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 labor market shocks to the world economy in recent times. 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, including high frequency surveys over the pandemic. In 2013, workers were randomly assigned to receive six months of sector-specific vocational training, in one of eight high productivity sectors. We document that over the pandemic, employment and earnings margins follow V-shaped dynamics, whereby the outcomes of treated (skilled) workers are more severely impacted by lockdowns, they recover more quickly between lockdowns, and remain resilient to the shock as the economy recovers. Cumulatively over the pandemic, skilled workers spend 61% more time than controls employed in one of our study sectors, and their total earnings are 17% higher. We explore supply- and demand-side mechanisms through which the returns to skills are maintained through the crisis. We document that skilled workers are more exposed to the shock because they are more likely to be laid off during the first lockdown as firms respond to the rapid, severe and uncertain shock by immediately laying off higher earning workers. However, skilled workers recover quickly because of their greater accumulation of sector-specific experience pre-pandemic, and the certifiability of their skills that allows 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 economic times and times of crisis. *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. Globally, at the height of the pandemic, it is estimated to have led to the loss of 144 million jobs 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 even more severe in lower-income settings, even if many of those countries 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 labor supply side, mobility restrictions reduced worker's ability to travel to work. On the labor demand side, it reduced the ability of firms to conduct face-to-face trade, disrupted supply chains between firms, and ultimately caused firms to face huge uncertainty over their survival prospects in the midst of a speedy and severe economic shock.[1]

Much has been documented about how the economic toll of the pandemic has been unevenly distributed because of its varying impacts by income or gender, including in low-income settings [Egger *et al.* 2021, Josephson *et al.* 2021]. We study the issue in Uganda and focus in on a narrower source of heterogeneity – skills – that is of intrinsic interest, a primitive for differences across other dimensions such as occupation or income, and more amenable to direct policy intervention. To do so we exploit a randomized skills training intervention, implemented six years prior to the pandemic, tracking young jobseekers for a decade from 2012 through to 2022, to study how skilled and unskilled workers were differentially impacted by the pandemic, and unpack the mechanisms driving the returns to skills through the crisis.

There are good reasons to expect skilled and unskilled workers to be differentially impacted by the labor market turbulence caused by the pandemic. On the labor supply side, skilled and unskilled workers might differ in their resilience to the shock because during their pre-pandemic careers they accumulate differing amounts of labor market experience, attachment to good firms and sectors, search capital, earnings and savings. Moreover, skilled workers can have more certifiable skills, enabling them to reallocate to new firms more easily after any employment loss; on the other hand, their skills might be less transferable across sectors and so it could be harder for them to take up new opportunities as the economy recovers from the pandemic. On the labor demand side, workers might be differentially exposed to the shock because the sectors they work in differ in their reliance on face-to-face trade, because the firms/jobs they work in are differentially exposed to supply chain disruptions, or because in order 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 utilizing pre-pandemic data to study the returns to skills acquired through vocational training and firm-sponsored training

[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.

[Alfonsi *et al.* 2020], and to study how vocational training impacted job search strategies [Bandiera *et al.* 2023]. In this paper our core analysis is based on the same panel of workers tracked over four waves of data collection from 2012 until 2018. To understand whether the returns to skills survive a large aggregate shock, during the pandemic 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. The pandemic survey waves collected additional information on their experiences of the pandemic.

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 the offer of receiving vocational training at one of five reputable vocational training institutes (VTIs) throughout Uganda. Each VTI could offer standard six-month training 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 vocational training, and 95% of workers completed training 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 in which we offered vocational training. These firms are tracked over a decade from 2012: four survey waves were implemented pre-pandemic, and in order to understand labor demand responses to the shock, we implemented two further survey waves during the pandemic. We use this data to help shed light on demand-side mechanisms through which skilled workers are differentially exposed and resilient to the pandemic shock.

Third, our study setting, Uganda, shares many of the hallmarks of lower-income economies throughout Sub Saharan Africa: a young population, a scarcity of good jobs, vocational training being a key route through which young people acquire skills post labor market entry, and an absence of social insurance schemes meaning that ensuring resilience to the transmission of aggregate shocks through labor market outcomes is key for lifetime welfare. In common with other settings, Uganda also experienced lock-downs during the pandemic. The first occurred early on, in April/May 2020, and the second in June/July 2021 around the time of 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.

Our first set of results briefly re-examine pre-pandemic differences in skills and labor market outcomes between treated and control workers. We do so in order to establish differences in skills, labor market attachment and other outcomes between skilled (treated) and unskilled (control) workers on the eve of the pandemic. We document that 66% of controls report having some sector-specific skills, and this rises to nearly 100% for those that took up vocational training. Furthermore, using a sector-secific 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 that these skills differences translate into differential task assignments within firms pre-pandemic. On labor market outcomes, in our final pre-pandemic survey measured 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 (a 72% increase over controls); (ii) have 25% higher total monthly earnings than controls. 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 in unemployment; (ii) accumulate 117% more experience working in one of the eight study sectors; (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, and this is uncorrelated to treatment. While attrition rises to 31% in the pandemic waves, nearly all of this occurs between waves 4 and 5. We then observe almost zero additional attrition over the three survey waves fielded during the pandemic. On most margins and survey waves we find little evidence of heterogeneous attrition by treatment and control, either in the pre-pandemic or pandemic survey waves.

Examining the dynamics of labor market outcomes for skilled and unskilled workers, we find: (i) employment and earnings margins dip severely around each lockdown; (ii) there is a V-shaped recovery in employment and earnings for skilled and unskilled workers; (iii) however, skilled workers are more severely impacted by each lockdown; (iv) yet, skilled workers also recover more quickly between lockdowns, and as the economy recovers from the pandemic overall.

More precisely, on the extensive margin of whether workers are employed in one of the study sectors – as a marker of continuing to be employed in a more productive sector – we see pronounced differences between skilled and unskilled workers through the pandemic. On the eve of the pandemic, skilled workers were 22pp more likely to be employed in a study sector ($p = .000$). They maintain this advantage over controls throughout the pandemic except when the first and second lockdown are in place. After each lockdown, treated workers recover more speedily to regain employment in the study sectors. In February 2022 when Uganda is coming out of the pandemic, skilled workers are 17pp more likely to be employed in a study sector ($p = .000$).

The differential employment dynamics between treated and control workers are driven by wage/self-employment. In contrast, in all periods of the pandemic, control workers remain more likely to be engaged in insecure and low wage casual work than skilled workers. During the pandemic skilled workers do not switch into casual work, so we find no evidence of skilled workers downgrading their skills (or reducing their reservation wage) in response to an aggregate downturn, as has been documented in high-income contexts [Huckfeldt 2022].

To quantify the returns to skills over the pandemic, we calculate the differential cumulative labor market impacts over the pandemic between skilled and unskilled workers, so essentially integrating over the dynamic treatment effects. We find that skilled workers spend 61% more time than unskilled workers employed in one of our study sectors, their total earnings are 17% higher, and this is driven by earnings from wage/self-employment, that are 28% higher. We show the robustness of these core results to addressing selective attrition on non-observables.

The bottom line is that the returns to skills acquired through vocational training survive the pandemic, gaps in labor market outcomes between skilled and unskilled workers are maintained over the crisis, and cumulative outcomes between them continue to diverge. By building on a pre-pandemic field experiment with data collection through the pandemic, we show that the returns to skills go beyond measured contemporaneous labor market earnings outcomes (as would be included in any IRR calculation) but also include building resilience and insurance among workers to aggregate shocks even as severe, rapid and uncertain as the Covid-19 pandemic.

Our third set of results examine the mechanisms through which the returns to skills are maintained during the pandemic. We distinguish between: (i) those relating to the differential characteristics or behaviors of skilled and unskilled workers, or supply-side mechanisms; (ii) those relating to 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 more labor market attachment than unskilled controls, resulting in them having greater experience working in good sectors and in good firms, had also accumulated different search capital and higher savings. All these channels might cause skilled and unskilled workers to differ in their resilience to the shock. To examine these mechanisms, we follow Hainmueller [2012] and reweight controls to create balanced samples where control group data is reweighted to match pre-pandemic covariate moments among compliers.

We find 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. Nearly all the impact on the cumulative experience in these study sectors over the pandemic is explained by this margin of labor market attachment. This suggests the accumulation of sector-specific skills is important for individual resilience to job loss due to the pandemic shock [Topel 1991, Neal 1995, Kletzer 1998]. We find more limited evidence that pre-pandemic experience of good jobs *per se*, or of good worker-firm matches explain the returns to skills over the pandemic on the extensive margin of retaining employment in good sectors. However, both forms of labor market attachment explain slightly more of the returns to skills over the pandemic in terms of total earnings, or earnings specifically from wage/self-employment.

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 evidence that search behaviors of skilled or unskilled workers robustly differ during the crisis.

The other key supply-side mechanism we consider is that skills obtained through vocational training are certified – workers receive certificates of course completion from VTIs. These credibly certify the skills workers have, enabling them to be more mobile in the labor market, again potentially aiding their resilience to the shock if the sectors they are skilled in are able to recover from the shock. We find evidence along these lines: workers with vocational training certificates are significantly more likely to make job transitions in wage employment across firms in the same sector over the first lockdown. Hence the V-shaped recovery on employment for skilled workers is not driven by them being re-employed at the same firm post-lockdown, but rather because of their ability to take up new employment opportunities in the same sector but at a different firm – one that is able to recruit skilled workers later in the pandemic.

On demand-side mechanisms, we first examine whether the initial sector of employment matters for 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 of the pandemic. 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 in on differences solely in the quality of firms skilled and unskilled 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, to establish why skilled workers were more exposed to the shock, we further exploit the firm-side data to understand whether and how firms differentially retain, layoff and recruit skilled and unskilled workers in response to the first lockdown. We find that over the first lockdown firms are far more likely to respond to the shock by immediately laying off the highest earning workers: 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 unprecedented uncertainty over the scale of the pandemic and the need to urgently reduce wage bills in the face of falling profits. Hence skilled workers remain resilient despite being more likely to be immediately fired at the outset of the crisis. Our firm-side data confirms that later in the pandemic, surviving firms attempt to recruit workers with experience in their sector – that skilled workers can take advantage of to a greater extent given their certifiable skills, and their greater accumulation of sector-specific experience.

Our work contributes to three classes of literature. The first is the large body of work examining labor market impacts of the pandemic, where much has been documented about how the economic toll of the pandemic 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 more 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].

We build on these studies by exploiting experimental evidence to document the causal impact of skills on labor market dynamics over the pandemic, and 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 in December 2018, through to August 2021. In contrast to our findings, they report 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 is to the body of work examining long run returns to training [Ibarrarán *et al.* 2019, Aizer *et al.* 2021, Kugler *et al.* 2022, Silliman and Virtanen 2022]. Our results speak 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 during a crisis, we are able to 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 – are key to generating 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 central to driving the returns to skill, 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 to the crisis. Other mechanisms such as savings and search behavior might play less of a role during the crisis because of the speed and severity of the pandemic shock.

Finally, we draw inspiration from an established literature on labor market dynamics of displaced workers [Jacobsen *et al.* 1993, Farber 1997, Kletzer 1998]. Some of this 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. Most relevant for our work, this literature has also considered heterogeneous impacts of job loss by worker skills [Seim 2019], job content [Blien *et al.* 2021, 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]

[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].

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, that are detailed enough for us to understand multiple mechanisms relevant for the differential labor market dynamics for skilled and unskilled workers during the pandemic. When presenting our results we discuss the relationship to these earlier sets of work, while keeping in mind throughout that the pandemic shock obviously has characteristics that make it distinct from more slow moving aggregate downturns, or more localized episodes of mass layoffs.

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 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]

[3] The eligibility criteria were being aged 18-25, having completed between 7 and 11 years of education, not being in full-time schooling, 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 score on each dimension and a total score 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.

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 over 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; (ii) having between 1 and 15 employees (plus a firm 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, and they constitute a source of stable employment for young workers in Uganda: 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 took place from June to September 2012 when workers applied to the offer of vocational training. Training took place between January and July 2013 at the VTIs we partnered with. Workers were subsequently tracked over four surveys pre-pandemic, fielded 24, 36, 48 and 68 months after baseline (12, 24, 36 and 55 months after treated workers graduated from their vocational training courses). 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, as well as search behaviors and expectations. The pandemic survey waves also include 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 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 tracking 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]

[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,

2.3 Design

Our field experiment follows an oversubscription design where we randomly assigned eligible applicants to the offer of receiving 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 vocational 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. Given their pre-intervention earnings, our sample of individuals could not typically self-finance this kind of vocational training. Courses were full-time, and worker attendance was monitored. Upon graduation, vocational trainees receive a certificate verifying their acquired 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. The second group of trained workers were upon graduation offered light touch offers to match for job interviews with firms in our firm-side sample. The impact of such 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 took place, we pool both interventions and show robustness of key results in each of these original treatment arms.

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: attrition 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 attrition is low pre-pandemic: 12% of workers attrit by the 68-month fourth follow-up, and this is uncorrelated to treatment. The remaining Columns show that: (i) attrition

and in April 2020 a food distribution scheme to aid the 1.5mn 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 been a beneficiary of it.

rises to 31% in the pandemic waves; (ii) nearly all of this occurs between waves 4 and wave $L1$ – 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 plausibly 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, and whether they reside in Kampala.

On most margins and survey waves we find little evidence of heterogeneous attrition between treatment and control, either in the pre-pandemic or pandemic survey waves. However, those with any sector-specific skills and those resident in Kampala at baseline are significantly less likely to be tracked until survey wave 4. Table A3 re-examines balance on 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 examine 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. To ensure high course completion rates, for each worker, VTIs were paid half the training fee at the start of training, and half at the end, conditional on them having trained the worker. This staggered timing of payments ensured 95% of workers completed training courses conditional on enrolment. Table A4 shows correlates of take-up. Individuals with lower than median cognitive ability score, lower than median locus of control score, 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 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]

[6] This pre-pandemic attrition rate compares favorably to other studies conducted in good economic times such as Attanasio *et al.* [2011] (18%), and Card *et al.* [2011] (38%). Indeed, in the meta-analysis of McKenzie [2017], all but one study have attrition rates above 18%. 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, that report attrition rates of 7 and 15% [Alfonsi *et al.* 2023, Chakravorty *et al.* 2023].

[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%.

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 existing 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 individual skills using a sector-specific skills test we developed in conjunction with skills assessors of written and practical occupational tests in Uganda. Each test comprises seven questions (combining 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 conducted on all workers (including controls) at third follow-up, so measuring persistent skills accumulation. There is no differential attrition by treatment into the test. Table 4 reports the results. In Panel A we report the ITT estimates $\widehat{\beta}$ from (1), and in Panel B we report the corresponding ATT estimates.[9]

Before administering the test, we asked a filtering question to workers on 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

[8] All regressions control for the training implementation round and dummies for 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 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).

100% for those offered vocational training, as measured three years post-intervention. All workers that 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 vocational training significantly increase their measurable sector-specific skills by 19% (or .28σ of test scores). Columns 1 and 2 in Panel B of Table 4 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 .41σ of test scores).[10]

To better establish how the distribution of skills among the treated group is impacted, 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 intense vocational training. We do not claim controls are entirely without skills valued in labor markets, especially because by the eve of the pandemic, they have accumulated six years of potential labor market experience.

Tasks One way to validate that these acquired skills are relevant for our study sectors is to consider tasks workers conduct at work, conditional on them being employed. We also measure such tasks at work in the third follow-up survey. To do so for each of the eight study sectors, we construct a list of 30 to 40 tasks performed by workers (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 away 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 right hand side of each panel), and other tasks 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 is in line with vocationally trained and control workers having different skills, but both are relevant for the workplace. This also highlights that skilled and unskilled workers might have

[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 refers to all main job spells reported at third follow-up (so there is one job spell per worker and only employed individuals are included in the sample), where workers were asked to report which tasks they performed in each employment spell they had in the year prior to the survey.

different amounts of occupation specific human capital, that can impact labor market dynamics after job loss [Huckfeldt 2022, Braxton and Taska 2023]. This is an issue we come back to when studying mechanisms through which the returns to skills are maintained 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). To do so, we exploit the fact that in the pre-pandemic survey waves we asked workers to recall their labor market outcomes over 12 months, so effectively constructing 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 experience of working 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 come back 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, during and just after the first lockdown. In wave $L2$ these periods span just prior to, during, and just after the second lockdown. Given our focus on returns to skills over the pandemic, we

[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.

estimate the following specification by 2SLS in time-frame t from survey waves $L1$, $L2$ and R:[14]

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

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, where we also report the p-value on a test of the null for not rejecting that 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: $\beta_1 = \beta_7$.

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 time periods covered and the grey shaded regions refer to each lockdown.

Panel A examines whether individuals are employed in any form of work. 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%, and 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, more exposed to and hit harder by the shock going into the first lockdown.[15]

However, 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' on employment exactly matches the timing of lockdowns, with the severity of employment impacts during the first lockdown being greater than for the second, in line with the first lockdown being

[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].

[15] Employment levels in our data closely match those reported for urban vocational trainees in Uganda in Alfonsi *et al.* [2023]. For example they report that pre-pandemic employment rates were around 77%, and they fell by around 40pp in the first lockdown.

more stringently enforced. Comparing levels of outcomes around each lockdown, we observe a V-shaped recovery in employment outcomes for both skilled and unskilled workers, with the depth of the V-shaped employment shock being greater for skilled workers.

Panel B narrows in 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 more pronounced differences between skilled and unskilled workers through the pandemic. More precisely, as the corresponding regression specification in 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 when lockdowns are in place. After each lockdown ends, skilled workers recover more speedily to regain employment in the study sectors. In February 2022 when Uganda was coming out of the pandemic, skilled workers were 17pp more likely to be employed in a study sector ($p = .000$).

Panels C and D examine employment types. Panel C confirms 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.[16] Moreover, Panel D shows that there is no rapid influx of complier or control workers into casual wage employment in either lockdown. Neither group of prime age workers appear to move down the job ladder. Moreover, casual/informal work does not act as more of a buffer for skilled than unskilled workers. We see that in all time periods of the pandemic, control workers remain more likely to be engaged in casual work than skilled workers. Hence in response to the turmoil caused by the pandemic, skilled workers do not downgrade and switch into casual work, as has been documented for US workers in response to job loss [Huckfeldt 2022], and has been documented in response to trade shocks in middle-income contexts [Dix-Carneiro *et al.* 2024]. We return to this issue of skills downgrading when we later examine search behavior of workers.[17]

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

[16] 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 is never statistically different in any time frame of the pandemic, including during the first or second lockdown.

[17] As detailed in the Appendix, when examining differential impacts of skills by gender, shifts into casual employment are the margin on which the most striking gender differentials exist. Among men, we find 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 Chakravorty *et al.* [2023] in the context of rural India.

dip dynamics as for employment outcomes, whereby skilled workers are more severely impacted by lockdowns, they recover more quickly between lockdowns, and as the economy recovers overall.

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 is near zero, so that in proportionate terms skilled workers have larger earnings losses during lockdowns relative to the preceding time frame. In line with the earlier results, the first lockdown has more severe impacts on earnings than does the second.

Panel B focuses on earnings from wage/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.

Panel C conditions earnings on being in wage/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. Finally, Panel D reconfirms that skilled workers do not shift into casual work, hence the earnings from casual work remain higher for control workers throughout.

Validation Using Worker Expectations To validate these findings on employment and earnings outcomes, in the Appendix we present additional results examining whether the patterns align with worker expectations on job offer arrival rates and earnings conditional on 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, which further helps explain why they do not downgrade and switch towards casual employment.

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 is significantly different 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.[18]

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.

4.5 Implications

We highlight three implications of our findings. First, the employment and earnings dynamics for these prime age workers in Uganda are similar to those found for workers in the US, whereby there were large employment losses in the trough of the pandemic recession, and skilled workers experienced a relatively speedy recovery [Chetty *et al.* 2023].

Second, on all employment and earnings margins, we cannot reject that the ATT effects are the same in the first and last time-frames of the pandemic. Hence the magnitudes of treatment effects of skills on labor market outcomes remain the same at the end of the pandemic as at its start.[19] However, as shown in Section 3, the pre-pandemic trends in labor market outcomes were upward – not flat as over the pandemic period. In the Appendix we present additional results to estimate the impacts of the shock on skilled and unskilled workers relative to a counterfactual absent the pandemic. We do so using the pre-pandemic survey waves to project labor market outcomes in a counterfactual without the pandemic, and then contrast the level of their projected outcome to their actual outcome in our final survey wave, February 2022. This exercise reveals: (i) skilled (unskilled) workers' likelihood to be employed in one of the study sectors is 37% (49%) below trend; (ii) skilled (unskilled) workers have earnings from wage/self employment that are

[18] 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.

[19] The p-values on these tests on the null that $\beta_1 = \beta_7$ are shown at the foot of each Column in Table A6. In these two time frames at either end of the pandemic, the ATT effects on individuals main employment being in a study sector are 22.0 vs. 16.6pp ($p = .402$), on their main activity being in wage/self employment they are 8.8 vs. 8.9pp ($p = .980$), and on their monthly earnings from wage/self employment they are 19.9 vs. 15.8pp ($p = .781$).

34% (45%) below trend.[20]

Third, we can contrast our results 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 they report ITT estimates and 60% of treated workers in their study 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 struck. 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.

4.6 Extensions and Robustness

We present two further sets of result 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 that selective attrition may confound our results using multiple approaches following Blattman *et al.* [2020].

5 Mechanisms: Supply Side

To understand why the returns to skills are maintained through the pandemic we consider two types of mechanism: (i) those relating to differential characteristics or behaviors of skilled and unskilled workers, or supply-side mechanisms; (ii) those relating to firm behaviors having differential impacts on skilled and unskilled workers, or demand-side mechanisms. Supply-side mechanisms help explain the resilience to the shock that skilled workers display. Demand-side mechanisms help

[20] These magnitudes are slightly higher than the broader literature on the dynamic labor market outcomes for displaced workers that typically suggest – using administrative data from high-income settings – that workers experience long run earnings losses between 15% and 30% [Jacobsen *et al.* 1993, Couch and Plaszek 2010, Davis and von Wachter 2011].

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.

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 have been trained, so accumulate sector-specific skills. They also have greater experience of good jobs in wage/self-employment – irrespective of their sector of training – so build knowledge of the wider labor market. 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 in Hainmueller [2012] to create balanced samples where the control group data is reweighted to match pre-pandemic covariate moments among compliers. To account for other background sources of worker heterogeneity that potentially correlate with the reweighting covariate, when reweighting for continuous covariates we first regress the covariate on worker characteristics (that are either measured at baseline or are time invariant). 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 6.[21]

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, in Column 2 we see that the impact on the cumulative experience over the pandemic in these study sectors can then be explained by this margin of labor market attachment: the ATT estimate is not statistically different from zero and the reweighted estimate of the cumulative impact reduces this differential entirely. This builds on the earlier finding that conditional on working in the same sector, the composition of tasks that skilled and unskilled workers conduct within firms fundamentally differ (Figure A2) and suggests the accumulation of sector-specific skills is important for individual resilience to job loss due to the pandemic shock [Topel 1991, Neal 1995, Kletzer 1998]. This is so for retaining attachment to good sectors. For earnings, after

[21] The individual baseline characteristics controlled for 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.

accounting for sector-specific experiences, in Column 5 we see that the cumulative impact of skills on total earnings are only slightly affected, reducing from 17% to 16%. 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%.[22]

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 spend 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 spend 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.

Experience of Good Jobs To separate out experience in good sectors from experience in good jobs *per se*, Panel C of Table 6 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, from Column 2 we see 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 on this dimension of the returns to skills. On the margin of total earnings, we see from Column 5 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]. If for example skilled workers are on average in higher quality matches that pay correspondingly well, then earnings can be 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) that workers have 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 spend 61% more time over the pandemic in good sectors, accounting for this form of

[22] Our finding helps validate the claim in Barrera-Osorio *et al.* [2022] that the returns to skills among job seekers randomly assigned into a job training program focused on service sectors 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.

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 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 a greater buffer of 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 re-weight 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 6 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 help explain the non-shift into casual work.

Search Behavior Our earlier work showed that in good economic times pre-pandemic, the search behavior of skilled and unskilled workers differs [Bandiera *et al.* 2022]. Specifically, 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 over the pandemic [Hensvik *et al.* 2021].[23]

In the pandemic surveys we asked individuals about search effort and whether they were directing their search to 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.

[23] Hensvik *et al.* [2021] examine job search behavior in Sweden via a large online job board. They find workers respond to a 40% drop in posted vacancies by searching more intensively. Examining directed search behaviors, they find jobseekers search more towards resilient occupations, or towards those that can be performed from home.

[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).

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 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.

5.2 Skills Certification

In our earlier work examining returns to skills acquired through vocational training, 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. This mechanism might remain relevant during the pandemic, helping skilled workers to remain resilient to the severe downturn in their employment during the first lockdown. However, the pandemic might have very different impacts across sectors, and skills acquired through intense sector-specific vocational training may leave workers with less transferable human capital across sectors, slowing down their ability to take up new opportunities as the economy recovers from the pandemic.[25]

On transitions from unemployment back into employment, the earlier results in Figure 2 already demonstrate that although skilled workers are more impacted on employment margins in both lockdowns, they also bounce back into employment more quickly after each lockdown. To examine the separate issue of job transitions, we use the time frames either side of each lockdown and restrict the sample to individuals engaged in wage employment before *and* after each lockdown (so in time frames 1 and 3, or in time frames 4 and 6) and examine whether they report working: (i) at the same firm pre- and post-lockdown; (ii) in a different firm but in the same sector pre- and post-lockdown; (iii) in a different sector (and hence a different firm) pre- and post-lockdown.[26]

Column 1 of Table 7 shows that among controls that 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

[25] Along similar lines but in the context of far more slow moving structural change in the economy, Braxton and Taska [2023] show that technological change can account for a large share of earnings decline following job loss – because of the lack of suitability of worker skills to changing employment opportunities.

[26] In these specifications, we control for strata, survey month, the implementation round workers were assigned to, their desired sector of application at baseline, and their baseline demographics (age, married, whether they have any children, are employed, and have a high cognitive test score). 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.

same firm, despite their greater accumulation of sector-specific human capital. 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 actually very similar to those documented among US workers [Bick and Blandin 2023].[27]

It is useful to contrast these dynamics with those around the second lockdown: on most dimensions these suggest more persistence in wage employment with the same firm for skilled and unskilled workers. For example, among controls that were wage employed before and after the second lockdown, 93% remain employed in the same firm, and this does not differ to skilled workers. As a result, around 5% of workers transition to another firm in the same sector or to another sector. All this is in line with the second lockdown being far less disruptive than the first.

Finally, in Column 4 we consider transitions from wage to self-employment pre- and post-lockdowns. In line with the earlier evidence, we find no evidence that skilled workers make such transitions at a differential rate than controls, among whom 7 to 8% do so over each lockdown.

This evidence complements our earlier work highlighting that in good economic times, certification is a key channel driving the returns to skills [Alfonsi *et al.* 2020]. These results emphasize that the certification mechanism continues to drive some of the returns to skills in bad times.

5.3 Experiences of the Pandemic

In the Appendix, we present additional results exploring the possibility that treated and control workers might experience the pandemic differently and this can help explain some of their differential outcomes. For example, this might stem from them residing in different regions, or even within region residing in locations where lockdowns are differentially enforced.

6 Mechanisms: Demand Side

We now examine the extent to which demand-side mechanisms help explain the returns to skills over 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 on the eve of the pandemic.

[27] 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, at least double the rate of any previous episode in the preceding quarter of a century.

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

To shed light on demand-side mechanisms we build on data from firms collected as part of the original two-sided field experiment. We tracked a sample of small and medium sized firms operating in the eight sectors in which we offered vocational training. A representative sample of such firms from these sectors were first surveyed between October 2012 and June 2013, and then 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 (in comparison per capita annual income in Uganda in 2012 was around $800). 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 – so already selected on survival [McKenzie and Paffhausen 2019]. Panel C shows that half of firm owners are women – because the service sectors covered include hairdressing and catering that are female dominated. The average age of owners is in the mid 30s.

Panel D focuses on other firm characteristics relevant for their exposure to the pandemic shock (as measured at first follow-up). In terms of face-to-face trade, Column 1 shows firms report having around 17 customers per week, but there is enormous variation in this over firms and within a firm over time – the maximum number of customers reported in a good week is near double that of the average number. As part of our pre-pandemic surveys, we asked firms about their 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. Firms do not operate in isolation: firm owners reported having around one social or business tie, and more than half of these links between firms are ones that involve supply chains, because firm owners report buying/selling from/to the link. These firms could be more exposed to supply chain 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 further attrition of firms between the two pandemic survey waves. 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 similar characteristics related to employment, profit and revenues as all firms in our original sample.[28]

Column 4 then shows the characteristics of non-attriting firms as measured in the first time frame of recall in our first pandemic survey. On the eve of the pandemic in February/March 2020, non-attriting firms had grown to have double the number of employees since baseline ($p = .000$), their profits and revenues had significantly increased ($p = .015$, $p = .000$), and their customers per week had also near doubled since baseline ($p = .000$). However their revenues per worker had not risen in real terms ($p = .431$). Importantly, their wage bill as a share of revenue had risen from 68% pre-pandemic to nearer 95% on its eve.

Representativeness Firms in wave 5 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 between May and July 2017, 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 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, Column 7 shows the percentile at which surviving firms in our panel lie in the distribution of census firms. As expected, the firms we track 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 lie in the 92nd percentile of profits, and above the 90th percentiles in terms of revenues and revenues per worker. On the one hand, 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.[29]

[28]Columns 1 to 3 of Table A12 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.

[29]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.

6.2 Sectors and Firm Quality

A number of earlier results already hint at the importance of sectoral allocations in determining the resilience of workers to the pandemic: supply-side mechanisms explaining the resilience of skilled workers include their greater accumulation of sector-specific human capital, and the certification of their skills that allows them to more easily transition across firms in the same sector around the first lockdown. Over and above this, sectors can differ in their reliance on face-to-face trade, or their vulnerability to supply chain disruptions, and this will affect the dynamics of employment opportunities across sectors [Bloom *et al.* 2022]. We now examine in more detail why initial sectoral assignments might matter for resilience to the shock.

Figure A4 shows dynamics of firm openings and employment levels over the pandemic by sector, where we distinguish between sectors with high levels of customer interaction (motor mechanics, catering, hairdressing and tailoring), and low levels of customer interaction (construction, welding, plumbing and electrical wiring). Firms in sectors with higher levels of interaction are more severely impacted by the lockdown on both margins. Among surviving firms in those sectors, employment levels remain between 50 to 65% of their pre-pandemic level, while firms in sectors with relatively lower levels of customer interaction recover to nearly the same, or greater, employment levels. In terms of employment, 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 and workers 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 2017. In Panel A we consider firm characteristics relevant for their exposure to the pandemic shock. Comparing characteristics related to customers and supply chains we see that the most affected sector has firms that have a greater number of customers ($p = .000$), but have the same number of supply chain links to other firms ($p = .817$). This helps explain why the tailoring sector – where the act of supply often involves customer interaction – is more impacted than the welding sector. Panel B considers measures of firm performance and here we see that beyond being more exposed to customer interactions, firms in the most affected sector are significantly smaller, with lower revenues and less profitable 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 whether each of their employees is skilled or not. We see that less affected sectors have a greater number of skilled workers, and a greater share of all employees that are considered skilled.

Taking this evidence as a whole 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 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 first examine the overall role of initial sectoral assignments in explaining the cumulative impacts on labor market outcomes over the pandemic, we reweight the sectoral composition of controls on the eve of the pandemic to match that of compliers (combining all 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. The result in 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 (Column 2). 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 (Columns 5 and 6).

We next examine the specific role of firm quality, conditional on sector of application. 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 about such a rich set of characteristics of their employers. We are able to 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 are reduced after reweighting for this basic measure of firm quality: the cumulative impact on being employed in the study sectors falls from 61% to 38%, and cumulative impacts on total earnings almost halves from 17% to 10%.

In short, initial sectoral allocations are more important for explaining the returns to skills on the extensive margin of retaining attachment to study sectors over the pandemic, while initial firm quality is more important for explaining returns to skills on the margin of total earnings.[30]

6.3 Labor Demand

We now consider the dynamics of labor demand for skilled and unskilled workers, that is key to understanding the greater exposure of skilled workers to the pandemic shock, and the V-shaped recovery around the first lockdown for workers on employment and earnings margins. To establish the dynamics of firm outcomes, in Figure 4 we use our firm-side 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, with the first lockdown shown in shaded gray. 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

[30] Taken together, our results 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.

recovery – in July 2020 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 directly to the speed and severity of the pandemic shock and the uncertainty induced, as well as the persistent impacts of the shock on job creation and growth.

Panel B of Figure 4 examines how this translates into labor demand in the study sectors. Employment levels are at 55% of their pre-pandemic level during the first lockdown. This represents an enormous shock in the space of just two months. Recovery is slower than on the operating margin, with labor demand rising to around 80% 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:

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

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).[31]

Column 1 shows the likelihood of a firm operating in the first lockdown falls 53% relative to February 2020, when 87% of firms were operating, with this recovering between the first and second lockdown. Column 2 shows that for surviving firms, there is a sharp reduction in labor demand during the first lockdown and a slow recovery thereafter. Labor demand falls by 53% in the first lockdown relative to February 2020, remains 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 during the first lockdown. Both recover steadily as firms exit the first lockdown. By April 2021, firm revenues and profits are both significantly recover in levels from the depth of the first lockdown in April 2020 (p = .000, .011 respectively).

6.4 Matching the Dynamics of Labor Demand and Labor Supply

So how did firms survive through from the depth of the first lockdown? A key margin of response is in terms of labor demand – not just in terms of the number of employees but also their skills composition. To see this, we examine how the demand-side employment dynamics match the supply-side employment dynamics documented earlier. More precisely, Panel C of Figure 4 overlays the changes in labor demand from Panel B with changes in the 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. We see that employment rates of both complier and

[31] 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.

control workers fall further in the first lockdown than among firms in our study sectors, but the V-shaped recovery is similar in both the worker and firm data. This is in line with the earlier finding that over the course of the pandemic, certification allows skilled workers to reallocate across firms in the same sector.

We can go further and compare demand- and supply-side earnings dynamics as well. Panel D of Figure 4 shows the evolution of earnings conditional on employment, overlaying information from: (i) our firm-side data, where we can see the evolution of earnings of workers that *remain employed* in firms in the study sectors; (ii) the evolution of earnings for skilled and unskilled workers from our worker-side data, as shown earlier. To again aid the comparison, each series is normalized to one in the first time frame. Now we see a sharp divergence in earnings between treated workers and workers that remain employed in our study sector firms. The divergence is pronounced: earnings conditional on employment for treated workers follow a V-shaped recovery, as emphasized earlier. In contrast, earnings for workers that remain employed in our study sector firms follow a L-shaped pattern, remaining at 70% of the average level of all employees in February 2020 with no trend towards recovery between lockdowns.

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 ($p = .325$).

6.4.1 Retention

Matching the earnings dynamics from the firm- and worker-side data points to a changed composition of workers employed by firms that survived the pandemic. To examine this directly, we exploit the fact that in our pandemic firm surveys we asked firms to describe hires and layoffs over two periods: (i) between March 2020 and November 2020, so spanning either side of the first lockdown; (ii) December 2020 to June 2021, so between the first and second lockdown.

The results are in Table 12. Panel A focuses on the retention and recruitment of workers. 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 row shows firm's attempts to recruit workers were more muted over the first lockdown than between lockdowns.

Panel B examines characteristics of laid off workers. For firms that laid off a worker, the majority laid off highly experienced workers – that correlates with the most skilled workers. Hence we do not see those with tenure or skills being protected from job loss, but rather the opposite. This is because more experienced and 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 earlier using our worker-side data on the pre-pandemic period where we documented the skill premium in earnings of 25% (Table 4). This is also confirmed using data on the roster of workers in the firms we track. For example this shows that pre-pandemic, base earnings increase significantly with tenure with the firm, and in whether the worker is reported as being skilled by the firm owner.

In short, in the face of a severe, rapid and highly uncertain aggregate shock, firms laid off the highest earning workers first during the first lockdown. This is consistent with skilled workers being more exposed to the pandemic because firms needed to reduce wage bills in the face of a rapidly evolving shock, as firms had moved from a situation of high profitability to one of just breaking even. In sharp contrast, Panel B of Table 12 shows that very few unskilled workers were laid off over the pandemic. Linking back to Panel C of Figure 4, the data on worker retention thus helps explain: (i) why skilled workers were hit harder than typical employees during the lockdown; (ii) why our group of unskilled workers were hit harder than typical employees given they have greater potential labor market experience. These results also help explain the L-shaped pattern of earnings conditional on retained employment within the firm (Panel D of Figure 4).[32]

6.4.2 Recruitment

The other side of firms' strategies relate to labor hires over the pandemic. Panel C of Table 12 examines 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 on supply-side mechanisms: (i) that skills certification is a key channel through which the returns to skills are maintained, enabling workers to 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. This all matches the dynamic patterns of worker recovery in Panel C of Figure 4.

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 D: 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 – those with skills and experience – over the first lockdown. These results match the dynamic patterns shown in Panel D of Figure 4. The recovery in earnings is much faster for compliers than for typical workers employed in these firms because the composition of workers in firms tilts towards unskilled workers.

[32] 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 we 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 median age 25, 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.

6.4.3 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], again consistent with the kind of job mobility of skilled workers across firms in the same sector documented earlier.

To quantify how successful this FIFO strategy was for firms, we return to the firm-side 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 first 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 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 – so back to the ratio at baseline.

It is natural to expect retention and recruitment responses to differ across firms depending on their anticipated exposure to the shock, even more so given the actual observed heterogeneity in operating likelihood and employment across sectors shown earlier. However, exploring various forms of *ex ante* heterogeneity across firms on the eve of the pandemic generally leads to the same conclusion that firms responded rather homogeneously: by first firing higher earning skilled workers. All firms – irrespective of sector or pre-pandemic profitability – adopt this FIFO strategy in order to rapidly reduce wage bills. An explanation might be the unprecedented speed and severity of the shock. This makes it difficult for any given firm owner to confidently predict their own survival probability, as firms face huge uncertainty.

On the first point, we show it is hard to predict firm survival through the pandemic even based on a rich set of baseline covariates.[33] On the second point, we asked firm owners their expectations over the pandemic, and can examine how these expectations evolve over two periods: (i) between March 2020 and November 2020, so spanning either side of the first lockdown; (ii) December 2020 to June 2021, so between the first and second lockdown. 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, and this only fell to 29% during 2021.

[33] This result is shown in Column 4 of Table A12: we see 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. Hence we (as the econometrician) cannot reliably predict the *ex ante* survival probability of any given firm.

Finally, we note that beyond laying off the highest earning workers in a FIFO strategy, firms have other potential margins of response to the pandemic. In the Appendix we present additional results exploring this possibility, finding little evidence of adjustments in payment methods, the timing of payments, or reductions in hourly wages or piece rates.

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 because the frequency and diversity of viral outbreaks is increasing over time. 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 dissipate, so it remains vital to understand how to build resilience to such shocks. This is especially so 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 and personal savings imply the welfare impacts of aggregate shocks are most severe.

We present evidence from a field experiment in Uganda demonstrating that skills acquired through vocational training help build resilience to shocks even as severe as the Covid-19 pandemic. We do so among a group of workers that have 8-10 years of potential work experience when the pandemic struck. These are prime age or high attachment 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 severely impacted by lockdowns – because they are higher earnings workers and so are fired first by firms looking to survive the pandemic shock – 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 the Pandemic Once we factor in the resilience that skilled workers have to aggregate shocks, the returns to training are even higher than documented in the body of work evaluating such interventions over 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 over an intense six month period, and workers were selected into the evaluation sample based on their willingness to undertake this training rather than take up other short term labor market opportunities. Hence, for well-designed and targeted training interventions, the fact that such forms of skills acquisition enable workers to be resilient to aggregate shocks even of the scale as the Covid-19 pandemic, bolsters the case for skilling workers as early in their careers as possible, rather than reskilling them only as a reaction to job loss.

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 to the crisis. 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.

Design of Social Insurance Our study context is Uganda, a youthful developing country. We thus build on a rich empirical literature on the dynamics of displaced workers that 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 other forms of formal safety net, it is in such settings that the demand for social insurance is high. Such 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 and targeted incentives for firms to hoard labor in the face of the kinds of rapidly emerging aggregate crisis that developing countries are frequently subject to.

A Appendix

A.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 in our final survey wave, February 2022. Figure A3 shows projections for compliers and reweighted controls for two key outcomes: whether the worker is employed in one of our study sectors, and their total earnings from wage/self-employment. Taking outcomes across the first five survey waves (2013 to 2021)we use a power function to project the path labor market outcomes would have been predicted to take. We overlay these with the actual paths of each outcome over all survey waves. The resulting gaps between projected and actual outcomes imply: (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.

A.2 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 $L1$, so between the lockdowns, the magnitude of this effect is 1.27 (on a 0-10 scale), representing a 27% increase over controls (reweighted for compliance probability). The divergence in beliefs along this margin more than doubles between skilled and unskilled workers later in the pandemic. Columns 2 to 4 show treatment effects on the other key margin of expectations: expected earnings if workers were able to transition into their most preferred study sector job. Among those taking up vocational training, we see that in each pandemic survey wave, 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 larger 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 early in the pandemic measured in wave $L1$.

A.3 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 labour 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 across genders is in terms of 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 Chakravorty *et al.* [2023] in the context of rural India. We find these differential shifts into casual work across genders 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.[34]

Desired Sector of Training Workers that 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 that 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

[34] Alfonsi *et al.* [2023] track 700 young urban vocational trainees in Uganda – these graduated from similar VTIs and having 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, 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].

during the pandemic. Given desired sector of work correlates highly to the sector treated workers are actually trained in, this also proxies 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 that desired to work in manufacturing and services. The most notable divergence again occurs with respect to shifts into casual work. For those that 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 pandemics, 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 extensive margins of employment among those only offered vocational training (Columns 2 and 3), while the cumulative impacts of skills on earnings from wage/self-employment are slightly larger among those additionally offered matching (Columns 5 and 6).

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 median cognitive ability. Skilled workers that are also above median cognitive ability are far less likely to downgrade and 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.

A.4 Robustness to 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, 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.[35]

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 control. 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 control. 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 control 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 effectively assumes attriters are positively selected in control, while being 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 even more extreme assumptions that there is a .5SD difference in outcomes between complier and control attriters. It is only under such an extreme assumption that attriters in the control group 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

[35] This procedure amounts to running a first stage where attrition is predicted using baseline characteristics that are relevant for whether we were able to 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 get 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.

being no selective attrition on unobservables over time among treated and control groups.

A.5 Experiences of the Pandemic

Skilled and unskilled might experience the pandemic differently. Columns 1 to 3 of Table A11 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 more directly 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 then ask about coping strategies employed during the lockdowns. We see no differences between skilled and unskilled workers in terms of them reporting having to reduce the number or size of meals, having to sell assets, or moving location in the period prior to the survey. Finally, we examine whether workers differ in their expectations of the economic recovery from the pandemic. 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 is in sharp contrast to the differential expectations of skilled and unskilled workers about their own labor market outcomes (Table A7).

A.6 Other Margins of Firm Response

Beyond laying off the highest earning workers, firms have other potential margins of response to the pandemic. To explore this possibility in our data, 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 their hours at work ($p = .001$). We can also explore the issue further by drawing in data from Alfonsi *et al.* [2023] that was collected over the pandemic from graduates of vocational training institutes in Uganda. Focusing in on the most 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, consistent with downward nominal wage rigidity even in the presence of the aggregate shock; (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.[36]

[36] The sample in Alfonsi *et al.* [2023] consists of 561 young and individuals who graduated between 2014-19 from five vocational training institutes in the Central and Eastern regions of Uganda, and also received nationally accredited skills certificates. They received training in electrical wiring (27%), motor mechanics (23%), food and hospitality (17%), plumbing (14%), tailoring (9%), construction (7%), hairdressing (3%), and welding (less than 1%). They are on average 25 years old, 33% are women, 37% are married and 45% have children.

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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 standard deviation for continuous outcomes. The reported p-value 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 value 0 if the worker is assigned to the Control group, and 1 for workers assigned to the corresponding treatment group and the independent variables are the variables in Columns 1 to 5 (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)
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 or not. 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 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, and 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
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 or not. 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 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, and 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 (that 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 that 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, 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: 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 reweighed in this exercise. 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 7: Job Transitions 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

Column 4: in wage employment pre-lockdown AND in wage/self employment post-lockdown, in either of the two lockdowns

	Same firm	Same sector, different firm	Different sector	Wage employment to self-employment
	(1)	(2)	(3)	(4)
Skilled x wave L1	-.180**	.194***	-.014	.035
	(.082)	(.060)	(.063)	(.043)
Skilled x wave L2	.010	.031	-.041	.018
	(.054)	(.044)	(.034)	(.041)
Reweighted control mean, L1	.866	.057	.077	.080
Reweighted control mean, L2	.926	.052	.021	.068
Number of observations	406	406	406	735

Notes: We report 2SLS ATT estimates, where robust standard errors are in parentheses, and all data are based on survey waves L1 and L2. The sample in Columns 1 to 3 is restricted to workers that are wage employed in the pre- and post-lockdown time frames, in either of the two surveys. The sample in Column 4 is restricted to workers that are wage employed in the pre- lockdown time frame, and either wage or self-employed in the post-lockdown time frame, in either of the two surveys. 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 outcome in Column 4 is a dummy equal to one if the respondent was wage employed pre-pandemic but then became self-employed post-pandemic. 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. 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 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 ar at, as measured at survey wave 5. In Panel D, outcomes are measured at 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 are 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 skilled workers	.611	.775	[.000]
	(.439)	(.297)	

Notes: All data comes 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 total number of employees at the firm. 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 (that 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 reweighed in this exercise. 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, firm age, whether the owner is female, the owner's age, and a dummy for whether the firm is located in Kampala. To account for missing firm 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 a test of 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

	March - November 2020	December 2020 - June 2021	[p-value]
	(1)	(2)	(3)
A. Retention and Recruitment of Workers			
Share of employees still employed at firm	.633	.749	[.000]
	(.330)	(.307)	
Tried recruiting new workers since lockdown	.141	.212	[.000]
B. Laid Off Workers			
Substantial experience in firm	.787	.788	[.983]
Experience in same sector	.170	.162	[.772]
Unskilled	.023	.009	[.146]
C. Last Hired Worker			
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]
D. 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 equality of means between Mar20-Nov20 and Dec20-Jun21. 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

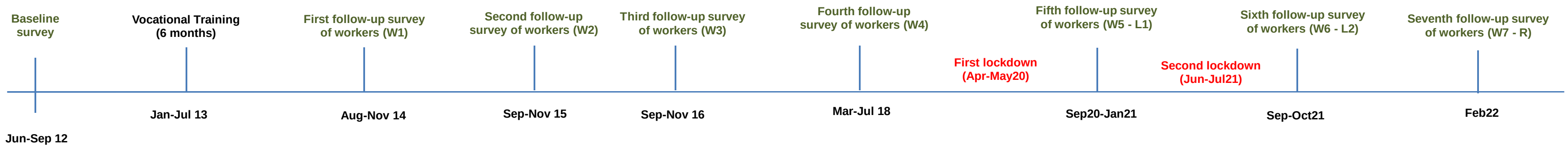
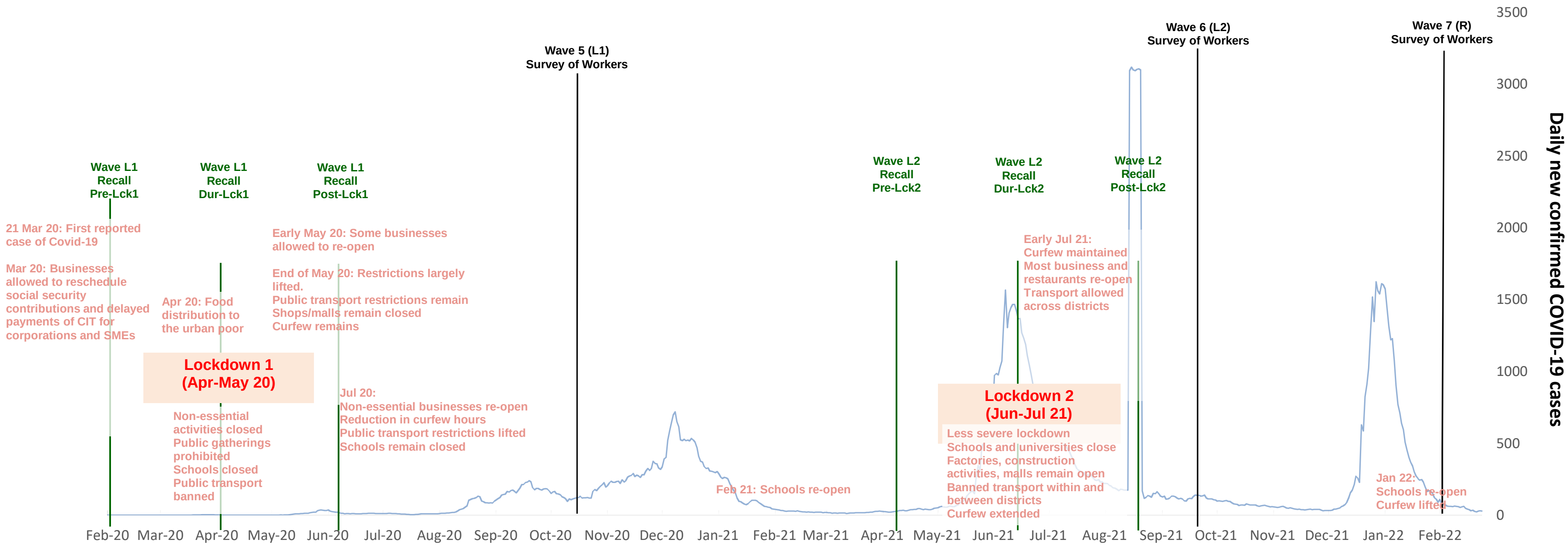
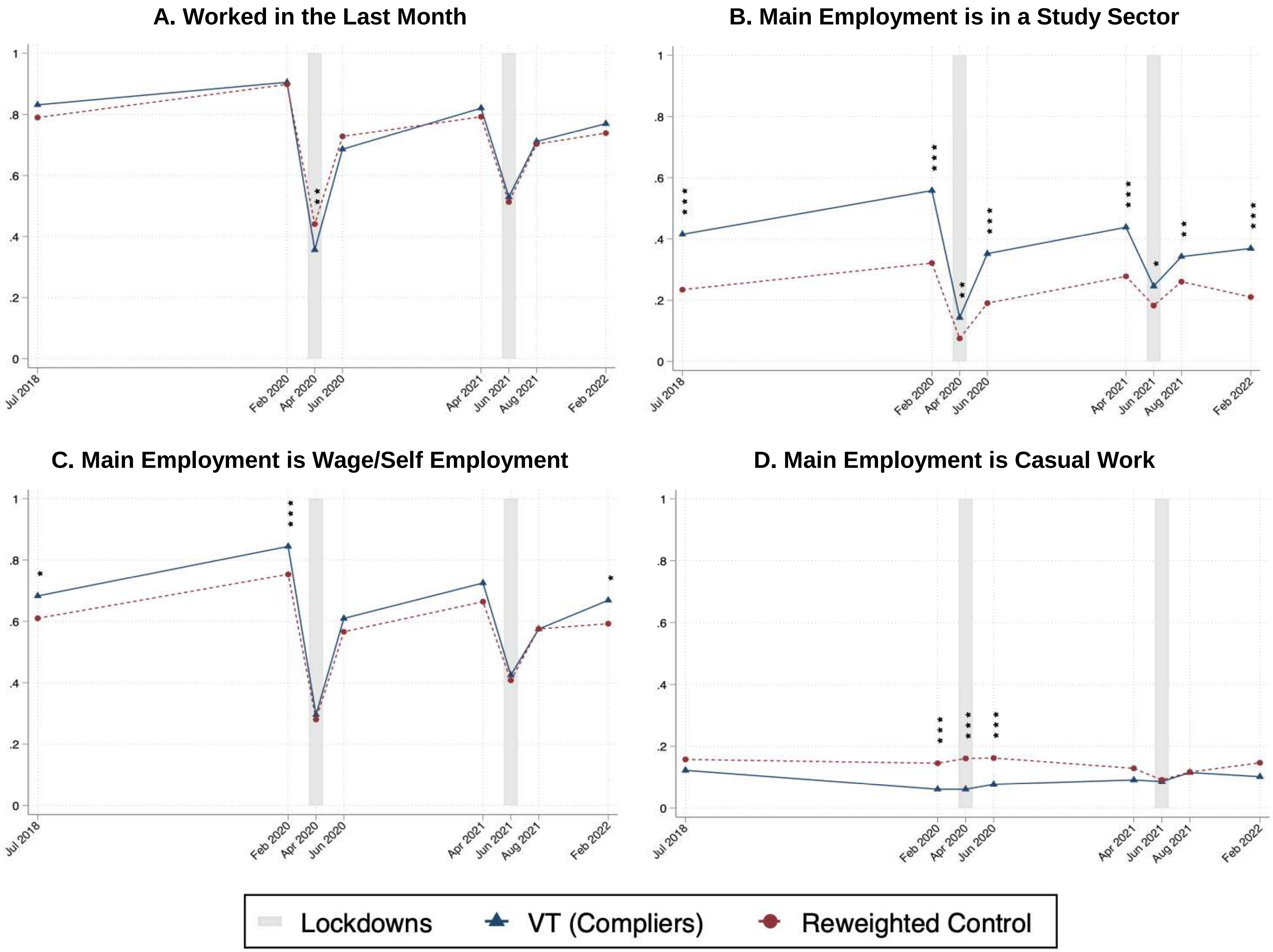


Figure 1B: Surveys, Confirmed Covid-19 Cases and Policy Responses



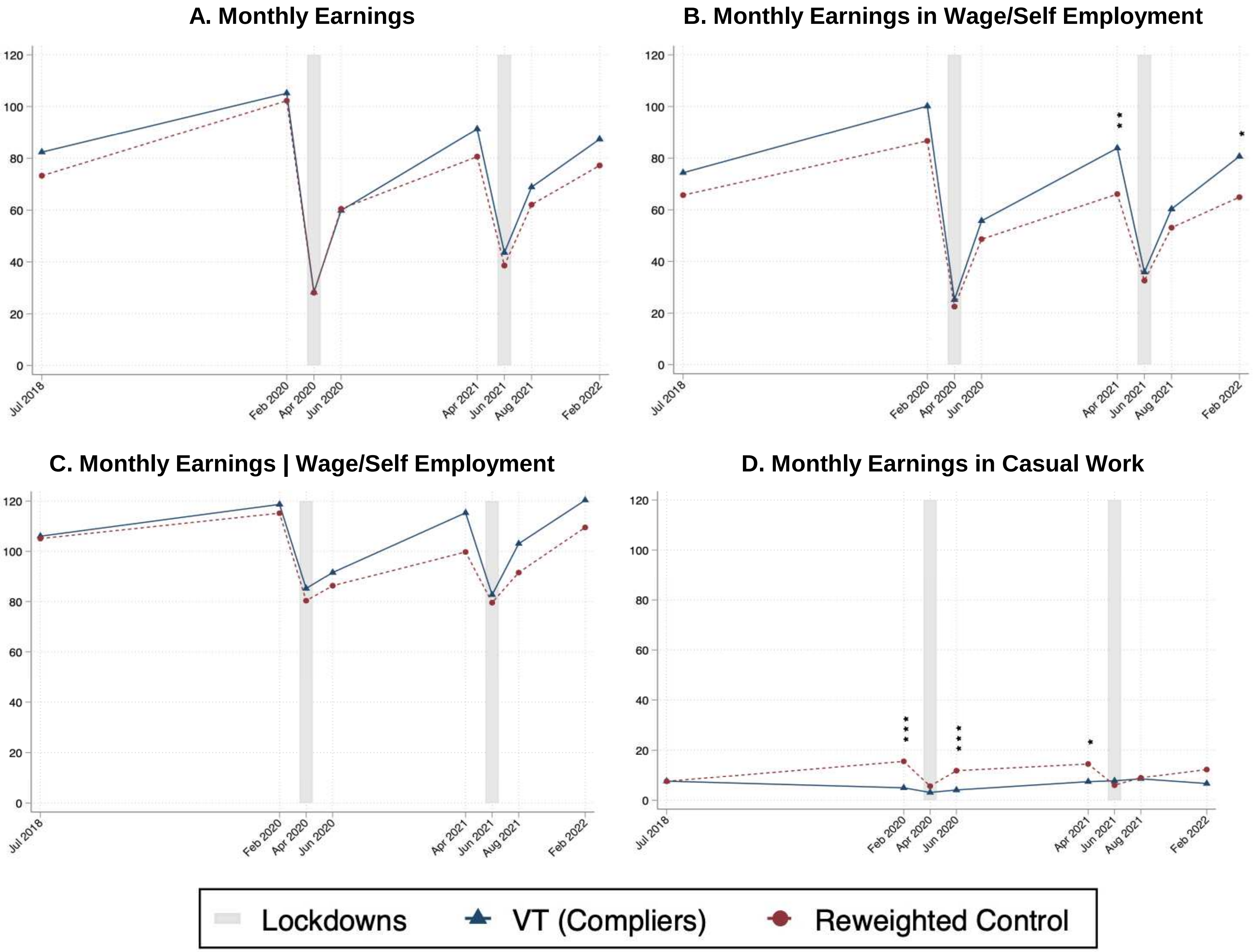
Source for Covid Cases Time Series: Our World in Data [https://ourworldindata.org/covid-cases]

Figure 2: Employment Outcomes over the Pandemic



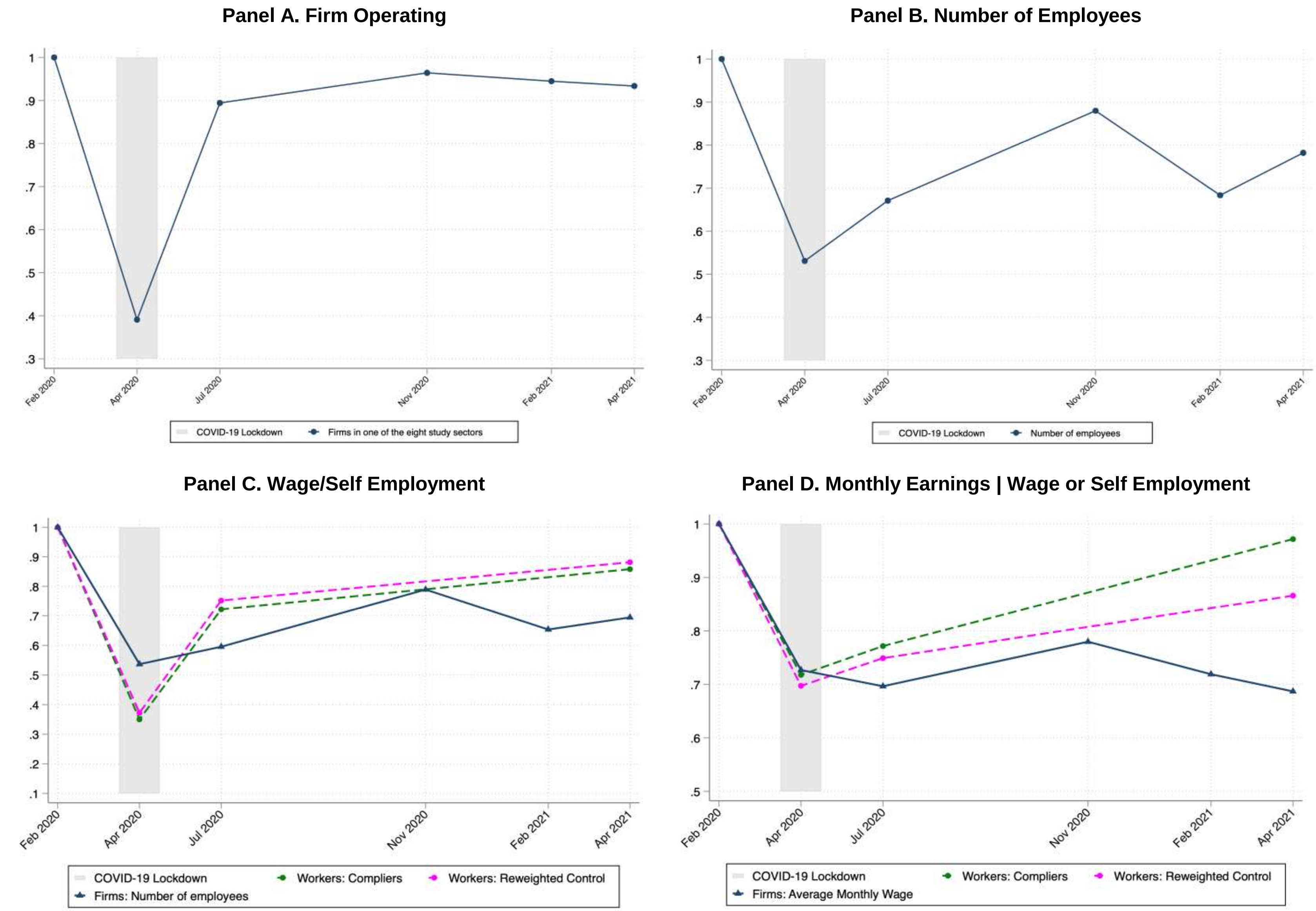
Notes: In each Panel we compare mean outcomes for compliers to the offer of vocational training to controls, where controls are reweighted by their probability of compliance. The first data point corresponds to Wave 4 conducted in 2018 before the pandemic survey waves. The stars in each time frame report the significance of these unconditional differences in each period. The grey 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 grey 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 D use data from the fifth and sixth follow-ups of the firm-side surveys. Panels C and D overlay this with data from the worker-side pandemic surveys. In all Panels, firm and worker outcomes are normalized to be one at their February 2020 levels. In Panel D, the top 1% of earnings values reported in the worker-side surveys are trimmed. The grey shaded region corresponds to the first lockdown. 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 engaged in a work activity or are actively seeking employment). The UNHS was fielded between June 2012 and June 2013. Our baseline survey was fielded between June and September 2012. In the UNHS respondents are considered to have attended vocational training if the highest grade completed is post-primary specialized training/diploma/certificate or post-secondary specialized training/diploma/certificate. 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 parenthesis throughout. The variable in Column 8 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. 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 value 0 if the worker is assigned to the Control group and taking value 1 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 are 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 vontrol (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 equal to 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 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). 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 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, 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 this exercise. 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 utilized 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 utilized 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: 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 utilized 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 was unable to 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 6 months and within 1 year, respectively. 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.

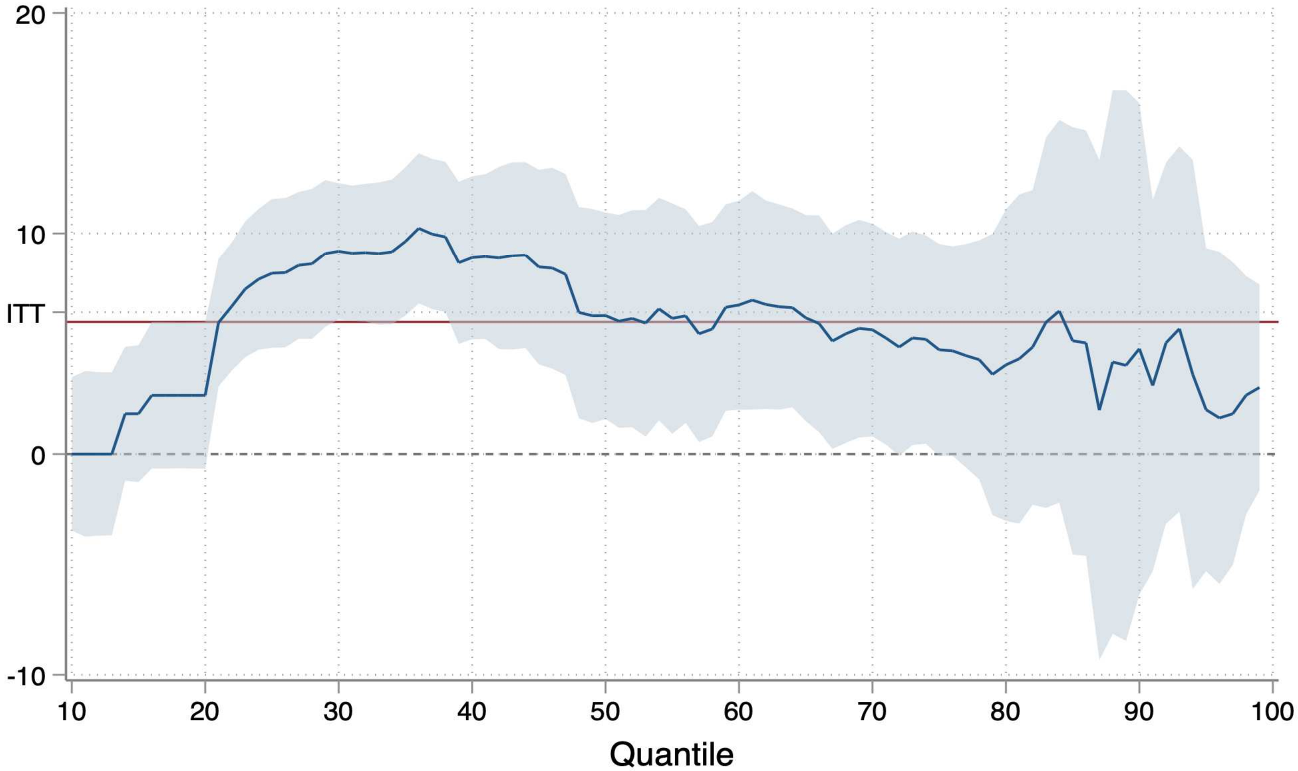
Table A12: Attrition and Survival of Firms

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 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.

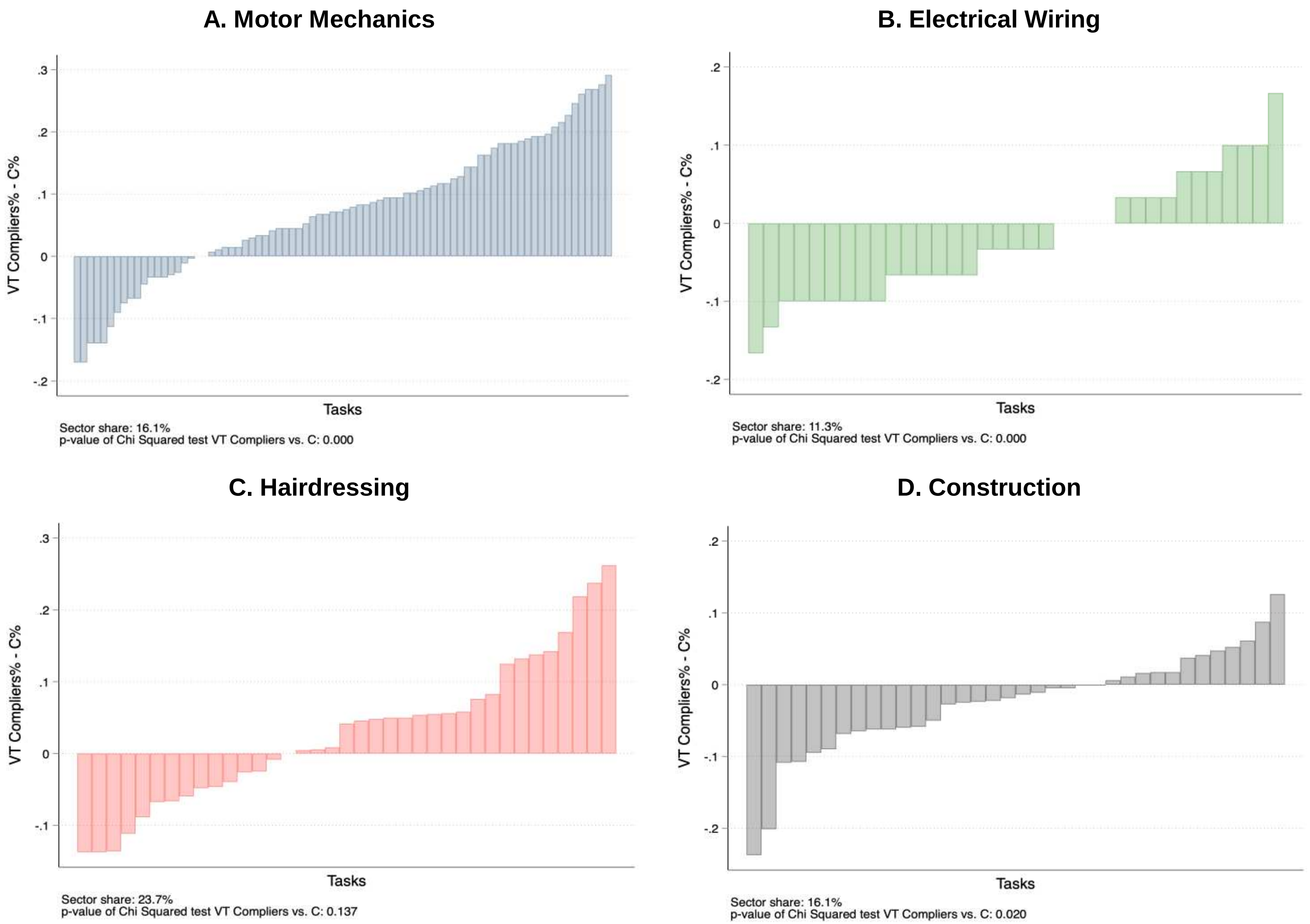
Figure A1: QTE on Sector-Specific Skills



Notes: The Figure reports quantile treatment effect estimates of the offer of training on the sector-specific skills test score (that 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 that 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

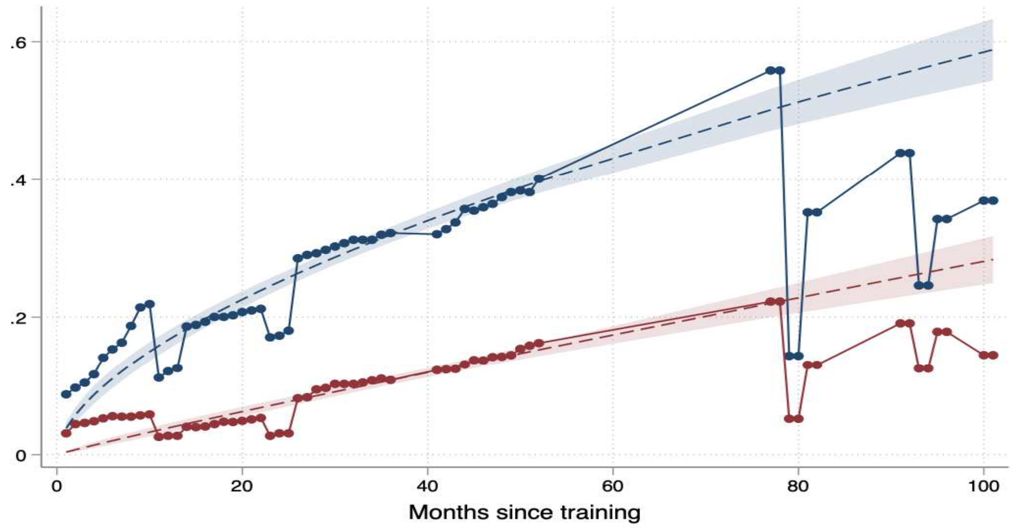
Y Axis = VT% - C% Performing a Given Task in the Firm



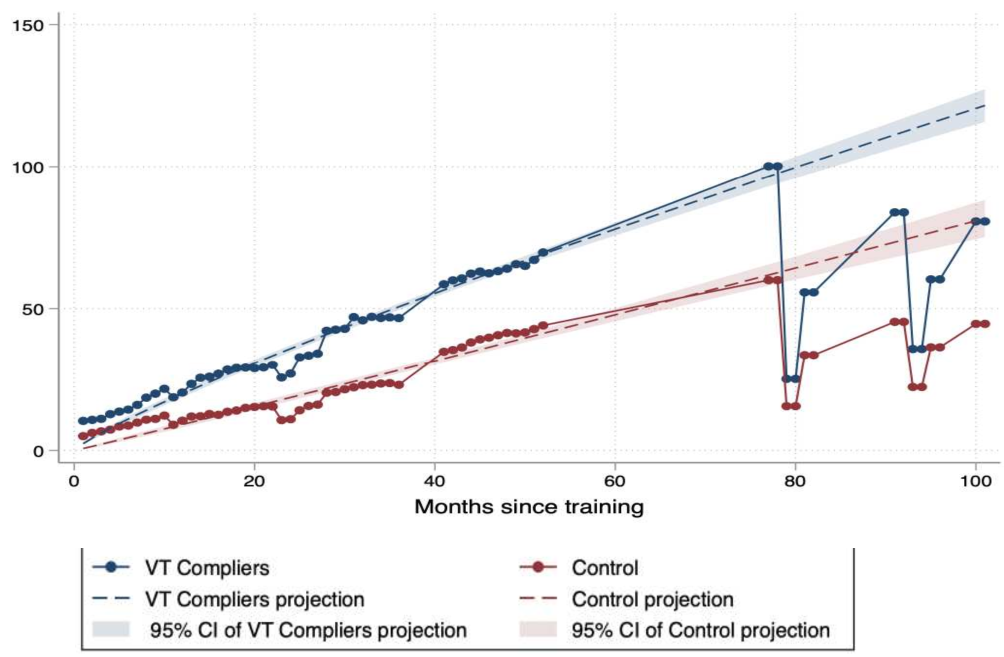
Notes: In the third worker follow-up survey we compiled a sector-specific list of tasks that workers in each sector are expected to be able to perform. We ask respondents whether they are able to perform each skill, for the sector in which they are employed. Each bar in the graph represents a different 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



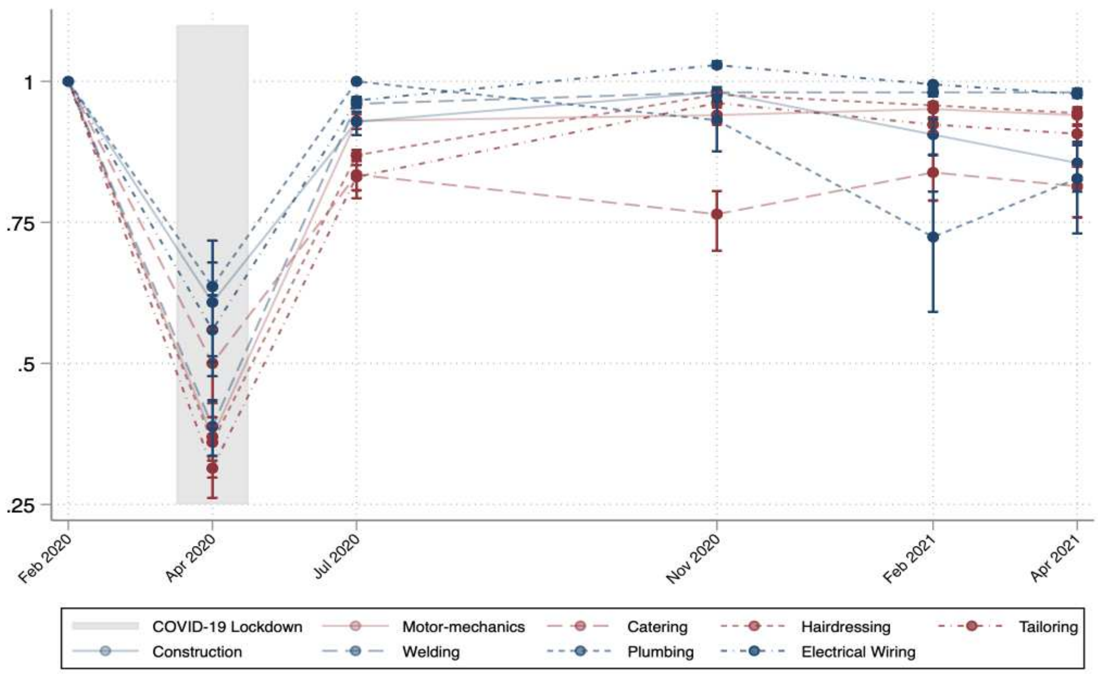
B. Monthly Earnings from Wage/Self Employment



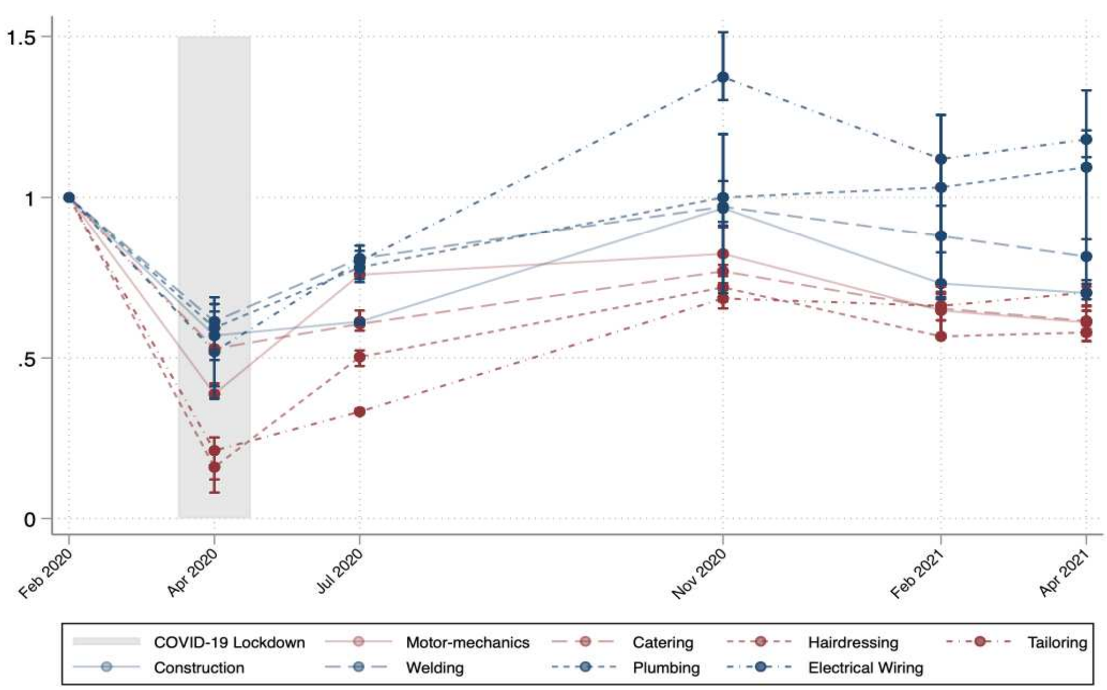
Notes: The projections use data from all worker surveys. Monthly data was collected from waves 1 to 4. From survey wave L1 (2020) onwards, respondents were asked to recall information about the last month's activity. For the pandemic survey waves we interpolate outcomes for missing months. We plot trends 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.