The Returns to Skills During the Pandemic: Experimental Evidence from Uganda*

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Abstract

The Covid-19 pandemic represents one of the most significant recent shocks to the world labor market. We present evidence from a field experiment to understand whether and why skilled and unskilled workers were differentially impacted by the shock, in the context of a low-income economy, Uganda. We leverage a panel of workers and firms, tracked from 2012 to 2022, and our data include high frequency surveys over the pandemic. In 2013, workers were randomly assigned to six months of vocational training, in one of eight high productivity sectors. We document that over the pandemic, employment and earnings follow V-shaped dynamics: treated (skilled) workers were impacted more by lockdowns but recovered faster between them, though their outcomes remained below pre-pandemic levels in February 2022. Cumulatively over the pandemic, skilled workers spend 61% more time than controls employed in one of the study sectors, and earn 17% more than controls. We explore supply and demand mechanisms that sustained returns to skills. We find that skilled workers were more likely to be laid off early in the pandemic as firms responded to the severe uncertainty by laying off the highest earners. However, skilled workers recover from this job loss because of their greater accumulation of sector-specific experience pre-pandemic, and the certifiability of their skills enabling them to switch employers in the same sector during the crisis. Our findings have implications for understanding the returns to skills acquired through vocational training in good and bad economic times. *JEL: J24, O12.*

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1 Introduction

The Covid-19 pandemic represents one of the most significant shocks to the world economy in the last few decades. At its height, the pandemic led to an estimated loss of 144 million jobs globally, with hours worked falling by 20% and both margins remaining below pre-pandemic levels through to at least 2022 [ILO 2021, 2022]. These impacts were worse in lower-income countries, even if many of them were not as strongly affected in terms of official case rates for Covid-19. The pandemic disrupted labor markets through both supply and demand channels. On the supply side, mobility restrictions reduced worker's ability to travel to work. On the demand side, the pandemic reduced the ability of firms to conduct face-to-face trade, disrupted supply chains, and ultimately caused huge uncertainty over firm's survival prospects.[1]

The uneven economic toll of the pandemic by household income or gender, including in low-income settings is well documented [Egger *et al.* 2021, Josephson *et al.* 2021]. We study the issue in Uganda and focus in on a key source of heterogeneity: skills, that is of intrinsic interest, a primitive for differences across dimensions such as occupation or income, and more amenable to policy intervention. Specifically, we exploit a randomized skills training intervention, implemented six years prior to the pandemic, and track young jobseekers from 2012 through to 2022. We use this to study how the pandemic differentially impacted skilled and unskilled workers, and unpack the mechanisms driving the returns to skills through the crisis.

There are good reasons for skilled and unskilled workers to be differentially impacted. Skilled and unskilled workers might differ in their resilience to the shock due to their pre-pandemic accumulation of differing amounts of labor market experience, attachment to good firms and sectors, search capital, earnings and savings. Skilled workers also can have more certifiable skills, enabling them to switch to new firms more easily; at the same time, their skills might be less transferable across sectors and so they may have difficulty in taking up new opportunities as the economy recovers from the pandemic. Workers might be differentially exposed to the shock because the sectors they work in differ in their reliance on face-to-face trade, because their firms or jobs are differentially exposed to supply chain disruptions, or because to survive in times of unprecedented uncertainty, firms are differentially likely to lay off workers with different skills and hence earnings.

Our analysis builds on our earlier work from the same project using pre-pandemic data to study the returns to skills acquired through vocational and firm-sponsored training [Alfonsi *et al.* 2020], and to study how vocational training impacted job search strategies [Bandiera *et al.* 2023]. Our core analysis is based on the same panel of workers tracked over four waves from 2012 until 2018. To understand whether the returns to skills survive a large aggregate shock, during the pandemic

[1] Altig *et al.* [2020] quantify the scale of the pandemic shock using measures of economic uncertainty. Constructing such indicators for the US and UK before and during the pandemic, they suggest the economic impact of the pandemic was unprecedented. The reasons for this are twofold: the suddenness and scale of the economic shock, primarily through job losses, and the severity of the economic contraction relative to the size of the mortality shock.

we implemented three additional (phone) surveys: in late 2020, late 2021 and early 2022. The resulting 10-year panel of 1100 workers allows us to build a rich picture of the dynamic evolution of worker skills, employment, earnings, sectoral allocations, expectations, search behavior and savings in good times and bad.

Three study features are key to our analysis.

First, at baseline, workers in our study were young job seekers (aged 20 on average), equally split by gender, and with limited labor market experience and skills. The original field experiment followed an oversubscription design where in 2013 we randomly assigned individuals to an offer of receiving vocational training at one of five reputable vocational training institutes (VTIs) throughout Uganda. Each VTI could offer standard six-month courses in eight sectors across manufacturing and services: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering. Applicants were randomly assigned to either receive sector-specific vocational training or not. 65% of workers take-up the offer of training, and 95% of workers completed their courses conditional on enrolment. Given our focus is on the returns to skills over the pandemic, we consider ATT estimates of differences in outcomes between compliers that take-up vocational training courses (who we refer to as skilled or treated workers), and controls (who we refer to as unskilled or control workers).

Second, the field experiment is a two-sided labor market study in which we also track 2000 SMEs in the eight study sectors where we offered vocational training. These firms are tracked over a decade from 2012: we implemented four survey waves pre-pandemic, and to understand labor demand responses to the shock, we implemented two further survey waves during the pandemic. We use this data to illuminate demand-side mechanisms through which skilled workers are differentially exposed and resilient to the pandemic shock.

Third, our study setting, Uganda, shares many hallmarks of economies throughout Sub Saharan Africa, including an absence of social insurance, meaning that resilience in labor market outcomes to aggregate shocks is key for lifetime welfare. Uganda also experienced lock-downs during the pandemic. The first occurred in April/May 2020, and the second in June/July 2021 around the second peak of cases. In our pandemic surveys, we purposefully collected information on labor market outcomes recalled before, during, and just after each lockdown enabling us to reconstruct a detailed picture of labor market dynamics over the crisis.

Our first set of results briefly re-examine pre-pandemic differences between treated and control workers. This establishes differences in skills, labor market attachment and other outcomes on the eve of the pandemic. 66% of controls report having some sector-specific skills, and this rises to 100% for those that took up vocational training. Using a sector-specific skills test we designed, we show that, relative to controls, compliers have a 23% increase in their measurable skills (or $.41\sigma$ of test scores). Conditional on sector of employment, we show these skills differences translate into differential task assignments within firms pre-pandemic. On labor market outcomes, in our final pre-pandemic survey from March to July 2018, around 55 months after workers graduate from vocational training, skilled workers: (i) are 18.1pp more likely to be working in one of the eight study sectors and, (ii) have 25% higher total monthly earnings. Finally, we consider how these translate into cumulative impacts of treatment across all four pre-pandemic survey waves from 2014 to 2018. We find that skilled workers: (i) spend 20% fewer months unemployed, (ii) accumulate 117% more experience working in one of the eight study sectors, and (iii) accumulate 59% higher earnings.

Our second set of results document how labor market outcomes differ between skilled and unskilled workers over the pandemic. To do so reliably, we note that pre-pandemic only 12% of workers attrit by the fourth follow-up in 2018. While attrition rises to 31% in the pandemic waves, nearly all of this occurs between waves 4 and 5. We observe almost zero additional attrition over the three pandemic survey waves.

Examining the dynamics of labor market outcomes we find that: (i) employment and earnings margins crash around each lockdown; (ii) there is a V-shaped recovery in employment and earnings for all workers around each lockdown; (iii) skilled workers are harmed more by each lockdown, but recover more quickly between lockdowns; (iv) as the economy begins recovering in February 2022, the levels of employment and earnings of skilled and unskilled workers remain substantially lower than pre-pandemic.

To quantify the returns to skills over the pandemic, we calculate the differential cumulative labor market impacts between skilled and unskilled workers, essentially integrating over the dynamic treatment effects. We find that skilled workers spend 61% more time employed in one of our study sectors than unskilled workers, and their total earnings are 17% higher. We show the robustness of these core results to addressing selective attrition on non-observables.

Our key takeaway is that the returns to skills acquired through vocational training survive the pandemic. These returns go beyond measured contemporaneous labor market earnings outcomes (as would be included in any IRR calculation) but also include building resilience and insuring workers against aggregate shocks even as severe as the Covid-19 pandemic.

This still leaves open the question of the overall impacts of the shock on the labor market outcomes. At one extreme, the shock might be severe but brief: once the economy reopened, labor market outcomes returned to their pre-pandemic levels. Hence it would be as if the pandemic caused the labor market to freeze, but then it thawed quickly upon reopening. Our data emphatically rejects this view, showing that despite the resilience of skill returns, the pandemic caused persistent losses for workers. Three pieces of evidence confirm this interpretation.

First, during the pandemic, skilled workers experienced flat or declining labor market outcomes, with employment rates in study sectors 19pp lower and total earnings 17% lower by February 2022 compared to pre-pandemic levels. Unskilled workers saw slightly larger declines on each margin. Second, comparing actual outcomes to pre-pandemic projections reveals lasting impacts, with skilled (unskilled) workers 37% (49%) less likely to be employed in study sectors and earnings 34% (45%) below trend. These magnitudes are at the top end of estimates from the literature on dynamic labor market outcomes for displaced workers typically using administrative data from high-income settings [Jacobsen *et al.* 1993, Couch and Plaszek 2010, Davis and von Wachter 2011]. Third, skilled workers demonstrated greater mobility across firms in the same sector during the first lockdown, consistent with the role of certifiable skills in facilitating job mobility. However, by February 2022 skilled workers also significantly shift into casual work, reflecting skills downgrading, similar to patterns observed in the US and middle-income countries after economic shocks [Huckfeldt 2022, Dix-Carneiro *et al.* 2024].

The remainder of our analysis examines mechanisms through which the returns to skills are maintained during the pandemic. We distinguish between mechanisms relating to: (i) differential characteristics or behaviors of skilled and unskilled workers, or supply-side mechanisms, and (ii) firm behaviors that have differential impacts on skilled and unskilled workers, or demand-side mechanisms. We shed light on demand-side mechanisms by drawing on data from firms collected as part of the original two-sided field experiment.

On supply-side mechanisms, we build on the fact that on the eve of the pandemic, skilled workers had accumulated greater experience working in good sectors and in good firms, and accumulated different search capital and higher savings. These channels might cause skilled and unskilled workers to differ in their resilience to the shock. We examine these mechanisms in turn by following Hainmueller [2012] and reweighting controls to match pre-pandemic covariate moments among compliers along any given dimension.

We find that the accumulation of sector-specific experience pre-pandemic can account for much of the subsequent returns to skills over the pandemic on the extensive margin, suggesting the accumulation of sector-specific skills matters for individual resilience to job loss due to the pandemic shock [Topel 1991, Neal 1995, Kletzer 1998]. We find a more limited role for pre-pandemic experience of good jobs *per se*, or of good worker-firm matches in explaining the returns to skills over the pandemic.

We find no evidence that the greater savings accumulated pre-pandemic by skilled workers explains the returns to skills over the pandemic. Nor do we find that search behaviors of skilled or unskilled workers differ during the crisis. Finally, we confirm weak interactions between health, skills and labor market outcomes. We establish that: (i) pre-pandemic, there was no differential in self-reported health between skilled and unskilled workers; (ii) over the pandemic, we find no evidence that concerns about health or Covid risks impacted job search behavior or job preferences.

On demand-side mechanisms, we first examine whether the initial sector of employment affects the returns to skills over the pandemic. To establish why sectors might matter, we compare firm characteristics between the hardest and least hit sectors in terms of employment impacts. Harder hit sectors have firms that are smaller, less profitable and more reliant on face-to-face customer interactions. Accounting for overall differences in sectoral assignment of skilled and unskilled workers on the eve of the pandemic explains nearly all of their greater retained employment in study sectors over the pandemic, but has muted impacts on explaining the returns to skills on earnings margins. In contrast, focusing on differences solely in the quality of firms workers are employed in on the eve of the pandemic, we explain around half of the returns on the margin of total earnings over the pandemic.

Finally, we exploit the firm data to understand whether and how firms differentially retain, layoff and recruit skilled and unskilled workers in response to the shock. We find that in the first lockdown firms are far more likely to immediately lay off the highest earning workers, that is those most experienced or skilled. We find that all firms – irrespective of sector – adopt this kind of first-in-first-out (FIFO) strategy in the face of the pandemic and the urgent need to reduce wage bills on account of falling profits. Our firm-side data confirms that later in the pandemic, surviving firms attempt to recruit workers with experience in their sector, and skilled workers are poised to take advantage of this given their certifiable skills and greater accumulation of sector-specific experience pre-pandemic.

Our work contributes to three classes of literature. The first is the large body of work examining labor market impacts of the pandemic, including how its economic toll has been unevenly distributed because of its varying impacts across groups in high-income settings [Adams-Prassl *et al.* 2020, Alon *et al.* 2022, Blundell *et al.* 2022, Chetty *et al.* 2023] and low-income settings [Egger *et al.* 2021, Josephson *et al.* 2021, Mahmud and Riley 2023]. Evidence on differential impacts of the pandemic across the distribution of worker skills is scarcer and largely limited to high-income settings [Couch *et al.* 2020]. In low-income settings, a few studies have tracked vocational trainees over the pandemic, with a focus on differential impacts by gender [Alfonsi *et al.* 2023, Chakravorty *et al.* 2023, Lang and Sether 2024].

We build on these studies by exploiting experimental evidence to document causal impacts of skills on labor market dynamics over the pandemic, and to provide new insights on open questions on the underlying supply- and demand-side mechanisms driving the returns to skills over the crisis. The most closely related paper is Barrera-Osorio *et al.* [2022], who link applicants randomly allocated into a job training program focused on service sectors in Cali, Colombia, to monthly administrative records on employment. They track workers from June 2017, through their graduation in training courses from December 2018, through to August 2021. In contrast to our findings, they report that the returns to skills disappear – or are even negative – during the pandemic. We discuss the relationship to these earlier sets of work while presenting our results.

Our second contribution speaks directly to concerns that the returns to interventions might vary due to their interaction with aggregate shocks [Rosenzweig and Udry 2020]. By evaluating the returns to the same offer of vocational training in good times and bad, we document that returns to skills are sustained. However, the mechanisms driving the returns in good times and bad differ. In our earlier work, we documented that supply-side mechanisms, such as certification and job search behavior, help generate returns to vocational training in times of economic stability [Alfonsi *et al.* 2020, Bandiera *et al.* 2022]. In contrast, over the pandemic we find that while skills certification remains important because it enables workers to switch firms in the same sector, additional mechanisms such as skilled workers greater accumulation of sector-specific experience and their greater likelihood to work at better firms are also key to ensuring the resilience of skilled workers. Other mechanisms such as savings and search behavior might play less of a role because of the speed and severity of the pandemic shock.

Finally, we draw inspiration from the literature on labor market dynamics of displaced workers [Jacobsen *et al.* 1993, Farber 1997, Kletzer 1998]. Some of this work has focused on how dynamics vary with labor market conditions or the business cycle or in the presence of correlated shocks across workers in the form of mass layoffs. This literature has also considered heterogeneous impacts of job loss by worker skills [Seim 2019], job content [Athey *et al.* 2023], occupation-specific human capital [Huckfeldt 2022, Braxton and Taska 2023], or demand-side characteristics such as firm quality [Schmieder *et al.* 2023].[2]

We contribute to this literature in two ways. First, earlier work is almost exclusively based in high- or middle-income settings, with far more limited evidence from the poorest countries where the highest risks of job loss actually exist [Donovan *et al.* 2023, Gerard *et al.* 2023, Carranza and McKenzie 2024]. Second, we take insights from this body of work to rich panel data on workers and firms, to simultaneously understand how supply- and demand-side mechanisms interact to drive labor market dynamics for skilled and unskilled workers during the pandemic. This highlights that firms respond to the uncertainty created by the shock using first-in-first-out firing strategies, but that skilled workers recover form this given their certifiable skills and greater accumulation of sector-specific experience pre-pandemic.

The paper is organized as follows. Section 2 describes our data and design of the field experiment. Section 3 estimates pre-pandemic differences in skills and labor market outcomes for those offered vocational training relative to controls. Section 4 documents how labor market outcomes differ between skilled and unskilled workers over the pandemic. Sections 5 and 6 examine supply- and demand-side mechanisms sustaining the returns to skills over the pandemic. Section 7 concludes. The Appendix presents further results and robustness checks.

2 Setting and Design

2.1 Sample

Workers Our core analysis utilizes a panel of workers, tracked since 2012 when they were labor market entrants, and collected as part of an earlier field experiment evaluating a vocational training intervention [Alfonsi *et al.* 2020]. In the field experiment, we advertized an offer of potentially receiving six months of sector-specific vocational training, sponsored by the NGO BRAC, at one

[2] Key papers on dynamics over the business cycle include Beaudry and DiNardo [1991], Kahn [2010], Davis and von Wachter [2011] and Oreopoulos *et al.* [2012], and those on mass layoffs include Couch and Placzek [2010], Carrington and Fallick [2014] and Lachowska *et al.* [2020].

of five vocational training institutes (VTIs) across Uganda. Eligible applicants were on average aged 20 in 2012, 43% were women, and disadvantaged young job seekers were targeted.[3] Table 1 shows their labor market outcomes at baseline: unemployment rates were over 60% (Column 1), with a reliance on insecure casual work rather than wage or self-employment. Average monthly earnings were $6, corresponding to less than 10% of the Ugandan average in 2012.[4]

Firms To understand demand-side mechanisms driving the returns to skills over the pandemic, we draw on data from firms also collected as part of the original field experiment. As detailed later, firms were tracked four times from 2012 to 2018, and twice further during the pandemic. To draw this sample in 2012, we conducted a census of firms in 15 urban labor markets, selecting firms: (i) operating in one of the manufacturing and service sectors in which we offered sector-specific vocational training, and (ii) having between one and 15 employees (plus an owner). The second restriction largely excludes micro-entrepreneurs and ensures a focus on small and medium sized firms that are central to employment generation in Uganda.

We end up with a sample of 2300 firms, that in aggregate employ 6000 workers at baseline, with the average firm size being three (plus a firm owner). These types of firm offer good jobs: earnings are higher in these sectors than many others. They collectively employ about 16% of workers aged 20-30, a percentage that more than doubles if we exclude youth involved exclusively in agriculture.

2.2 Timeline

Figure 1A shows the study timeline: the baseline worker survey occurred in 2012 ahead of the vocational training, which started in January 2013 at the partner VTIs. Workers were tracked over four surveys pre-pandemic, fielded 24, 36, 48 and 68 months after baseline. During the pandemic we ran three further waves of (phone) surveys: from September 2020 to January 2021 (wave 5), in September/October 2021 (wave 6), and in February 2022 (wave 7).

In each survey we ask questions on labour market outcomes such as employment, earnings, sectoral allocations, search behaviors and expectations. The pandemic surveys included modules related to experiences of the pandemic.

Figure 1B narrows in on the timeline over the pandemic, overlaying it with the time series of confirmed Covid-19 cases and periods of lockdown. The first lockdown occurred in April/May

[3]The eligibility criteria were being aged 18-25, having completed from 7 to 11 years of education, not being in full-time school, being poor – using a poverty score based on family size, assets owned, type of building lived in, village location, fuel used at home, number of household members in school, monthly wage and education of the household head. Applicants were ranked on a 1-5 scale on each dimension and a total score was computed. A relative threshold score (varying by geography) was used to select eligibles. Table A1 describes baseline characteristics of our sample: the vast majority were out of school and had never received vocational training.

[4]Table A1 compares our sample to those aged 18-25 in the Uganda National Household Survey from 2012/3. The program is well targeted: our sample has worse labor market outcomes at baseline (Columns 7 to 9), and that remains the case when we compare to youth in the UNHS that report being labor market active.

2020 (between waves 4 and 5), and the second in June/July 2021 (between waves 5 and 6). The second lockdown is considered to have been less strict.

In waves 5 and 6 we asked questions in relation to three time frames of recall, so we tracked individual labor market outcomes with high frequency. The periods of recall in wave 5 span the eve of the pandemic, during, and just after the first lockdown. Hence for expositional ease, we refer to wave 5 as wave $L1$. The recall periods in wave 6 span the time before, during, and just after the second lockdown. Hence we refer to wave 6 as wave $L2$. As Covid-19 cases returned to near zero and the economy began recovering by February 2022, we refer to wave 7 as wave R.[5]

2.3 Design

Our field experiment follows an oversubscription design where we randomly assigned eligible applicants to the offer of vocational training at one of five reputable VTIs. Each VTI could offer standard six-month training courses in eight sectors covering manufacturing and services. Applicants were randomly assigned to receive the training, using a stratified randomization where strata are region of residence, gender and education.

Vocational Training The vocational training intervention provides workers six months of sector-specific training in one of eight sectors: welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering. Our intervention partner BRAC covered training costs, at $470 per trainee. Courses were full-time, and worker attendance was monitored. Upon graduation, trainees receive a certificate verifying their skills. As Alfonsi *et al.* [2020] document, in good times there are high returns to having certifiable skills from reputable VTIs in these urban labor markets.

Vocational Training and Matching Within those assigned to training, the original field experiment included a second stage of randomization. In a first group, graduating trainees transitioned into the labor market unassisted. A second group received light touch offers to match for job interviews with firms in our firm sample. The impact of the matching on job search and outcomes in the pre-pandemic period is studied in Bandiera *et al.* [2023]. In this paper given our focus on the returns to skills during the pandemic, more than six years after the interventions occurred, we pool both and show the robustness of key results in each treatment arm.

[5] Uganda had very limited policy responses to support firms and workers during the pandemic. In March 2020, some formal firms were allowed to reschedule social security contributions and delay payments for three months, and in April 2020 a food distribution scheme to aid the 1.5million urban poor was started. In our firm sample, only 6% of firms interviewed over the pandemic report either applying or receiving support. Similarly, in our sample of workers, very few report having applied for the food distribution scheme or having benefitted from it.

2.4 Balance, Attrition and Compliance

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

Attrition We consider attrition in two periods: pre-pandemic from baseline until the fourth follow-up (March to July 2018), and over the three pandemic survey waves. Column 1 of Table 2 shows that pre-pandemic attrition is low: 12% of workers attrit by the 68-month fourth follow-up, and this is uncorrelated to treatment. The remaining Columns show that: (i) attrition rises to 31% in the pandemic waves; (ii) nearly all of this occurs between waves 4 and wave $L1$, and we then have close to zero further attrition through to our final survey wave R; (iii) during the pandemic, controls are 8-9pp more likely to attrit than those offered vocational training.[6]

In Table 3 we consider differential attrition between treatment and control groups. To do so we re-estimate the correlates of attrition between baseline and waves 4 to R, further including interactions between baseline characteristics and treatment. The baseline characteristics we consider are those that could affect behaviors and labor market outcomes during the pandemic: whether the worker reports having any sector-specific skills, their cognitive skills, their perceived locus of control, gender, their desired sector of training, whether they reside in Kampala, and whether they were employed at baseline.

On most margins and survey waves we find little evidence of heterogeneous attrition between the treatment and control groups, either before or during the pandemic. However, those with any sector-specific skills and resident in Kampala at baseline are significantly less likely to be tracked until survey wave 4. Table A3 re-examines balance of baseline labor market outcomes of non-attriters by survey waves 4 to R. In line with little selective attrition by treatment status, on each outcome there are no significant differences between treatment and control groups among non-attriters. We later show the robustness of our results to alternative approaches addressing selective attrition on non-observables.

Compliance 65% of workers take-up the offer of vocational training. The VTIs were paid half the training fee at the start and half after the worker completed the training, resulting in a 95% completion rate conditional on enrolment. Table A4 shows correlates of take-up. Individuals with lower cognitive ability, lower locus of control, or resident outside Kampala are more likely to take-up the offer. Given our focus is on the returns to skills over the pandemic, our analysis mostly considers ATT estimates, so the differential impact between compliers taking-up vocational

[6]This pre-pandemic attrition rate compares favorably to studies conducted in good economic times such as Attanasio *et al.* [2011] (18%), and Card *et al.* [2011] (38%). In the meta-analysis of McKenzie [2017], all but one study have attrition rates above 18%. During the pandemic period, our close to zero attrition rate replicates studies based on administrative data [Barrera-Osorio *et al.* 2022] and compares favorably to studies tracking similar populations, which report attrition rates of 7% and 15% [Alfonsi *et al.* 2023, Chakravorty *et al.* 2023].

training (who we refer to as skilled or treated workers) relative to controls (who we refer to as unskilled or control workers). Whenever we present descriptive statistics on controls, we reweight their outcomes to account for their likelihood to comply based on the results from Table A4.[7]

3 Pre-pandemic Outcomes

We first establish the impacts of vocational training on pre-pandemic labor market outcomes. This period is one of continued economic growth in Uganda, so these results map to the literature on long run returns to vocational training [Ibarrarán *et al.* 2019, Aizer *et al.* 2021, Kugler *et al.* 2022, Silliman and Virtanen 2022]. We use OLS to first estimate the following ITT specification for outcome y_{isw} for worker i in strata s in survey wave w:

$$y_{isw} = \alpha + \beta VT_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{isw}, \tag{1}$$

where VT_i is a dummy equal to one if worker i is assigned to the offer of vocational training, y_{is0} is the baseline value of the outcome (where available), $\mathbf{x}_{is0}$ are baseline characteristics of the individual, and λ_s are strata fixed effects. To estimate ATTs, we run a 2SLS specification where we replace the offer of vocational training with whether the worker took up the offer, and instrument take-up with the randomized offer of vocational training, VT_i. We present robust standard errors as randomization is at the individual level.[8]

Sector-Specific Skills In our earlier work using data from this project [Alfonsi *et al.* 2020], we showed how vocational training translates into skills accumulation. We briefly review those results. We measure skills using a sector-specific skills test developed in conjunction with skills assessors of written and practical occupational tests in Uganda. Each test comprises seven questions (multiple choice and more complex questions). Workers had 20 minutes to complete the test, and we convert answers into a 0-100 score. The test was given to all workers (including controls) at third follow-up, measuring persistent skills accumulation. There is no differential attrition by treatment into the test. Table 4 reports the results. Panel A reports the ITT estimates $\widehat{\beta}$ from (1), and Panel B reports ATT estimates.[9]

[7] The compliance rate is slightly higher than that in Barrera-Osorio *et al.* [2022] on applicants to a job training program for service sectors in Cali, Colombia. In the meta-analysis of McKenzie [2017], most studies have training completion rates between 70 and 85%.

[8] All regressions control for the training implementation round and dummies for the month of interview. We control for the following baseline characteristics: desired sector of training, marital status, whether they have children, whether they are in work, and whether they score above median on the cognitive test score. For each covariate we also include a dummy for whether it is missing at baseline.

[9] We developed the sector-specific skills tests with skills assessors from the Directorate of Industrial Training, the Uganda Business and Technical Examinations Board, and the Worker's Practically Acquired Skills Testing Board. To ensure the test would not be biased towards merely capturing theoretical/attitudinal skills taught only in VTIs, assessors were instructed to: (i) develop questions to assess psychomotor domain, e.g. trainees ability to perform

Before administering the test, we asked workers whether they had *any* skills relevant for the study sectors. The dependent variable in Column 1 of Table 4 is a dummy equal to one if the worker reported having skills for any sector. As reported at the foot of the Table, 66% of controls report having skills relevant for some sector, and reassuringly this rises to close to 100% for those offered vocational training, as measured three years post-intervention. All workers who reported having sectoral skills took the test: others were assigned a score of 11 assuming they would answer the test at random. Column 2 shows workers offered training significantly increase their measurable sector-specific skills by 19% (or $.28\sigma$ of test scores). Columns 1 and 2 in Panel B show that among those taking up vocational training, nearly all report having some sector-specific skills, and their skill measure is 23% higher than controls when we reweight for their compliance probability (or $.41\sigma$ of test scores).[10]

Figure A1 shows the corresponding quantile treatment effects regression. The distribution of measurable skills shifts rightward: only at the lowest and highest levels of skills among controls does the offer of vocational training have insignificant impacts. For expositional ease we refer to compliers and controls as skilled and unskilled workers respectively – to emphasize that compliers have acquired skills through six months of sector-specific and vocational training. We do not claim controls are entirely without market-valued skills, as by the eve of the pandemic, they have accumulated six years of potential labor market experience.

Tasks To validate that these acquired skills are relevant to our study sectors, we consider tasks workers conduct at work. We measure these tasks in the third follow-up survey. For each of the eight study sectors, we construct a list of 30 to 40 worker tasks (based on the O*NET task list).[11] For any given task j in sector k, we construct the share of workers reporting to perform task j, separately for those taking up vocational training and controls. Figure A2 graphs the difference in these shares for each task j, color coding the Figure by sector. We focus on the four most prominent study sectors of employment.[12]

In each sector we see a divergence from the zero line in the differences in these shares: within a sector, there are some tasks performed relatively more by vocationally trained workers (at the

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 with VTI trainees and workers employed in our study sectors (neither group overlapped with our evaluation sample).

[10] We further note that: (i) workers offered vocational training and matching have no different skills accumulation to those only offered vocational training; (ii) the offer of vocational training has no impact on other dimensions of human capital such as the Big-5 personality traits, cognitive ability (as constructed from a 10-question version of the Raven's progressive matrices test) and other psychological traits.

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

[12] The data refer to all main job spells reported at the 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.

right hand side of each panel), and others performed relatively more by controls (at the left hand side of each panel). In three of the four sectors, a Chi-squared test rejects the null that the task composition of workers is the same between vocationally trained and control workers. This highlights that skilled and unskilled workers might differ in their occupation specific human capital, which can impact labor market dynamics after job loss [Huckfeldt 2022, Braxton and Taska 2023]. This is an issue we return to when studying how the returns to skills endure through the pandemic.

Labor Market Outcomes We consider labor market outcomes in the final pre-pandemic survey, at wave 4 and so measured from March to July 2018, around 55 months after workers graduate from vocational training. In Panel A of Table 4, Columns 3 and 4 show that those offered vocational training: (i) are 12.1pp more likely to be working in one of the study sectors (a 50% increase over controls); (ii) have total monthly earnings 18% higher than controls. Panel B shows that compliers: (i) are 18.1pp more likely to be working in one of the eight study sectors (a 72% increase over controls); (ii) have total monthly earnings 25% higher than controls. This confirms the persistent impacts on labor market outcomes of vocational training in times of economic stability.

Finally, we consider how skills translate into cumulative impacts on outcomes across all four pre-pandemic survey waves, from wave 1 (2014) to wave 4 (2018). In the pre-pandemic survey waves we asked workers to recall their labor market outcomes over 12 months, so we can construct a panel data set of employment spells and earnings histories, based either on monthly or quarterly recall data depending on the outcome and survey wave. From Columns 5 to 7 in Panel A we see that those offered vocational training: (i) spend 14% fewer months in unemployment; (ii) accumulate 83% more work experience in one of the study sectors; (iii) accumulate 42% higher earnings than controls. From Panel B we see that skilled workers: (i) spend 20% fewer months in unemployment; (ii) accumulate 117% more experience of working in one of the study sectors; (iii) accumulate 59% higher earnings than controls. These cumulative differences in labor market attachment to good sectors, and the resources available to workers, can determine the dynamics of their labor market outcomes during the pandemic – all issues we return to.[13]

4 Labor Market Outcomes Over the Pandemic

4.1 Estimation

During the pandemic our surveys ran from September 2020 to January 2021 (wave $L1$), September/October 2021 (wave $L2$), and February 2022 (wave R). In waves $L1$ and $L2$ key questions were asked for three time-frames of recall. In wave $L1$ these periods span the eve of the pandemic,

[13] Table A5 confirms that on all but one dimension of pre-pandemic outcomes, there are no statistically significant differences between workers with and without match offers.

during and just after the first lockdown. In wave $L2$ these periods span just prior to, during, and just after the second lockdown. We estimate the following specification by 2SLS in time-frame t from survey waves $L1$, $L2$ and R:[14]

$$y_{ist} = \alpha + \sum\nolimits_{t=1}^{t=7} \beta_t Skilled_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{ist}, \qquad (2)$$

where $Skilled_i$ indicates whether worker i took up the offer of vocational training. We instrument $Skilled_i$ with the randomized offer of vocational training (VT_i) and all other covariates are as previously described. We report robust standard errors. This specification enables us to trace the dynamic returns to skills over seven time-frames t of the pandemic. Given the estimated coefficients of interest are $\{\widehat{\beta}_t\}_{t=1}^{t=7}$, we graphically present unconditional differences between compliers and controls reweighted for their compliance probability. The regression estimates from (2) are shown in Table A6. To establish the constancy of the *impact of skills* on outcomes over the pandemic, in Table A6 we report the p-value on a test of whether treatment effects on the eve of the pandemic in the first time frame in wave $L1$ (February/March 2020), are the same as in wave R (February 2022), when the economy is recovering, H_0: $\beta_1 = \beta_7$. To establish whether workers fully recover in the *level* of outcomes over the pandemic, we report the p-value on a test of whether $\overline{y}_1 = \overline{y}_7$ for skilled and unskilled workers.

4.2 Employment

Motivated by the literature showing that following job loss, re-employment probabilities often depend on the aggregate state of the macroeconomy [Beaudry and DiNardo 1991, Kahn 2010, Davis and von Wachter 2011, Oreopoulos *et al.* 2012], we first focus on outcomes related to the extensive margin of employment. Figure 2 shows unconditional differences in each time frame for four outcomes along this margin between compliers and reweighted controls. As a point of comparison we also show the outcome from the final pre-pandemic survey wave 4. The x-axis is scaled to match the periods covered and the gray shaded regions refer to each lockdown.

Panel A examines whether individuals are employed. Pre-lockdown 1, both vocational trainees and controls have employment rates close to 85% – reflecting that when the pandemic struck they were prime age workers with six years of potential experience and high labor market attachment. During the first lockdown, employment rates for unskilled workers drop to 45%. The corresponding regression specification in Table A6 shows that employment rates drop even more for skilled workers, who are 13.4pp less likely to still be in employment ($p = .006$). Hence skilled workers are in proportionate terms, hit harder by the shock going into the first lockdown.

After the end of lockdown 1, employment rates of skilled and unskilled workers follow similar trajectories, with both dipping again during the second lockdown. The 'double dip' exactly

[14] Recall bias is unlikely to correlate to treatment given individuals were assigned to treatment six years earlier. Moreover, recall bias is less of a concern in relation to salient events [Beegle *et al.* 2012].

matches the timing of lockdowns, with the severity of the impacts for the first lockdown being greater than for the second, in line with the first being more stringently enforced. Comparing levels of outcomes around each lockdown, we observe a V-shaped recovery in employment outcomes for both groups, with the depth of the V-shaped employment shock being greater for skilled workers. However, the recovery is incomplete (so $\overline{y}_7 < \overline{y}_1$): in February 2022, employment rates remained 16pp lower for skilled workers than on the eve of pandemic in February 2020 ($p = .000$).

Panel B focuses on whether skilled and unskilled workers are employed in one of the study sectors – as a marker of working in a more productive sector, and gaining valuable labor market experience. On this margin we see pronounced differences between skilled and unskilled workers through the pandemic. As Table A6 shows, on the eve of the pandemic treated workers were 22pp more likely to be employed in a study sector ($p = .000$). They maintain this advantage over controls throughout, except during the lockdowns. After each lockdown, skilled workers recover more quickly in regaining employment in the study sectors. In February 2022 skilled workers were 17pp more likely than unskilled workers to be employed in a study sector ($p = .000$). However, neither set of workers recover in levels: in February 2022 employment rates in study sectors remain 19pp lower for skilled workers than on the eve of pandemic in February 2020 ($p = .000$).

The remaining Panels examine employment types. Panel C confirms that the differential employment dynamics between skilled and unskilled workers are driven by wage/self-employment, and this is itself largely driven by wage employment rather than workers shifting into self-employment.[15]

Panel D shows trends in casual work. To begin with, we note that unskilled workers engage in casual work at higher rates at the outset of the pandemic. This gap is maintained over the first lockdown with employment rates in casual work being significantly higher for unskilled workers around the first lockdown. However, by the time of the second lockdown these employment rates almost converge as skilled workers shift into casual work at later stages of the pandemic. By the end of the pandemic in February 2022, employment rates in casual work are 4pp higher for skilled workers than on the eve of pandemic in February 2020 ($p = .000$). This kind of skills downgrading and switch into casual work has been documented for US workers in response to job loss [Huckfeldt 2022], and in response to trade shocks in middle-income contexts [Dix-Carneiro *et al.* 2024].

On all employment margins, we cannot reject that the ATT effects are the same in the first and last time-frames of the pandemic, as shown in Table A6. Hence the magnitudes of treatment effects of skills on these outcomes remain the same at the end of the pandemic as at its start.

[15] More precisely, if we separately examine self-employment as an outcome over the time frames of the pandemic, we find: (i) on the eve of the pandemic, self-employment is far less prevalent than wage employment among controls (27% versus 48%); (ii) on the eve of the pandemic, skilled workers are not more likely to be self-employed than unskilled workers; (iii) the differential likelihood of skilled and unskilled workers to be self-employed never differs statistically in any time frame of the pandemic, including during the first or second lockdowns.

4.3 Earnings

A risk individuals face from job loss is permanently lower earnings – the 'scarring effects' of recessions [Ruhm 1991, Jakobsen *et al.* 1993, Davis and von Wachter 2011]. We examine this in Figure 3 where we repeat the earlier analysis but for earnings outcomes. The underlying regression estimates are shown in Columns 5 to 7 in Table A6. These follow very similar V-shaped and double dip dynamics for employment outcomes, with skilled workers more severely impacted by lockdowns, recovering more quickly between lockdowns, but there is no full recovery in levels, as earnings outcomes in February 2022 remain below what they were on the eve of pandemic.

Panel A of Figure 3 shows the dynamics of total monthly earnings (from all forms of employment). In nearly all time frames skilled workers have higher monthly earnings than unskilled workers. It is again the case that in the depth of each lockdown, the gap in total earnings between skilled and unskilled workers approaches zero, so that in proportionate terms, skilled workers have larger earnings losses during lockdowns. In line with the earlier results, the first lockdown suppresses earnings more than does the second. Finally, we continue to find that the recovery in levels is far from complete by the end of our study period. In February 2022 total earnings remain 17% lower for skilled workers than they were on the eve of the pandemic ($p = .026$), with the corresponding figure for unskilled workers being 24% ($p = .000$).

Panel B focuses on earnings from wage and self-employment only (including zeros). In line with the earlier extensive margin results, skilled workers retain significantly higher earnings than unskilled workers pre-lockdown 1, and as the economy recovers. In February 2022, skilled workers' monthly earnings from wage/self-employment are 16% higher than for unskilled workers, so back to close to the pre-pandemic differential. However they do not recover to their pre-pandemic level: instead they remain 19% lower than on the eve of the pandemic ($p = .016$), while unskilled workers remain 22% lower ($p = .017$).

Panel C conditions earnings on wage and self-employment. As in Panel A we see that over the pandemic, in nearly all time frames skilled workers have higher earnings than unskilled workers. Moreover, this is a margin of outcome for which there is a full recovery in levels by February 2022 for skilled and unskilled workers.

Finally, Panel D shows that earnings from casual work remain higher for control workers just pre- and post the first lockdown, but these earnings gaps disappear around the second lockdown – in line with the earlier evidence that skilled workers downgrade their skills and shift into casual work around the second lockdown.

Validation Using Worker Expectations To validate these findings on employment and earnings outcomes, in the Appendix we present results examining whether the patterns align with worker expectations on job offer arrival rates and earnings conditional on wage employment. These confirm that through the pandemic, skilled workers have higher expected job offer arrival rates from firms in sectors in which they have been trained (or wanted to be trained in for controls), and have higher expected earnings conditional on being employed in their preferred study sector. Given that, in many job search models, the minimum expected earnings from employment map to a worker's reservation wage, our data suggests skilled workers retain higher reservation wages than unskilled workers throughout the pandemic for wage employment. Hence any shift into causal work is not driven by a fall in their reservation wage.

4.4 Cumulative Impacts

To summarize the returns to skills over the pandemic, we calculate the cumulative difference in labor market outcomes over the pandemic between skilled and unskilled workers. To do so we estimate the following 2SLS specification for individual i in strata s and time-frame t:

$$\sum_{t=1}^{t=7} y_{ist} = \alpha + \beta Skilled_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{ist}, \tag{3}$$

where we again instrument $Skilled_i$ with the randomized offer of vocational training, VT_i. We take the pandemic period to be February 2020 until February 2022. The time frames of our pandemic surveys cover 14 of these months (including the most turbulent times around both lockdowns), and we interpolate outcomes over the other 11 to construct cumulative impacts using a constant imputation, namely we assume the treatment effect remains constant from any given time frame until the month before the next time frame is measured.

The results are in Table 5 where we show the four margins of employment from Figure 2 (Columns 1 to 4) and three of the earnings margins from Figure 3 (Columns 5 to 7). For each outcome we show the ATT effect from (3). In the lower part of the table we then show the implied cumulative treatment effect. Focusing on those margins where the ATT estimate differs significantly from zero we see that over the pandemic: (i) skilled workers spend 61% more time than unskilled workers employed in one of our study sectors; (ii) their total earnings are 17% higher; (iii) their earnings from wage/self-employment are 28% higher.[16]

The bottom line is that the returns to skills acquired through vocational training survive the pandemic, and further widen cumulative gaps in labor market outcomes between skilled and unskilled workers. These cumulative impacts are quantitatively important, despite skilled workers being hit harder by each lockdown. This speaks to their resilience during the pandemic.[17]

[16] The estimated returns to skills are very similar if we: (i) restrict the analysis to the 14 months of the pandemic covered in the time frames of data; (ii) linearly interpolate treatment effects between time frames.

[17] We can repeat the analysis using non-experimental variation in other dimensions of skills across workers. This helps establish how specific are the returns over the pandemic to skills acquired through vocational training. We consider two skills dimensions, and focus on estimating cumulative outcomes by each skill among controls only. The first uses our baseline measure of cognitive ability, from which we split workers between high (above median) and low (below median) cognitive ability. The second contrasts workers with an external versus an internal locus of control. Estimating the cumulative returns to these dimensions of cognitive and non-cognitive skills among controls, we find that: (i) on the extensive margin outcome of employment, cumulative employment outcomes are

Extensions and Robustness We present two further sets of results in the Appendix. First, we discuss how the returns to skills vary across subgroups such as: (i) gender, given this has been a key focus of earlier work – this largely confirms that our main results hold across genders, with the most striking contrast across genders being greater shifts into casual work among skilled women relative to skilled men; (ii) desired sector of employment in manufacturing versus services; (iii) region of residence; (iv) whether workers are additionally offered matching; (v) another dimension of skills – cognitive ability. Second, we address concerns over attrition. We earlier documented that although attrition rises between our last pre-pandemic survey in 2018 and our first pandemic survey, attrition is near zero across the three waves of pandemic surveys. This helps ameliorate the concern that the estimated dynamic labor market impacts are driven by attrition alone. Moreover, we earlier showed no strong evidence of differential attrition by treatment and control based on observables. The double dip dynamic impacts documented on both employment and earnings margins further help ameliorate the concern that attrition might drive the impacts, or that there is any steady fade out of the return to skills over the pandemic. Nevertheless, in the Appendix we address concerns related to attrition using multiple approaches following [Blattman *et al.* 2020].

4.5 Did Labor Markets Just Freeze?

Having established that the returns to skills survived the pandemic, this still leaves open the broader question of the overall impacts of the pandemic on the labor market outcomes of workers. At one extreme, the shock might be viewed as severe but essentially brief: the pandemic caused the labor market to freeze in time, but it recovered quickly upon reopening – as documented for prime age workers in the US [Chetty *et al.* 2023]. The other view is that, despite the survival of returns to skills, the pandemic caused persistent losses to workers, impacting their future trajectories.

Our results strongly suggest the latter interpretation. Pre-pandemic labor market trends for both skilled and unskilled workers were upward, unlike the flat or declining trends during the pandemic shown in Figures 2 and 3. We provide two further pieces of evidence on the persistent impacts of the pandemic.

4.5.1 Post-pandemic Recovery

One way to benchmark workers' recovery from the pandemic is to use pre-pandemic data to project labor market outcomes in a counterfactual absent the pandemic, and then contrast projected and actual outcomes from February 2022. Figure A3 shows projections for compliers and reweighted controls for two key outcomes: employment in one of our study sectors, and total earnings from wage/self-employment. Using data across the first five survey waves, we use a power function

just 1% different between high and low cognitive ability workers, or between those with external and internal locus of control; (ii) cumulative total earnings are around 11% lower over the pandemic for those with high cognitive skills or an external locus of control, although results are imprecise given we only use control workers.

to project the path labor market outcomes would have taken. We overlay these with the actual paths of each outcome. The resulting gaps between projected and actual outcomes imply lasting impacts of the pandemic: (i) skilled (unskilled) workers' likelihood to be employed in one of the study sectors is 37% (49%) below trend; (ii) skilled (unskilled) workers have total earnings that are 34% (45%) below trend. These magnitudes are at the top end of estimates from the established literature on dynamic labor market outcomes for displaced workers typically using administrative data from high-income settings – these find long run earnings losses between 15% and 30% [Jacobsen *et al.* 1993, Couch and Plaszek 2010, Davis and von Wachter 2011].

4.5.2 Worker Mobility

A second key way in which the pandemic shock can have persistent impacts on labor market trajectories is through worker mobility – either in the reallocation of workers across firms and sectors, or through transitions from productive wage employment into self-employment, casual work or unemployment.

Firm and Sectoral Reallocations To examine the reallocation of workers across firms and sectors and how this differs between skilled and unskilled workers, we focus on the time frames before and after each lockdown and restrict the sample to those in wage employment before *and* after each lockdown (so in time frames 1 and 3, or in time frames 4 and 6). We then examine whether, pre- and post-lockdown, they report working: (i) at the same firm; (ii) in a different firm but in the same sector; (iii) in a different sector (and hence a different firm).[18]

Column 1 of Table 6 shows that among controls who were wage employed before and after the first lockdown, 87% remain employed in the same firm. The ATT estimate shows skilled workers are 18pp *less* likely to remain at the same firm pre- and post- the first lockdown (p = .029). Hence the V-shaped recovery on employment of skilled workers is not because they are re-hired by the same firm. Rather, as Column 2 shows, skilled workers are significantly more likely to leave their original firm and transition across firms in the same sector than controls (p = .001). The magnitude of this impact is 19pp, more than four times the rate of such transitions among controls over the first lockdown (5.7%). The results in Column 3 confirm that very few workers transition to another sector around the first lockdown. These rates of job transition are very similar to those documented among US workers [Bick and Blandin 2023].[19]

[18] As the specifications are conditional on employment, selective attrition from pre- to post- each lockdown is a concern. To address the issue we include interactions between the baseline covariates and survey wave.

[19] If we assume individuals wage employed in both time frame 3 (post first lockdown) and time frame 4 (pre-second lockdown) are actually employed by the same firm, then we can repeat the exercise to examine job transitions from time frame 1 to time frame 6, so over both lockdowns. Doing so generates similar conclusions: skilled workers are 32.1pp more likely to be employed at a different firm but in the same sector over both lockdowns (p = .000), but are no more likely than controls to switch wage employment across sectors, or to shift into self-employment. Bick and Blandin [2023] use the online Real-Time Population Survey to study employer reallocation during the pandemic in the US. They find that 26% of pre-pandemic workers were working for a new employer one year into the pandemic,

In our earlier work examining returns to skills pre-pandemic, we documented that in good times returns are partly generated because these skills are certified [Alfonsi *et al.* 2020]. As a result, workers are more mobile: they experience quicker transitions back into employment when unemployed. The results in Table 6 show this mechanism remains relevant during the pandemic, helping skilled workers to build resilience to the severe downturn in their employment during the first lockdown.[20]

Transitions Out of Wage Employment The second half of Table 6 examines transitions from productive wage employment into other forms of work or unemployment, and how this differs by skilled and unskilled workers. We consider workers that were in wage employment pre-lockdown. We find no evidence that skilled workers transition into self-employment at a differential rate than controls. However, we find that skilled workers are significantly more likely to switch into causal work around the second lockdown – in line with the evidence in Panel D in Figure 2. This is a second important route through which persistent effects of the shock can be generated for skilled workers and again highlights how the data rejects the hypothesis that labor markets simply just froze temporarily during the pandemic. Finally, Column 7 shows there are large flows of workers into unemployment – over 20pp – around each lockdown. Although this is not different between skilled and unskilled workers, it remains the case that for many workers of prime working age when the pandemic shock struck, their labor market trajectories were worsened with persistent consequences for them and a loss of human capital utilization for the economy as a whole.

4.6 Roadmap

The remainder of the paper drills down to understand why the returns to skills endured through the pandemic. This reveals which workers were most exposed, and the factors leading to skilled workers being more resilient to it and hence the shock having less persistent effects on them. We consider: (i) supply-side mechanisms, namely those relating to differential characteristics or behaviors of skilled and unskilled workers; (ii) demand-side mechanisms, namely those relating to firm behaviors having differential impacts on skilled and unskilled workers. Supply-side mechanisms help explain the resilience to the shock that skilled workers display. Demand-side mechanisms help explain both how initial exposure to the shock, and differential resilience to it differ for workers by their skill levels. We shed light on demand-side mechanisms by building on data from firms collected as part of the original two-sided field experiment.[21]

at least double the rate of any previous episode in the preceding quarter of a century.

[20] The certification mechanism also shows up in terms of the speed of transitions from unemployment back into employment. The results in Figure 2 already demonstrated that although skilled workers are more impacted on employment margins in both lockdowns, they also bounce back into employment more quickly after each lockdown.

[21] We thus contrast our results and approach to Barrera-Osorio *et al.* [2022], who find that among job seekers randomly assigned into a job training program focused on service sectors in Colombia, the returns to skills disappear during the pandemic. Indeed, given that they report ITT estimates and 60% of treated workers in their study

5 Mechanisms: Supply Side

5.1 Labor Market Attachment

Between 2013 and the eve of the pandemic, treated workers accumulate greater labor market attachment than controls. They have greater experience working in the good sectors in which they were trained, and so they accumulate sector-specific skills. They also have greater experience of good jobs in both wage and self-employment, irrespective of their sector of training. Their more productive work histories mean they acquire different search capital, and they accumulate more savings than controls. All these margins might lead skilled and unskilled workers to differ in their resilience to the pandemic. We examine this set of explanations by considering how our ATT estimates of cumulative treatment effects of skills reduce as we reweight controls to have the same distribution of characteristics as compliers, as measured in the last pre-pandemic survey wave.

We follow the approach of Hainmueller [2012] to create balanced samples where the control group data is reweighted to match pre-pandemic covariate moments among compliers. To account for differential attrition and other background sources of worker heterogeneity that potentially correlate with the reweighting covariate, when reweighting for continuous covariates we first regress the covariate on a set of worker characteristics (either measured at baseline or that are time invariant, and that can also predict attrition). We then split the distribution of residuals into deciles and use this to reweight controls so the distribution of residual deciles corresponds to that of the compliers. Non-compliers are not reweighted in this exercise. The results are in Table 7.[22]

Sector-specific Experience In Panel A we show the baseline ATT impacts on each cumulative labor market outcome. In Panel B we reweight controls to match the (residualized) cumulative labor market experience compliers have in the eight study sectors pre-pandemic. On the extensive margin, Column 2 shows the impact on the cumulative experience over the pandemic in these study sectors is explained by this margin of labor market attachment: the reweighted ATT estimate is not statistically different from zero and the implied cumulative impact is reduced entirely. This builds on the earlier finding that the composition of tasks that skilled and unskilled workers conduct

comply, their ATT impacts are actually negative. This is despite such training having large returns pre-pandemic on employment and earnings. Barrera-Osorio *et al.* [2022] suggest three reasons for this, but their data does not allow them to distinguish between them: (i) the training program was relatively short; (ii) service sectors were hardest hit; (iii) sample workers graduated from their training courses around December 2018, so had little labor market experience before the pandemic. We make progress on these issues when studying the mechanisms behind our findings because: (i) our workers are assigned to training in both manufacturing and service sectors; (ii) we examine how labor market dynamics of skilled and unskilled workers differ with their experience in study sectors or in wage employment more broadly; (iii) the firm-side data allows us to understand firm responses during the pandemic and how they differed between skilled and unskilled workers, and so help drive the dynamic impacts on workers shown so far.

[22] The individual baseline characteristics controlled for are age, whether the individual is married, whether they have children, are employed at baseline, and whether they have a higher than median cognitive test score, and their desired sector of application. We also control for implementation round, strata fixed effects.

within firms in the same sector differ significantly (Figure A2) and suggests the accumulation of sector-specific skills matters for resilience to job loss [Topel 1991, Neal 1995, Kletzer 1998]. This is so for retaining attachment to good sectors. For earnings, after accounting for sector-specific experiences, in Column 5 we see that the cumulative impact of skills on total earnings are only slightly affected. Column 6 shows that cumulative impacts on earnings from wage/self employment are more impacted when accounting for sector-specific experience accumulated pre-pandemic: the estimated cumulative impact of skills then falls from 28% to 21%.[23]

Experience of Good Jobs To separate out experience in good sectors from experience in good jobs, Panel C of Table 7 repeats the exercise with an alternative measure of labor market attachment: labor market experience in wage/self-employment – irrespective of sector – from baseline to the last pre-pandemic survey. On the extensive margin, Column 2 shows this form of labor market attachment only explains around half the subsequent cumulative impacts of skills over the pandemic, so is a less important mechanism than sector-specific experience in this dimension of the returns to skills. On the margin of total earnings, Column 5 shows that pre-pandemic experience of good jobs explains around half the cumulative impact of skills, so more than the effect of pre-pandemic sector-specific experience.

Experience of Good Matches While it is natural to think of labor market attachment as capturing the accumulation of sector-specific skills or those from good jobs, it might also capture workers' experience of good matches with employers [Kletzer 1998]. For example, if skilled workers are on average in higher quality matches that pay correspondingly well, then earnings are more likely to fall following job loss [Jovanovic 1979, Schmeider *et al.* 2023]. To distinguish this explanation from the accumulation of sector-specific skills, we proxy good worker-firm matches using the average duration of employment spells (in months) from baseline to the last pre-pandemic survey, and then reweight controls to match this among compliers. Panel D shows the resulting cumulative impacts of skills: while the baseline estimate suggested treated workers spent 61% more time over the pandemic in good sectors, accounting for this form of pre-pandemic experience, the reweighted estimate reduces to 44%. On the total earnings margin in Column 5, the cumulative impacts of skills on total earnings fall from 17% to 12%. Hence the returns to skills narrow on the

[23] Our finding thus supports the claim in Barrera-Osorio *et al.* [2022] that the returns to skills among job seekers randomly assigned into a job training program disappeared during the pandemic partly because their sample of workers graduated from training courses in December 2018, and so had little labor market experience before the pandemic struck. To get a clear sense of the differential accumulation of sector-specific experience between skilled and unskilled workers, Figure A5 shows the share of months workers spent in any given sector pre-pandemic. The top panel shows this for compliers: each row corresponds to the sector the worker was trained in, the columns show the share of months spent in each sector. Depending on the sector of training, workers spent between 25% (plumbing) and 89% (construction) of all working months employed in their sector of training. The off diagonal entries show that workers trained in one sector spend almost no time in the other study sectors. Rather, as the final Column shows, when not working in their sector of training they spend time in other occupations, often related to the retail sector or as taxi drivers.

earnings margin when accounting for a history of good matches, but the returns to skills in terms of attachment to good sectors are far more driven by the accumulation of sector-specific skills.

Savings An additional consequence of treated workers accumulating more labor market experience and earnings pre-pandemic, is that they also enter the pandemic with more savings. This can impact their ability to weather the economic stresses of the pandemic, and help finance costly search behaviors [Lentz and Tranaes 2005, Lise 2013]. To explore whether savings help explain resilience, we consider how our ATT estimates of cumulative treatment effects change if we reweight controls to have the same residualized distribution of savings as complier treated workers as measured in our last pre-pandemic survey wave. The result in Panel E of Table 7 shows that the cumulative impacts on working in the eight study sectors remain almost unchanged from the baseline estimates (61% vs. 60%), as do the cumulative impacts on total earnings (17% vs. 16%). Moreover, reweighting for savings also does not explain the non-shift into casual work.

5.2 Search Behavior

Our earlier work showed that in good economic times, search behaviors of skilled and unskilled workers differ [Bandiera *et al.* 2022]. Skilled workers search more intensively and direct their search towards higher quality firms. All this leads skilled workers to have accumulated different search capital on the eve of the pandemic. Hence differences in outcomes over the pandemic between skilled and unskilled workers might be due to their continued use of different search behaviors – as has been documented for workers in high-income settings [Hensvik *et al.* 2021].

In the pandemic surveys we asked individuals about search effort and whether they were directing their search towards particular sectors, firms or locations. We find little evidence that skilled and unskilled workers differ in their search behavior along either margin (Table A10).[24] The one exception is that in the final survey wave R as the economy recovers, skilled workers are significantly more likely to report directing their search towards firms in the eight study sectors ($p = .039$). This is a result we return to below when considering how the resilience of skilled workers relates to the dynamics of labor demand for skilled and unskilled workers over the pandemic.

An implication of this set of results relates to the generalizability of evaluations of training as aggregate conditions vary [Rosenzweig and Udry 2020]. By evaluating the returns to the same offer of vocational training in good times [Alfonsi *et al.* 2020, Bandiera *et al.* 2022] and during a crisis, we show that although returns to skills acquired through vocational training are sustained over both periods, the mechanism by which this is so differs. In good times search behaviors differ

[24] On search intensity, they do not differ over the pandemic in terms of whether they are searching for work. Conditional on actively searching, skilled and unskilled workers also do not differ on how many days they spend searching, the number of applications they send, or job offers received. On whether workers strategically revise the value of employment they attach to different sectors or firms and so engage in directed search, treated and control workers also do not differ in terms of whether they report searching for work in the eight study sectors, in formal firms, in the informal sector and in Kampala (Columns 5 to 8).

between skilled and unskilled workers and this is a key mechanism generating returns, while in the pandemic crisis, this mechanism is far more muted – perhaps because the speed and severity of the pandemic mean that returns to search effort and alternative strategies are far more uncertain.

5.3 Health and Other Experiences of the Pandemic

In the Appendix we consider health and labor market interactions, establishing that: (i) pre-pandemic, there was no differential in self-reported health between skilled and unskilled workers; (ii) over the pandemic, there is no evidence that concerns about health or Covid risks impacted job search behavior or job preferences. This is unsurprising given Covid-19 case rates were relatively low in Uganda (Figure 1B). Finally, we present results exploring the possibility that treated and control workers might experience the pandemic differently on other margins.

6 Mechanisms: Demand Side

We now examine the extent to which demand-side mechanisms help explain the returns to skills through the pandemic. We consider how skilled and unskilled workers were differentially exposed to the pandemic because of the sectors and firms they were employed in. This complements the earlier focus on their accumulated experience in different sectors and jobs in the pre-pandemic period. To then understand their resilience to the shock, we examine how they were differentially treated by firms over the pandemic – in terms of layoffs and recruitment.

6.1 Firm Data

As part of the original two-sided field experiment, we tracked a representative sample of small and medium sized firms operating in the eight sectors in which we offered vocational training. They were first surveyed between October 2012 and June 2013, and three times further between 2014 and 2017. The last pre-pandemic survey took place between May and July 2017. During the pandemic we ran two waves of (phone) surveys: October-December 2020 (wave 5), and May-July 2021 (wave 6). In each, we asked questions related to three time-frames of recall, enabling us also to track firm outcomes with high frequency. In wave 5 these time-frames of recall are February 2020, April 2020 and July 2020, so spanning just before, during and after the first lockdown. In wave 6 the time frames of recall are November 2020, February 2021 and April 2021, so between the first and second lockdown.

Firm Characteristics Column 1 of Table 8 describes our sample of 2307 firms at baseline. From Panel A we see that the average firm in our study sectors employs three workers, with monthly profits of $221. Panel B shows that at baseline around a third of the firms operate in manufacturing and half are in Kampala. Firms are six years old at baseline. Panel C shows that half of firm owners are women – because the service sectors covered are female dominated. The average owner is in their mid 30s.

Panel D focuses on other firm characteristics relevant for their exposure to the pandemic. In terms of face-to-face trade, Column 1 shows firms report having around 17 customers per week, but there is enormous variation over firms and within a firm over time. The maximum number of customers reported in a good week is nearly double the average number. We asked firms about ties to other firms that could take two forms: (i) a family/social tie to another firm owner; and/or (ii) a business relationship where the firms were linked via buying/selling inputs, or sharing machines, employees or information. Firm owners reported having around one social or business tie, and more than half involve supply chain relationships. These firms could be more exposed to disruptions in the pandemic.

Attrition We next consider the firms tracked from baseline through to the pandemic. Firm attrition pre-pandemic is relatively low: 16% of firms attrit by the fourth follow-up. Attrition rises to 28% in the pandemic, but nearly all of this occurs between waves 4 and 5. We have close to zero attrition of firms between the pandemic surveys. Column 2 of Table 8 shows the *baseline* characteristics of those firms that did not attrit by wave 5, our first pandemic survey. On most margins, at baseline non-attriters have characteristics similar to firms in our original sample.[25]

Column 4 then shows the characteristics of non-attriting firms as measured in the first time frame of recall in our first pandemic survey. By March 2020, non-attriting firms had grown significantly with almost double the number of employees, profits, and customers per week since baseline. Importantly, their revenues per worker had not risen in real terms, and their wage bill as a share of revenue had risen from 68% pre-pandemic to nearer 95% on its eve.

Representativeness By wave 5 firms are no longer representative of firms in the study sectors on the eve of the pandemic. To gauge how positively selected surviving firms are, we exploit the fact that alongside our last pre-pandemic survey wave we also conducted a new census of firms operating in the same labor markets and sectors, using the same sampling approach as our 2012 census. We can thus compare characteristics of firms that we tracked and that survived until February/March 2020 to firms in the second census.

This information is shown in the remaining Columns of Table 8. Column 6 shows firm characteristics in the census, and Column 7 shows the percentile of surviving firms in the distribution of census firms. As expected, tracked firms over time are positively selected. For example, census firms have 4.1 employees in 2017; tracked firms have 5.5 employees on average, corresponding to the 84th percentile of census firms. Tracked firms are in the 92nd percentile of profits, and above

[25] Columns 1 to 3 of Table A13 show correlates of firm attrition pre-pandemic, and then over each survey wave. Across periods, attrition is uncorrelated to firm size, and negatively correlated to firm age.

the 90th percentiles in terms of revenues and revenues per worker. This degree of positive selection of tracked firms needs to be borne in mind for interpreting demand-side factors driving the returns to worker skills. On the other hand, tracked workers from our sample have also acquired six years of potential experience by the pandemic, and have also moved up the job ladder into larger firms.[26]

6.2 Sectors and Firm Quality

The earlier results highlight that sectoral allocations are key to workers' resilience during the pandemic, with much of the supply-side pull coming from the accumulation of sector-specific human capital and skill certification, which enabled easier transitions across same-sector firms during the first lockdown. We now examine another way initial sectoral assignments impact resilience: firms' varying reliance on face-to-face trade and vulnerability to supply chain disruptions [Bloom *et al.* 2022].

Figure A4 shows dynamics of firm openings and employment over the pandemic by sector, where we distinguish between sectors with high and low levels of in-person customer interaction. Firms in sectors with higher levels of interaction are more severely impacted by the first lockdown. In these firms, employment levels remain between 50 to 65% of their pre-pandemic level, while firms in sectors with lower levels of customer interaction recover to nearly the same, or greater, employment levels. The most impacted sector is tailoring, in which employment is at just over 50% of its level in April 2021 relative to February 2020, and the least impacted sector is welding, in which employment is actually 20% higher in April 2021 relative to February 2020. Hence the pandemic leads to a reallocation of employment opportunities across sectors.

Table 9 shows how firm characteristics differ between most and least affected sectors, as measured in the last pre-pandemic survey wave. In Panel A we consider firm characteristics relevant for exposure to the shock. Comparing characteristics related to customers and supply chains we see that the most affected sector has firms with more customers ($p = .000$), but the same number of supply chain links ($p = .817$). Panel B considers measures of firm performance and here we see that firms in the most affected sector are significantly smaller, with lower revenues and profits than firms in the least affected sector. Panel C shows how the composition of workers by skill levels differ across the most and least affected sectors. This is based on owner's assessment of their employees' skills. Less affected sectors have more skilled workers, and a greater share of employees that are considered skilled.

This evidence suggests skilled workers might be more resilient partly because they are less exposed to firms reliant on customer-facing exchange, and to smaller and less profitable firms – i.e. lower quality firms. Firm quality might matter for resilience to the pandemic because high quality firms might be more able to retain productive matches with employees over the first

[26] In the final pre-pandemic survey, the median size of firms that complier and control workers are employed in are 4 and 3 respectively. 21% (18%) of compliers (controls) are employed in firms of size 5-9.

lockdown, and earnings losses could arise if workers are displaced from high wage firms and later hired by lower wage firms [Davis and von Wachter 2011, Schmeider *et al.* 2023].

To quantify the overall role of initial sectoral assignments in explaining the cumulative impacts on outcomes over the pandemic, we reweight the sectoral composition of controls on the eve of the pandemic to match that of compliers (combining non-study sectors into a composite ninth sector). As sectors are unordered, we do not residualize as we did for continuous outcomes when reweighting controls. Panel A of Table 10 shows our baseline estimates. Panel B shows that accounting for sectoral differences between skilled and control workers can account for nearly all the cumulative impacts on employment in the study sectors over the pandemic. However, the differential sectoral composition explains none of the differential returns to skills along margins of total earnings or earnings specifically from wage/self-employment.

We next examine the role of firm quality. To be clear, we cannot measure firm quality in our worker-side data in comparable detail as from our firm-side data because workers were not asked to characterize their employers in detail. We can, however, construct a cruder index of the firm quality that individuals are employed in on the eve of the pandemic based on two easily observable characteristics: the size of the firm and whether it is formal. As Panel C of Table 10 shows, the estimated cumulative impacts of skills shrink after reweighting for this measure of firm quality: the cumulative impact on being employed in the study sectors falls from 61% to 38%, and the cumulative impacts on total earnings almost halves from 17% to 10%.

In short, sectoral allocations matter for explaining the returns to skills on the extensive margin of retaining attachment to study sectors over the pandemic, while firm quality matters more for explaining returns to skills on the margin of total earnings.[27]

6.3 Labor Demand

We now consider the dynamics of labor demand for skilled and unskilled workers. We start by using our firm surveys to present unconditional firm outcomes over the six time frames of the pandemic, where we normalize each outcome to one in the first time frame, February-March 2020.

Panel A of Figure 4 shows the share of firms that remain operating in each period (solid line). The pandemic hit firms severely: only 40% of firms in our study sectors remained in operation during the first lockdown. They then experience a V-shaped recovery after the end of the first lockdown, 90% of firms were back in operation and this remained steady over the remaining time frames until April 2021. However, 7% of firms – even the positively selected ones we track – stopped operating by April 2021, speaking to the severity of the pandemic shock and the uncertainty induced. We overlay this with labor demand in the study sectors (dashed line). Employment levels are at 55% of their pre-pandemic level during the first lockdown – an enormous shock in the

[27] Our results thus provide nuance to those in Barrera-Osorio *et al.* [2022]. Our findings suggest that the returns to skills vary across services and manufacturing because of the greater reliance of service sectors to face-to-face trade, but also because firms in such sectors are smaller and less profitable.

space of just two months. Recovery is slower than on the operating margin, with labor demand rising to 70% of the pre-pandemic level in these firms.

Table 11 shows the regression adjusted equivalent of these results for outcome y for firm f in sector s in time frame t:

$$y_{fst} = \alpha + \sum\nolimits_{t=2}^{t=7} \beta_t time\_frame_t + \delta \mathbf{x}_{fs0} + \lambda_s + u_{fst}, \tag{4}$$

where the omitted time frame t is February 2020, $\mathbf{x}_{fs0}$ are baseline characteristics of the firm and λ_s are sector fixed effects, and we estimate robust standard errors. Outcomes are measured in absolute amounts (so not normalized to one in the omitted period as in Figure 4).[28]

Column 1 shows in the first lockdown, the share of firms operating falls by 53pp relative to February 2020, but firms recover between the first and second lockdowns. Column 2 shows that for surviving firms, labor demand falls sharply during the first lockdown and then slowly recovers. Labor demand falls by 53% in the first lockdown, remaining 41% lower in July 2020 (when the number of firms operating is only 10% lower). On the eve of the second lockdown labor demand remains 30% lower than in February 2020. Column 3 shows that firm revenues plummet during the first lockdown, with profits falling to nearly zero. Both recover steadily after the first lockdown. By April 2021, firm revenues and profits are both significantly recovered in levels from the depth of the lockdown in April 2020 ($p = .000$, $.011$ respectively).

6.4 Matching Labor Demand and Supply Dynamics: Firms' First-in-First-Out Strategy

To fully understand how firms survived the trough of the first lockdown, we now directly compare demand-side employment dynamics with the supply-side employment dynamics documented earlier. Panel B of Figure 4 overlays changes in labor demand from Panel A with changes in wage/self employment of skilled and unskilled workers as measured in our worker-side data. To aid the comparison, each series is normalized to one in the first time frame. Employment rates of complier and control workers fall further in the first lockdown than among firms in our study sectors, but the V-shaped recovery is similar in both data sources. This supports the earlier finding that over the course of the pandemic, skilled workers reallocate across firms in the same sector.

We also compare demand- and supply-side earnings dynamics. Panel C of Figure 4 shows the evolution of earnings conditional on employment using: (i) firm-side data on workers who *remain employed* in study-sector firms; (ii) worker-side data on skilled and unskilled workers. To again aid the comparison, each series is normalized to one in the first time frame. A sharp divergence emerges: earnings conditional on employment for treated workers recover in a V-shaped pattern,

[28] The baseline firm characteristics in $\mathbf{x}_{fs0}$ are whether it operates in Kampala, firm age, whether the owner is female, and the firm owner's age.

while earnings for workers who remain employed in study-sector firms show an L-shaped pattern, remaining at 70% of the average level of all employees in February 2020 with no trend towards recovery between lockdowns.[29]

The corresponding regression result from the firm-side data is in Column 5 of Table 11. We see persistent falls in the monthly earnings for the average employee at firms over the pandemic: earnings fall 40% in the first lockdown relative to February 2020, and this persists across time frames including until April 2021. In line with a L-shaped impact, we cannot reject the null that the earnings impact is the same in April 2021 as in the trough of the first lockdown ($p = .325$).

Retention Matching the earnings dynamics from the firm- and worker-side suggests a shift in the composition of workers employed by firms that survived the pandemic. To examine this directly, we use data from our pandemic firm surveys, where firms reported hires and layoffs over two periods: (i) March 2020 to November 2020, covering the first lockdown; (ii) December 2020 to June 2021, between the first and second lockdowns. In short, we find that in the face of a severe aggregate shock, firms first laid off the highest earning workers.

The results are in Panel A of Table 12 which focuses on worker retention. We see that 63% of employees stayed with the firm over the first lockdown, and 75% of employees stayed with the firm between lockdowns, a significant increase in retention over these phases of the pandemic ($p = .000$). The next few rows examine characteristics of laid off workers. For firms that laid off a worker, the majority laid off highly experienced workers – correlating with the most skilled workers. Contrary to expectations, tenure and skills did not protect workers from job loss. This is because more experienced/skilled workers have higher earnings: either because their base earnings are higher or because they can obtain a higher piece rate in some sectors. This was shown in pre-pandemic worker data, where the skill premium in earnings was 25% (Table 4). Data from firms' roster of workers also confirms that, pre-pandemic, base earnings significantly increased with tenure and when workers were classified as skilled by the firm owner.

The data on worker retention reveals why skilled workers were hit harder than typical employees during the lockdown and why our group of unskilled workers, despite having more potential labor market experience, were also disproportionately affected. Firms laid off the highest-earning workers first during the initial lockdown to quickly reduce wage bills in response to the severe shock, as profitability plummeted. This also explains the L-shaped earnings pattern (Figure 4, Panel C), where earnings for retained employees remained low after the first drop.[30]

[29] This result lines up closely with Bassi *et al.* [2021] who find that employee earnings in late 2020 were 30% lower than pre-lockdown in a representative sample of carpenters, welders and grain-millers in the same context.

[30] It was feasible for firms to first lay off more experienced and higher skilled workers, and keep operating with a smaller group of less experienced and skilled employees. To see this, note that workers in our sample are older and more skilled than many employees in the firm-side data in the last pre-pandemic firm survey in 2017. For example, workers in our evaluation sample have a median age of 25, while the median age of employees is 23, with 39% of employees being below age 21, the youngest worker in our sample. Furthermore, 29% of employees are reported by firm owners as being unskilled.

Recruitment The other side of firms' strategies during the pandemic was adjusting hiring as part of broader labor demand dynamics. This is examined in Panel B of Table 12. We first see that firm's attempts to recruit workers were more muted over the first lockdown than between lockdowns. The next few rows examine characteristics of the last recruited worker. Firms were more likely to recruit workers with experience in the same sector between the first lockdown and November 2020, than between lockdowns ($p = .000$). This exactly matches with two earlier results: (i) that skilled workers switch firms within the same sector over the first lockdown; (ii) that as the economy recovers, skilled workers report directing their search towards firms in the study sectors.

These changes in the composition of employed workers are reflected in earnings differences between last hired and last laid off workers, as shown in Panel C of Table 12: the average monthly earnings of hired workers are \$30, while the monthly earnings of laid off workers are \$49. This is further consistent with firms laying off the highest earning workers over the first lockdown.

Firms' FIFO Strategy The kind of first-in-first-out (FIFO) strategy we document is entirely counter to last-in-first-out strategies often observed as firms respond to slow moving shocks [Buhai *et al.* 2014]. At the same time, if it is common knowledge that skilled workers are laid off first, then this information can actually aid their re-employment at other firms later during the pandemic [Gibbons and Katz 1991, Oyer and Schaefer 2011, Carrington and Fallick 2014], consistent with the job mobility of skilled workers across firms in the same sector documented earlier.

To quantify how successful this FIFO strategy was, we return to the firm data and consider the dynamics of firm outcomes in Table 11. We already saw that firm profits fell to nearly zero during the first lockdown, but recovered steadily as firms exited the lockdown. By April 2021, firm profits are significantly higher than in the depth of the first lockdown ($p = .011$). Column 6 of Table 11 examines how changes in skills composition of retained employees translate into the ratio of wage bills to revenues. As described earlier, at baseline this ratio was 68% but on the eve of the pandemic had risen to 95%. Given the response of firms of immediately laying off the highest earning workers, we see that in the first lockdown the wage/revenue ratio fell by 27% relative to February 2020, and had fallen by 43% by April 2021 – and so back to the ratio at baseline.

Although retention and recruitment responses might have varied based on firms' anticipated exposure to the shock and sectoral differences, we consistently find that firms laid off higher-earning skilled workers first, regardless of sector or pre-pandemic profitability, using a FIFO strategy to reduce wage bills. An explanation might be the unprecedented speed and severity of the shock meaning that firm owners: (i) are unable to confidently predict their survival probability; (ii) face huge uncertainty. On (i), we show it is hard to predict firm survival even based on a rich set of baseline covariates.[31] On (ii) we can examine how the expectations of firm owners evolve over

[31] This result is in Column 4 of Table A13: larger and older firms, those in manufacturing, with male owners, older owners and fewer customers are more likely to survive the pandemic. However covariates explain less than 15% of the variation in firm survival.

the pandemic. We find that: (i) during 2020, 40% of firms believed the economy would rebound within six months, but these expectations worsened in the first half of 2021 ($p = .003$); (ii) the expectation of a new total lockdown increased in 2021 relative to 2020; (iii) during 2020, 37% of firms believed they were very unlikely to re-open following any new total lockdown.

In the Appendix, we analyze and rule out other potential firm responses, such as adjustments in payment methods, timing, or wage reductions.

Skilled Workers and FIFO Despite firms' FIFO strategy, skilled workers remain resilient to job loss during the first lockdown because the returns to skills are maintained through the various mechanisms documented earlier. This raises two issues: (i) are there productivity losses from the destruction of pre-pandemic worker-firm matches? (ii) what kinds of firms (in the same sector) do skilled workers reallocate to?

On (i), to the extent that earnings reflect individual productivity in our study sectors, then the fact that earnings for skilled workers recover to 90% of their pre-pandemic level by April 2021 suggests, all else equal, productivity losses are small. However, productivity losses for the economy arise as skilled workers switch to causal employment over the second lockdown and many skilled workers remain unemployed even as the economy recovers (Table 6).

On (ii), as skilled workers transition to firms in the same sector, these firms could be: (i) of the kind represented in our firm survey; (ii) larger firms; (iii) firms that started up in the pandemic. Our data is not well suited to distinguish these cases because our pandemic worker surveys do not have data on the size of firm workers were employed at. However, we can illuminate the issue by comparing the distribution of earnings in our tracked firms during the pandemic to the distribution of earnings of complier and unskilled workers at the same moment in time. This comparison is in Panel D of Figure 4 for three prominent sectors: motor-mechanics, hairdressing and construction. For hairdressing and construction sectors, given the overlap in earnings distributions, skilled workers might move to the firms similar to those in our firm survey. This appears less likely for skilled workers in the motor mechanics sector, where the bulk of the earnings distributions do not coincide, suggesting those workers might have transitioned to larger employers.

7 Discussion

Lower-income countries are susceptible to aggregate shocks of many forms, but few impact the economy with as much speed, severity and uncertainty as viral outbreaks [Altig *et al.* 2020]. This is a huge concern for human well-being. Between 1980 and 2013, there were over 12,000 recorded outbreaks of 215 human infectious diseases, comprising 44 million cases across 219 countries [Smith *et al.* 2014]. In recent times, major economic disruptions have been caused by SARS (2003), H1N1 (2009), MERS (2012), Ebola (2014), Zika (2016) and of course Covid-19. None of the fundamental drivers of outbreaks are likely to disappear, so it is vital to understand how to build resilience to them, especially in labor markets given that employment outcomes centrally determine well-being, and especially so in lower-income contexts where a lack of social safety nets can lead to severe welfare impacts of aggregate shocks.

We show that skills acquired through vocational training in Uganda help build resilience to shocks even as severe as the Covid-19 pandemic. We do so among a group of prime-age workers, for whom recovery from the shock is critical to lifetime welfare. They also have fewer opportunities to return to education or acquire training than younger workers. We document that over the crisis, employment and earnings margins follow V-shaped dynamics for skilled and unskilled workers. However, while skilled workers are more impacted by lockdowns because they are fired first by firms looking to survive the pandemic, they also recover more quickly between lockdowns, and remain resilient to the shock as the economy recovers. In short, the positive pre-pandemic returns to skills survive through the pandemic.

We draw a number of implications of general interest from our findings.

Skills and Aggregate Shocks Once we factor in skilled workers' resilience to aggregate shocks, the returns to training are even higher than documented in work evaluating interventions during good economic times. In our earlier work, using a standard approach valuing the benefits of skills via earnings, we documented the IRR to the vocational training intervention to be 30% in the pre-pandemic steady state [Alfonsi *et al.* 2020]. To provide a sense of how this IRR is sustained over bad times, we note that over the pandemic skilled workers have 17% higher earnings than unskilled workers (Table 5), while pre-pandemic this earnings gain was 59% (Table 4). However, this does not value the utility gains from the insurance provided: skilled workers remain resilient to shocks as severe, rapid and uncertain as the Covid-19 pandemic.

The resilience that skills interventions build might be in contrast to other anti-poverty interventions whose returns could dissipate during aggregate shocks. That is not to say that *any* training intervention will generate such returns over good times and bad: many training interventions have been found to generate relatively low returns [McKenzie 2017, Carranza and McKenzie 2024]. As discussed in our earlier work, our intervention might generate especially high returns because we collaborated with the most reputable VTIs throughout Uganda, enabled individuals to build sector-specific human capital, and we selected workers into the evaluation based on their willingness to undertake this training rather than take up other short-term labor market opportunities. Hence, the fact that such forms of skills acquisition enable workers to be resilient to an aggregate shock of the scale as the Covid-19 pandemic, bolsters the case for skilling workers as early in their careers as possible.

Mechanisms in Good Times and Bad Our results show the mechanisms driving the returns to skills differ in good times and bad. This speaks directly to wider concerns over returns to interventions varying due to their interaction with aggregate shocks [Rosenzweig and Udry 2020].

In our earlier work, we documented that supply-side mechanisms – such as certification and job search behavior – are key to generating returns to vocational training in times of economic stability [Alfonsi *et al.* 2020, Bandiera *et al.* 2022]. Over the pandemic we find that certification remains critical because it allows skilled workers to switch firms in the same sector. In addition, the accumulation of sector-specific skills, the sectoral composition of firms, and firm quality – whereby skilled workers are less exposed to smaller, less profitable firms, and firms more reliant on customer-facing exchange – are all also key to ensuring the resilience of skilled workers. These findings speak to the concern that if training programs are overly job-specific, the skills provided may hinder workers adaptation to shocks [Acemoglu and Autor 2011, Hanushek *et al.* 2017, Deming and Noray 2020]. We find this not to be the case because of the multiple mechanisms through which skills continue to matter through the aggregate shock.

Throughout, we have recognized that the pandemic shock was unique in its speed, severity and uncertainty faced by workers and firms. This has implications for the supply- and demand-side mechanisms we uncover driving the returns to skills. Other supply-side mechanisms – such as skilled workers accumulating more savings or different job search capital – might be more relevant to how they cope with more gradual economic downturns. On demand-side mechanisms, the severity of the crisis led initially to a major loss of employment opportunities for skilled workers as firms laid off the highest earning workers – those experienced and skilled – to quickly reduce wage bills. In a more slow moving economic downturn, firms should be better able to adopt alternative last-in-first-out strategies and retain their most valuable employees.

Policy We extend a rich empirical literature on the dynamics of displaced workers which is almost exclusively based in high- or middle-income settings, to a low-income setting in sub Saharan Africa, where the highest risks of job loss actually exist [Donovan *et al.* 2023, Gerard *et al.* 2023, Carranza and McKenzie 2024]. Absent formal safety nets, it is in such settings that the demand for social insurance is high. Such worker-targeted policies are now beginning to be implemented and our results have implications for the kind of workers that might gain most from social insurance, and the value of complementary policies in such contexts, such as skills certification. Finally our results have implications for firm-side policies, such as incentive schemes making it easier for firms to avoid FIFO strategies and be able to hoard labor when faced with the kinds of aggregate crisis that developing economies are frequently subject to.

A Appendix

A.1 Validation: Worker Expectations

One way to validate the results for employment and earnings outcomes is to examine whether the patterns align with worker expectations on job offer arrival rates and earnings conditional on employment. We do so for all workers irrespective of their employment status, ensuring results are not driven by composition effects. For the pandemic survey waves, expectations on both margins are measured on survey date (not in relation to each time-frame). Table A7 shows these results, where we focus on ATT estimates.

Starting with beliefs over the job offer arrival rate from firms in sectors in which the worker has been trained (or wanted to be trained in for controls), Column 1 shows how over each period of the pandemic, skilled workers have significantly higher beliefs than unskilled controls. In wave $L1$ the magnitude of the effect is 1.27 (on a 0-10 scale), a 27% increase over controls (reweighted for compliance probability). This divergence in beliefs more than doubles between skilled and unskilled workers later in the pandemic. Columns 2 to 4 show treatment effects on expected earnings if workers could transition into their most preferred study sector job. Among compliers, we see that in each pandemic survey, they significantly revise upwards their minimum expected earnings, their maximum expected earnings are revised upwards by a greater extent, and their average expected earnings shift forward. We again observe a divergence in beliefs along this margin between skilled and unskilled workers later in the pandemic: the gap in expected earnings is twice in magnitude in wave R relative to that in wave $L1$.

A.2 Heterogeneity

Gender One of the major lessons from the pandemic, across high- and low-income settings, was the gendered nature of impacts of lockdowns because: (i) women's labor force participation was more affected because the sectors they engage in are more sensitive to social distancing [Alon *et al.* 2022]; (ii) the unequal distribution of housework and care duties [Adams-Prassl *et al.* 2020]. That might be especially relevant in the Ugandan setting where schools were locked down for a long period. The first set of results in Table A8 thus consider how the returns to skills over the pandemic vary by gender. In Panels A and B we see that the cumulative ATT effects of skills are larger for women on many margins. The most striking contrast is in shifts into casual work. Among men, we see that skilled workers are 26% less likely to shift into casual work. However, among women, skilled workers are 40% *more* likely to shift into casual work than controls. This is exactly in line with the findings of Alfonsi *et al.* [2023] in the context of urban Uganda, and those of Chakravorty *et al.* [2023] in the context of rural India. We find these differential shifts into casual work lead to earnings from casual work for skilled women to rise slightly relative to unskilled women, while they fall for skilled men by 37%. Overall, our findings thus confirm the earlier evidence that hard-earned progress towards women's employment and earnings parity can be set back by temporary but aggregate shocks – even for skilled women.[32]

[32] Alfonsi *et al.* [2023] track 700 young urban vocational trainees in Uganda – these graduated from similar VTIs and followed similar sector-specific courses as in our work. Chakravorty *et al.* [2023] study the dynamic labor market outcomes for 2000 vocational trainees in India, focusing on a sample of rural youth. Our results by gender are also in line with the evidence on differential impacts of job loss across genders in high-income settings,

Desired Sector of Training Workers who originally desired to be trained in one of the manufacturing sectors in which we offered vocational training might differ in other unobserved ways from those who desired to work in one of the service sectors in which we offered training – say because the latter has more face-to-face trade taking place that could also be differentially impacted during the pandemic. Given desired sector of work correlates highly to the sector treated workers are actually trained in, this also proxies for whether the individual spends most of their working life in manufacturing or service sectors.

In Panels C and D we see that extensive margin impacts are similar across those who desired to work in manufacturing and services. The most notable divergence again occurs with respect to shifts into casual work. For those who preferred to work in manufacturing, skilled workers spend 28% less time in casual work, in line with our baseline results. In contrast, among those that preferred to work in services, skilled workers spend 15% more time in casual work. Both sets of skilled workers retain a large advantage over the pandemic to unskilled workers in terms of total earnings and earnings from wage/self-employment.

Region of Residence To explore whether locations help explain the returns to skills over the pandemic, we consider how our estimates of cumulative treatment effects change if we reweight controls to have the same region of residence as treated workers as measured in our last pre-pandemic survey. Panel E shows that the cumulative impacts on working in the eight study sectors remain almost unchanged from the baseline estimates (61% vs. 63%). There are also only a slight change in the cumulative impacts on total earnings (17% vs. 18%).

Matching We next consider whether cumulative treatment effects differ between those offered vocational training and those additionally offered matching. In Panels F and G we see slightly larger treatment effects on the extensive margins of employment among those only offered vocational training, while the cumulative impacts of skills on earnings from wage/self-employment are slightly larger among those additionally offered matching.

Cognitive Skills Finally we consider splitting by a cross sectional correlate of compliance: cognitive ability at baseline. In Panels H and I we see on some key margins, that the cumulative impacts of skills for those above median cognitive ability are larger than for those below the median. Skilled workers who are also above median cognitive ability are far less likely to switch into casual work, although even among those with below median cognitive ability, skilled workers retain 10% higher total earnings over the pandemic than unskilled workers.

where women tend to experience greater and persistent earnings losses, as well as a greater propensity to shift into part-time or marginal employment [Illing *et al.* 2023].

A.3 Robustness to Attrition

We address concerns related to attrition using multiple approaches following Blattman *et al.* [2020] and using the sample through the three pandemic survey waves (i.e. waves 5, 6 and 7). The results are shown in Table A9 where each row corresponds to our key cumulative outcomes. As a point of comparison, Column 1 shows our baseline estimate of the ATT effects over the pandemic. Column 2 shows the results to be almost unchanged when we drop the controls ($\mathbf{x}_{is0}$). Column 3 shows that our core results also barely change when using inverse probability weighting (IPWs) to correct for selective attrition.[33]

In the remaining Columns we make various assumptions on missing observations to examine robustness to differing degrees of selective attrition on unobservables in a bounding exercise in the spirit of Manski bounds. In Column 4 we replace all missing values in both complier and control groups with the average outcome for non-attriters in the control group. This effectively assumes that among compliers, attriters are negatively selected on outcomes (relative to non-attriters), but there is no negative selection of the attriters in the control group. As is intuitive, the ATT estimates are slightly lower than in Column 1, but the ATT impacts on cumulative outcomes remain positive and significant.

In Column 5, we assign to attriters in the control group an outcome .1SD higher than the mean outcome among control non-attriters, while attriters among compliers are assigned an outcome .1SD lower than the mean outcome of control non-attriters. This assumes attriters are positively selected in control and negatively selected among compliers, so that there is a .2SD difference in outcomes between complier and control attriters. Our baseline estimates on employment are robust to this conservative approach, while ATT estimates on earnings remain positive but not significant. Column 6 shows that when we make the opposite imputation – i.e., we assign to attriters in treatment an outcome .1SD higher than the control mean, and to attriters in control an outcome .1SD lower than the control mean – our estimated treatment effects are similar to Column 1 and highly significant. Columns 7 and 8 repeat the analysis but under the more extreme assumption that there is a .5SD difference in outcomes between complier and control attriters. It is only under such an extreme assumption that control attriters outperform the control non-attriters by .25SD that the ATT effect on employment become insignificant.

In summary, the results from the bounding exercises show our findings are robust to plausible degrees of selective attrition on unobservables. This reinforces the earlier direct evidence of there being no selective attrition on unobservables over time among treated and control groups.

[33] This procedure amounts to running a first stage where attrition is predicted using baseline characteristics that are relevant for whether we could trace respondents but are excluded from the set of controls $\mathbf{x}_{is0}$. In a second stage, we then reweight observations in the ATT regression analysis so that those non-attriters with a higher predicted probability of attriting receive a higher weight in the estimation. As in Alfonsi *et al.* [2020], we predict attrition separately at waves $L1$, $L2$ and R, using the following excluded predictors: a dummy for orphan status, a dummy for whether anyone in the household has a phone, and a dummy for whether the respondent was willing to work in multiple sectors at baseline.

A.4 Health and Other Experiences of the Pandemic

We consider the role of health interacting with labor market outcomes, and whether these interactions differ between skilled and unskilled workers. In Table A11 we first consider self-reported health in the third worker survey wave (2016). Pre-pandemic, we find no difference in skilled and unskilled workers' reported health status (Columns 1 and 2). We then examine health and search behaviors over the pandemic. Across all time periods, we find no evidence of differential behaviors between skilled and unskilled workers.[34]

Skilled and unskilled might experience the pandemic differently in other ways. Columns 1 to 3 of Table A12 focus on experiences of lockdown. In Column 1 we see that skilled workers are 14pp *more* likely to report that during the first lockdown, everything was completely shut down except for essentials (relative to 69% of controls reporting this). In Columns 2 and 3 we asked about difficulties experienced during each lockdown. The responses from controls in waves $L1$ and $L2$ are in line with the notion that the second lockdown was less strict. We find no difference in reports of the severity of each lockdown from skilled and unskilled workers in terms of getting to food markets, but skilled workers are 7.6pp more likely to report difficulty in being able to buy food during the first lockdown. Columns 4 to 6 ask about coping strategies. We see no differences between workers in terms of them reporting having to reduce the number or size of meals, having to sell assets, or moving in the period prior to the survey. Finally, we examine whether workers differ in their expectations of economic recovery. At the outset of the pandemic, 27% of control workers expected the economy to rebound within six months (Column 7) and 66% of controls expected it to rebound within a year. We see no differences in these expectations between treated and control workers. This contrasts sharply to the differential expectations of these groups of workers about their own labor market outcomes (Table A7).

A.5 Other Margins of Firm Response

To explore other margins of firm response to the pandemic, we asked firm owners about other changes. They report no change in payment methods for workers ($p = .808$) but in the second period, there is a significant increase from 9.5% to 22.6% of firm owners reporting allowing employees more flexibility in hours at work ($p = .001$). We can explore the issue further by drawing in data from Alfonsi *et al.* [2023] that was collected over the pandemic from graduates of VTIs in Uganda. Using a comparable sample of trained workers in that data, we find: (i) 90% of skilled workers report no reductions in hourly wages or piece rates during the pandemic; (ii) 95% report no changes in the timing of payments; (iii) 99% report no changes in payment mode; (iv) 89% report no changes in other non-pecuniary benefits.

[34] For example, they report similar responses to questions about not engaging in job search due to health, moving to locations with better healthcare or safety from Covid, worries about contracting Covid, and changes in job preferences due to Covid (Columns 3 to 6).

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Table 1: Baseline Balance on Labor Market Histories

Means, standard deviation 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	Any work in the last month	Any regular wage employment in the last month	Any self employment in the last month	Any casual work in the last month	Total regular earnings in last month [USD]	Total earnings in last month [USD] | wage/self employment	F-test of joint significance
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
All Workers	**1140**	.386	.136	.040	.259	5.87	38.1	
						(17.8)	(31.5)	
Control	**448**	.399	.117	.038	.298	5.02	34.8	
						(15.6)	(25.8)	
Offered Vocational Training	**692**	.378	.149	.041	.233	6.42	39.6	{.240}
						(19.0)	(33.8)	
		[.917]	[.098]	[.631]	[.106]	[.137]	[.353]	
Number of observations		1132	1132	1132	1132	1117	125	

Notes: Data is from the baseline worker survey. Columns 1 to 6 report the mean of each worker outcome, and the standard deviation for continuous outcomes. The reported p-values are derived from an OLS regression of the outcome of interest on a treatment dummy of whether the worker was offered vocational training, randomization strata dummies and a dummy for the implementation round. Robust standard errors are reported throughout. Column 7 reports the p-value from F-Tests of joint significance of all regressors from an OLS regression where the dependent variable is a dummy taking the value of zero if the worker is assigned to the Control group, and one for workers assigned to the corresponding treatment group and the independent variables are the variables in Columns 1 to 5 (the variable in Column 6 is dropped as it is missing for individuals who were not wage or self-employed in the month prior the survey). Robust standard errors are calculated. In Column 4, casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing, slashing compounds, and any type of agricultural labor such as farming, animal rearing, fishing, and agricultural day labor. In Column 5, workers who report doing no work in the month prior the survey have a value of zero for total earnings. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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: Attrition

OLS regression coefficients, robust standard errors in parentheses

	Outcome: worker attrited by			
	2018 (Wave 4)	2020 (Wave L1)	2021 (Wave L2)	2022 (Wave R)
	(1)	(2)	(3)	(4)
Offered Vocational Training	-.005	-.077***	-.090***	-.091***
	(.020)	(.027)	(.027)	(.028)
Cognitive Ability (above median = 1)	.009	.025	-.022	-.020
	(.019)	(.027)	(.028)	(.028)
Locus of Control (above median = 1)	-.065**	-.140***	-.087**	-.088**
	(.030)	(.040)	(.040)	(.040)
Any sector-specific skills	.012	.011	.013	.011
	(.018)	(.026)	(.026)	(.027)
Gender (male = 1)	.023	.140*	.106	.127
	(.073)	(.082)	(.083)	(.082)
Preferred training sector (manufacturing = 1)	-.014	-.086**	.007	-.007
	(.031)	(.043)	(.044)	(.044)
Employed at baseline	-.016	-.057**	-.043	-.033
	(.020)	(.027)	(.027)	(.028)
Mean of outcome in Control group	.118	.312	.310	.317
Strata and Implementation round dummies	Yes	Yes	Yes	Yes
Other baseline characteristics	Yes	Yes	Yes	Yes
Test of joint significance of baseline characteristics [p-value]	[.877]	[.042]	[.085]	[.119]
Number of observations	1140	1140	1140	1140

Notes: The outcome is whether the worker attrits from the sample between baseline and a given survey wave. We control for a treatment dummy of whether the worker was offered vocational training and the individual characteristics controlled for are mostly measured at baseline. The cognitive ability measure is based on a test, and we convert scores to a dummy indicating whether the individual is above the median score. The Locus of Control measure is calculated using Rotter's [1996] scale, so a higher score indicates a more external locus of control. We convert scores to a dummy indicating whether the individual is above the median score or not. The dummy for whether the individual reports having any sector-specific skills is measured at the third follow-up. The preferred training sector being manufacturing is a dummy equal to one if the sector of interest reported at baseline was either motor-mechanics, plumbing, construction, welding, or electrical work. It is equal to zero otherwise. The other baseline characteristics controlled for are age, and dummies for whether the worker is married, has any children, is employed, or if the worker resides in Kampala. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. OLS specifications are estimated and robust standard errors are reported in parentheses. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 3: Heterogeneous Attrition

OLS regression, p-values reported

	Attrited by 2018 (Wave 4)	Attrited by 2020 (Wave L1)	Attrited by 2021 (Wave L2)	Attrited by 2022 (Wave R)
	(1)	(2)	(3)	(4)
t-test of significance between treatment dummy and:				
Cognitive ability (above median = 1)	.807	.306	.127	.225
Locus of control (above median = 1)	.216	.466	.505	.306
Any sector-specific skills	.047	.138	.450	.033
Gender (male = 1)	.604	.527	.427	.238
Preferred training sector (manufacturing = 1)	.111	.433	.670	.319
Resident in Kampala at baseline	.033	.715	.204	.034
Employed (any activity) at baseline	.280	.131	.470	.333
Mean of outcome in Control group	.118	.312	.310	.317
Joint F-test	.025	.650	.473	.075
Strata and Implementation round dummies	Yes	Yes	Yes	Yes
Other baseline characteristics	Yes	Yes	Yes	Yes
Number of observations	1140	1140	1140	1140

Notes: The outcome is whether the worker attrits from the sample between baseline and a given survey wave. In each cell we report the p-value on a t-test of significance between the treatment dummy of whether the worker was offered vocational training and characteristics of the worker. Characteristics controlled for are mostly measured at baseline. The cognitive ability measure is based on a test, and we convert scores to a dummy indicating whether the individual is above the median score. The Locus of Control measure is calculated using Rotter's [1996] scale, so a higher score indicates a more external locus of control. We convert scores to a dummy indicating whether the individual is above the median score or not. The dummy for whether the individual reports having any sector-specific skills is measured at the third follow-up. The preferred training sector being manufacturing is a dummy equal to one if the sector of interest reported at baseline was either motor-mechanics, plumbing, construction, welding, electrical work. It is equal to zero otherwise. The other baseline characteristics controlled for are age, and dummies for whether the worker is married, has any children, is employed, or if the worker resides in Kampala. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. OLS specifications are estimated and robust standard errors are reported in parentheses. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 4: Labor Market Outcomes Pre-pandemic

ITT and ATT estimates, robust standard errors in parentheses

	Skills in 2016 (wave 3)		Impacts in 2018 (wave 4)		Cumulative Effects 2014 to 2018		
	Has any sector-specific skills	Sector-specific skill test score (0-100)	Main activity in last month is work in any of the eight sectors	Total earnings in last month (USD)	Months unemployed	Months in which main activity was in any of the eight sectors	Monthly earnings from wage/self-employment (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ITT							
Offered Vocational Training	.225***	5.98***	.121***	13.0**	-3.91***	5.03***	528***
	(.042)	(2.12)	(.040)	(6.55)	(1.05)	(.874)	(132)
Panel B: ATT							
Vocationally Trained	.319***	8.49***	.181***	18.6**	-5.37***	6.90***	760***
	(.056)	(2.87)	(.058)	(9.19)	(1.40)	(1.15)	(185)
Control mean (SD)	.663	30.7 (21.3)	.240	72.0 (75.0)	27.3	5.99	1263
Reweighted control mean (SD)	.890	37.5 (20.6)	.253	73.0 (77.0)	27.3	5.90	1281
Number of observations	755	755	1008	935	737	737	526

Notes: Panel A reports OLS ITT estimates, while Panel B reports 2SLS ATT estimates, where robust standard errors are in parentheses. The outcome in Column 1 is a dummy for whether the individual reports having any sector-specific skills, measured at third follow-up. The outcome in Column 2 is a sector-specific skill test score (which ranges from 0 to 100), administered in the third follow-up. The skills test assesses worker skills in the sector of training for treated workers or in the most preferred sector of training for controls. For those who report having no sector-specific skills, we assume they answer the test at random and so obtain a score of 11. In Columns 3 and 4, the dependent variables are labor market outcomes in 2018 (Wave 4). In Columns 5, 6, and 7, the outcomes are cumulative labor market outcomes from the first to the fourth follow-up, among a balanced panel of workers tracked over that period. At the foot of each column we report the mean (standard deviation) for each outcome among controls, and the reweighted mean (standard deviation) for each outcome among controls, where we reweight observations by their probability of compliance. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. In Columns 1 to 4 we also control for survey month. In Column 4, we control for the dependent variable at baseline, setting the missing values equal to 0 and including a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 5: Cumulative Labor Market Outcomes Over the Pandemic

ATT estimates, robust standard errors in parentheses

	Has done any work	Main activity is work in any of the eight sectors	Main activity is wage or self-employment	Main activity is casual work	Total earnings (USD)	Earnings in wage/self employment (USD)	Earnings in casual work (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT: Vocationally Trained	-.211	1.89***	.235	-.476*	132	184**	-52.1
	(.369)	(.481)	(.43)	(.274)	(81.1)	(80.2)	(34.9)
Interpolated effects over 25 months							
Constant imputation	-.271	3.41***	.419	-.752	262*	358**	-95.7
	(.704)	(.918)	(.825)	(.532)	(153)	(151)	(67.7)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.51%***	***60.5%***	***2.93%***	***-21.4%***	***16.6%***	***28.1%***	***-31.4%***
Number of observations	708	607	708	708	683	683	683

Notes: The top Panel reports 2SLS ATT estimates, where robust standard errors are in parentheses. The lower panel reports interpolated estimates covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time-frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time frames. The reweighted control mean reweights observations by their probability of compliance. The Implied Treatment Effect is calculated dividing the ATT by the reweighted control mean. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age and, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 6: Worker Mobility During the Pandemic

ATT panel regression coefficients, robust standard errors in parentheses

Columns 1 to 3: in wage employment pre- AND post-lockdown, in either of the two lockdowns

Columns 4-7: in wage employment pre-lockdown, in either of the two lockdowns

	Firm and Sectoral Allocations			Transitions from Wage Employment to:			
	Same firm	Same sector, different firm	Different sector	Wage employment	Self-employment	Casual work	Unemployment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Skilled x wave L1	-.180**	.194***	-.014	-.049	.035	-.061	.078
	(.082)	(.060)	(.063)	(.076)	(.043)	(.040)	(.069)
Skilled x wave L2	.010	.031	-.041	-.082	.018	.089***	-.035
	(.054)	(.044)	(.034)	(.082)	(.041)	(.028)	(.073)
Reweighted control mean, L1	.866	.057	.077	.539	.080	.104	.277
Reweighted control mean, L2	.926	.052	.021	.708	.068	.000	.214
N. of observations	406	406	406	735	735	735	735

Notes: We report 2SLS ATT estimates, where robust standard errors are in parentheses, and all data are from survey waves L1 and L2. The sample in Columns 1 to 3 is restricted to workers who are wage employed in the pre- and post-lockdown time frames, in either of the two surveys. The sample in Columns 4 to 7 is restricted to workers that are wage employed in the pre- lockdown time frame. The outcome in Column 1 is a dummy equal to one if the respondent was wage employed in the same firm pre- and post-lockdown. The outcome in Column 2 is a dummy equal to one if the respondent was wage employed in the same sector but in a different firm pre- and post-lockdown. The outcome in Column 3 is a dummy equal to one if the respondent was wage employed in a different sector pre- and post-lockdown. The outcomes in Columns 4 to 7 are dummies equal to 1 if the respondent transitioned from being wage employed pre-lockdown to being wage employed, self-employed, engaged in casual work, or unemployed, post-lockdown. Each Column corresponds to one of these four activity types. In all specifications we control for randomization strata, implementation round, survey month, the desired sector at application, and the following worker characteristics at baseline: age and, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. Interaction terms are included between the six covariates controlled for at baseline and survey wave to account for differential attrition. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 7: Labor Market Attachment

Outcomes: Cumulative monthly outcomes over the pandemic (March 2020 to February 2022)

ATT estimates

	Has done any work	Main activity is work in any of the eight sectors	Main activity is wage or self-employment	Main activity is casual work	Total earnings (USD)	Earnings in wage/self employment (USD)	Earnings in casual work (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Baseline imputed effects over 25 months	-.271	3.41***	.419	-.752	262*	358**	-95.7
	(.704)	(.918)	(.825)	(.532)	(153)	(151)	(67.7)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.51%***	***60.5%***	***2.93%***	***-21.4%***	***16.6%***	***28.1%***	***-31.4%***
B. Reweight by sector-specific experience	-.721	-1.30	-.803	-.007	249	278	-29.0
	(.776)	(1.16)	(.917)	(.560)	(183)	(185)	(54.1)
Reweighted control mean	18.3	6.37	15.0	3.20	1591	1333	258
Implied Treatment Effect (%)	***-3.94%***	***-20.4%***	***-5.35%***	***.219%***	***15.7%***	***20.9%***	***-11.2%***
C. Reweight by all experience in wage/self employment	-1.11	1.66	-.940	-.219	147	224	-77.7
	(.758)	(1.08)	(.899)	(.549)	(180)	(182)	(57.8)
Reweighted control mean	18.3	6.37	15.0	3.20	1591	1333	258
Implied Treatment Effect (%)	***-6.07%***	***26.1%***	***-6.27%***	***-6.84%***	***9.24%***	***16.8%***	***-30.1%***
D. Reweight by length of average employment spell	-.968	2.88***	.084	-1.05**	196	343*	-146**
	(.777)	(1.12)	(.892)	(.536)	(201)	(199)	(71.9)
Reweighted control mean	18.9	6.60	15.4	3.34	1701	1425	276
Implied Treatment Effect (%)	***-5.12%***	***43.6%***	***.545%***	***-31.4%***	***11.5%***	***24.1%***	***-52.9%***
E. Reweight by savings	-.298	3.40***	.415	-.754	251	344**	-92.4
	(.697)	(.934)	(.807)	(.501)	(166)	(166)	(60.6)
Reweighted control mean	17.9	5.68	14.3	3.55	1575	1268	307
Implied Treatment Effect (%)	***-1.66%***	***59.9%***	***2.90%***	***-21.2%***	***15.9%***	***27.1%***	***-30.1%***
Number of observations	708	607	708	708	683	683	683

Notes: Each panel reports interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time-frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time-frames. The reweighted control mean reweights observations by their probability of compliance. The implied treatment effect is calculated dividing the ATT by the reweighted control mean. In Panels B to E, we reweight Controls such that the distribution of the residualized reweighting variable is equivalent to that of compliers. When reweighting for continuous covariates, we first regress the covariate on worker characteristics (that are either measured at baseline or are time invariant) and then split the distribution of residuals into deciles -- using this to reweight controls so the distribution of residual deciles corresponds to that of the compliers. Non-compliers are not reweighted. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 8: Firm Characteristics

Means, standard deviations in parentheses
p-value on t-test of equality of means

	Baseline (Oct '12 - Jan '13)	W5 Non-attriters, outcome at baseline (Oct '12 - Jan '13)	Test of equality [1 =2]	Non-attriters, outcome at W5 (Feb - Mar '20)	Test of equality [1 =4]	Census (May-Jul '17)	Percentile of Census firms that the W5 non attriters are at
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of firms	2,307	1,068		1,065		1,191	
A. Employment, Profit and Revenues							
Number of employees	2.84	2.97	[.126]	5.50	[.000]	4.10	*84th percentile*
	(2.29)	(2.35)		(10.4)		(7.81)	
Monthly profits (USD)	221	232	[.433]	266	[.015]	121	*92nd percentile*
	(357)	(374)		(657)		(133)	
Revenues (USD)	522	547	[.439]	1010	[.000]	267	*97th percentile*
	(847)	(879)		(5310)		(358)	
Revenues per worker (USD)	203	207	[.726]	191	[.431]	75.2	*95th percentile*
	(308)	(322)		(435)		(70.9)	
Wage bill/Revenues	.683	.704	[.685]	.945	[.000]		
	(1.16)	(1.42)		(1.27)			
B. Firm Characteristics							
Manufacturing	.339	.380	[.020]	.388	[.006]	.251	
In Kampala	.522	.526	[.828]	.491	[.113]	.618	
Firm age	6.63	7.23	[.004]	14.2	[.000]	9.77	
	(5.33)	(6.26)		(6.26)		(6.04)	
C. Firm Owners							
Female owner	.530	.520	[.587]	.520	[.607]	.485	
Owner age	34.5	34.6	[.767]	41.6	[.000]	36.7	*77th percentile*
	(7.56)	(7.83)		(7.84)		(7.91)	
D. Exposure to the Pandemic							
Number of customers per week	16.8	15.5	[.313]	29.8	[.000]		
	(38.3)	(23.2)		(58.8)			
Maximum number of customers in a good week	29.1	28.1	[.485]				
	(36.8)	(34.9)					
Number of social or business ties to other firms	1.09	1.15	[.099]				
	(.874)	(.900)					
Number of supply chain ties	.589	.598	[.739]				
	(.780)	(.792)					

Notes: All data comes from the firm-side surveys or the second census of firms conducted in 2017. Column 1 reports firm outcomes at baseline, for firms operating in one of the eight study sectors. Column 2 reports firm outcomes at baseline for those firms that do not attrit by the first pandemic firm survey, or fifth survey overall. Column 3 reports the p-value of the t-test comparing the means in Columns 1 and 2. Column 4 reports outcomes for non-attriting firms in the first pandemic firm survey, or firth survey overall. Column 5 reports the p-value of the t-test comparing the means in Columns 1 and 4. Column 6 reports outcomes for firms in the 2017 firm census, for firms operating in one of the eight study sectors. Column 7 reports the percentile of data from the Census of firms that the wave 5 non-attriters outcomes, as measured at survey wave 5. In Panel D, outcomes are measured at the first follow-up. The number of customers per week is the number of customers that made purchases at the firm in the last week, while the maximum number of customers in a good week is the maximum number of customers the firm typically has in a week when demand is particularly high. Our firmside surveys ask firms to list and answer questions about a maximum of five firms with whom they interact/communicate. In Panel D, the number of social or business ties to other firms is the number of firms that surveyed firms then list as part of their network. The number of supply chain ties is the number of the firms within the network that sell/buy inputs from the surveyed firm. All monetary variables are deflated at 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 9: Firms in Most and Least Affected Sectors

Firm characteristics at Wave 4 (2017)
Means, standard deviations in parentheses
p-value on t-test of equality of means

	Most Affected Sector: Tailoring Firms	Least Affected Sector: Welding Firms	Test of equality [1 =2]
	(1)	(2)	(3)
A. Exposure to the Pandemic			
Number of customers per week	14.2	8.15	[.000]
	(16.4)	(8.18)	
Number of supply chain ties	1.73	1.70	[.817]
	(1.40)	(1.23)	
B. Employment, Profit and Revenues			
Number of employees	1.53	3.63	[.000]
	(1.91)	(2.02)	
Firm Age	11.9	12.0	[.920]
	(5.46)	(5.12)	
Revenues (USD)	278	1295	[.000]
	(386)	(1235)	
Monthly profits (USD)	125	366	[.000]
	(141)	(376)	
C. Composition of Workers by Skill Level (owner reported)			
Number of skilled workers	1.62	2.83	[.000]
	(1.64)	(1.73)	
Share of workers that are skilled	.611	.775	[.000]
	(.439)	(.297)	

Notes: All data come from the fourth firms survey in 2017. In Panel A, the number of customers per week is the number of customers that made purchases at the firm in the last week. Our firm survey asks firms to list and answer questions about a maximum of five firms with whom they interact/communicate. In Panel A, the number of supply chain ties is the number of the firms within the network that sell/buy inputs from the surveyed firm. In Panel C, the number of skilled workers refers to the number of workers that the owner identified as being skilled in the employee roster that we conducted in 2017. The share of skilled workers is the number of workers that the owner identified as being skilled divided by the firm's total number of employees. Column 3 reports the p-value of the t-test of the quality of means between tailoring and welding firms. All monetary variables are deflated at 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 10: Sectors and Firm Quality

Outcomes: Cumulative monthly outcomes over the pandemic (March 2020 to February 2022)

ATT estimates

	Has done any work	Main activity is work in any of the eight sectors	Main activity is wage or self-employment	Main activity is casual work	Total earnings (USD)	Earnings in wage/self employment (USD)	Earnings in casual work (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Baseline imputed effects over 25 months	-.271	3.41***	.419	-.752	262*	358**	-95.7
	(.704)	(.918)	(.825)	(.532)	(153)	(151)	(67.7)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.51%***	***60.5%***	***2.93%***	***-21.4%***	***16.6%***	***28.1%***	***-31.4%***
B. Reweight initial sector	-.716	-.252	-.199	-.570	270*	357**	-87.3
	(.643)	(.920)	(.747)	(.457)	(147)	(147)	(53.4)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-4.00%***	***-4.47%***	***-1.39%***	***-16.2%***	***17.1%***	***28.1%***	***-28.6%***
C. Reweight firm quality (size+formality)	-.996	2.57**	.056	-1.04*	160	337*	-177**
	(.802)	(1.15)	(.922)	(.553)	(202)	(200)	(78.1)
Reweighted control mean	18.8	6.72	15.4	3.36	1639	1354	284
Implied Treatment Effect (%)	***-5.30%***	***38.2%***	***.364%***	***-31.0%***	***9.76%***	***24.9%***	***-62.3%***
Number of observations	708	607	708	708	683	683	683

Notes: Each panel reports interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time frames not questioned about. The reweighted control mean reweights observations by their probability of compliance. The implied treatment effect is calculated dividing the ATT by the reweighted control mean. In Panel B, we reweight Controls such that the distribution of the reweighting variable (pre-pandemic sector of employment) is equivalent to that of compliers. In Panel C, the firm quality index captures two characteristics of the last firm that the worker was employed at before the pandemic: its size and whether it was formal. When reweighting the firm quality index, we first regress firm size on firm characteristics (which are either measured at baseline or are time invariant) and then split the distribution of residuals into deciles. We then regress the dummy of whether the firm was formal and obtain the residuals. We use these two sets of residuals to reweight the controls so that the distribution of residual deciles corresponds to that of the compliers. Non-compliers are not reweighted. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 11: Firm Dynamics Over the Pandemic

OLS Panel regression coefficients, robust standard errors in parentheses

	Operating	Number of Employees	Revenues	Profits	Monthly Earnings of Employee	Wage Bill / Revenues
	(1)	(2)	(3)	(4)	(5)	(6)
February 2020			reference period			
April 2020 (during first lockdown)	-.529***	-2.93***	-737***	-207***	-28.4***	-.260***
	(.017)	(.591)	(199)	(35.5)	(7.00)	(.096)
July 2020	-.088***	-2.29***	-582***	-158***	-24.7***	-.139**
	(.015)	(.392)	(181)	(25.1)	(6.08)	(.067)
November 2020	.051***	-1.17***	-180	-29	-16.5***	-.355***
	(.013)	(.391)	(199)	(42.9)	(6.03)	(.073)
February 2021	.033**	-1.94***	-275	-79.4*	-21.3***	-.376***
	(.013)	(.367)	(202)	(43.3)	(6.11)	(.068)
April 2021	.023*	-1.69***	-266	-59.1	-23.6***	-.404***
	(.014)	(.449)	(202)	(52.5)	(6.09)	(.060)
Mean in February 2020	.869	5.58	1010	266	70.4	.946
April 2020 = April 2021 [p-value]	[.000]	[.033]	[.000]	[.011]	[.325]	[.120]
Baseline firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	6577	5006	4508	4508	3717	3468

Notes: All data comes from the fifth and sixth round of firm-side surveys. OLS estimates are shown with robust standard errors in parentheses. All specifications control for the following baseline firm characteristics: a dummy for whether it operates in a manufacturing sector, age, whether the owner is female, the owner's age, and a dummy for whether the firm is in Kampala. To account for missing firm variables at baseline, we set the missing values equal to zero and include a dummy for whether the variable was missing at baseline. At the foot of each Column we report a test of the equality of coefficients between the April 2020 and April 2021 time frames. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 12: Retention and Recruitment of Workers

p-values of test of equality in square brackets

	Mar-Nov 2020	Dec 2020-Jun 2021	[p-value]
	(1)	(2)	(3)
A. Retention and Laid Off Workers			
Share of employees still employed at firm	.633	.749	[.000]
	(.330)	(.307)	
Laid off workers:			
Substantial experience in firm	.787	.788	[.983]
Experience in same sector	.170	.162	[.772]
Unskilled	.023	.009	[.146]
B. Recruitment and Last Hired Workers			
Tried recruiting workers since lockdown	.141	.212	[.000]
Last hired workers:			
Experience in same sector	.422	.239	[.000]
Experience in other sector	.082	.139	[.097]
No experience, but vocationally trained	.034	.100	[.018]
Unskilled	.463	.522	[.275]
C. Earnings			
First month earnings of last/average hired worker	31.8	29.8	[.571]
	(33.1)	(31.6)	
Avg monthly earnings of laid off workers		49.2	
		(41.1)	

Notes: All data comes from the fifth and sixth round of firm-side surveys. The sample covers firms in the eight study sectors. In Panel C, outcomes are conditional on the firm having tried to recruit new workers in the indicated period. In Column 3, we report the test of the equality of means between March 2020-November 2020 and December 2020-June 2021. All monetary variables are deflated at 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 1A: Timeline of Worker Surveys and Interventions

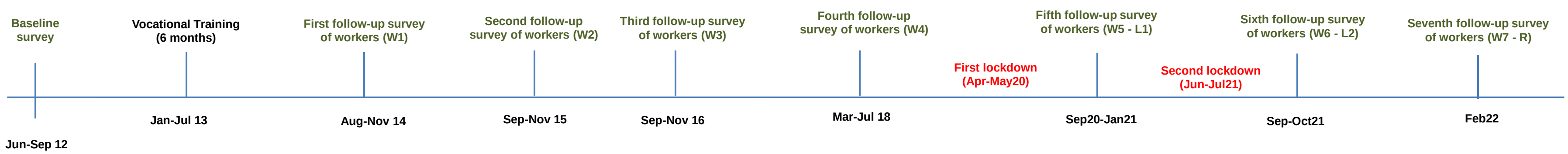
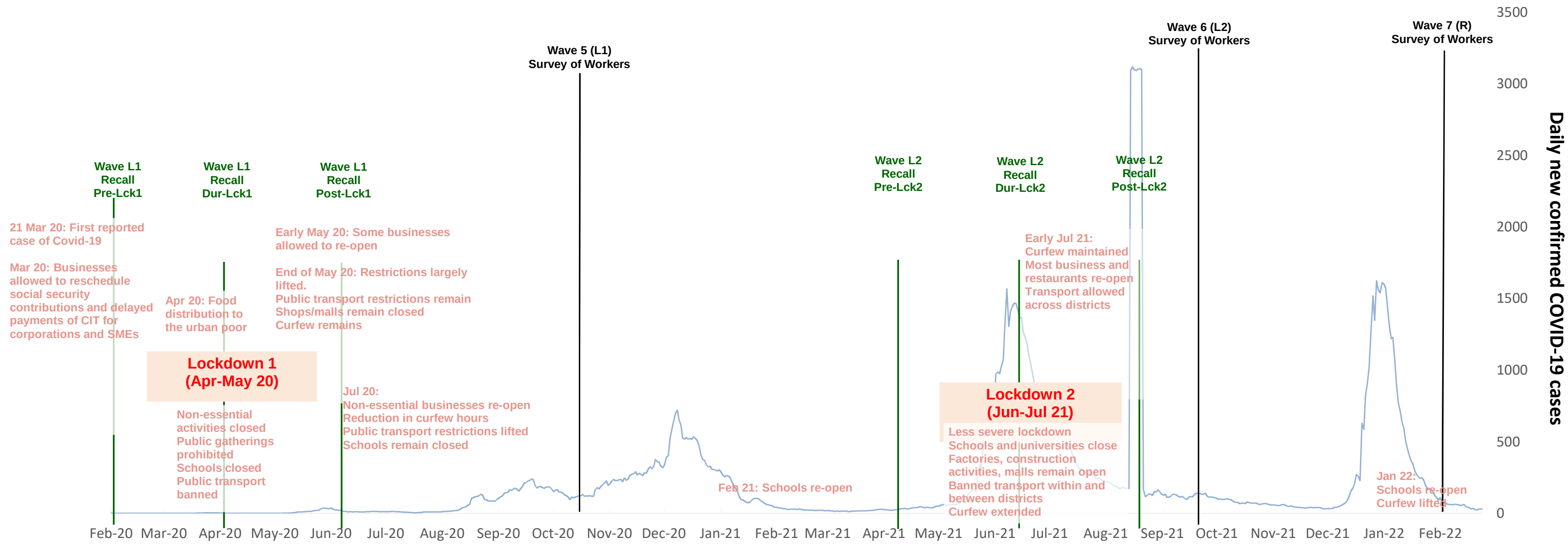
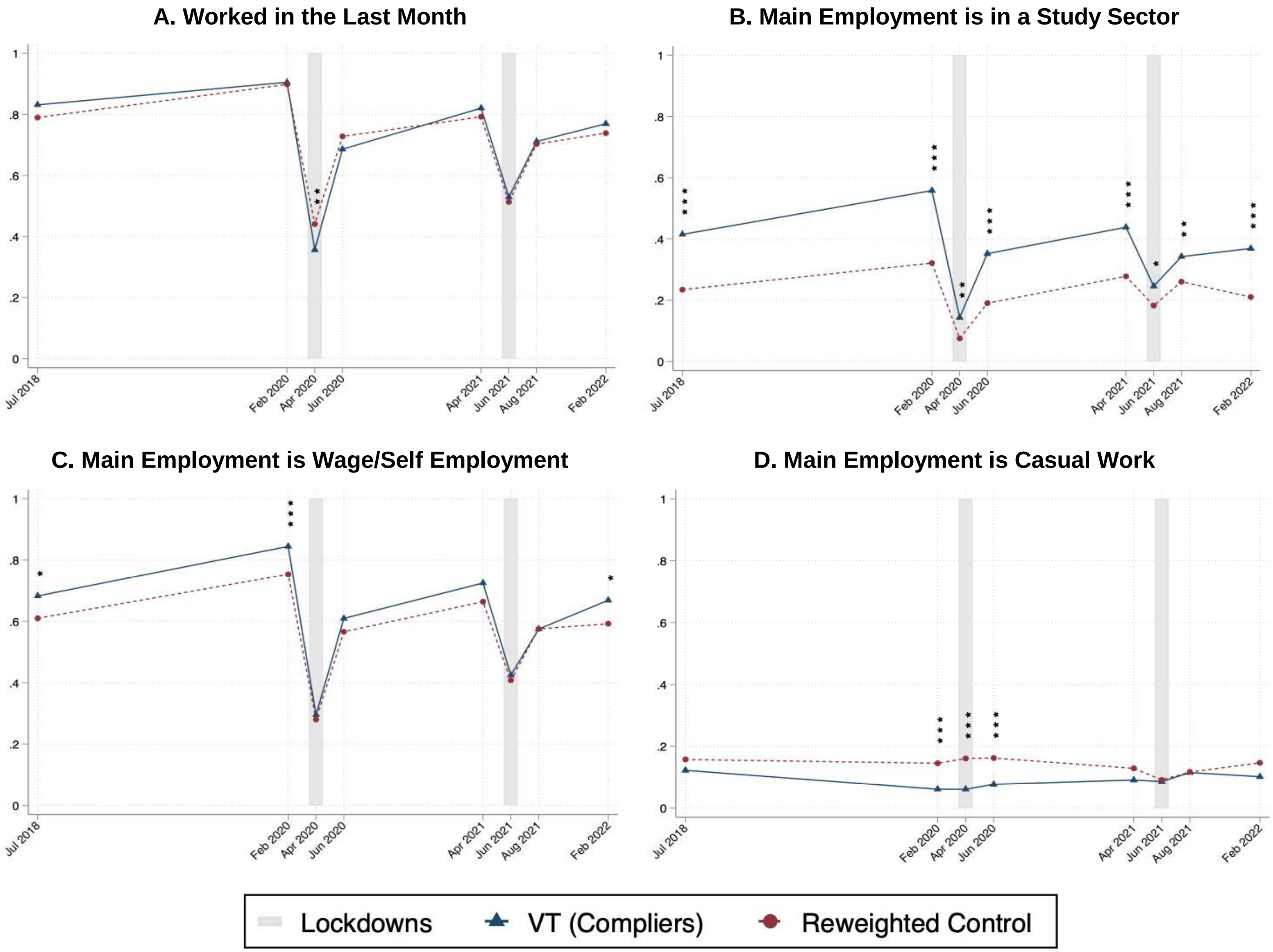


Figure 1B: Surveys, Confirmed Covid-19 Cases and Policy Responses



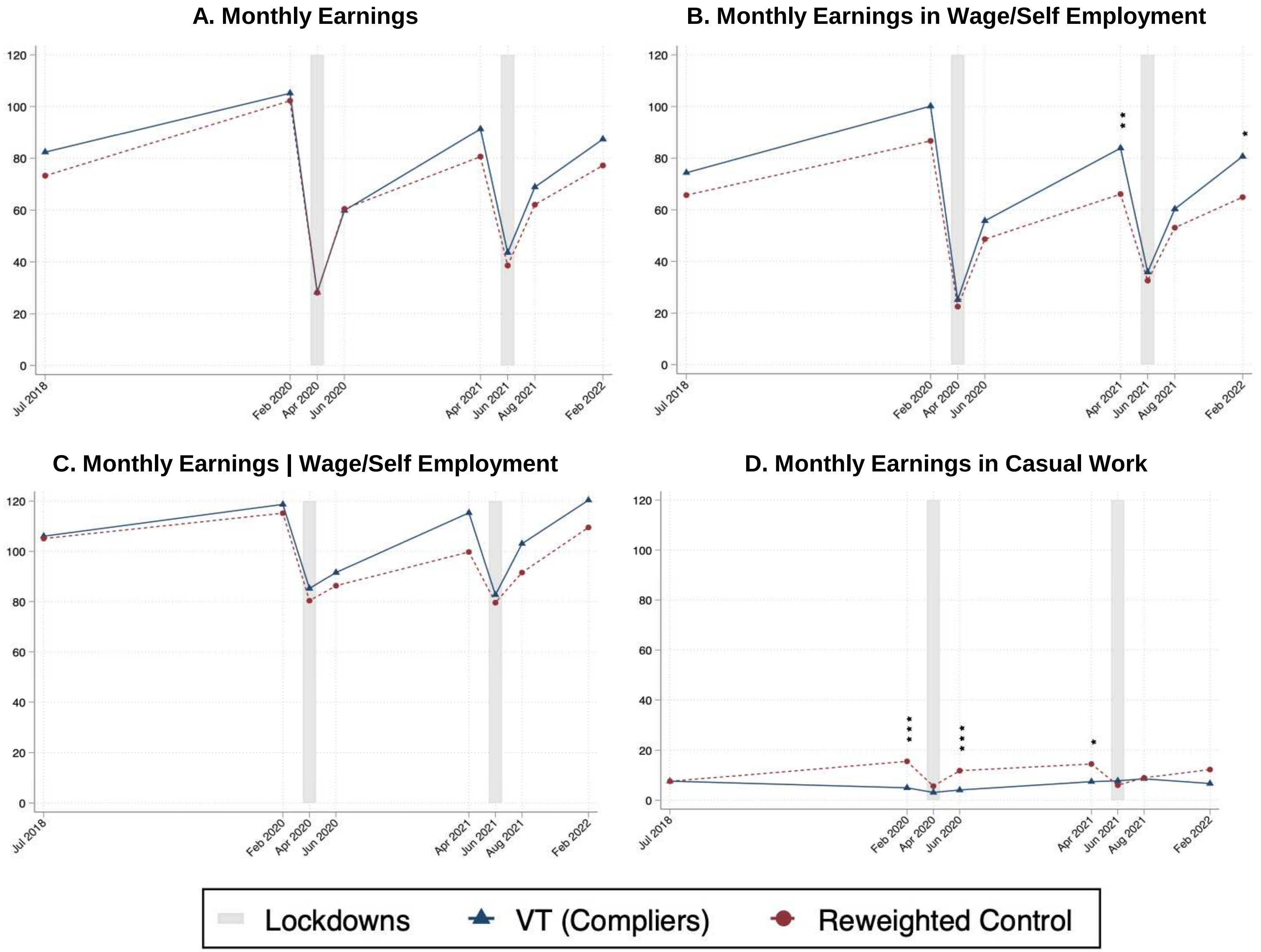
Source for Covid Cases Time Series: Our World in Data [https://ourworldindata.org/covid-cases]

Figure 2: Employment Outcomes over the Pandemic



Notes: In each Panel we compare mean outcomes for compliers to the offer of vocational training to controls, where controls are reweighted by their probability of compliance. The first data point corresponds to Wave 4 conducted in 2018 before the pandemic survey waves. The stars in each time frame report the significance of these unconditional differences in each period. The gray shaded regions correspond to the first and second lockdowns.

Figure 3: Earnings Outcomes over the Pandemic



Notes: In each Panel we compare mean outcomes for compliers to the offer of vocational training to controls, where controls are reweighted by their probability of compliance. The first data point corresponds to Wave 4 conducted in 2018 before the pandemic survey waves. The stars in each time frame report the significance of these unconditional differences in each period. The gray shaded regions correspond to the first and second lockdowns. All monetary variables are deflated at 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 4: Firm Dynamics Over the Pandemic

Notes: Panels A to C use data from the fifth and sixth follow-ups of the firm-side surveys. Panels B and C overlay this with data from the worker-side pandemic surveys. In Panels A to C, firm and worker outcomes are normalized to be one at their February 2020 levels. In Panel C, the top 1% of earnings values reported in the worker-side surveys are trimmed. The gray shaded region corresponds to the first lockdown. Panel D overlays the monthly earnings of the average worker in our firms' sample at April 2021 (sixth follow-up of the firm-side surveys) with the monthly earnings of our workers' sample in the same period (sixth follow-up of the worker-side surveys). Outliers beyond the interquartile range are winsorized. The firm sample is restricted to firms who were open and operating in April 2021 in three sectors: motor-mechanics, hairdressing, and construction. The workers' sample is restricted to VT compliers and controls who were wage employed in one of the three sectors in April 2021. All monetary variables are deflated at 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 A1: 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	Has had any wage employment in the last week	Total earnings from wage employment in the last month
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Our Baseline 2012									
A. Aged 18-25	1,067	20.2	.432	.036	.015	.039	.358	.148	4.29
		(1.91)	(.496)	(.185)	(.122)	(.193)	(.480)	(.355)	(15.8)
Uganda National Household Survey 2012/13:									
B. Aged 18-25	4,696	21.1	.465	.395	.309	.062	.681	.293	9.13
		(2.32)	(.499)	(.489)	(.462)	(.241)	(.466)	(.455)	(28.2)
C. Aged 18-25 and labor market active	3,456	21.4	.475	.448	.207	.064	.902	.389	12.2
		(2.33)	(.499)	(.497)	(.405)	(.245)	(.297)	(.488)	(32.0)

Notes: We report the mean (standard deviation) of individual characteristics 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 working or seeking employment). The UNHS was fielded between June 2012 and June 2013. Our baseline survey was fielded between June and September 2012. In the UNHS respondents are considered to have attended vocational training if the highest grade completed is post-primary specialized training/diploma/certificate or post-secondary specialized training/diploma/certificate. The top 1% of earnings values are trimmed in both samples. All monetary variables are deflated at 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 A2: Baseline Balance on Worker Characteristics

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

	Number of workers	Age [Years]	Gender (=1 male)	Married	Has child(ren)	Currently in school	Ever attended vocational training	Cognitive Test Score	F-test of joint significance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All Workers	1,140	20.1	.567	.038	.117	.018	.038	.562	
		(.252)	(.009)	(.019)	(.027)	(.012)	(.024)	(.054)	
Control	448	20.1	.596	.028	.103	.011	.042	.562	
		(.260)	(.010)	(.020)	(.029)	(.013)	(.025)	(.055)	
Offered Vocational Training	692	20.0	.548	.044	.126	.023	.035	.563	{.377}
		(.119)	(.009)	(.011)	(.019)	(.008)	(.012)	(.029)	
F-test of joint significance		{.821}	{.993}	{.054}	{.139}	{.283}	{.625}	{.534}	

Notes: Data is from the baseline worker survey. Columns 2 to 8 report the mean value of each worker characteristic, derived from an OLS regression of the characteristic of interest on a treatment dummy. 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 in parentheses throughout. The variable in Column 8 is a dummy equal to one if the applicant scored at the median or above on a cognitive test administered with the baseline survey. The test consisted of six literacy and six numeracy questions. Column 9 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 the value zero if the worker is assigned to the Control group and taking value one for workers assigned the offer of vocational training, and the independent variables are the variables in Columns 2 to 8. 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 A3: Baseline Balance for Non Attriters, by Survey Wave

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

		Number of workers	Any work in the last month	Any regular wage employment in the last month	Any self employment in the last month	Any casual work in the last month	Total regular earnings in last month [USD]	Total earnings in last month [USD] | wage/self employment
			(1)	(2)	(3)	(4)	(5)	(6)
Non attriters: wave 4	*Control*	**395**	.394	.120	.041	.288	5.05	35.4
			(.060)	(.033)	(.022)	(.059)	(1.39)	(12.0)
	Offered Vocational Training	**617**	.385	.152	.043	.239	6.51	39.5
			(.030)	(.022)	(.013)	(.027)	(1.06)	(6,78)
			[.854]	[.095]	[.721]	[.282]	[.135]	[.463]
Non attriters: wave 5 (L1)	*Control*	**308**	.428	.127	.042	.320	5.76	38.6
			(.064)	(.036)	(.025)	(.064)	(1.87)	(15.5)
	Offered Vocational Training	**534**	.386	.156	.040	.247	6.63	40.7
			(.034)	(.025)	(.015)	(.031)	(1.26)	(8.40)
			[.499]	[.245]	[.914]	[.130]	[.454]	[.539]
Non attriters: wave 6 (L2)	*Control*	**309**	.436	.130	.042	.313	5.64	36.6
			(.065)	(.039)	(.025)	(.064)	(1.96)	(12.0)
	Offered Vocational Training	**539**	.399	.159	.039	.252	6.99	42.1
			(.034)	(.025)	(.015)	(.031)	(1.23)	(7.69)
			[.603]	[.193]	[.942]	[.222]	[.201]	[.452]
Non attriters: wave 7 (R)	*Control*	**306**	.446	.138	.039	.315	6.35	36.8
			(.063)	(.037)	(.023)	(.062)	(1.85)	(11.3)
	Offered Vocational Training	**536**	.391	.150	.041	.250	6.50	39.7
			(.034)	(.025)	(.015)	(.031)	(1.25)	(7.44)
			[.319]	[.519]	[.821]	[.212]	[.718]	[.558]

Notes: Data is from the baseline worker survey. Columns 1 to 6 report the mean of each worker characteristic, where standard errors are derived from an OLS regression of the characteristic of interest on dummy variables for the treatment groups. All regressions include strata dummies and a dummy for the implementation round. The comparison group in these regressions is control workers. Robust standard errors are reported throughout. In Column 4, casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing compounds. Casual work also includes any type of agricultural labor such as farming, animal rearing, fishing, and agricultural day labor. In Column 5, workers who report doing no work in the month prior the survey (or only doing casual or unpaid work) have a value of zero for total earnings. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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 A4: Compliance

OLS regression coefficients, robust standard errors in parentheses

	(1) Take-up Offer of Vocational Training
Age at baseline	-.009
	(.010)
Married at baseline	-.028
	(.114)
Any child at baseline	-.063
	(.073)
Employed at baseline	.007
	(.040)
Gender (male = 1)	.120
	(.136)
Resides in Kampala at baseline	-.205*
	(.123)
Preferred training sector (manufacturing = 1)	.025
	(.063)
Cognitive ability (above median=1)	-.080**
	(.037)
Locus of control (above median=1)	-.064*
	(.038)
Mean outcome	.655
Strata and implementation round dummies	Yes
Number of observations (workers)	692

Notes: Data is from the baseline worker survey for workers offered vocational training. OLS regression estimates are reported with robust standard errors in parentheses. The cognitive ability variable is a dummy equal to 1 if the applicant scored at the median or above on a cognitive test administered with the baseline survey. The test consisted of six literacy and six numeracy questions. The non-cognitive skills indicator is built using the locus of control (LOC) score calculated using Rotter's (1996) LOC scale. A higher score indicates a more external LOC. The dummy equals one if the respondent answered above the median in the locus of control question. The preferred training sector being manufacturing is a dummy equal to one if the sector of interest reported at baseline was either motor-mechanics, plumbing, construction, welding, electrical work. It is zero otherwise. In all specifications we control for randomization strata and implementation round. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A5: Labor Market Outcomes Pre Covid-19 by Matching Intervention

ITT and ATT estimates, robust standard errors in parentheses

	Skills (wave 3, 2016)		Impacts in 2018		Cumulative Effects 2014 to 2018		
	Has any sector-specific skills	Sector-specific skill test score (0-100)	Main activity in last month is work in any of the eight sectors	Total earnings in last month (USD)	Months unemployed	Months in which main activity was in any of the eight sectors	Monthly earnings from wage/self-employment (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ITT							
T1: Offered Vocational Training	.234***	5.01**	.125***	11.7*	-4.79***	5.43***	420***
	(.044)	(2.20)	(.042)	(6.99)	(1.17)	(1.05)	(156)
T2: Offered Vocational Training + Matched	.205***	7.92***	.115**	15.1*	-2.74**	4.50***	670***
	(.049)	(2.64)	(.047)	(7.88)	(1.3)	(1.09)	(176)
Panel B: ATT							
T1: Vocationally Trained	.314***	6.72**	.180***	16.1*	-6.22***	7.05***	578***
	(.054)	(2.80)	(.059)	(9.37)	(1.48)	(1.30)	(205)
T2: Vocationally Trained + Matched	.329***	12.8***	.185**	23.6*	-4.04**	6.67***	1046***
	(.073)	(4.07)	(.073)	(12.1)	(1.90)	(1.57)	(269)
p-value: T1=T2 (ATT)	[.759]	[.060]	[.931]	[.457]	[.231]	[.819]	[.104]
Control mean (SD)	.663	30.7 (21.3)	.240	72.0	27.3	5.99	1263
Reweighted control mean (SD)	.664	30.9 (21.4)	.235	73.2	27.3	5.90	1281
Number of observations	755	755	1008	935	737	737	526

Notes: Panel A reports OLS ITT estimates, while Panel B reports 2SLS ATT estimates, where robust standard errors are in parentheses. The outcome in Column 1 is a dummy for whether the individual reports having any sector-specific skills, measured at the third follow-up. The outcome in Column 2 is a sector-specific skill test score (that ranges from 0 to 100), administered in the third follow-up. The sector relates to the sector of training for treated workers or the most preferred sector of training for controls. In Columns 3 and 4, the dependent variables are labor market outcomes in 2018 (Wave 4). In Columns 5, 6, and 7 the outcomes are cumulative labor market outcomes from the first to the fourth follow-up, among a balanced panel of workers tracked over that period. At the foot of each column we report the mean (standard deviation) for each outcome among controls, and the reweighted mean (standard deviation) for each outcome among controls, where we reweight observations by their probability of compliance. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. In Columns 1 to 4 we also control for survey month. In Column 4, we control for the dependent variable at baseline, setting the missing values equal to 0 and including a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A6: Labor Market Outcomes Over the Pandemic

Panel regression coefficients (ATT), robust standard errors in parentheses

	Has done any work	Main activity is work in any of the eight sectors	Main activity is wage or self-employment	Main activity is casual work	Total earnings (USD)	Earnings in wage/self employment (USD)	Earnings in casual work (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Vocationally Trained x pre-lock1 (Feb-Mar 20)	-.007	.220***	.088**	-.093***	8.56	19.9*	-11.3**
	(.031)	(.048)	(.041)	(.031)	(11.3)	(11.2)	(4.82)
Vocationally Trained x during-lock1 (Apr-May 20)	-.134***	.045	-.024	-.108***	-1.84	-.745	-1.09
	(.049)	(.031)	(.045)	(.031)	(7.37)	(7.23)	(1.83)
Vocationally Trained x post-lock1 (Jun-Jul 20)	-.066	.149***	.02	-.084**	1.92	9.01	-7.09*
	(.045)	(.045)	(.049)	(.033)	(8.89)	(8.59)	(3.72)
Vocationally Trained x pre-lock2 (Apr-May 21)	.034	.146***	.053	-.024	13.3	20.3**	-7.01
	(.041)	(.046)	(.047)	(.032)	(10.5)	(10.0)	(5.17)
Vocationally Trained x during-lock2 (Jun-Jul 21)	.016	.044	-.013	.023	7.17	3.55	3.62
	(.05)	(.04)	(.05)	(.028)	(7.41)	(7.05)	(3.08)
Vocationally Trained x post-lock2 (Aug-Sep 21)	.045	.081*	.019	.015	11.4	10.5	.850
	(.045)	(.045)	(.05)	(.031)	(9.04)	(8.85)	(3.73)
Vocationally Trained x recovery (Feb 22)	.051	.166***	.089*	-.038	12.5	15.8	-3.26
	(.043)	(.044)	(.049)	(.034)	(10.6)	(9.92)	(5.68)
Reweighted control mean, Feb-Mar 2020	.898	.321	.753	.145	102	86.7	15.6
p-value of F-test of joint significance	[.077]	[.000]	[.200]	[.000]	[.527]	[.093]	[.082]
Feb-Mar 20 = Feb 22 [p-value]	[.282]	[.402]	[.980]	[.227]	[.799]	[.781]	[.274]
Number of observations	5898	5754	5898	5898	5839	5839	5839

Notes: We report 2SLS ATT estimates, where robust standard errors are in parentheses. At the foot of each column, we report the reweighted mean (standard deviation) for each outcome among controls, where we reweight observations by their probability of compliance. In all specifications we control for randomization strata, implementation round, survey month, period fixed effects, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. At the foot of each Column, we also report the p-value from an F-test of the joint significance of the seven interactions reported in the table. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A7: Expectations on Employment and Earnings Over the Pandemic

Panel regression coefficients (ATT), robust standard errors in parentheses

	Expected probability of getting a job in the training sector in the next 12 months (0-10 scale)	Min Expected Earnings in sector of application	Max Expected Earnings in sector of application	Avg Expected Earnings in sector of application
	(1)	(2)	(3)	(4)
Vocationally Trained x L1	1.27***	21.1***	44.7***	32.9***
(September 2020-January 2021)	(.314)	(7.41)	(14.8)	(11.0)
Vocationally Trained x L2	2.34***	41.1***	72.5***	58.0***
(September-October 2021)	(.329)	(7.65)	(15.0)	(11.1)
Vocationally Trained x R	2.70***	49.1***	82.5***	67.2***
(February 2022)	(.315)	(7.15)	(12.2)	(9.41)
Reweighted control mean, L1	4.67	83.8	150	118
Vocationally trained, L1 = R [p-value]	[.001]	[.006]	[.049]	[.017]
Vocationally trained, L1 = L2 [p-value]	[.018]	[.057]	[.184]	[.106]
Vocationally trained, L2 = R [p-value]	[.418]	[.441]	[.603]	[.526]
Number of observations	2516	2365	2361	2346

Notes: We report 2SLS ATT estimates, where robust standard errors are in parentheses. At the foot of each column, we report the reweighted mean (standard deviation) for each outcome among controls, where we reweight observations by their probability of compliance. In all specifications we control for randomization strata, implementation round, survey month, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. At the foot of each Column, we also report the p-value from a test of equality across survey waves for those offered vocational training. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A8, Part 1: Heterogeneous Impacts on Labor Market Outcomes Over the Pandemic

Outcomes: Cumulative monthly outcomes over the pandemic (March 2020 to February 2022)
ATT estimates

	Has done any work	Main activity is work in any of the eight sectors	Main activity is wage or self-employment	Main activity is casual work	Total Earnings (USD)	Earnings in Wage/Self Employment (USD)	Earnings in Casual Work (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Imputed effects over 25 months	-.271	3.41***	.419	-.752	262*	358**	-95.7
	(.704)	(.918)	(.825)	(.532)	(153)	(151)	(67.7)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.51%***	***60.5%***	***2.93%***	***-21.4%***	***16.6%***	***28.1%***	***-31.4%***
A. Men	-.784	3.45***	.282	-1.17	166	318	-152
	(.747)	(1.21)	(.979)	(.743)	(216)	(212)	(105)
Reweighted control mean	19.8	6.23	15.2	4.49	1944	1532	412
Implied Treatment Effect (%)	***-3.96%***	***55.4%***	***1.86%***	***-26.1%***	***8.54%***	***20.8%***	***-36.9%***
B. Women	.653	3.67**	.103	.537	392**	392**	.064
	(1.53)	(1.45)	(1.58)	(.636)	(167)	(165)	(41.4)
Reweighted control mean	13.7	4.28	12.4	1.27	730	673	57.1
Implied Treatment Effect (%)	***4.77%***	***81.4%***	***.811%***	***40.4%***	***56.0%***	***60.9%***	***.113%***
C. Desired sector: manufacturing	-.371	3.52***	.723	-1.19*	186	342*	-155
	(.764)	(1.17)	(.961)	(.712)	(210)	(207)	(97.1)
Reweighted control mean	19.1	6.04	14.8	4.19	1857	1466	391
Implied Treatment Effect (%)	***-1.94%***	***58.3%***	***4.89%***	***-28.4%***	***10.0%***	***23.3%***	***-39.6%***
D. Desired sector: services	-.396	2.82*	-.659	.262	173	178	-5.10
	(1.51)	(1.52)	(1.59)	(.727)	(172)	(171)	(50.1)
Reweighted control mean	14.8	4.66	13.1	1.73	831	757	73.8
Implied Treatment Effect (%)	***-2.68%***	***60.5%***	***-5.03%***	***15.1%***	***20.8%***	***23.5%***	***-6.91%***
E. Region of residence	-.104	3.56***	.536	-.689	284*	372**	-88.2
	(.663)	(.883)	(.763)	(.475)	(154)	(154)	(57.0)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-.581%***	***63.1%***	***3.75%***	***-19.6%***	***18.0%***	***29.2%***	***-28.9%***
Number of observations	708	607	708	708	683	683	683

Notes: Each panel reports interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time frames not questioned about. The reweighted control mean reweights observations by their probability of compliance. The implied treatment effect is calculated dividing the ATT by the reweighted control mean. In Panels B-E, we reweight Controls such that the distribution of the reweighting variable is equivalent to that of compliers. Non-compliers are not reweighted in this exercise. In Panel C the preferred training sector being manufacturing is if the sector of interest reported at baseline was either motor-mechanics, plumbing, construction, welding, or electrical work. In Panel D the preferred training sector being services is if the sector of interest reported at baseline was either hairdressing, tailoring or catering. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A8, Part 2: Heterogeneous Impacts on Labor Market Outcomes Over the Pandemic

Outcomes: Cumulative monthly outcomes over the pandemic (March 2020 to February 2022)

ATT estimates

	Has done any work	Main activity is work in any of the eight sectors	Main activity is wage or self-employment	Main activity is casual work	Total Earnings (USD)	Earnings in Wage/Self Employment (USD)	Earnings in Casual Work (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Imputed effects over 25 months	-.271	3.41***	.419	-.752	262*	358**	-95.7
	(.704)	(.918)	(.825)	(.532)	(153)	(151)	(67.7)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.51%***	***60.5%***	***2.93%***	***-21.4%***	***16.6%***	***28.1%***	***-31.4%***
F. T1: Offered Vocational Training	-.182	4.04***	.954	-1.11**	264*	349**	-84.9
	(.756)	(.984)	(.878)	(.555)	(154)	(149)	(74.8)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.02%***	***71.6%***	***6.67%***	***-31.5%***	***16.7%***	***27.4%***	***-27.8%***
G. T2: Offered Vocational Training + Matched	-.324	2.91**	-.020	-.442	353	477**	-123*
	(.944)	(1.30)	(1.13)	(.749)	(235)	(237)	(74.2)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.81%***	***51.6%***	***-.140%***	***-12.6%***	***22.4%***	***37.5%***	***-40.3%***
H. Cognitive Ability (above median=1)	-.345	3.71***	.545	-.881	303	331	-27.8
	(.964)	(1.23)	(1.14)	(.756)	(232)	(240)	(69.6)
Reweighted control mean	18.4	5.49	14.6	3.61	1565	1333	232
Implied Treatment Effect (%)	***-1.88%***	***67.6%***	***3.73%***	***-24.4%***	***19.4%***	***24.8%***	***-12.0%***
I. Cognitive Ability (below median=1)	-.431	2.11	-.479	-.017	159	310	-151
	(1.05)	(1.39)	(1.18)	(.701)	(217)	(204)	(123)
Reweighted control mean	17.3	5.99	14.2	3.11	1596	1216	380
Implied Treatment Effect (%)	***-2.49%***	***35.2%***	***-3.37%***	***-.547%***	***9.96%***	***25.5%***	***-39.7%***
Number of observations	708	607	708	708	683	683	683

Notes: Each panel reports interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time frames not questioned about. The reweighted control mean reweights observations by their probability of compliance. The implied treatment effect is calculated dividing the ATT by the reweighted control mean. In Panels F-I, we reweight Controls such that the distribution of the reweighting variable is equivalent to that of compliers. Non-compliers are not reweighted. In Panels H and I, the samples are split by workers with above and below the median cognitive test score, that is if the applicant scored at the median or above/below on a cognitive test administered with the baseline survey. The test consisted of six literacy and six numeracy questions. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A9: Robustness to Attrition

Outcomes: Cumulative monthly outcomes over the pandemic (March 2020 to February 2022)

ATT estimates, robust standard errors in parentheses

					Imputation of attriters			
					+/- .1 SD		+/- .25 SD	
	Main specification	No controls	IPW	Treatment = Control	Control outperforms	Treatment outperforms	Control outperforms	Treatment outperforms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Main activity in last month is work in any of the eight sectors	3.41***	3.51***	3.42***	2.51***	1.73***	3.06***	.726	4.06***
	(.918)	(.901)	(.910)	(.621)	(.564)	(.558)	(.573)	(.560)
Total earnings in last month (USD)	262*	277*	257	256**	3.64	286***	-208**	498***
	(153)	(154)	(162)	(115)	(102)	(102)	(104)	(102)
Earnings from wage/self employment in last month (USD)	358**	368**	363**	330***	81.7	357***	-125	563***
	(151)	(152)	(159)	(114)	(101)	(100)	(103)	(101)

Notes: The data is from the fifth, sixth and seventh worker follow-up surveys. We report 2SLS ATT estimates, where robust standard errors are in parentheses. We report interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time frames of pandemic surveys. In Columns 1 to 3, we use a constant imputation method, so assuming each outcome remains constant between time frames not questioned about. In the other columns, we impute missing data for the attriters using the control mean (Column 4), assuming that controls outperform compliers by 0.2SD and vice versa (Columns 5 and 6), and assuming that controls outperform compliers by 0.5SD and vice versa (Columns 7 and 8). In all specifications we control for randomization strata, implementation round and desired sector at application. In all specifications from Column 2 onwards we also control for the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A10: Search Behavior Over the Pandemic

Panel regression coefficients (ATT), robust standard errors in parentheses

	Search Intensity (last month)				Directed Search			
	Searched	Days spent searching	Applications sent	Job offers received	Searched in one of the eight main sectors	Searched in the formal sector	Searched in the informal sector	Searched in Kampala
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vocationally Trained x L1	.041	1.41	-	-	-	-	-	-
(September 2020-January 2021)	(.049)	(1.02)						
Vocationally Trained x L2	-.027	2.09*	-.252	.054	.007	-.024	.027	-.027
(September-October 2021)	(.045)	(1.23)	(.256)	(.120)	(.037)	(.038)	(.039)	(.028)
Vocationally Trained x R	.022	-.170	.147	-.016	.070**	.055	.011	.016
(February 2022)	(.044)	(1.51)	(.204)	(.050)	(.034)	(.035)	(.037)	(.024)
Reweighted control mean in L2	.288	7.71	1.08	.219	.160	.189	.170	.083
p-value of F-test of joint significance	[.724]	[.186]	[.436]	[.839]	[.117]	[.232]	[.761]	[.510]
Number of observations	2526	737	1684	1683	1686	1663	1659	1686

Notes: The data is from the fifth, sixth and seventh worker follow-up surveys. Survey wave 5 (L1) was conducted between September 2020 and January 2021 and spans the first lockdown, while survey wave 6 (L2) was conducted between September 2021 and October 2021 and spans the second lockdown. We report 2SLS ATT estimates, where robust standard errors are in parentheses. The dependent variable in Column 1 is a dummy equal to one if the respondent was actively searching for a job in the month prior to the survey. In Columns 2, 3 and 4, the dependent variable is the number of days that the respondent spent searching, number of job applications sent, and number of job offers received, respectively, in the last month. These outcomes are conditional on having actively searched for a job in the last month. Questions on the number of applications and number of job offers were not asked in survey wave L1. The outcomes in Columns 5, 6, 7 and 8 are also conditional on having searched in the last month and were not asked in survey wave L1. In all specifications we control for randomization strata, implementation round, survey month, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. At the foot of each column, we report the reweighted control mean at survey wave L2, where we reweight using compliance probabilities. At the foot of each column, we also report the p-value from an F-test of joint significance of the three interactions reported in the table. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A11: Health

ATT estimates, robust standard errors in parentheses

	Pre-pandemic health in 2016 (wave 3)		Health and Search Behavior Over the Pandemic			
	(1) Self-reported Health (0-10)	(2) Unable to perform normal activity due to health	(3) Not searching for a job due to health	(4) Moved to a location with better healthcare or safer in terms of Covid (conditional on moving)	(5) Extremely worried about contracting Covid	(6) No change in ideal job preferences due to Covid
ATT: Vocationally Trained	.278	-.008				
	(.236)	(.035)				
Vocationally Trained x L1			-.001	.022	-.066	
(September 2020-January 2021)			(.037)	(.047)	(.057)	
Vocationally Trained x L2			.011	.063	-.011	-.009
(September-October 2021)			(.020)	(.050)	(.059)	(.042)
Vocationally Trained x R			-.007	.013	.031	.028
(February 2022)			(.017)	(.046)	(.047)	(.046)
Reweighted control mean in W3	7.42	.192				
Reweighted control mean in L2			.021	.014	.357	.795
p-value of F-test of joint significance			[.874]	[.526]	[.492]	[.827]
Number of observations	996	996	1781	510	1780	1688

Notes: The data utilized is from the third, fifth, sixth and seventh worker follow-up surveys. Survey wave 3 is a pre-pandemic survey conducted in 2016. Survey wave 5 (L1) was conducted between September 2020 and January 2021 and spans the first lockdown, while survey wave 6 (L2) was conducted between September 2021 and October 2021 and spans the second lockdown. We report 2SLS ATT estimates, where robust standard errors are in parentheses. The dependent variable in Column 1 comes from a self-reported health score that ranges from 0 to 10, where respondents were asked to describe the state of their physical health in the last few days. In Column 2, the dependent variable is a dummy equal to 1 if the respondent reported being unable to perform normal activity for at least seven days due to illness/injury. The dependent variable in Column 3 is a dummy variable equal to 1 if the worker reported they were not actively looking for a job because of health reasons (e.g. looking for a job or working can increase the probability of infection). Column 3 is restricted to the sample of workers who were not actively looking for a job in the last 30 days. The dependent variable in Column 4 is a dummy equal to 1 if the respondent listed a better healthcare system or lower risk of COVID-19 infections as reasons for moving to a different location. Column 4 is conditional on having moved since the second lockdown (for L2) or since November 2021 (for R). In Column 5, the dependent variable is a dummy variable equal to 1 if the respondent reported being extremely worried about contracting COVID-19 in the workplace. The dependent variable in Column 6 is a dummy variable equal to 1 if the worker said that COVID-19 did not change their preferences over their ideal job. In all specifications we control for randomization strata, implementation round, survey month, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. At the foot of each column, we report the reweighted control mean at survey wave L2, where we reweight using compliance probabilities. At the foot of each Column, we also report the p-value from an F-test of joint significance of the interactions reported in the table. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A12: Experiences of the Pandemic

Regression coefficients (ATT), robust standard errors in parentheses

	Lockdowns			Coping Strategies			Expectations	
	Lockdown strictly implemented	Difficult to go to food market during lockdown	Unable to buy food during lockdown	Reduce number or size of meals	Sold assets	Moved	Expects economy to rebound in six months	Expects economy to rebound in one year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vocationally Trained x L1	.140***	.030	.076***	.014	-.016	.026	.036	.069
	(.045)	(.047)	(.026)	(.039)	(.050)	(.045)	(.045)	(.047)
Vocationally Trained x L2	-	.003	.007	.018	.005	.023	-.021	.034
		(.049)	(.022)	(.049)	(.049)	(.036)	(.044)	(.051)
Vocationally Trained x R	-	-	-		-.053	.054	-.020	-.014
					(.049)	(.040)	(.050)	(.048)
Reweighted control mean in L1	.685	.675	.054	.820	.572	.277	.274	.657
Reweighted control mean in L2	-	.497	.048	.612	.481	.135	.226	.468
Reweighted control mean in R	-	-	-		.411	.165	.468	.665
p-value of F-test of joint significance	-	[.810]	[.015]	[.887]	[.739]	[.482]	[.794]	[.445]
Number of observations	838	1686	1686	1686	2518	2526	2525	2525

Notes: The data is from the fifth, sixth and seventh worker follow-up surveys. Survey wave 5 (L1) was conducted between September 2020 and January 2021 and spans the first lockdown, while survey wave 6 (L2) was conducted between September 2021 and October 2021 and spans the second lockdown. We report 2SLS ATT estimates, where robust standard errors are in parentheses. In Column 1 the strictness of the lockdown is equal to one if the respondent said that during the first lockdown everything was completely shut down except for essentials. In Column 2 the outcome is a dummy equal to one if the respondent had difficulties in going to the food market during the lockdown. The dependent variable in Column 3 is a dummy equal to one if the respondent could not buy food during the lockdown either due to shortages in markets, because prices were too high, or because household income had dropped. The outcome in Column 4 is equal to one if the respondent reported to have reduced the number or size of their meals during the total lockdown. The dependent variables in Columns 5 and 6 are whether the respondent sold any asset or livestock to generate income and whether they moved since March 2020 (for L1), since June 2021 (for L2), and since November 2021 (for R). The dependent variables in Columns 7 and 8 are dummy variables equal to 1 if the respondent said it was very likely or moderately likely that the economy would rebound within six months and within one year. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. At the foot of each Column we report the reweighted control mean at each survey wave, where we reweight using compliance probabilities. At the foot of each column, we also report the p-value from an F-test of joint significance of the three interactions reported in the table. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

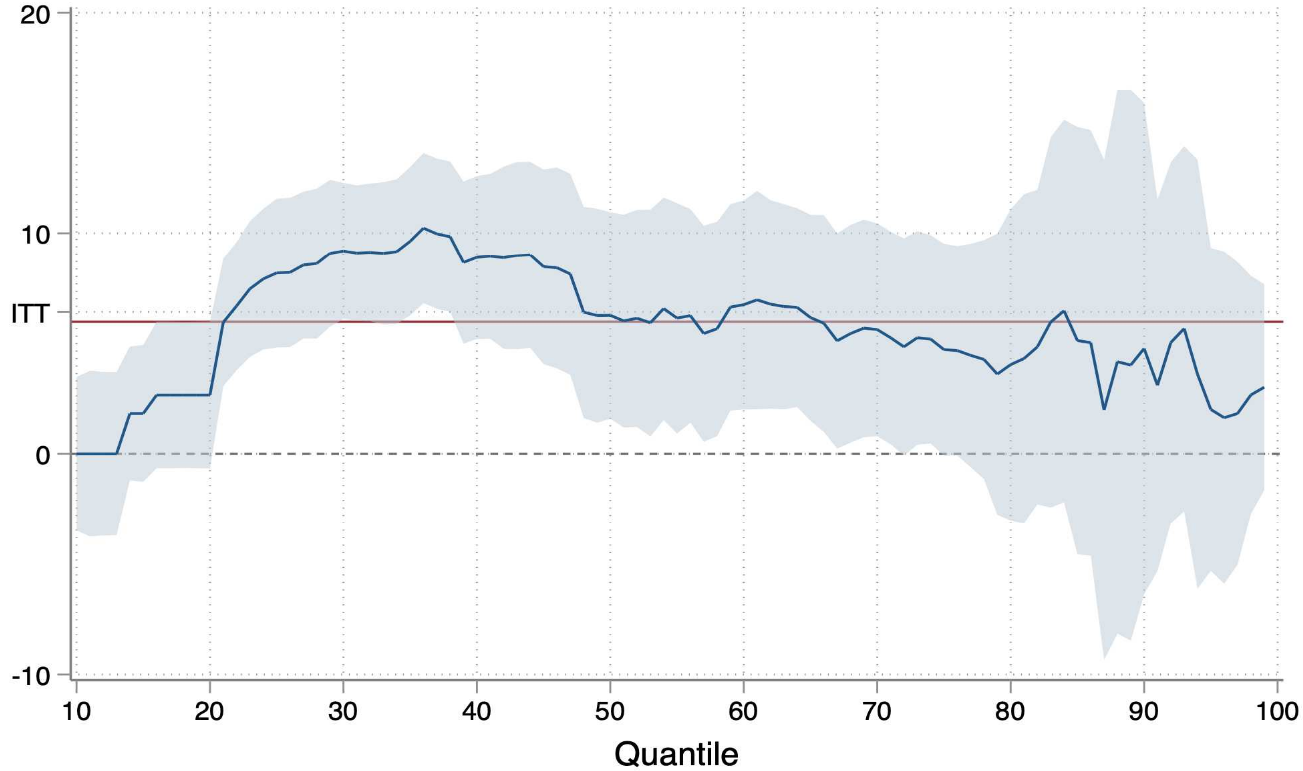
Table A13: Attrition and Survival of Firms

OLS regression coefficients, robust standard errors in parentheses

	Outcome: Firm attrited by			Outcome: Firm Survival
	2017 (Wave 4)	2020 (Wave 5)	2021 (Wave 6)	2021 (Wave 6)
	(1)	(2)	(3)	(4)
Number of Employees	.004	-.006	-.005	.009*
	(.004)	(.005)	(.004)	(.005)
Log Monthly Profits (USD)	-.033***	.027**	.019	-.018
	(.010)	(.012)	(.011)	(.014)
Manufacturing	-.021	-.009	-.038*	.128***
	(.018)	(.022)	(.022)	(.026)
In Kampala	.149***	-.028	-.054***	-.018
	(.016)	(.021)	(.020)	(.025)
Firm Age	-.006***	-.005***	-.005***	.005**
	(.002)	(.002)	.002	(.002)
Female Owner	-.044**	.035	.021	-.055**
	(.018)	(.021)	(.021)	(.025)
Owner Age	.002	.001	-.001	-.003*
	(.001)	(.001)	(.001)	(.002)
Wage Bill / Revenues				.009
				(.006)
Number of customers per week				-.001***
				(.000)
Number of supply chain ties				.010
				(.014)
Mean outcome	.157	.284	.272	.670
Test of joint significance of firm characteristics [p-value]	.000	.025	.000	[.000]
R-squared	.058	.081	.103	.144
Number of observations (firms)	1860	1860	1860	1409

Notes: All data is from the firm side surveys. OLS estimates are shown with robust standard errors in parentheses. The outcome in Columns 1, 2 and 3 are whether the firm attrits between baseline and survey waves 4, 5 and 6 respectively. Firm owners can attrit at each survey wave 4, 5, and 6 either because they cannot be located, or are recorded as deceased, mentally ill, or having moved abroad. The outcome in Column 4 is whether the firm survives until firm survey wave 6, conditional on being open in the last pre-pandemic survey wave (Wave 4) and on not attriting in either wave 5 or wave 6. The covariates included in all Columns are collected at baseline, and we additionally control for a dummy for firms that were not approached at all. To account for the missing variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. In Column 4 the number of customers per week is the number of customers that made purchases at the firm in the last week, as collected at the first follow-up. Our firm survey also asked firms to list and answer questions about a maximum of five firms with whom they interact/communicate. The number of supply chain ties is the number of the firms within the network that sell/buy inputs from the surveyed firm. All monetary variables are deflated at 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. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Figure A1: QTE on Sector-Specific Skills

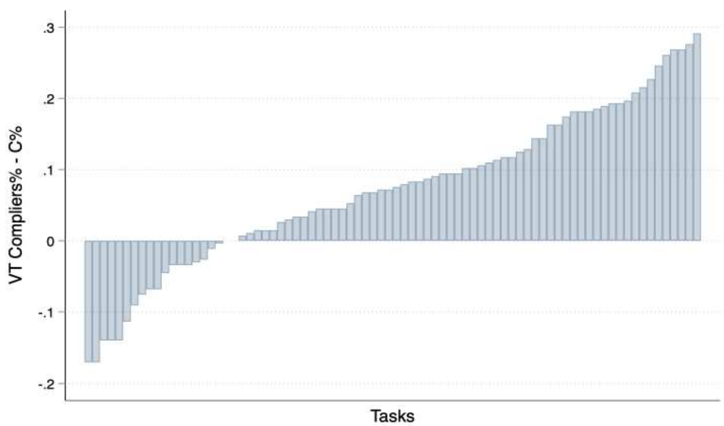


Notes: The Figure reports quantile treatment effect estimates of the offer of training on the sector-specific skills test score (which ranges from 0 to 100) and 95% confidence intervals. The tests were administered in the third follow-up. The sector relates to the sector of training for treated workers or the most preferred sector of training for controls. All workers who reported having sectoral skills took the test: others were assigned a score of 11 assuming they would answer the test at random. Hence we remove the first ten quantiles from the figure of QTEs. In this specification we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline.

Figure A2: Tasks Performed by Vocationally Trained and Control Workers

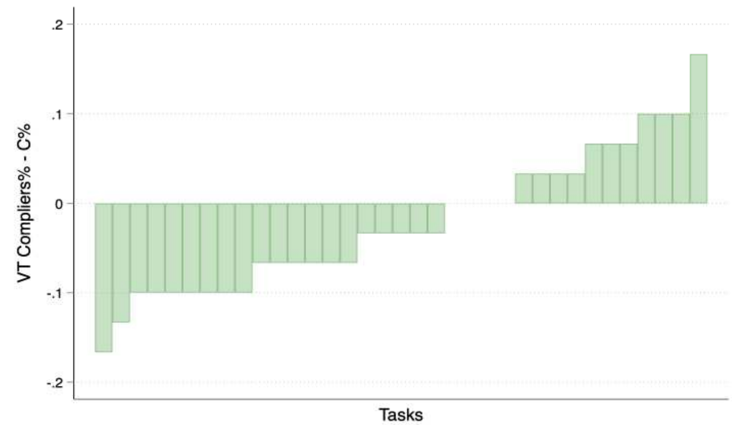
Y Axis = VT% - C% Performing a Given Task in the Firm

A. Motor Mechanics



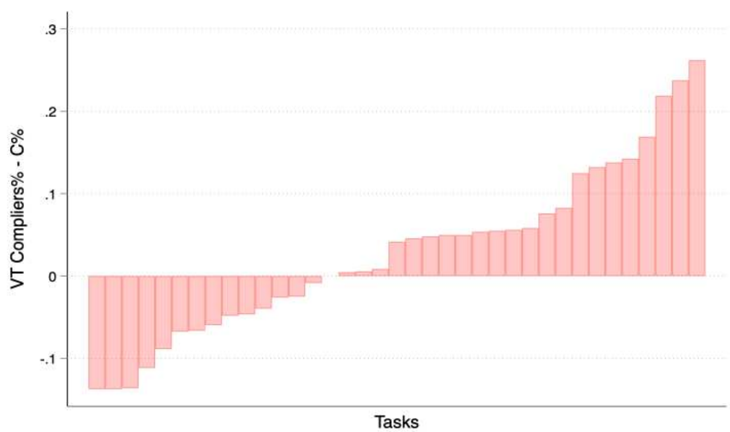
Sector share: 16.1%
p-value of Chi Squared test VT Compliers vs. C: 0.000

B. Electrical Wiring



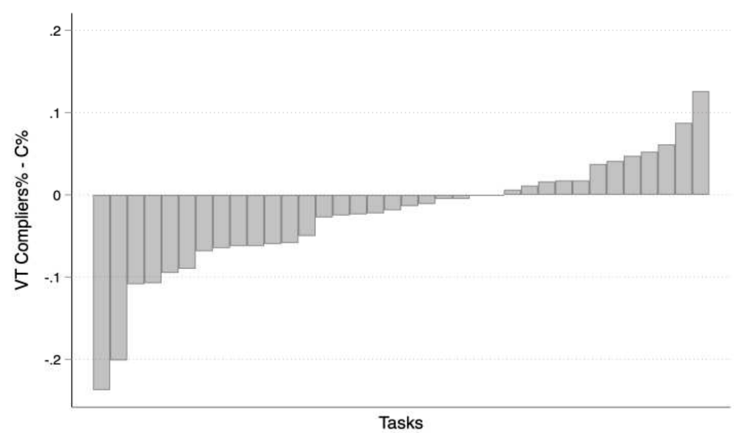
Sector share: 11.3%
p-value of Chi Squared test VT Compliers vs. C: 0.000

C. Hairdressing



Sector share: 23.7%
p-value of Chi Squared test VT Compliers vs. C: 0.137

D. Construction

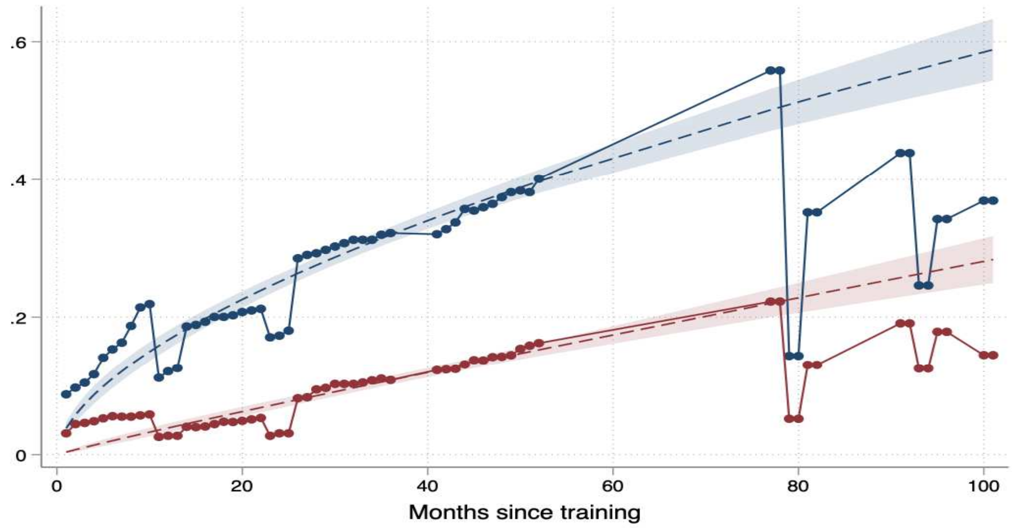


Sector share: 16.1%
p-value of Chi Squared test VT Compliers vs. C: 0.020

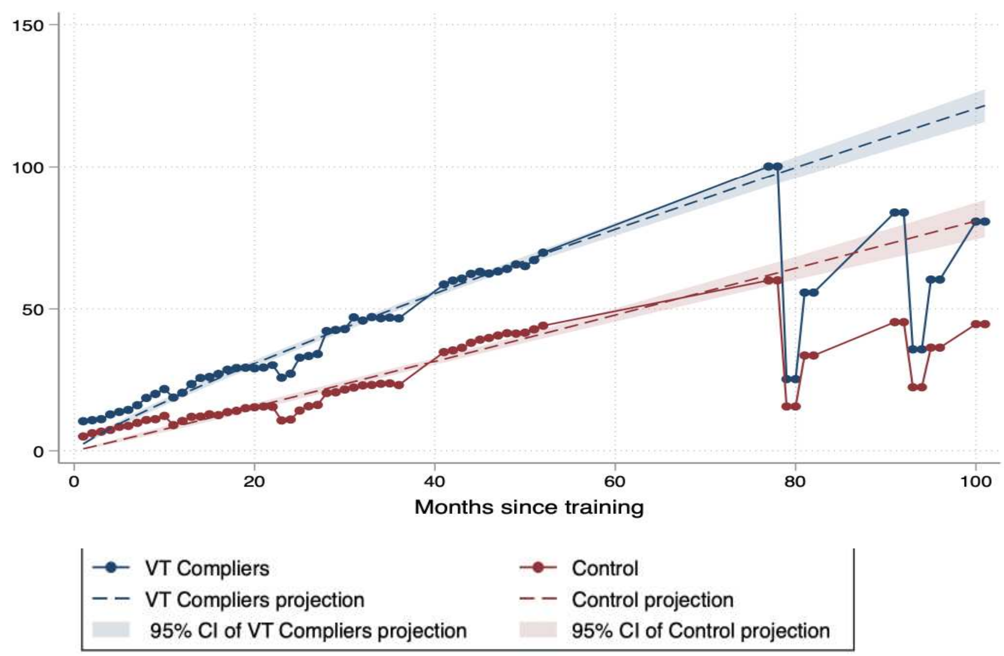
Notes: In the third worker follow-up survey we compiled a sector-specific list of tasks that workers in each sector are expected to be able to perform. We ask respondents whether they can perform each task, for the sector in which they are employed. Each bar in the graph represents a different task. The Figures plot the difference in the share of workers performing each given task while employed, between workers who received vocational training and controls. 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). In each Panel we report a Chi-squared test that the distribution of tasks across trained and untrained workers is the same.

Figure A3: Projected Outcomes in Counterfactual Absent Covid-19

A. Main Employment is in a Study Sector



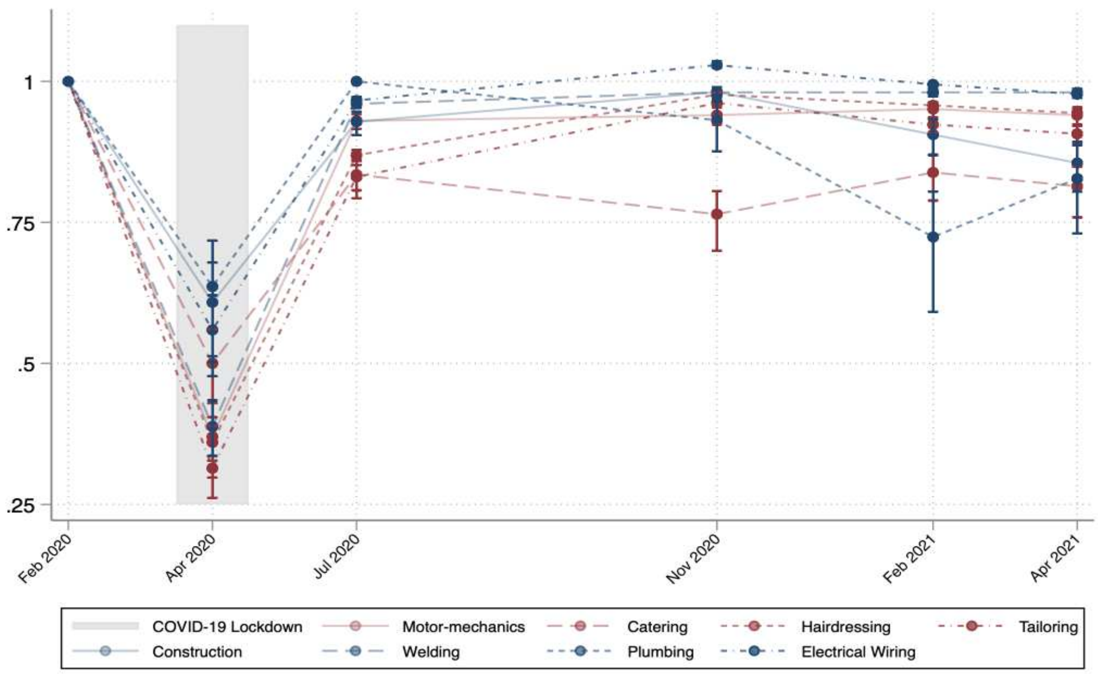
B. Monthly Earnings from Wage/Self Employment



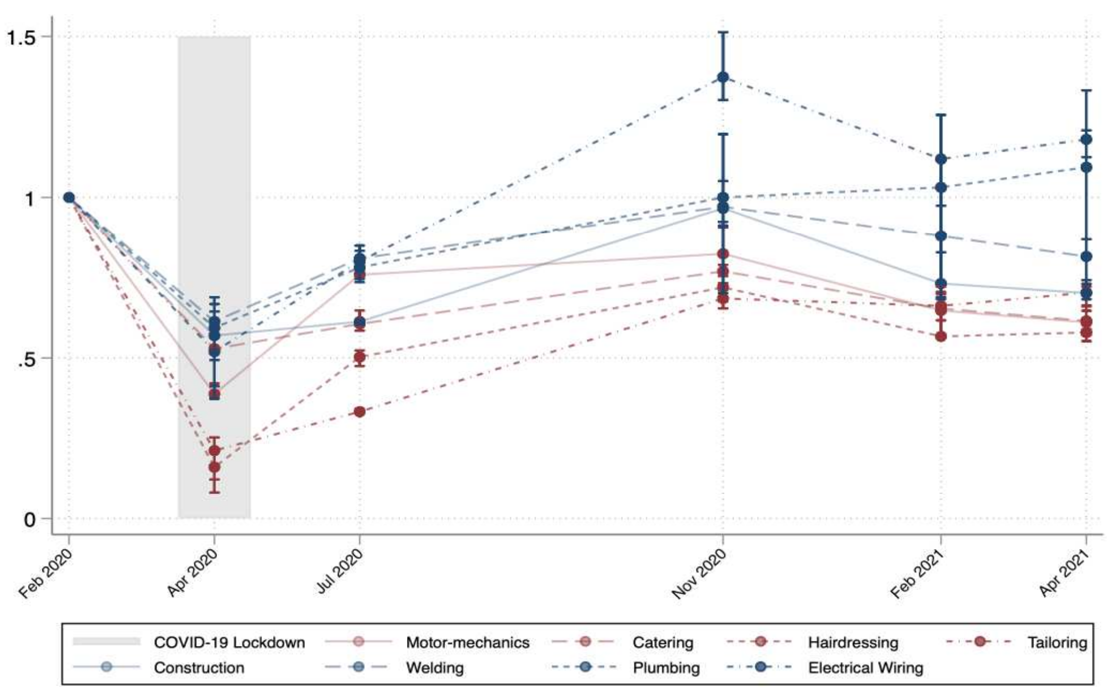
Notes: The projections use data from all worker surveys. Monthly data was collected from waves 1 to 4. From survey wave L1 (2020) onward, respondents were asked to recall information about the last month's activity. For the pandemic survey waves, we interpolate outcomes for missing months. We plot trends and projections for compliers and controls, where controls are reweighted for their probability of compliance, and 95% confidence intervals of the projections are shown. The projections were estimated with a power function using data up until the last pre-pandemic period. All monetary variables are deflated at 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 A4: Firm Dynamics Over the Pandemic, by Sector

Panel A. Firm Operating



Panel B. Number of Employees



Notes: The data is from the fifth and sixth follow-up firm surveys, where the grey shaded region refers to the first lockdown. In each Panel, outcomes are normalized to one in February 2020. The blue shaded sectors refer to sectors with low frequency of customer interactions: plumbing, electricity, construction, and welding. The red shaded sectors represent the sectors with high frequency customer interactions: catering, tailoring, hairdressing, and motor-mechanics. Panel A shows the share of firms operating in each sector, and Panel B shows the number of employees in the average firm in the sector (conditional on the firm being open). 95% confidence intervals are reported.

Figure A5: Sectoral Experiences in Wage/Self-Employment Pre-pandemic

VT COMPLIERS

Share of all months spent in wage/self-employment pre-pandemic (waves 1 to 4)

Sector in which the worker was trained

	MOT	PLU	CAT	TAI	HAI	CON	ELE	WEL	Top Three Other Sectors
MOT	27%	1%	3%	0%	4%	11%	2%	0%	BOD (13%), RET (11%), OWN (5%)
PLU	2%	25%	5%	0%	1%	10%	2%	1%	BOD (16%), RET (12%), CAR (6%)
CAT	0%	0%	43%	4%	7%	0%	0%	0%	RET (15%), EDU (14%), OTS (6%)
TAI	0%	0%	5%	50%	8%	1%	8%	0%	RET (8%), OFF (6%), EDU (6%)
HAI	0%	0%	4%	0%	73%	1%	0%	0%	RET (12%), OTH (2%), EDU(2%)
CON	5%	0%	0%	0%	0%	89%	0%	0%	OTH (6%)
ELE	1%	0%	2%	0%	4%	8%	49%	0%	RET (12%), OTH (5%), OWN (3%)
WEL	0%	0%	6%	6%	0%	0%	0%	43%	BOD (24%), OWN (5%), STR (3%)

CONTROLS

Share of all months spent in wage/self-employment pre-pandemic (waves 1 to 4)

Sector in which the worker desired to be trained in

	MOT	PLU	CAT	TAI	HAI	CON	ELE	WEL	Top Three Other Sectors
MOT	12%	0%	6%	2%	5%	6%	3%	3%	BOD (17%), RET (7%), FAC (5%)
PLU	0%	0%	11%	0%	9%	0%	0%	0%	EDU (34%), RET (20%), OWN (13%)
CAT	0%	0%	5%	1%	7%	7%	5%	0%	RET (26%), OTS (9%), BOD (9%)
TAI	0%	0%	7%	7%	4%	0%	0%	0%	RET (16%), OTH (15%), EDU (14%)
HAI	0%	0%	15%	8%	20%	1%	0%	0%	RET (17%), OWN (13%), CLE (5%)
CON	0%	0%	11%	0%	0%	29%	0%	0%	MAN (17%), OFF (10%), OWN (8%)
ELE	1%	0%	5%	0%	5%	7%	9%	1%	BOD (9%), FAC (9%), RET (9%)
WEL	0%	0%	0%	0%	11%	0%	0%	0%	RET (33%), OWN (23%), STR (13%)

Study Sectors

MOT	MOTOR-MECHANICS
PLU	PLUMBING
CAT	CATERING
TAI	TAILORING
HAI	HAIRDRESSING
CON	CONSTRUCTION
ELE	ELECTRICAL WIRING
WEL	WELDING

Other Sectors

BOD	BODA BODA / TAXI DRIVER
RET	RETAIL SHOP WORKER
FAC	FACTORY WORK
STR	STREET FOOD MAKING AND VENDING
EDU	EDUCATION / TEACHER
MAN	OTHER MANUFACTURING
OFF	OFFICE WORK
OWN	OWNER OF RETAIL SHOP
OTH	OTHER
OTS	OTHER SERVICES
CLE	CLEANER / HOUSEKEEPER

Notes: The data used is from the four pre-pandemic worker survey waves. Each panel shows the share of months workers spend in any given sector in the pre-pandemic period. The top panel shows this for compliers: each row corresponds to the sector the worker was trained in; the columns show the share of months spent in each sector. The lower panel repeats the exercise for controls, where each row corresponds to the sector in which the worker desired to be trained in. At the right of each row in each panel we show the most common other sectors (outside the study sectors) that workers spend the most time wage/self-employed in.