1% wages and capital stock values are excluded.

Table 3: Characteristics of Apprenticeships

A. Availability	
Worker received on-the-job training at the current firm	.499
Duration of on-the-job training [months]	10
B. Payments	
In the first month of training, the worker:	
Was paid	.197
Was unpaid	.513
Was paying the firm owner	.291
Earnings (conditional on > 0) [US$] (median)	39.2 (40.1)
Amount worker was paying to owner (conditional on > 0) [US$] (median)	51.3 (33.3)
C. Trainers	
Who was mainly involved in training the worker:	
Firm owner only	.459
Other employees only	.091
Firm owner as well as other employees	.451

Notes: The data is from the first firm follow-up, and the sample is restricted to those workers employed in Control firms. The sample includes 957 workers employed in 333 firms. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. The top 1% monetary values are excluded.

Table 4: Skills

OLS regression coefficients, IPW estimates, robust standard errors in parentheses

	On-the-job Training		Sector-Specific Skills Test			Firm-Specific Skills
	Received OTJ-T at First Employer	Position in First Job is "Trainee"	Report Some Skills	ITT	ATE: Offered Worker-Firm Match	Skills Transferability
	(1)	(2)	(3)	(4)	(5)	(6)
T2: Firm Trained	.142***	.220***	.105***	1.93	4.05	.104
	(.052)	(.041)	(.032)	(1.53)	(3.03)	(.080)
T3: Vocationally Trained	-.009	-.015	.278***	7.00***	-	.170**
	(.048)	(.029)	(.026)	(1.34)	-	(.069)
T4: Vocationally Trained + Matched	-.058	-.027	.256***	7.14***	60.5***	.136*
	(.047)	(.028)	(.029)	(1.57)	(16.1)	(.082)
T5: Untrained, Matched	-.046	-.030	.017	.999	6.93	.001
	(.051)	(.029)	(.032)	(1.52)	(9.87)	(.111)
Mean (SD) Outcome in Control Group	.400	.092	.596	30.1 (22.9)	30.1 (22.9)	0
Control for Baseline Value	No	No	No	No	No	No
P-values on tests of equality:						
Firm Trained = Vocationally Trained	.004***	.000***	.000***	.001***		.176
N. of observations	938	940	2,178	2,178	1,663	669

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline, second and third worker follow-up survey. We report OLS regressions, where we use inverse probability weighting and robust standard errors are reported in parentheses. All regressions include strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. In Columns 1 and 2 we use information on the first employment spell reported by a worker in the post-intervention period (so the sample only includes workers that had at least one job in the post-intervention period). In Column 1 the dependent variable is a dummy=1 if the worker reported having received on the job training at her first employer. In Column 2 the dependent variable is a dummy=1 if the worker reported being a "Trainee" when asked about her position at her first employer. In Column 3 we report a linear probability model on whether the respondent reports having any sector specific skills or not at second and third follow-up. In Columns 4 and 5 the dependent variable is the skills test score, from the test administered to workers in the second worker follow-up. For the regressions in Columns 4 and 5 workers that reported not having any sector specific skills are assigned a test score equal to what they would have got had they answered the test at random. Workers that refused to take the skills test are excluded from the regressions in Columns 4-6. Column 4 reports OLS estimates, while in Column 5 we report 2SLS regressions, where we instrument treatment take-up with the original treatment assignment. We also control for the following baseline characteristics of workers: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, a dummy for whether the worker was employed at baseline, and a dummy for whether the worker scored at the median or above on the cognitive test administered at baseline. The weights for the IPW estimates are computed separately for attrition at first, second and third follow-up. The instruments for the IPW estimates are whether the worker was an orphan at baseline, a dummy if anyone in the household of the worker reported having a phone at baseline, a dummy for whether the worker reported being willing to work in more than one sector at the time of their original application to the VTIs and dummies for the survey team the worker's interview was assigned to in each of the three follow-up survey rounds. At the foot of each Column we report p-values on the null that the impact of the vocational training is equal to the impact of firm training (T2=T3).

Table 5: Extensive Margin Impacts on Employment

OLS regression coefficients, IPW estimates, robust standard errors in parentheses

	Has done any paid work in the last month	Has done any wage employment in the last month	Any self employment in the last month	Has done any casual work in the last month	Employed at firm they were matched to
	(1)	(2)	(3)	(4)	(5)
T2: Firm Trained	.065***	.073***	-.015	.001	.115***
	(.025)	(.023)	(.018)	(.015)	(.012)
T3: Vocationally Trained	.105***	.071***	.039**	-.009	N/A
	(.022)	(.021)	(.017)	(.013)	
T4: Vocationally Trained + Matched	.071***	.046**	.030	-.001	.010**
	(.025)	(.023)	(.019)	(.014)	(.004)
T5: Untrained, Matched	.062**	.049**	.032*	-.014	.007**
	(.025)	(.023)	(.019)	(.015)	(.003)
Mean Outcome in Control Group	.438	.271	.153	.107	.000
Control for Baseline Value	Yes	Yes	Yes	Yes	No
P-values on tests of equality:					
Firm Trained = Vocationally Trained	.111	.910	.004***	.493	N/A
N. of observations	3,915	3,915	3,915	3,915	3,021

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and first three follow-up worker surveys. We report OLS regressions, where we use inverse probability weighting and robust standard errors are reported in parentheses. All regressions control for the value of the outcome at baseline (except in Column 5), as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. We also control for the following baseline characteristics of workers: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, a dummy for whether the worker was employed at baseline, and a dummy for whether the worker scored at the median or above on the cognitive test administered at baseline. The weights for the IPW estimates are computed separately for attrition at first, second and third follow-up. The instruments for the IPW estimates are whether the worker was an orphan at baseline, a dummy if anyone in the household of the worker reported having a phone at baseline, a dummy for whether the worker reported being willing to work in more than one sector at the time of their original application to the VTIs and dummies for the survey team the worker's interview was assigned to in each of the two follow-up survey rounds. In Column 4 casual work includes any work conducted in the following tasks where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing the compound. Casual work also include any type of agricultural labor such as farming, animal rearing, fishing and agricultural day labor. At the foot of each Column we report p-values on the null that the impact of the vocational training is equal to the impact of firm training (T2=T3).

Table 6: Total Effect Impacts on Employment

OLS regression coefficients, IPW estimates, robust standard errors in parentheses

	Number of months worked in the last year	Number of hours worked in wage employment in the last week	Hourly wage rate [USD]	Total earnings in the last month [USD]	Total earnings in the last month [USD]
	(1)	(2)	(3)	(4)	(5) LATE
T2: Firm Trained	.534**	2.508	.055***	6.288**	13.670**
	(.259)	(1.590)	(.019)	(2.543)	(5.402)
T3: Vocationally Trained	1.107***	3.241**	.047***	11.364***	-
	(.233)	(1.493)	(.013)	(2.30)	-
T4: Vocationally Trained + Matched	.599**	2.402	.034***	8.018***	66.886***
	(.259)	(1.623)	(.013)	(2.608)	(23.315)
T5: Untrained, Matched	.709***	2.602	.008	5.267**	36.201**
	(.257)	(1.660)	(.012)	(2.429)	(16.789)
Mean Outcome in Control Group	4.49	17.6	.090	28.7	28.7
Control for Baseline Value	No	Yes	Yes	Yes	Yes
P-values on tests of equality:					
Firm Trained = Vocationally Trained	.031**	.661	.670	.066*	-
N. of observations	3,915	3,769	3,726	3,747	2,901

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and first three follow-up worker surveys. We report OLS regressions, where we use inverse probability weighting and robust standard errors are reported in parentheses. All regressions control for the value of the outcome at baseline (except in Column 2), as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. We also control for the following baseline characteristics of workers: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, a dummy for whether the worker was employed at baseline, and a dummy for whether the worker scored at the median or above on the cognitive test administered at baseline. The weights for the IPW estimates are computed separately for attrition at first, second and third follow-up. The instruments for the IPW estimates are whether the worker was an orphan at baseline, a dummy if anyone in the household of the worker reported having a phone at baseline, a dummy for whether the worker reported being willing to work in more than one sector at the time of their original application to the VTIs and dummies for the survey team the worker's interview was assigned to in each of the three follow-up survey rounds. In Column 3 the hourly wage rate is computed as total earnings in the last month divided by total hours worked in the last month. Individuals with no hours worked (and no labor income) in the last month are assigned a value of zero. In Column 4 workers who report doing no work in the past month (or only did unpaid work in the last month) have a value of zero for total earnings. The top 1% of earnings values are excluded. At the foot of each Column we report p-values on the null that the impact of the vocational training is equal to the impact of firm training (T2=T3). All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 7: Worker Beliefs and Job Search

OLS regression coefficients, IPW estimates, robust standard errors in parentheses

	Job Offer Probability	Offered Wages			Search Intensity and Method	
	Expected probability of finding a job in the next 6 months (0 to 10 scale)	Minimum expected monthly earnings [USD]	Maximum expected monthly earnings [USD]	Average expected monthly earnings (triangular distribution) [USD]	Has actively looked for a job in the last year	Main channel through which looked for a job is formal [yes=1]
	(1)	(2)	(3)	(4)	(5)	(6)
T2: Firm Trained	.645***	.370	.268	.389	-.001	.013
	(.137)	(2.191)	(4.099)	(3.280)	(.025)	(.008)
T3: Vocationally Trained	1.859***	14.319***	23.759***	20.730***	.102***	.019**
	(.124)	(2.063)	(3.727)	(3.056)	(.023)	(.008)
T4: Vocationally Trained + Matched	1.867***	13.783***	23.088***	18.950***	.079***	.013
	(.134)	(2.212)	(4.055)	(3.295)	(.025)	(.008)
T5: Untrained, Matched	-.091	2.931	2.345	-.028	.001	.002
	(.133)	(2.346)	(4.057)	(3.315)	(.025)	(.007)
Mean Outcome in Control Group	2.783	41.612	73.821	57.658	.509	.017
Control for Baseline Value	Yes	Yes	Yes	Yes	No	No
P-values on tests of equality:						
Firm Trained = Vocationally Trained	.000***	.000***	.000***	.000***	.000***	.442
N. of observations	3,770	2,627	2,622	2,233	3,914	3,913

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and first three follow-up worker surveys. We report OLS regressions, where we use inverse probability weighting and robust standard errors are reported in parentheses. All regressions control for the value of the outcome at baseline (except in Columns 5 to 8), as well as strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. We also control for the following baseline characteristics of workers: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, a dummy for whether the worker was employed at baseline, and a dummy for whether the worker scored at the median or above on the cognitive test administered at baseline. The weights for the IPW estimates are computed separately for attrition at first, second and third follow-up. The instruments for the IPW estimates are whether the worker was an orphan at baseline, a dummy if anyone in the household of the worker reported having a phone at baseline, a dummy for whether the worker reported being willing to work in more than one sector at the time of their original application to the VTIs and dummies for the survey team the worker's interview was assigned to in each of the three follow-up survey rounds. In Column 4 we assume a triangular distribution to calculate the average expected monthly earnings. At the foot of each Column we report p-values on the null that the impact of the vocational training is equal to the impact of firm training (T2=T3). All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 8: Structural Estimates of the Job Ladder Search Model

Two-step estimation procedure in Bontemps, Robin and van den Berg [2000]

		Steady State: November 2015 (Data from Third and Second FUP)			
Panel A: Parameter Estimates		**Control**	**Firm Trained**	**Vocationally Trained**	**Untrained, Matched**
		(1)	**(2)**	**(3)**	**(4)**
Job destruction rate (monthly):	δ	.0272	.0259	.0242	.0192
		(.0030)	(.0037)	(.0021)	(.0028)
Arrival rate of job offers if UNEMPLOYED (monthly):	λ_0	.0190	.0191	.0238	.0181
		(.0019)	(.0024)	(.0019)	(.0025)
Arrival rate of job offers if EMPLOYED (monthly):	λ_1	.0388	.0377	.0434	.0441
		(.0096)	(.0117)	(.0082)	(.0145)
Interfirm competition for workers	κ_1	1.426	1.456	1.793	2.297
Unemployment Rate	u	.5892	.5757	.5041	.5142
Panel B: Function and Income Estimates					
Average (sd) monthly OFFERED wage [USD]	***F(.)***	44.8	47.0	46.3	40.3
		(37.4)	(43.6)	(41.9)	(41.8)
Average (sd) monthly ACCEPTED wage [USD]	***G(.)***	63.7	68.9	70.7	69.2
		(45.5)	(54.5)	(54.5)	(57.3)
Treatment Effect Impact on Annual Income [USD]			**36.8**	**106.7**	**89.4**
% Impact:			**11.7%**	**34.0%**	**28.5%**
Panel C: Firm Productivity Distribution					
Average (sd) firm productivity	***P(.)***	174.7	122.7	270.6	93.9
		(610.4)	(452.5)	(1112.2)	(294.5)
% Impact:			**-30%**	**55%**	**-46%**

Notes: The dataset is a cross-section of workers, and for each worker it contains information on: spell type (employment, unemployment), spell duration (in months), earnings in employment spells (in USD), dates of transitions between spells and type of transition: (i) job to unemployment, (ii) unemployment to job, or (iii) job to job. Wages are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. The dataset contains at most two spells (and one transition) per individual. The data comes from the second and third follow-up survey of workers, and the initial spell is identified as the (employment or unemployment) spell that was ongoing in November 2015. Spells are right censored at the date of interview. Spells are left censored at 1 August 2014. Casual and agricultural occupations are coded as unemployment. Self-employment is coded as employment (but self-employment spells are assigned a separate spell). The estimation protocol follows the two-step procedure in Bontemps, Robin and van den Berg [2000]: in the first step the G function is estimated non-parametrically from the data (so this is just the empirical CDF of observed wages for those workers that are employed in their first spell), and is then substituted into the likelihood function. In the second step, maximum likelihood is then conducted using information from both the first and second spells for each individual to recover the parameter estimates. In Panel B, the average yearly income is defined as the average monthly accepted wage (in levels) multiplied by the unemployment rate and multiplied by 12. The impact of the program on average income is defined as the difference in average income between the respective treatment groups and the control group. In Panel C, the productivity distribution is the one implied by the job ladder model. This is the (exogenous) productivity distribution needed to rationalize the observed distribution of wages in the model. A higher value of productivity means that the firm is more productive.

Table 9: Firm Side Outcomes, LATE Estimates

OLS IPW regression coefficients, standard errors clustered by sector-branch in parenthesis

	At Least One Worker Hired and Retained	Number of Employees	Log (Average Monthly Profits)	Log (Net Investment)
	(1)	(2)	(3)	(4)
T2: Wage Subsidy Offer	.090***	-.016	.132**	.024
	(.017)	(.153)	(.054)	(.114)
T4: Matched to Vocationally Trained Worker	-.002	.418	.094	.237
	(.004)	(.267)	(.083)	(.166)
T5: Matched to Untrained Worker	-.003	.351	.171*	.161
	(.003)	(.239)	(.092)	(.161)
Mean of outcome in T1 Control group	.000	1.44	191	60.8
Controls for baseline value of outcome	No	Yes	Yes	No
Number of firms	1,567	2,005	1,787	1,990

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. We report results from regressions run on a panel dataset including four rounds of follow-up data, and controlling for the value of the outcome at baseline when available. The Table shows 2SLS regressions coefficients, where we instrument treatment take-up with the original treatment assignment, and standard errors in parentheses (clustered by sector-BRAC branch). Take-up is equal to one if the firm was successfully contacted by the implementation team and offered to meet a matched worker. All regressions include baseline controls, branch and trade fixed-effects, survey wave fixed effects and dummies for month of interview. Baseline controls include owner's sex, owner's years of education and firm size at baseline. Standard errors are adjusted for heteroskedasticity and clustered at the branch-trade level. The instruments for the IPW estimates are a dummy for whether the owner reported at baseline an intention to relocate in the future, and the number of network firms reported at baseline. All monetary amounts are deflated and expressed in terms of the price level in January 2013 using the monthly Producer Price Index for the manufacturing sector (local market), published by the Uganda Bureau of Statistics. The monetary amounts are then converted in January 2013 USD (1USD=2385UGX). Monthly profits are truncated at 99th percentile. Net investments are truncated at the bottom percentile and at the 99th percentile. The dependent variable in Column 3 is the log of one plus average monthly profits. The dependent variable in Column 4 is the log of one plus net investment.

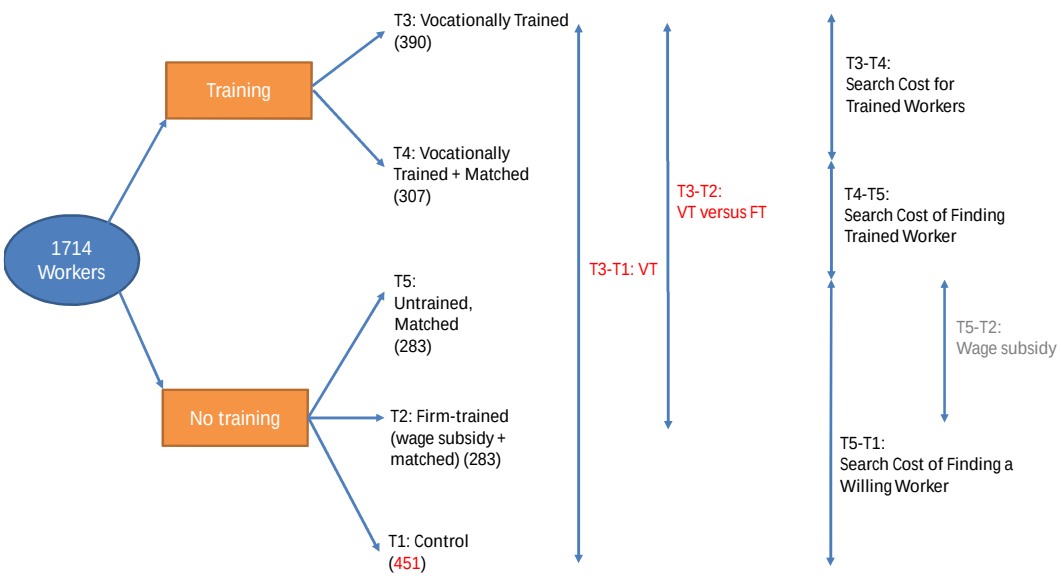
Table 10: Internal Rate of Return

	Firm Trained	Vocationally Trained	Vocationally Trained + Matched	Untrained, Matched
	(1)	(2)	(3)	(4)
Social discount rate = 5%				
Remaining expected productive life of beneficiaries	38 years	38 years	38 years	38 years
Panel A. External parameters				
Total cost per individual at year 0 [USD]:	**368**	**510**	**527**	**21**
(i) Training costs (for 6 months)	302	470	470	0
(ii) Program overheads costs	31	4	21	21
(iii) Foregone earnings (for 6 months) - average at baseline	36	36	36	0
Panel B. Estimated total earnings benefits				
1 NPV change in total earnings year 1 and beyond-forever (from structural model)	621	2122	1624	1508
2 Benefits/cost ratio	**1.69**	**4.16**	**3.09**	**73.13**
Social discount rate = 10%	0.97	2.40	1.78	42.20
3 Internal Rate of Return (IRR)	**0.097**	**0.247**	**0.183**	**4.336**
Panel C. Sensitivity				
Sensitivity to different expected remaining productive life of beneficiaries				
Remaining expected productive life = 20 years	**0.077**	**0.244**	**0.176**	**4.336**
Remaining expected productive life = 10 years	**0.000**	**0.210**	**0.128**	**4.336**
Sensitivity to different earnings				
Foregone earnings = 90th percentile at baseline (120USD)	**0.076**	**0.212**	**0.157**	**4.336**
Foregone earnings = double 90th percentile at baseline (241USD)	**0.056**	**0.176**	**0.130**	**4.336**
Foregone earnings = max earnings at baseline (794USD)	**0.012**	**0.096**	**0.069**	**4.336**
Foregone earnings = double max earnings at baseline (1588USD)	**NA**	**0.052**	**0.033**	**4.336**
Panel D. Programme Costs for IRR to equate social discount rate				
5 Total cost per individual at year 0 [USD]	**621**	**2123**	**1625**	**1508**
Sensitivity to different discount rates/time horizons				
Social discount rate = 10%	358	1224	937	870

Notes: Forgone earnings are calculated as the average monthly earnings at baseline (6 USD) multiplied by six (as the duration of both types of training was six months). The remaining expected productive life of beneficiaries is calculated as average life expectancy in Uganda in 2012 (58 years) minus average age in the sample at baseline (20 years). The computation of the IRR uses as input for the benefit the treatment effect impact on annual income from the structural model. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Figure 1: Experimental Design

A. Worker Side Design



B. Firm Side Design

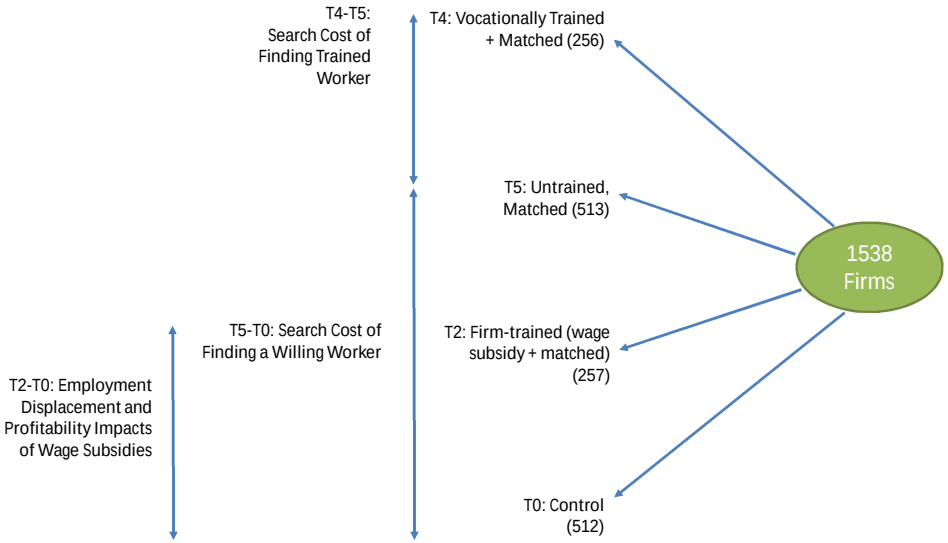
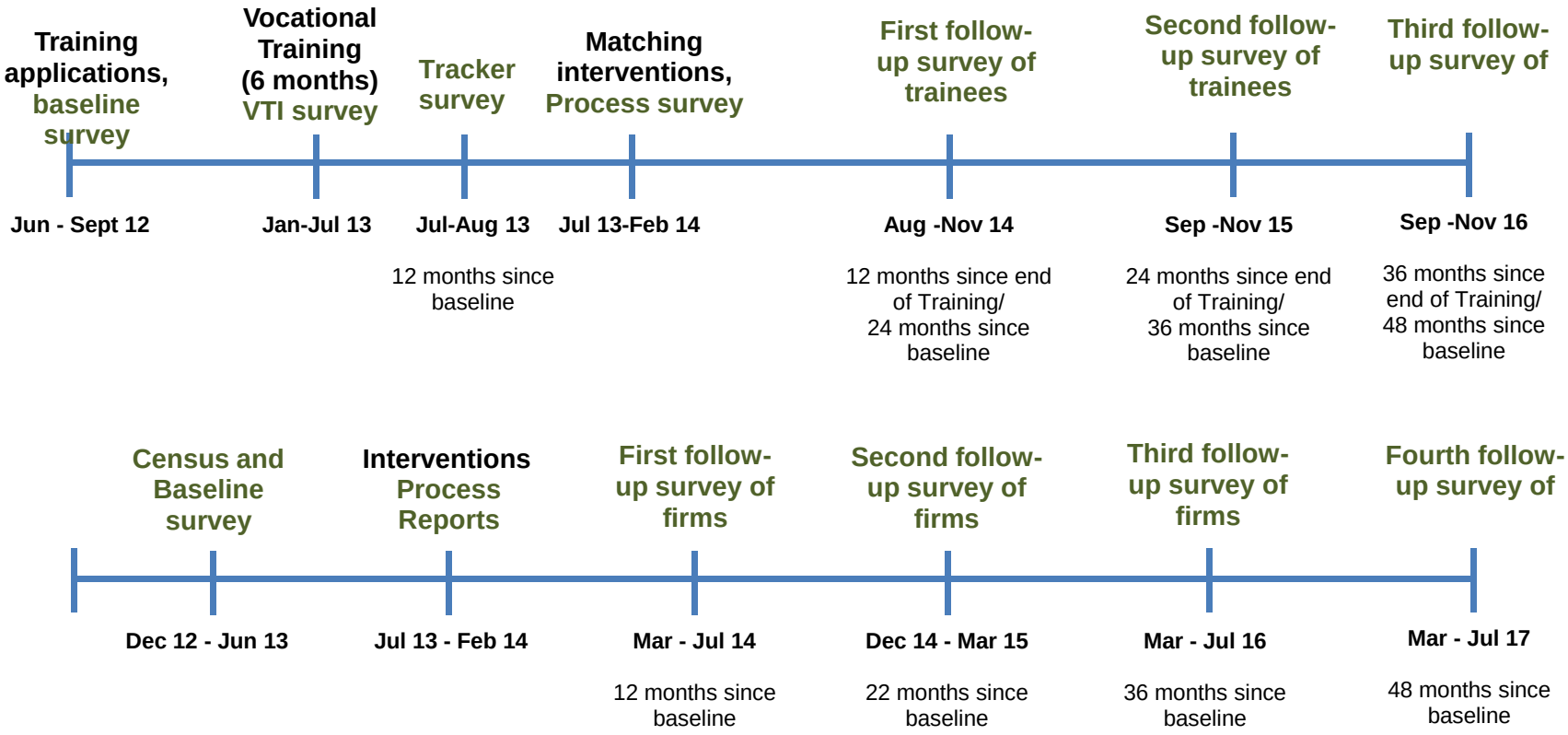


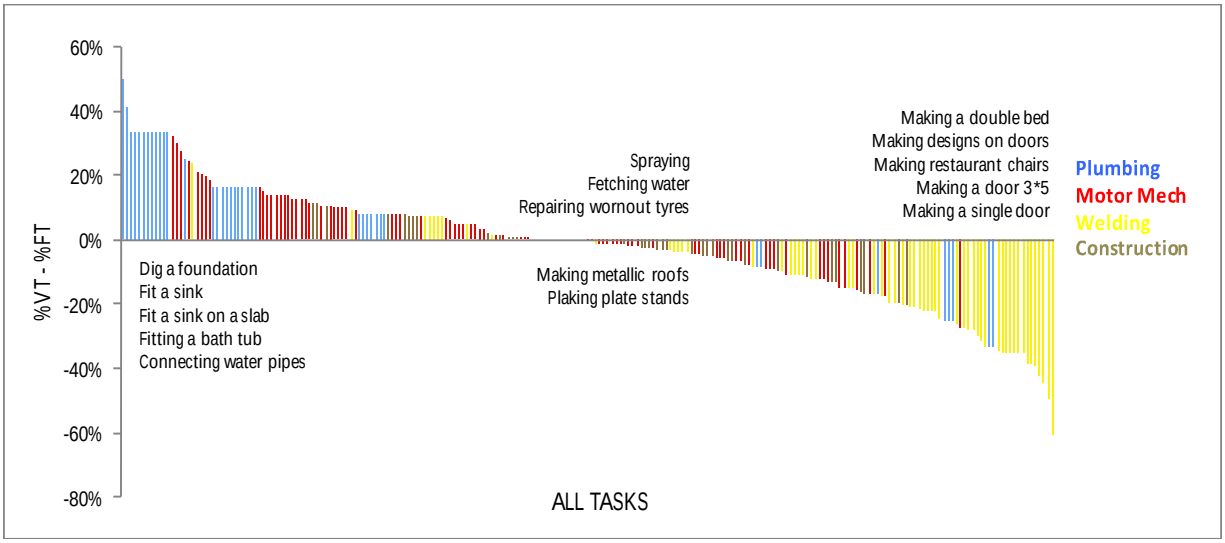
Figure 2: Timeline



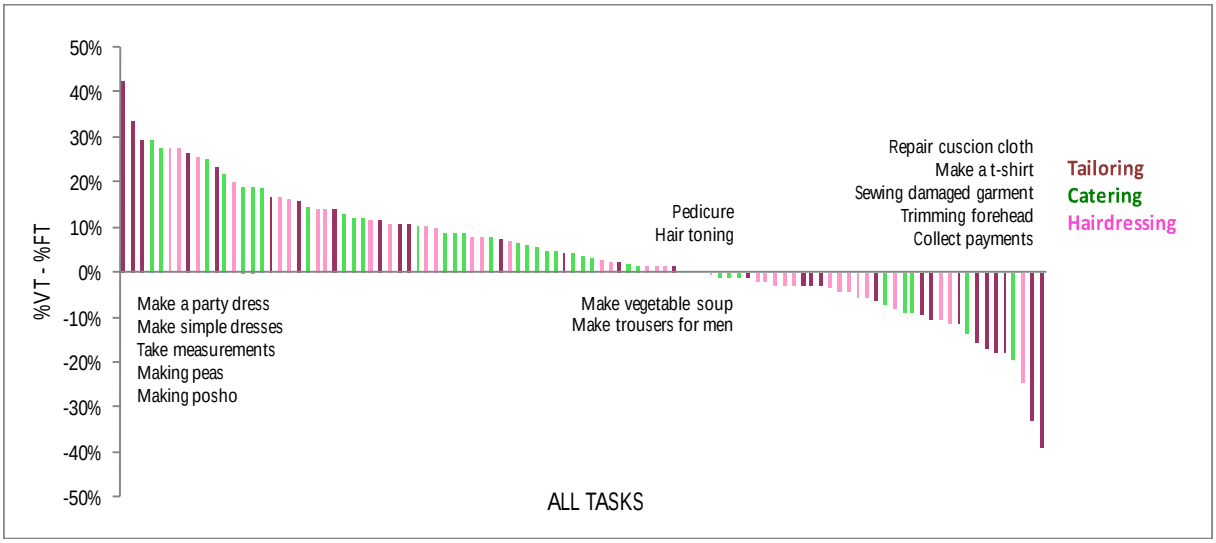
Notes: The timeline highlights the dates relevant for the main batch of worker applications and baseline surveys. A second smaller round of applications and baseline surveys were conducted in May and June 2013. The majority of trainees from the first round of applicants started training in January 2013, 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 April 2014. VTI surveys were collected towards the end of the training period while trainees were still enrolled at the VTIs. Workers from the second round of applicants were not included in the Tracker Survey. The remaining interventions (the matching treatments and on-the-job training placements, and both follow-up surveys) were conducted at the same time for workers from the first and second round of applicants. On the firms' timeline, the firm level interventions include: matching, vocational training and matching, and on-the-job training. There were two rounds of Match and Vocational Training and Match interventions, in line with the two batches of matched trainees from the vocational training institutes. The first round of the Vocational training and Match interventions took place in August-September 2013. The second round took place in December 2013-February 2014. The on-the-job training intervention took place in September-November 2013.

Figure 3: Most Common Tasks Performed

Panel A: Manufacturing



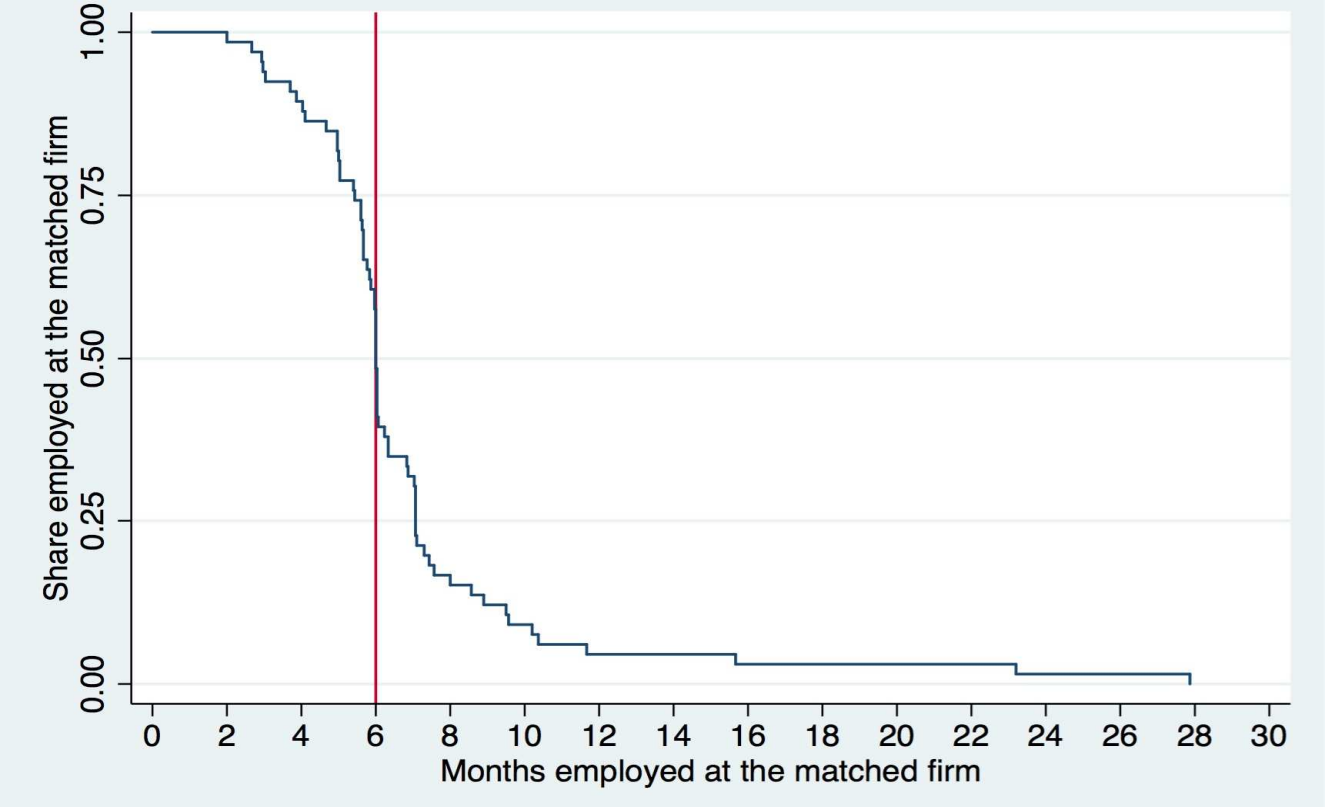
Panel B: Services



Notes: The Figures plot the difference in the percentage of workers performing each given task while employed, between workers who were assigned to receive Vocational Training (T3 and T4) , and workers who were assigned to receive on the job training (T2). The data refers to all main job spells reported at third follow-up (so there is one job spell per worker and only employed individuals are included in the sample), where workers were asked to report which tasks they performed in each employment spell they had in the year prior to the survey (the tasks had to be indicated from an initial list compiled together with BRAC). Manufacturing sectors are: motor-mechanics, plumbing, construction, electrical wiring and welding. Service sectors are: hairdressing, catering and tailoring.

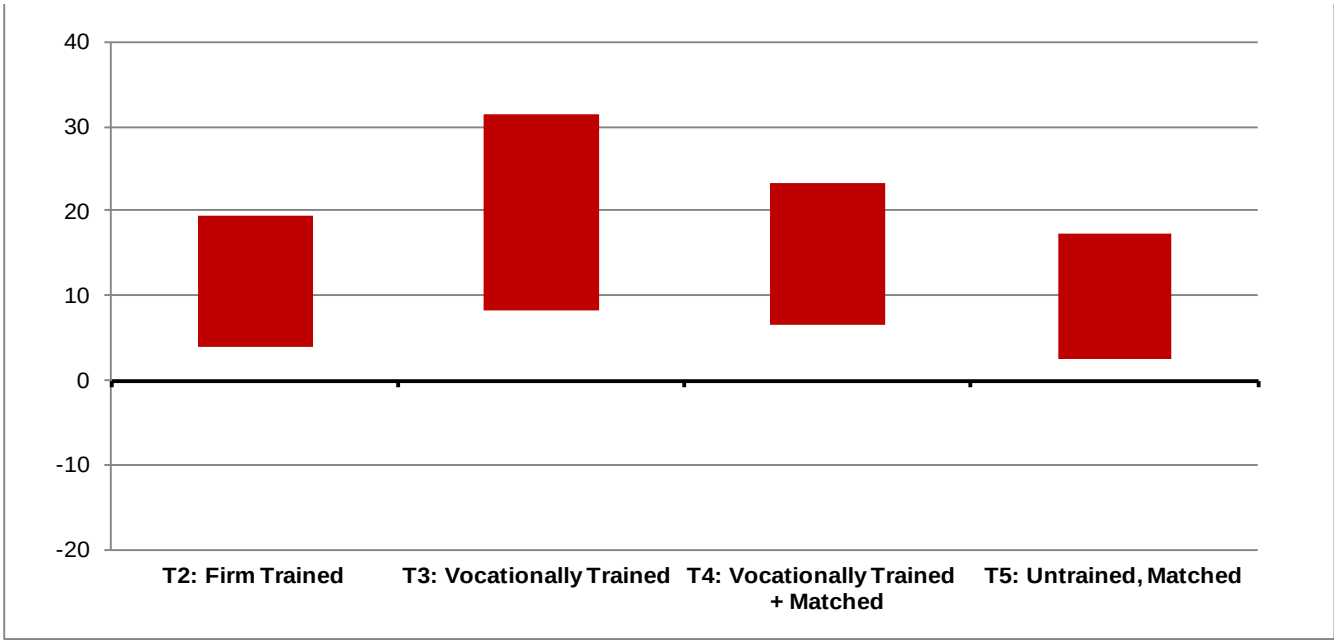
Figure 4: Survival Function for Apprentices in the Wage Subsidy Treatment

Workers Who Started a Job at the Matched Firm



Notes: The Figure plots the survival function for workers in T2:FT who started a job at the matched firm. The Figure is based on 67 workers (information on employment duration at the matched firm is missing for two workers).

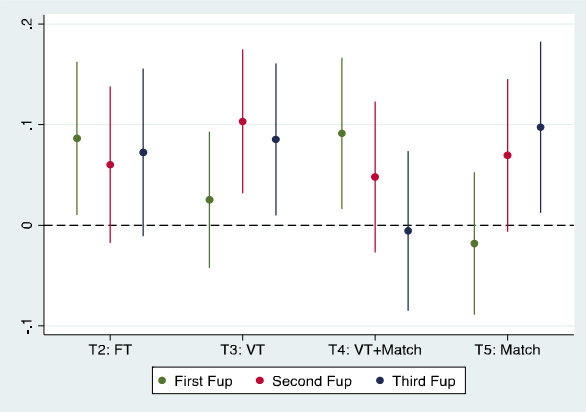
Figure 5: Productivity Bounds



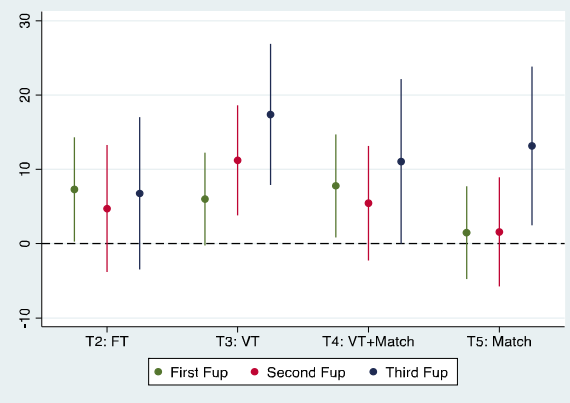
Notes: The Figure shows the upper and lower bound of the productivity effect reported in Table A7 for the full sample of workers. We assume that the non-program earnings of those individuals who would have found employment regardless of the Treatment (the "always employed") are at least as high as the non-program earnings of the individuals that were induced by the Treatment to switch from unemployment to employment (the "compliers"), so that the lower bound of the productivity effect corresponds to the earnings effect (see Table A7 for more details).

Figure 6: Treatment Effects on Key Outcomes, by Survey Wave

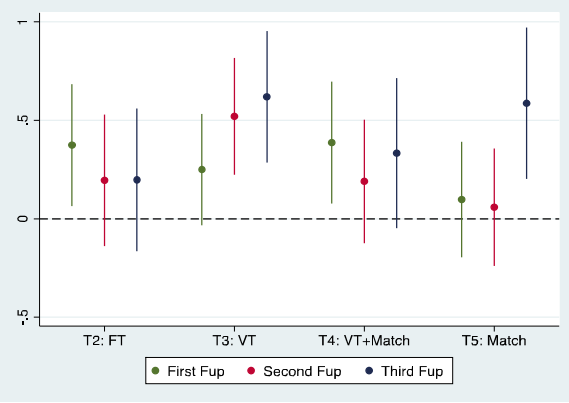
Panel A: Any Wage Employment in the Last Month



Panel B: Total Earnings in the Last Month



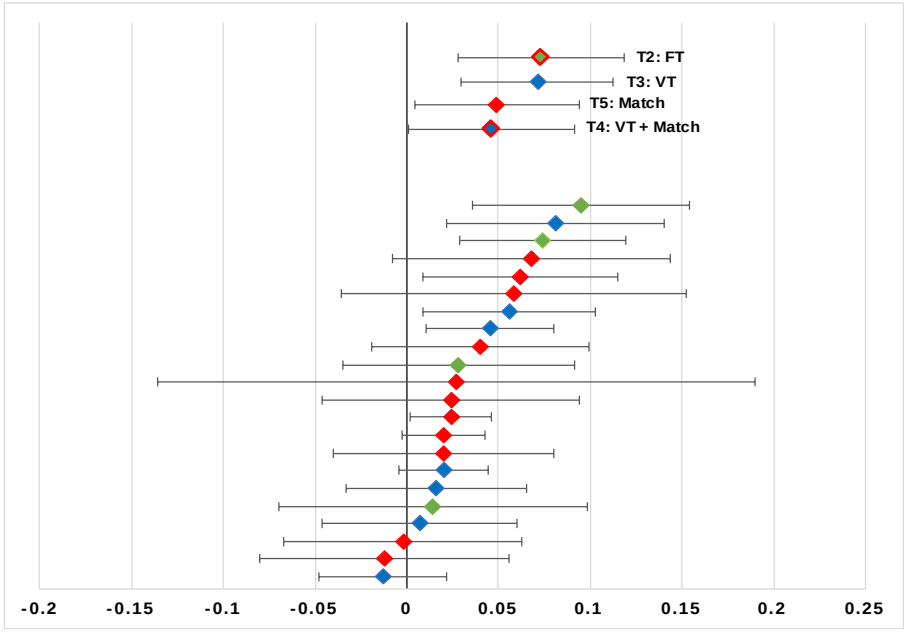
Panel C: Labor Market Index



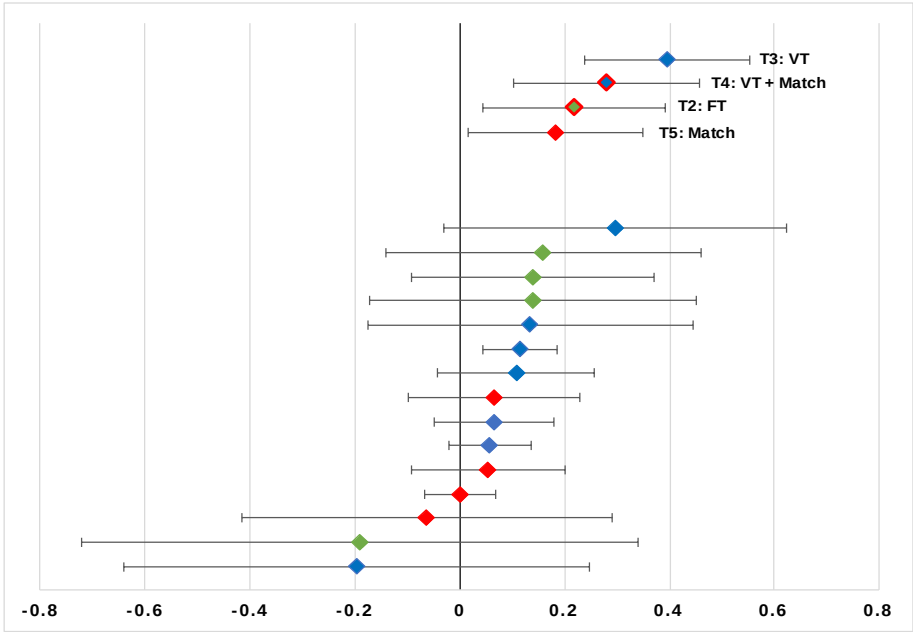
Notes: The data used is from the baseline and first three follow-up worker surveys. We report OLS regression coefficients and 95% confidence intervals, where we use inverse probability weighting and robust standard errors. Impacts on each outcome are estimated from the same panel regression using all three follow-up waves, where we interact the treatment indicators with dummies for the different follow-up waves. The Labor market index is computed using the following variables: any paid work in the last month (dummy), any wage employment in the last month (dummy), any casual work in the last month (dummy), hours worked in wage employment last week, hourly wage rate, total earnings in the last month. Hourly earnings and total earnings are set to zero for workers with no earnings. The index is constructed by converting each component into a z-score, averaging these and taking the z-score of the average. z-scores are computed using means and standard deviations from the control group at baseline. All regressions control for the value of the outcome at baseline, as well as strata dummies, a dummy for the implementation round and dummies for the month of interview. We also control for the following baseline characteristics of workers: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, a dummy for whether the worker was employed at baseline, and a dummy for whether the worker scored at the median or above on the cognitive test administered at baseline. The weights for the IPW estimates are computed separately for attrition at first, second and third follow-up. The instruments for the IPW estimates are whether the worker was an orphan at baseline, a dummy if anyone in the household of the worker reported having a phone at baseline, a dummy for whether the worker reported being willing to work in more than one sector at the time of their original application to the VTIs and dummies for the survey team the worker's interview was assigned to in each of the two follow-up survey rounds.

Figure 7: Comparison of Treatment Impacts to Meta-analysis by McKenzie [2017]

Panel A: Employment impacts



Panel B: Earnings impacts



Notes: The Figures compare the treatment impacts from this study to the treatment impacts reported in the meta-analysis by McKenzie [2017]. The green estimates correspond to wage subsidy programs, the blue estimates to vocational training programs, and the red estimates to job search and matching assistance programs. Panel A reports treatment impacts (ITT) on the probability of paid employment, together with 95% confidence intervals. The estimates from our study are taken from Column 2 of Table 4, where we use as outcome variable "Any wage employment in the last month". Alongside our estimates, Panel A further reports 22 estimates of treatment impacts taken from Table 1, 3 and 4 of McKenzie [2017]. These correspond to all the available program estimates for this outcome reported in McKenzie [2017], a part from the estimate from Galasso et al. [2004], which is omitted as no standard error is provided, and the estimate from Groh et al. [2016] with time frame 6 months, as that is estimated while the wage subsidy was still ongoing (while our estimates for T2: FT and all the other estimates for wage subsidy programs reported in the Figure refer to the period after the wage subsidy ended). Panel B reports treatment impacts (ITT) on earnings, in terms of percentage increase relative to the earnings level of the Control group, together with 95% confidence intervals. The estimates from our study are taken from Column 4 of Table 5, where we use as outcome variable "Total earnings in the last month". Alongside our estimates, Panel B further reports 15 estimates of treatment impacts taken from Table 1, 3 and 4 of McKenzie [2017]. These correspond to all the available program estimates for this outcome reported in McKenzie [2017], apart from the estimate from Groh et al. [2016] with time frame six months, as that is estimated while the wage subsidy was still ongoing (while our estimates for T2: FT and all the other estimates for wage subsidy programs reported in the Figure refer to the period after the wage subsidy ended), and the estimate from Maitra and Mani [2016], which is excluded as that is very large relative to all the other estimates: Maitra and Mani [2016] estimate a treatment impact on earnings of .957, with confidence interval [.056 ; 1.86]. However, this corresponds to only a $2.40 monthly increase in earnings in absolute terms, and so the large treatment impact is due to the women in their sample having extremely low earnings to begin with.

Table A1: Baseline Balance on Worker Characteristics

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

	Number of workers	Age [Years]	Married	Has child(ren)	Currently in school	Ever attended vocational training	F-test of joint significance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All Workers	1714	20.0	.040	.118	.016	.036	
		(.198)	(.016)	(.024)	(.008)	(.018)	
T1: Control	451	20.1	.027	.102	.011	.042	
		(.211)	(.016)	(.025)	(.009)	(.020)	
T2: Firm Trained	283	20.1	.040	.121	.018	.038	{.999}
		(.139)	(.014)	(.024)	(.009)	(.015)	
		[.970]	[.271]	[.260]	[.576]	[.897]	
T3: Vocationally Trained	390	20.0	.056*	.127	.018	.032	{.882}
		(.134)	(.014)	(.022)	(.008)	(.013)	
		[.781]	[.056]	[.339]	[.553]	[.461]	
T4: Vocationally Trained + Matched	307	20.0	.030	.123*	.029	.038	{.845}
		(.146)	(.012)	(.023)	(.011)	(.015)	
		[.975]	[.128]	[.075]	[.248]	[.792]	
T5: Untrained, Matched	283	20.0	.047*	.122	.007	.027	{.875}
		(.148)	(.015)	(.024)	(.007)	(.014)	
		[.429]	[.084]	[.201]	[.468]	[.359]	
F-test of joint significance		{.933}	{.243}	{.449}	{.445}	{.752}	

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. All data is from the baseline survey to workers. Column 1 reports the number of workers assigned to each treatment. Columns 2 to 6 report the mean value of each worker characteristic, derived from an OLS regression of the characteristic of interest on a series of dummy variables for each treatment group. All regressions include strata dummies and a dummy for the implementation round. The excluded (comparison) group in these regressions is the Control group. Robust standard errors are reported throughout. Column 7 reports the p-values from F-Tests of joint significance of all the regressors from an OLS regression where the dependent variable is a dummy variable taking value 0 if the worker is assigned to the Control group, and it takes value 1 for workers assigned to treatment group j (with j going from 2 to 5) and the independent variables are the variables in Columns 2 to 6. Robust standard errors are also calculated in these regressions. The p-values reported in the last row are from the F-test of joint significance of the treatment dummies in each Column regression where the sample includes all workers.

Table A2: External Validity

Means, standard deviations in parentheses

	Number of individuals	Age [Years]	Gender [Male=1]	Married	Currently in school	Ever attended vocational training	Has worked in the last week [Yes=1]	Has had any wage employment in the last week	Has done any casual work in the last week	Total earnings from wage employment in the last month [USD]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Baseline, aged 18-25	1608	20.1	.567	.038	.014	.037	.362	.136	.167	4.32
		(1.86)	(.496)	(.190)	(.116)	(.189)	(.481)	(.343)	(.373)	(16.4)
Uganda National Household Survey 2012/13:										
B. All, aged 18-25	4696	21.1	.535	.395	.309	.065	.667	.202	.473	6.16
		(2.32)	(.499)	(.489)	(.462)	(.247)	(.471)	(.402)	(.499)	(25.1)
C. Labor Market Active, aged 18-25	3167	21.4	.516	.455	.205	.067	.962	.291	.685	9.41
		(2.32)	(.500)	(.497)	(.404)	(.251)	(.192)	(.454)	(.465)	(31.7)

Notes: We present characteristics of individuals from three samples: (i) those individuals in our baseline sample aged 18-25; (ii) individuals aged 18-25 and interviewed in the Uganda National Household Survey 2012/13 (UNHS) conducted by the Ugandan Bureau of Statistics; (iii) individuals aged 18-25 and interviewed in the UNHS who self-report being active in the labor market (either because they are employed or actively seeking employment). The UNHS was fielded between June 2012 and June 2013. Our baseline survey was fielded between June and September 20112. In the UNHS respondents are considered to have attended vocational training if the highest grade completed is post-primary specialized training/diploma/certificate or post-secondary specialized training/diploma/certificate. In the baseline survey questions on employment status did not refer to work activities performed in the last week, but to work activities performed at the time of the survey. Therefore, for the baseline survey the variable "Has worked in the last week" corresponds to the worker being "Currently employed or involved in a work activity". Similarly, Columns 8-10 for the baseline survey are based on the most recent activity performed by the individual, conditional on him/her saying to be currently employed or involved in a work activity. For UNHS, the outcomes in Columns 8-10 are based on the main activity performed in the week before the survey. In Column 10 workers who report doing no work in the past month (or only did unpaid work in the last month) have a value of zero for total earnings.

Table A3: Firm Interests and Worker Take-Up of Treatments

	Vocational Training		Firm Training (wage subsidy + matched)			Matched		
	% Workers Offered Training	% Workers Who Took Up Training	% Workers Offered Treatment (as firm interested)	% Workers That Met at Least One Firm	% Workers Who Took Up Treatment	% Workers Offered Treatment (as firm interested)	% Workers That Met at Least One Firm	% Workers Who Took Up Treatment
Sample of Workers:	**All Workers**	**Offered Training**	**All Workers**	**Offered Treatment**	**Worker received an offer**	**All Workers**	**Offered Treatment**	**Worker received an offer**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
T3: Vocationally Trained	**96.4**	76.9	-	-	-	-	-	-
T4: Vocationally Trained + Matched	**95.8**	85.4	-	-	-	**12.4**	47.2	23.6
T2: Firm Trained (wage subsidy + matched)	-	-	**44.9**	80.3	71.1	-	-	-
T5: Untrained, Matched	-	-	-	-	-	**17.0**	31.6	25.0

Notes: The data used is from the tracker survey and process reports. The tracker survey was collected in July-August 2013, at the end of the main round of vocational training. Process reports were collected during the implementation of the firm-level interventions (September 2013-February 2014). In Columns 1, 3 and 6 the sample includes all workers assigned to the respective treatment groups. In Columns 2, 4 and 7 the sample includes those workers who could be traced and were offered the treatment by BRAC staff. For On-the-Job Training and Matching (Columns 4 and 7) the treatment offer was conditional on firms having first expressed interest in meeting the workers (so those workers matched to firms that were not interested in the program are not included, as they were not offered treatment). The same definitions apply for the calculation of the percentage of workers offered treatment in Columns 1, 3 and 6: so only workers that were traced and successfully informed about the treatment offer are considered as having been offered treatment. In Columns 5 and 8 the sample includes those workers who received an offer to start at the firm. In Column 2 the percentage of workers who took up training includes the workers who completed the 6 months vocational training. In Columns 5 and 8 the percentage of workers who took up treatment is calculated as the percentage of workers who accepted the offer received by the firm, and so started work/training at the firm.

Table A4: Attrition

OLS regression coefficients, robust standard errors in parentheses

	Worker attrited between baseline and third follow up	
	With covariates	Heterogeneous
	(1)	(2)
T2: Firm Trained	-.000	.002
	(.026)	(.035)
T3: Vocationally Trained	-.018	.022
	(.024)	(.034)
T4: Vocationally Trained + Matched	-.011	-.012
	(.027)	-.036
T5: Untrained, Matched	.013	.014
	(.027)	(.035)
High Score on Cognitive Test at Baseline [Yes=1]	.045**	.061*
	(.018)	(.032)
T2: Firm Trained X High Cognitive Score		-.004
		(.051)
T3: Vocationally Trained X High Cognitive Score		-.071
		(.047)
T4: Vocationally Trained + Matched X High Cognitive Score		.001
		(.051)
T5: Untrained, Matched X High Cognitive Score		-.002
		(.053)
Mean of outcome in T1 Control group	.134	.134
Strata and Implementation round dummies	Yes	Yes
Other baseline characteristics	Yes	Yes
Test of joint significance of baseline characteristics		
F-statistic	2.36	1.57
P-value	.070*	.195
Test of joint significance of Treatment X High Score interactions		
F-statistic		.80
P-value		.527
Number of observations (workers)	1,560	1,560

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. Data is from baseline, first, second and third follow-up of applicants to the vocational scholarships. Standard errors are adjusted for heteroskedasticity in all regressions. Other baseline characteristics include: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, and a dummy for whether the worker was employed at baseline. The variable High Score on Cognitive Test at Baseline is a dummy=1 if the applicant scored at the median or above on the cognitive test administered with the baseline survey.

Table A5: General Skills Test for Motor Mechanics

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

Table A6: Expectations

Columns 1 and 2: Means, standard deviations in parentheses

All amounts in 2012 USD

	Measured at Baseline	
	Average expected monthly earnings with current skill set (triangular distribution)	**Average expected monthly earnings if receives VTI Training (triangular distribution)**
	(1)	**(2)**
All Workers (Baseline Interview)	57.8	118.3
	(46.9)	(71.5)
N. of observations	1,243	1,411

Notes: Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and first three follow-up worker surveys. Columns 1 and 2 report the mean and standard deviation (in parentheses) of the average expected monthly earnings with the current skill set, or if the worker were to receive vocational training (assuming a triangular distribution of expected earnings). This is based on all workers interviewed at baseline (across all treatments). All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. The top 1% values of each variable are excluded from the analysis.

Table A7: Productivity Bounds

	Treatment impact on earnings	Treatment impact on paid employment	Earnings in the control group		Earnings effect	Composition effect		Productivity effect	
			10th percentile	90th percentile		Lower Bound	Upper Bound	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T2: Firm Trained	6.288	0.065	19.35	139.29	4.048	15.500	-15.500	-11.451	19.548
T3: Vocationally Trained	11.364	0.105	19.35	139.29	8.280	23.193	-23.193	-14.914	31.473
T4: Vocationally Trained + Matched	8.018	0.071	19.35	139.29	6.628	16.731	-16.731	-10.102	23.359
T5: Untrained, Matched	5.267	0.062	19.35	139.29	2.423	14.873	-14.873	-12.450	17.296
Mean of outcome in T1 Control Group	28.65	0.438							

Notes: We follow Angrist et al. [2006] and Attanasio et al. [2011] for the computation of the bounds on productivity. Specifically, we decompose the overall treatment effect on earnings into: (i) an extensive margin effect on employment; (ii) a composition effect, corresponding to the impact of the treatment on the selection of workers into employment; and (iii) a productivity effect, namely the causal impact on earnings for those employed. Column 1 reports the treatment effects on total earnings in the last month, and the estimates are taken from Column 4 of Table 6. Column 2 reports the extensive margin impact on paid employment, and the estimates are taken from Column 1 of Table 5. Following Attanasio et al. [2011], we define the overall treatment impact on earnings reported in Column 1 as: $E(LS|T = 1) - E(LS|T = 0)$, where T is the treatment variable, S stands for total earnings in the last month, and L is one for individuals in paid employment and zero for individuals not in paid employment. Also, we define the impact on paid employment reported in Column 2 as: $Pr(L = 1|T = 1) - Pr(L = 1|T = 0)$. The earnings effect is the change in the earnings of those employed, defined as: $E(S|L = 1,T = 1) - E(S|L = 1,T = 0)$. Following Attanasio et al. [2011], we can rewrite the earnings effect as: $E(S|L = 1,T = 1) - E(S|L = 1,T = 0) = \{[E(LS|T = 1) - E(LS|T = 0)] - [Pr(L = 1|T = 1) - Pr(L = 1|T = 0)] * E(S|T = 1)/Pr(L = 1|T = 1)\} / Pr(L = 1|T = 0)$. We use the treatment effects and control group means reported in Columns 1 and 2 as inputs in this formula to calculate the estimated earnings effects reported in Column 5. Following again Attanasio et al. [2011], the bounds for the composition effect are calculated as: (i) upper bound: $\{(E(S(p(0.90)) - E(S(p(0.10))) * [Pr(L = 1|T = 1) - Pr(L = 1|T = 0)] / Pr(L = 1|T = 1)\}$ and: (ii) lower bound $\{(E(S(p(0.10)) - E(S(p(0.90))) * [Pr(L = 1|T = 1) - Pr(L = 1|T = 0)] / Pr(L = 1|T = 1)\}$, where $E(S(p(90)))$ and $(S(p(10)))$ are, respectively, the mean total earnings for those in the 90th and 10th quantile in the control group (these quantiles are reported in Columns 3 and 4). Columns 6 and 7 then report bounds on the composition effect calculated using this formula. The upper bound of the productivity effect is the difference between the earnings effect and the upper bound of the composition effect. The lower bound of the productivity effect is the difference between the earnings effect and the lower bound of the composition effect. These are reported in Columns 8 and 9. Attanasio et al. [2011] further show that if we assume that the non-program earnings of those individuals who would have found employment regardless of the Treatment (the "always employed") are at least as high as the non-program earnings of the individuals that were induced by the Treatment to switch from unemployment to employment (the "compliers"), then the earnings effect corresponds to the lower bound of the productivity effect (see Attanasio et al. [2011] for more details). In Columns 1 and 2, earnings for individuals out of paid employment are equal to zero. In Columns 3 and 4, earnings for individuals out of paid employment are excluded. All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table A8: Robustness Checks

Dependent Variable: Labor Market Index
OLS regression coefficients, IPW estimates, robust standard errors in parentheses
Lee [2009] Bounds in brackets
Bootstrap pvalues in braces: unadjusted p-values (left) and Romano and Wolf [2016] adjusted p-values (right)

	(1) All	(2) Women	(3) Men	(4) Services	(5) Manufacturing	(6) Non-Kampala	(7) Batches
T2: Firm Trained	.260***	.191	.334**	.105	.394***	.361***	.264***
	(.099)	(.138)	(.140)	(.141)	(.139)	(.111)	(.099)
	[.122 ; .289]	[.222 ; .197]	[.092 ; .334]	[.187 ; .294]	[.227 ; .312]	[.234 ; .362]	
	{.007; .014}	{.170 ; .298}	{.018 ; .030}	{.459 ; .459}	{.005 ; .012}	{.002 ; .003}	{.006 ; .158}
T3: Vocationally Trained	.458***	.410***	.504***	.355***	.554***	.477***	.486***
	(.090)	(.122)	(.133)	(.131)	(.126)	(.099)	(.101)
	[.366 ; .487]	[.313 ; .462]	[.404 ; .505]	[.229 ; .397]	[.380 ; .515]	[.432 ; .511]	
	{.000 ; .000}	{.001 ; .004}	{.000 ; .001}	{.010 ; .028}	{.000 ; .000}	{.000 ; .000}	{.000 ; .001}
T3A: Vocationally Trained x Second Batch of Trainees							-.124
							(.158)
							{.784 ; .784}
T4: Vocationally Trained + Matched	.303***	.185	.399***	.201	.364***	.385***	.328***
	(.100)	(.139)	(.143)	(.145)	(.141)	(.114)	(.099)
	[.345 ; .385]	[.232 ; .324]	[.386 ; .456]	[.320 ; .385]	[.410 ; .361]	[.391 ; .430]	
	{.003 ; .006}	{.183 ; .298}	{.005 ; .016}	{.171 ; .294}	{.008 ; .017}	{.000 ; .003}	{.001 ; .047}
T5: Untrained, Matched	.242**	.335**	.178	.325**	.215	.184*	.248**
	(.096)	(.139)	(.134)	(.144)	(.131)	(.103)	(.097)
	[.236 ; .242]	[.334 ; .361]	[.169 ; .160]	[.347 ; .391]	[.167 ; .112]	[.151 ; .229]	
	{.009 ; .014}	{.017 ; .043}	{.176 ; .176}	{.022 ; .062}	{.095 ; .095}	{.070 ; .070}	{.008 ; .179}
Mean Outcome in Control Group	.801	.467	1.034	.481	.996	.714	.801
Control for Baseline Value	Yes	Yes	Yes	Yes	Yes	Yes	Yes
P-values on tests of equality:							
Firm Trained = Vocationally Trained	.057*	.121	.264	0.089*	.274	.320	.049**
N. of observations	3,606	1,595	2,011	1,482	2,112	2,870	3,606

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The data used is from the baseline and first three follow-up worker surveys. We report OLS regressions, where we use inverse probability weighting and robust standard errors are reported in parentheses. We report Lee [2009] bounds in brackets, where we implement a conditional Lee Bounds procedure that is able to condition on strata dummies in Columns 1-3 and 6-7, and to condition on region dummies and a dummy for having a level of education at the median or above at baseline in Columns 4-5. We report pvalues adjusted for multiple testing in braces. These are computed using the step-down procedure discussed in Romano and Wolf [2016], with 5000 bootstrap replications. In Columns 1-5 the dependent variable is the Labor Market Index that is computed using the following variables: any paid work in the last month (dummy), any wage employment in the last month (dummy), any casual work in the last month (dummy), hours worked in wage employment last week, hourly wage rate, total earnings in the last month. Hourly earnings and total earnings are set to zero for workers with no earnings. The index is constructed by converting each component into a z-score, averaging these and taking the z-score of the average. z-scores are computed using means and standard deviations from the control group at baseline. In those specifications we are able to control for the baseline value of the index. Manufacturing sectors are: motor-mechanics, plumbing, construction, electrical wiring and welding. Service sectors are: hairdressing, catering and tailoring. Workers are assigned to Manufacturing or Sevice sectors according to stated preferences over their ideal job, reported at baseline. In Column 6 we restrict the sample to labor markets outside of Kampala. All regressions include strata dummies, survey wave dummies, a dummy for the implementation round and dummies for the month of interview. We also control for the following baseline characteristics of workers: age at baseline, a dummy for whether the worker was married at baseline, a dummy for whether the worker had any children at baseline, a dummy for whether the worker was employed at baseline, and a dummy for whether the worker scored at the median or above on the cognitive test administered at baseline. Columns 1 and 4-7 further control for a complete set of strata dummies. Columns 1-3 further control for region dummies, a dummy for female and a dummy for having a level of education at the median or above at baseline. The weights for the IPW estimates are computed separately for attrition at first, second and third follow-up. The instruments for the IPW estimates are whether the worker was an orphan at baseline, a dummy if anyone in the household of the worker reported having a phone at baseline, a dummy for whether the worker reported being willing to work in more than one sector at the time of their original application to the VTIs and dummies for the survey team the worker's interview was assigned to in each of the three follow-up survey rounds. At the foot of each Column we report p-values on the null that the impact of the vocational training is equal to the impact of firm training (T2=T3). All monetary variables are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table A9: Spell Descriptives

	Steady State: November 2015 (Data from Second and Third FUP)
Unemployment rate (%)	52.25%
% of initial spells that are LEFT CENSORED	51.55%
% of initial spells that are RIGHT CENSORED	55.51%
% of initial UNEMPLOYMENT spells that are RIGHT CENSORED	60.08%
% of initial EMPLOYMENT spells that are RIGHT CENSORED	37.74%
Average duration of initial UNEMPLOYMENT spell [Months]	21.11
Average duration of initial EMPLOYMENT spell [Months]	15.55

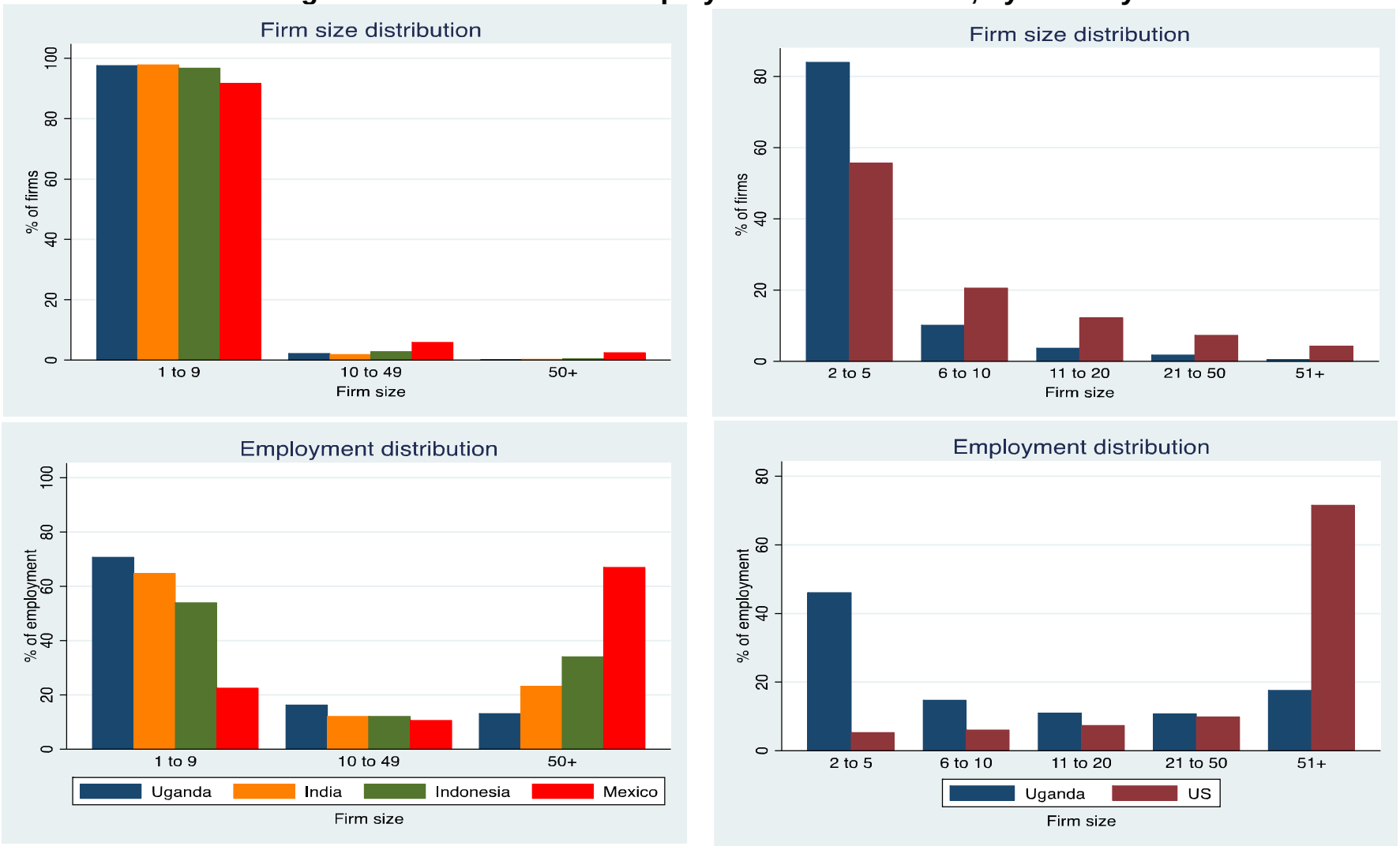
Notes: The descriptive statistics use data from the second and third worker follow-up. The dataset is a cross-section of workers, and for each worker it contains information on: spell type (employment, unemployment), spell duration (in months), earnings in employment spells (in USD), dates of transitions between spells, and type of transition: (i) job to unemployment, (ii) unemployment to job, or (iii) job to job. Wages are deflated and expressed in terms of August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. The statistics are computed for at most two spells (and one transition) per individual. The initial spell is identified as the (employment or unemployment) spell that was ongoing in November 2015. Spells are right censored at the date of interview. Spells are left censored at 1 August 2014. Casual and agricultural occupations are coded as unemployment. Self-employment is coded as employment (but self-employment spells are assigned a separate spell). The unemployment rate is defined as the percentage of the sample that was unemployed in their first spell.

Table A10: Firm-Side Selection

	Firm was Interested in Meeting a Matched Worker		
	(1)	**(2)**	**(3)**
T2: Wage Subsidy Offer	.306***	.306***	.779***
	(.037)	(.037)	(.215)
T4: Matched to Vocationally Trained Worker	.015	.016	.248*
	(.026)	(.026)	(.133)
Firm size		.012	.034*
		(.011)	(.018)
Log profits per worker		-.031	.024
		(.041)	(.041)
Log capital per worker		.039	.045
		(.034)	(.037)
T2: Wage Subsidy Offer x Firm Size			-.040
			(.028)
T4: Matched to Vocationally Trained Worker x Firm Size			-.034*
			(.019)
T2: Wage Subsidy Offer x Log profits per worker			-.232**
			(.093)
T4: Matched to Vocationally Trained Worker x Log profits per worker			.023
			(.095)
T2: Wage Subsidy Offer x Log capital per worker			.015
			(.085)
T4: Matched to Vocationally Trained Worker x Log capital per worker			-.068
			(.079)
Mean outcome in T5: Untrained, Unmatched group		.086	
Number of observations (firms)		789	

Notes: ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level. The Table reports results from OLS regressions. Firms in the Control group are not included in the analysis. All independent variables are measured at baseline. All regressions further include the following baseline controls: dummy for whether owner is female, years of education of owner, sector and branch dummies. The excluded category in the treatment dummies is the Match group. The top 1% monetary values are excluded. Manufacturing sectors are: motor-mechanics, plumbing, construction, electrical wiring and welding. Service sectors are: hairdressing, catering and tailoring.

Figure A1: Firm Size and Employment Distributions, by Country

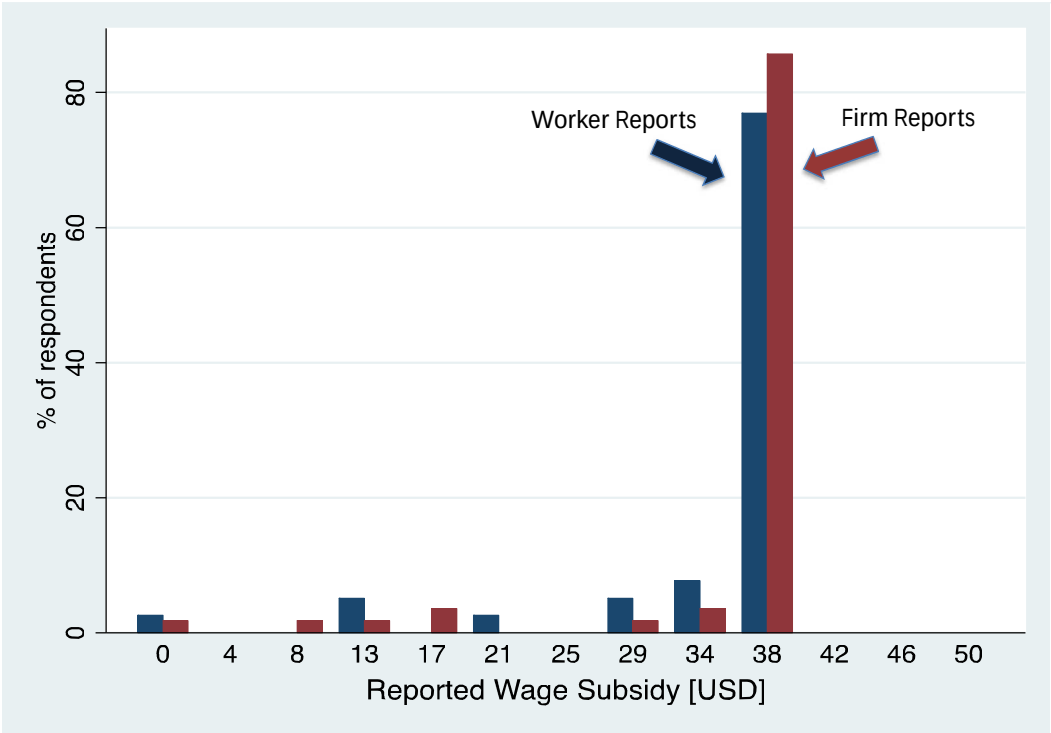


Notes: The data used to produce these figures is from the following sources: Uganda – 2010 Census of Business Establishments collected by the Uganda Bureau of Statistics; US - 2010 Business Dynamic Statistics collected by the US Census Bureau; India, Indonesia and Mexico - data are from Hsieh and Olken (2014). Data are from 2011 for India, 2006 for Indonesia and 2008 for Mexico. The firm size distribution reflects the % of firms that employ 0 to 9, 10 to 49, and 50+ employees. The employment distribution reflects the percentage of workers employed in firms that employ 0 to 9, 10 to 49, and 50+ employees.

Figure A2a: Wage Distribution of Unskilled Workers at Baseline



Figure A2b: Worker-Firm Wage Subsidy Splits



Notes: The top graph shows the distribution of unskilled workers' wages at first follow-up. The solid line is drawn at the suggested wage subsidy to workers under the on-the-job training treatment, and the dashed line indicates to the median (unskilled) wage at baseline. A Kernel density estimate of the distribution of wages is also shown. The lower histogram shows the reported monthly earnings of workers hired through the on-the-job training treatment, where the first bar is always the worker's self-reported wage, and the second bar is what the firm reports paying the worker.

Figure A3: Worker Spells Data

