Firm Responses to Uncertainty and Implications for Workers: Experimental Evidence from Uganda During the Pandemic

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Abstract

The Covid-19 pandemic represents one of the most rapid and severe shocks to ever hit labor markets. We study how firms reacted to the heightened uncertainty and the consequences for workers, in the context of a low-income economy: Uganda. Our analysis is based on a panel of firms and workers, tracked from 2012 to 2022, including high frequency surveys during the pandemic. We find the key common response of firms to the heightened uncertainty caused by the pandemic was to immediately lay off the highest earning workers, that is, the most experienced or skilled employees. We then study the differential impacts of such firm survival strategies on workers across the skills distribution, exploiting the fact that we randomly assigned individuals to the offer of vocational training in 2013. We find that high-skill trained workers were more likely to be laid off early in the pandemic given firms' survival strategies, but such trained workers recover from this job loss and remain resilient to the shock. Cumulatively over the pandemic period, trained workers spend 61% more time employed than controls, and earn 17% more. In short, the returns to training survive the pandemic. The mechanisms driving the resilience of trained workers are the certifiability of their skills and their greater accumulation of sector-specific experience: both of which enable them to remain resilient to the shock by switching employers within the same sector during the crisis. Our findings have implications for understanding firm responses to fast moving aggregate shocks in low income settings, and to understand what drives the returns to training in good and bad economic times. *JEL: J24, O12.*

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1 Introduction

The Covid-19 pandemic represents one of the deepest and fast moving shocks to the world economy in the last few decades. At its height, the pandemic led to an estimated loss of 144 million jobs globally, with hours worked falling by 20% and both margins remaining below pre-pandemic levels through to at least 2022 [ILO 2022]. These impacts were worse in lower-income countries, even if many of them were not as severely affected in terms of official case rates for Covid-19. Understanding how heightened uncertainty arising from aggregate shocks impacts labor markets is critical given that employment outcomes centrally determine well-being.[1]

We study the issue in the context of one such low-income country: Uganda, that enjoyed a period of sustained economic growth in the years prior to the pandemic. Uganda shares many hallmarks of labor markets throughout Sub Saharan Africa, including an absence of: (i) policies to support firms during crises, meaning that firms may need to resort to extreme coping strategies to survive the pandemic; (ii) social insurance to workers, meaning that resilience in labor market outcomes to aggregate shocks is key for lifetime welfare. We approach the issue in two stages. First, we examine the strategic responses of firms to heightened uncertainty caused by the pandemic, through changes in labor demand, the skills composition of retained employees, and wages. Second, we examine the consequent impacts on workers across the skills distribution, with a focus on the differential impacts on high-skill trained and low-skill untrained workers. Understanding how these impacts vary is important because the resilience of high-skilled workers plays a key role in sustaining productive worker-firm matches, preserving the value of human capital, and supporting long-run prospects for the growth of firms and the broader economy.

Our analysis builds on our earlier work from the same project that utilized pre-pandemic data to study the returns to training acquired through vocational and firm-sponsored training [Alfonsi *et al.* 2020], and to study how training impacted job search strategies [Bandiera *et al.* 2023]. Our core analysis is based on the same panel of firms and workers tracked from 2012 but exploits new rounds of high frequency data collected over the pandemic. On the firm-side, we build on our original study that tracked 2000 SMEs in eight study sectors across manufacturing and services over four survey waves pre-pandemic (from 2012 to 2018). Study sector firms operate in welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering sectors. These firms in aggregate employ 6000 workers at baseline, with the average firm size being three (plus a firm owner). On the worker-side, we build on our original study tracking 1100 workers over four survey waves pre-pandemic (from 2012 to 2018). This data collection started as part of a field experiment in which we randomly assigned individuals to the offer of standard six-month vocational training courses in 2013, in one of the eight study sectors. This enables us

[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.

to experimentally study the causal impacts of the aggregate shock on high-skill trained workers and low-skill untrained workers.

Our pandemic surveys were timed around the two lockdowns in Uganda during the pandemic. The first lock-down occurred in April/May 2020, and the second in June/July 2021. Surveys to our tracked firm owners were fielded in 2020 and 2021. Surveys to our tracked workers were implemented in 2020, 2021 and 2022. In these firm and worker pandemic surveys, we purposefully collected information on outcomes recalled before, during, and just after each lockdown enabling us to reconstruct granular labor market dynamics over the crisis. The resulting 10-year panel of firms allows us to build a rich picture of the dynamic evolution of the uncertainty firms face, their responses in terms of labor demand, and the skills composition of retained and laid off workers. The resulting 10-year panel of workers allows us to build a rich picture of the dynamic evolution of worker skills, employment, earnings, sectoral allocations, expectations, search behavior and savings. We know of no comparable data set that enables such an analysis of how the pandemic propagated to firms and workers in a developing country setting, and that allow the heterogenous impacts on trained and untrained workers to be studied experimentally.

Our first set of results exploit the firm-side data to understand how firm owners perceived the heightened uncertainty during the pandemic, and how they responded in order to survive. We find that the key strategic response of firms was to immediately lay off the highest earning workers, that is those most experienced or skilled employees. As a result: (i) by April 2021, labor demand was 30% lower than in February 2020; (ii) average earnings of workers that remain employed within firms follow an L-shaped pattern, remaining at 70% of the average level of employees in February 2020 with no trend towards recovery between lockdowns – as only the lowest skilled or more inexperienced workers were retained. In line with nominal downward wage rigidity [Kaur 2019], we find no evidence that firms were able to reduce wages. Rather they were reliant on reducing employment and lowering the skills composition of workers as key survival responses.

Our first key takeaway is that all firms – irrespective of their sector of operation or pre-pandemic profitability – adopt this kind of first-in-first-out (FIFO) strategy. All firms had an urgent need to reduce wage bills on account of falling demand and profits at the outset of the pandemic. Such FIFO strategies were successful in that: (i) over 90% of firms were able to remain in operation; (ii) by April 2021, firm revenues and profits had both steadily recovered in levels from the depth of the first lockdown.

The second part of our analysis uses the worker-side data to examine how firms' FIFO strategies differentially impact individual labor market dynamics over the pandemic. We exploit the fact that our data collection started as part of a field experiment in which we randomly assigned individuals to the offer of standard six-month vocational training courses in 2013, in one of the eight study sectors. 65% of workers took-up the offer of training, and 95% of workers completed their courses conditional on enrolment. We use this experimental variation to study how firms' FIFO strategies differentially impact high-skill trained workers and low-skill untrained workers.

We consider ATT estimates of outcomes between compliers that take-up vocational training (who we refer to as trained or treated workers), and controls (who we refer to as unskilled or control workers). To do so reliably, we note that pre-pandemic only 12% of workers attrit by the fourth follow-up survey in 2018. While attrition rises to 31% in the first pandemic survey, we observe no additional attrition over the three pandemic surveys. We also document the robustness of our core results to addressing selective attrition on non-observables.

We find that: (i) in line with firms's FIFO strategies, trained workers face greater initial exposure to the shock; (ii) there is a V-shaped recovery in employment and earnings outcomes for trained workers around each lockdown, and trained workers recover more quickly between lockdowns; (iii) the V-shaped recovery is *not* because trained workers are re-hired by their original employer post-lockdown; rather trained workers demonstrate greater mobility across firms in the same sector during the first lockdown, consistent with the role of certifiable skills in facilitating job mobility; (iv) by February 2022 trained workers also significantly shift into casual work, reflecting skills downgrading, similar to patterns observed in the US and middle-income countries after more slow-moving economic shocks [Huckfeldt 2022, Dix-Carneiro *et al.* 2024].

To quantify the returns to training over the pandemic, we calculate the differential cumulative labor market impacts between trained and control workers, essentially integrating over the dynamic treatment effects. We find that trained workers spend 61% more time employed in one of our study sectors than controls, and their total earnings are 17% higher. These cumulative impacts are around half of those documented for the pre-pandemic period 2013-18, over which trained workers accumulated 117% more experience in one of the study sectors, and their earnings from wage/self-employment were 59% higher earnings than controls.

Hence our second key takeaway is that the returns to skills acquired through vocational training survive the pandemic, despite such high-skilled workers being more exposed to the shock through firms' FIFO responses. These returns go beyond measured contemporaneous labor market earnings outcomes (as would be included in any IRR calculation) but also include building resilience and insuring workers against aggregate shocks even as rapid and severe as the Covid-19 pandemic.

Our results strongly refute the notion that the pandemic caused the labor market to merely freeze and quickly recover once lock-downs were over. Rather, our results raise the spectre of productive worker-firm matches that formed pre-pandemic being lost and not fully replaced. For example, comparing actual outcomes relative to projections based on pre-pandemic trends reveals lasting impacts of the shock, with trained (control) workers 37% (49%) less likely to be employed in study sectors and earnings 34% (45%) below trend – these magnitudes are at the top end of estimates from the literature on displaced workers that typically uses administrative data from high-income settings [Jacobsen *et al.* 1993, Couch and Plaszek 2010, Davis and von Wachter 2011]. To the extent that earnings reflect individual productivity, the fact that earnings for skilled workers remain well below trend suggests productivity losses are large. These losses are exacerbated by trained workers switching to casual employment over the second lockdown and many remaining unemployed even as the economy recovers.

The third part of our analysis examines mechanisms through which the returns to training are maintained. We build on the fact that on the eve of the pandemic, trained workers had accumulated greater experience in good sectors and in good firms, and accumulated different search capital and higher savings. These channels might cause treated and control workers to differ in their resilience to the shock. We examine these mechanisms following Hainmueller [2012], reweighting controls to match pre-pandemic covariate moments among compliers.

Our third key takeaway is that sector-specific experience accumulated before the pandemic plays a central role in sustaining the returns to training during the crisis. While trained workers were more mobile across firms in the same sector during the first lockdown, it is the depth of experience within the sector that explains much of their resilience [Topel 1991, Neal 1995, Kletzer 1998]. We find a more limited role for other mechanisms, such as trained workers having more pre-pandemic experience of good jobs *per se* or of good worker-firm matches, or trained workers using different search strategies to control workers during the crisis.

Our work contributes to three literatures. Our first is to the body of work examining labor market impacts of the pandemic, including how its economic toll has been unevenly distributed across groups in high- and low-income settings [Adams-Prassl *et al.* 2020, Egger *et al.* 2021, Alon *et al.* 2022, Blundell *et al.* 2022, Mahmud and Riley 2023, Chetty *et al.* 2024]. Evidence on impacts across the skills distribution is scarce and 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 impacts by gender [Alfonsi *et al.* 2023, Chakravorty *et al.* 2023].

We build on these studies by documenting causal impacts of training on labor market dynamics over the pandemic, and providing new insights on why high-skilled trained workers are more initially exposed to the shock, and the mechanisms enabling them to remain resilient to the crisis. The most closely related paper is Barrera-Osorio *et al.* [2022], who link applicants randomly allocated into a job training program in service sectors in Colombia, to monthly administrative records on employment. They track workers from June 2017, through their graduation from training in December 2018, through to August 2021. Counter to our findings, they report the returns to training disappear – or are even negative – during the pandemic despite such training having large returns pre-pandemic. We discuss the relationship to these earlier sets of work while presenting our results – our two-sided data collection helps explain some of these earlier findings, enables us to go deeper in studying mechanisms driving the returns to training over the pandemic, and to uncover firm-side responses to the shock that initially spark the negative impacts on workers.[2]

[2] Barrera-Osorio *et al.* [2022] suggest three reasons for the returns to training becoming negative in their setting, but their data does not allow them to distinguish between them: (i) the training program was relatively short; (ii) service sectors were hardest hit; (iii) sample workers graduated from their training courses around December 2018, so had little labor market experience before the pandemic. We make progress on these issues when studying the mechanisms behind our findings because: (i) the firm-side data allows us to understand firm responses during the pandemic and how they differed between treated and control workers, and so help understand which workers were

Our second contribution speaks directly to concerns raised in a nascent literature that the returns to interventions might vary due to their interaction with aggregate conditions [Rosenzweig and Udry 2020]. By evaluating the returns to the same offer of vocational training in good economic times and bad, we document that returns to training are halved in bad times, but remain positive. However, the mechanisms driving the returns in good times and bad differ. In our earlier work, we documented that mechanisms such as certification and job search behavior generate the returns to training in times of economic stability [Alfonsi *et al.* 2020, Bandiera *et al.* 2023]. In contrast, over the pandemic we find that while skills certification remains important because it enables workers to switch firms in the same sector, additional mechanisms such as trained workers greater accumulation of sector-specific experience is also key to ensuring resilience. Other mechanisms such as accumulated savings and search behavior might play less of a role in bad times because of the speed and severity of the pandemic shock.

Finally, we draw inspiration from the literature on labor market dynamics of displaced workers [Jacobsen *et al.* 1993, Farber 1997, Kletzer 1998, Schmieder *et al.* 2023]. This has shown how dynamics vary with labor market conditions or in the presence of correlated shocks across workers in the form of mass layoffs. This literature has considered heterogeneous impacts of job loss by worker skills [Seim 2019], job content [Athey *et al.* 2023], or occupation-specific human capital [Huckfeldt 2022, Braxton and Taska 2023]. We extend 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 work to our panels of workers and firms, to simultaneously understand how firms and workers interact to drive labor market dynamics for trained and untrained workers during the pandemic. This highlights that in a low-income setting firms respond to uncertainty using first-in-first-out firing strategies, but that trained workers recover from this higher initial exposure to the shock given their certifiable skills and greater accumulation of sector-specific experience pre-pandemic.

The paper is organized as follows. Section 2 describes our data. Section 3 documents firm responses to the heightened uncertainty caused by the pandemic. Section 4 describes the field experiment and reviews pre-pandemic differences in skills and labor market outcomes between treated and control workers. Section 5 documents treatment effects of training on labor market outcomes over the pandemic. Sections 6 studies mechanisms sustaining the returns to training. Section 7 concludes. The Appendix presents further results and robustness checks.

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more exposed to the shock; (ii) our workers are assigned to training in both manufacturing and service sectors; (iii) we examine how labor market dynamics of treated and control workers differ with their experience in study sectors or in wage employment more broadly.

2 Setting

2.1 Data Sources

Firms To establish firm responses to the pandemic, we draw on data from firms collected as part of the original two-sided field experiment [Alfonsi *et al.* 2020]. We drew this sample in 2012, conducting a census of firms in 15 urban labor markets and then selecting firms: (i) operating in one of the manufacturing and service sectors in which we offered vocational training, and (ii) having between one and 15 employees (plus an owner). The second restriction excludes micro-entrepreneurs and ensures we focus on small and medium sized firms that are central to employment generation in Uganda. We end up with a sample of 2300 firms, that in aggregate employ 6000 workers at baseline, with the average firm size being three (plus a firm owner). These types of firm offer good jobs: earnings are higher in these sectors than many others. They collectively employ about 30% of individuals aged 20-30 working outside of agriculture.

Workers To establish impacts on workers of the pandemic, we exploit a panel of workers tracked since 2012 when they were labor market entrants, and also collected as part of the two-sided field experiment. The experiment 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, and 43% were women. Table A1 shows their labor market outcomes at baseline: unemployment rates were over 60%, 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.[3]

2.2 Study Timeline

Figure 1A shows the study timeline. 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, and the second in June/July 2021. The second lockdown is considered to have been less strict. What is important to stress is that Uganda suffered relatively few cases of Covid-19. Indeed the first lockdown was imposed when cases remained close to zero. As we document later, the crisis can be thought of as much of an economic as a health shock. Uganda had very limited policy responses to support firms and workers during the pandemic. In March 2020, some formal firms were allowed to reschedule social security contributions and delay payments for three months, and in April 2020 a food distribution scheme to aid the 1.5million

[3]The eligibility criteria were being aged 18-25, having completed from 7 to 11 years of education, not being in full-time school, being poor – using a poverty score based on family size, assets owned, type of building lived in, village location, fuel used at home, number of household members in school, monthly wage and education of the household head. Applicants were ranked on a 1-5 scale on each dimension and a total score was computed. A relative threshold score (varying by geography) was used to select eligibles.

urban poor was started. In our firm sample, only 6% of firms report either applying or receiving support. Similarly, in our worker sample, very few report having applied for the food distribution scheme or having benefitted from it.

Firm Surveys The baseline survey to firms took place between October 2012 and June 2013, and they were tracked over three further surveys pre-pandemic. As Figure 1B shows, during the pandemic we ran two (phone) survey waves to firm owners. In each, we asked questions related to three time-frames of recall, enabling us also to track firm outcomes with high frequency – spanning just before, during and after the first lockdown, and between the first and second lockdown.

Worker Surveys The baseline survey to workers took place in 2012. For those assigned to treatment, vocational training began in January 2013 at the partner VTIs. Workers were tracked over four surveys pre-pandemic. As Figure 1B shows, during the pandemic we ran three (phone) survey waves. In the first two pandemic survey waves we asked questions in relation to three time frames of recall, so tracking individual labor market outcomes with high frequency – spanning the eve of the pandemic, during, and just after the first lockdown, and just before, during, and just after the second lockdown.

2.3 Firm Characteristics and Exposure to the Shock

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

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

Attrition We next consider the firms tracked from baseline through to the pandemic. Firm attrition pre-pandemic is relatively low: 16% of firms attrit by the fourth follow-up. Attrition rises to 28% in the pandemic, but nearly all of this occurs between waves 4 and 5. We have close to zero attrition of firms between the pandemic surveys. Column 2 of Table 1 shows the *baseline* characteristics of those firms that did not attrit by wave 5, our first pandemic survey. On most margins, at baseline non-attriters have characteristics similar to firms in our original sample. Column 4 then shows the characteristics of non-attriting firms as measured in the our first pandemic survey, with reference to the first time frame of recall on the eve of the pandemic. By February 2020, non-attriting firms had grown significantly with almost double the number of employees, profits, and customers per week since baseline. Importantly, their revenues per worker had not risen in real terms, but their wage bill as a share of revenue had risen from 68% pre-pandemic to nearer 95% on its eve.[4]

Representativeness By wave 5 firms are no longer representative of firms in the study sectors on the eve of the pandemic. To gauge how positively selected surviving firms are, we exploit the fact that alongside our last pre-pandemic survey we also conducted a new census of firms operating in the same labor markets and sectors, using the same sampling approach as our 2012 census. We can thus compare characteristics of firms that we tracked and that survived until February 2020 to firms in the second census. Column 6 of Table 1 shows firm characteristics in the census, and Column 7 shows the percentile of surviving firms that we track from baseline, in the distribution of census firms. As expected, tracked firms are positively selected. For example, census firms have 4.1 employees in 2017; tracked firms have 5.5 employees on average, corresponding to the 84th percentile of census firms. Tracked firms are in the 92nd percentile of profits, and above the 90th percentiles in terms of revenues and revenues per worker.

On the one hand, this positive selection of tracked firms needs to be borne in mind for interpreting survival strategies of firms in general, and how firm responses to the pandemic might have impacted workers. On the other hand, tracked workers from our sample have acquired six years of potential experience by the pandemic, and have moved up the job ladder into larger firms.[5]

3 Firm Dynamics Over the Pandemic

3.1 Uncertainty

Even pre-pandemic, firms in our context face uncertainty arising from demand volatility or productivity shocks. However, the unprecedented speed and severity of the pandemic means that firm owners faced even greater uncertainty during the crisis, and sometimes along new margins. Table 2 shows how the uncertainty firms face evolves over the pandemic, comparing responses in Oct-Dec 2020 (a few months after the end of the first lockdown) to those recorded in May-July 2021 (just prior to the second lockdown, and a year on from the end of the first lockdown). Panel

[4] Columns 1 to 3 of Table A3 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.

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

A examines the expectations of firm owners about the pandemic itself. We see that: (i) at the end of 2020 only 18% of owners expected a new lockdown within six months, with 58% of them viewing the possibility as unlikely; (ii) at the end of 2020, 40% of firms believed the economy would rebound within six months, but expectations worsened by the middle of 2021 ($p = .003$); (iii) at the end of 2020, 37% of owners believed they were unlikely to re-open following any new total lockdown, and this eased slightly by mid 2021 ($p = .000$).

Panel B examines owner expectations over firm outcomes. In terms of expected size (number of employees), we use information on expected hires and layoffs over the next six months to construct this expectation as a percentage of current firm size. Coming out of the first lockdown, owners expected firm size to recover – being 36% higher than the actual firm size as recorded in wave 5, and with a slightly smaller expectation for firm size in mid 2021 ($p = .019$). The next row examines expected sales, where we exploit the fact that in both surveys we asked owners about their expected sales relative to 2019: in 2020 owners expected sales to be around half the 2019 levels, but by mid-2021 they were expecting sales to have fully rebounded.

Finally, Panel C gives a sense of the supply chain disruptions faced. Owners report that 11% of their suppliers had closed down by late 2020, and these were only partially replaced by new suppliers over the pandemic.

All three sources of uncertainty make it difficult for owners to predict their firm's survival probability. To gauge this, we predict firm survival based on a rich set of baseline covariates. This result is in Column 4 of Table A3: larger and older firms, those in manufacturing, with male owners, older owners and fewer customers are more likely to survive the pandemic. However these covariates explain less than 15% of the variation in survival probabilities. While additional information might be privately observed to owners, the regression result highlights the degree of uncertainty still faced.

3.2 Labor Demand

To begin to understand how such uncertainty transmits through to the labor market, we start by considering the dynamics of firm's labor demand. We use our firm surveys to present unconditional firm outcomes over the six time frames of the pandemic, where we normalize each outcome to one in the first time frame, February-March 2020. Panel A of Figure 2 shows the share of firms that remained operating in each period (solid line). The pandemic hit firms severely: only 40% of firms in our study sectors remained in operation during the first lockdown. They then experienced a V-shaped recovery after the end of the first lockdown, 90% of firms were back in operation and this remained steady thereafter. However, 7% of firms – even the positively selected ones we track – stopped operating by April 2021, speaking to the severity of the pandemic shock and the uncertainty induced.

Panel A overlays this time series of operating firms with labor demand in these firms. Employment levels are at 55% of their pre-pandemic level during the first lockdown – an enormous shock in the space of just two months. Recovery is slower than on the operating margin, with labor demand rising to only 70% of the pre-pandemic level in these firms. In short, even among positively selected firms tracked into the pandemic and that survived the shock, firm sizes shrank.

Table 3 shows the regression adjusted equivalent of these results for outcome y for firm f in sector s in time frame t:

$$y_{fst} = \alpha + \sum_{t=2}^{t=7} \beta_t time\_frame_t + \delta \mathbf{x}_{fs0} + \lambda_s + u_{fst}, \tag{1}$$

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 2).[6]

These largely confirm the descriptive evidence: Column 1 shows in the first lockdown, the share of firms operating fell by 53pp relative to February 2020, but firms recovered between the first and second lockdowns. Column 2 shows that for surviving firms, labor demand fell sharply during the first lockdown and then slowly recovered. Labor demand fell by 53% in the first lockdown, remaining 41% lower in July 2020 (when the number of firms operating is only 10% lower). On the eve of the second lockdown labor demand remained 30% lower than in February 2020 – a considerable reduction in the average size of firms.

3.3 Firms' First-in-First-Out Strategy

To begin to unpack how firms survived such uncertainty through the first lockdown, and implications for workers, Panel B of Figure 2 uses the firm data to show the evolution of earnings of workers that *remaining employed* in these firms. To aid comparison with the firm outcomes shown in Panel A, the series is normalized to one in the first time frame. Earnings for workers who remain employed in study-sector firms show an L-shaped pattern, remaining at 70% of the average level of all employees in February 2020 with no trend towards recovery between lockdowns.

The corresponding regression result is in Column 3 of Table 3. We see persistent falls in the monthly earnings across employees retained at firms over the pandemic: earnings fall 40% in the first lockdown relative to February 2020, and this persists across time frames including until April 2021. In line with a L-shaped impact, we cannot reject the null that the earnings impact is the same in April 2021 as in the trough of the first lockdown ($p = .325$).

Retention This fall in average earnings of retained workers can arise from two sources: changes in the composition of workers, and falls in wages of existing workers. To explore changes in the composition of retained employees, we use data from our pandemic firm surveys, where firms

[6] 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.

reported hires and layoffs over two periods: (i) March 2020 to November 2020, covering the first lockdown; (ii) December 2020 to June 2021, between the first and second lockdowns. The results are in Table 4. Panel A first considers worker retention. We see that 63% of employees stayed with the firm over the first lockdown, and 75% of employees stayed with the firm between lockdowns, an increase in retention over these phases of the pandemic ($p = .000$). Hence many – but far from all – productive worker-firm matches that had formed pre-pandemic were preserved over the crisis.

The next rows examine characteristics of laid off workers. For firms that laid off a worker, the majority laid off highly experienced workers – correlating with the most skilled workers. Contrary to expectations, tenure and skills did not protect workers from job loss, and rather firms employed something like a first-in-first-out (FIFO) strategy in the first lockdown. Such responses help reduce wage bills because more experienced/trained workers have higher earnings: either because their base earnings are higher or because they can obtain a higher piece rate in some sectors. It was feasible for firms to first lay off more experienced and high-skill workers, and keep operating with a smaller group of less experienced and skilled employees. At the start of the pandemic, in the tracked firms 29% of employees were reported by firm owners as being unskilled. The median age of employees was 23, with 39% of employees being below age 21.

To explore the possibility of wage adjustments during the crisis, we draw in data from Alfonsi *et al.* [2023] that was collected over the pandemic from graduates of VTIs in Uganda. Using a comparable sample of trained workers, we find 90% of skilled workers report no reductions in hourly wages or piece rates during the pandemic. In short, these labor markets display downward nominal wage rigidity, even in the pandemic. Such rigidity is in line with evidence documented in other low-income contexts [Kaur 2019].

In short, in the face of a severe aggregate shock, firms first laid off the highest earning workers – corresponding to the most skilled or experienced workers. They did so during the initial lockdown to quickly reduce wage bills as profitability plummeted. This explains the L-shaped dynamics of the average earnings among retained workers in the firm (Panel B of Figure 2).

Recruitment The other side of firms' strategies during the pandemic was adjusting hiring as part of broader labor demand dynamics. This is examined in Panel B of Table 4. Firm's attempts to recruit workers were more muted over the first lockdown than between lockdowns. The next few rows examine characteristics of the last recruited worker. Firms were more likely to recruit workers with experience in the same sector between the first lockdown and November 2020, than between lockdowns ($p = .000$). Such opportunities to be rehired by firms in the same sector is something we further document later using our worker side data, showing that: (i) trained workers switch firms within the same sector over the first lockdown; (ii) as the economy recovers, trained workers report directing their search towards firms in the study sectors.

These changes in the composition of employed workers are reflected in earnings differences between last hired and last laid off workers, as shown in Panel C: the average monthly earnings of hired workers are \$30, while the monthly earnings of laid off workers were nearly 40% higher, at \$49. This is again consistent with firms laying off the highest earning workers over the first lockdown as part of FIFO strategies to survive the shock.

The Success of FIFO Strategies The kind of first-in-first-out strategy we document is entirely counter to last-in-first-out strategies often observed as firms respond to slow moving shocks in higher-income settings [Buhai *et al.* 2014]. To assess whether such FIFO strategies were successful in response to the pandemic, we re-consider the dynamics of firm outcomes in Table 3. On revenues and profits, Column 4 shows revenues plummeted during the first lockdown, with profits falling to nearly zero (Column 5). However, by April 2021, firm revenues and profits had both steadily recovered in levels from the depth of the lockdown in April 2020 (p = .000, .011 respectively). Indeed we cannot rule out that both are the same as on the eve of the pandemic in February 2020, although the point estimates are negative. Column 6 examines how changes in skills composition of retained employees translate into the wage/revenue ratio. As described earlier, at baseline this ratio was 68% but on the eve of the pandemic had risen to 95%. Given firms' response 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.[7]

In short, the dynamics of revenues, profits and wage bills all suggest: (i) recovery along these margins for surviving firms; (ii) these financial outcomes for firms follow very different patterns to those shown earlier on employment and the monthly earnings of retained workers.

Heterogeneity in FIFO Strategies Across Firms Finally, we consider heterogeneity across firms in the use of FIFO strategies along three margins: (i) forecast errors related to firm outcomes; (ii) pre-pandemic profitability; (iii) sector of operation. We do so to underpin the credibility of the interpretation of firm responses to the pandemic, and to inform our analysis of the consequences of these responses for workers.

On the first margin, we can measure the accuracy of expectations firm owners had in terms of their firm size or sales, comparing (in absolute terms) the difference in expectations on these margins to actual outcomes. We divide firms into above/below the median difference to capture firms with low/high forecast errors. Panel C of Figure 2 shows that both types of firm behave consistently with FIFO, with earnings of retained workers remaining at 60% of their level in February 2020 for firms with high forecast errors over firms size, and the corresponding figure being 75% for firms with low forecast error. Panel D repeats the analysis using measures of

[7] We also explored other margins of firm response to the pandemic beyond the use of FIFO strategies. We find that: (i) 95% of firm owners report no changes in the timing or method of payments; (ii) 99% report no changes in payment mode; (iii) 89% report no changes in other non-pecuniary benefits; (iv) between the first and second pandemic survey wave, there is a significant increase from 9.5% to 22.6% of firm owners reporting allowing employees more flexibility in hours at work ($p = .001$).

forecast errors based on sales: for both high/low forecast error firms we see earnings of retained workers falling to around 70% of their level in February 2020. On both margins, firms with high forecast errors: (i) continue reducing earnings of retained earnings even after the end of the first lockdown, while firms with more accurate forecasts recover more quickly; (ii) have more severe FIFO strategies, with retained earnings of employees being proportionately lower through the pandemic that for firms with more accurate forecasts.

Panels E and F consider earnings of retained workers splitting firms into high/low profits or high/low revenues per worker (within sector), as measured in the last pre-pandemic survey. Irrespective of the heterogeneity considered, the use of FIFO strategies is apparent across firms – even those that might have had access to more working capital and so better able to use alternative coping strategies to respond to the shock.

Finally, we consider heterogeneity across study sectors because they can vary in their exposure to the shock due to differential reliance on face-to-face trade and vulnerability to supply chain disruptions. Panels A and B in Figure A1 show dynamics of firm openings and employment over the pandemic by sector, where we distinguish between sectors with high and low levels of in-person customer interaction. Firms in sectors with higher levels of interaction are more severely impacted by the first lockdown. In these firms, employment levels remain between 50 to 65% of their pre-pandemic level, while firms in sectors with lower levels of customer interaction recover to employment levels between 70% and 95% of their pre-pandemic level. 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 electrical wiring, in which employment is almost unchanged in April 2021 relative to February 2020. Hence the pandemic leads to a reallocation of employment opportunities across sectors.

Panel C shows monthly earnings of retained workers, by firms in each sector. Firms in nearly all sectors display behaviors consistent with FIFO strategies during the first lockdown, although there is variation across sectors. In the depths of the first lockdown, earnings of retained workers fall to be between 45% (hairdressing) and 95% (construction) of average earnings on the eve of the pandemic in February 2020; by April 2021 earnings of retained workers remain at between 62% (tailoring) and 81% (construction and electrical wiring) of average earnings on the eve of the pandemic. Panel D relates changes in earnings of retained workers to changes in employment, by sector and each phase of the pandemic. We see that: (i) in the depth of the first lockdown sectors with larger falls in employment also have larger falls in earnings of retained workers; (ii) the same relationship holds in the second part of the pandemic.

To summarize, we consistently find that firms laid off higher-earning skilled workers first, consistent with them using FIFO strategies to survive the pandemic, regardless of the accuracy of the expectations over firm size and sales, their pre-pandemic profitability, and sector of operation.

4 Workers

4.1 Design of the Experiment

Our second set of results examine the implications of firm's FIFO strategies on workers – focusing on the differential impacts between high-skill trained workers and low-skilled untrained workers. We study the issue experimentally by exploiting the fact that our original field experiment randomly assigned eligible applicants to the offer of vocational training in 2013, at one of five reputable VTIs. Applicants were randomly assigned to receive the training, using a stratified randomization where strata are region of residence, gender and education. The VTIs could offer standard six-month training courses in the eight sectors covering manufacturing and services that our firm-side sample is drawn from.

Treatment The vocational training intervention provides workers six months of sector-specific training in one of the eight study sectors. Our intervention partner BRAC covered training costs, at $470 per trainee. Courses were full-time, and worker attendance was monitored. Upon graduation, trainees receive a certificate verifying their skills. As Alfonsi *et al.* [2020] document, in good times there are high returns to having certifiable skills from reputable VTIs in these urban labor markets. Within those assigned to training, the original field experiment included a second stage of randomization. In a first group, graduating trainees transitioned into the labor market unassisted. A second group received light touch offers to match for job interviews with firms in our firm sample. The impact of the matching on job search and outcomes in the pre-pandemic period is studied in Bandiera *et al.* [2023]. In this paper given our focus on the returns to training during the pandemic, more than six years after the interventions occurred, we pool both and show the robustness of key results in each treatment arm.

4.2 Balance, Attrition and Compliance

Balance Table A1 shows baseline labor market characteristics of workers in each treatment arm. Table A2 shows other background characteristics. In both cases, the samples are well balanced and normalized differences in observables are small.

Attrition We consider attrition in two periods: pre-pandemic from baseline until the fourth follow-up (March to July 2018), and over the three pandemic survey waves. Column 1 of Table A4 shows that pre-pandemic attrition is low: 12% of workers attrit by the 68-month fourth follow-up, and this is uncorrelated to treatment. The remaining Columns show that: (i) attrition rises to 31% in the pandemic waves; (ii) nearly all of this occurs between waves 4 and 5, and we then have close to zero further attrition through to our final survey wave; (iii) during the pandemic, controls are 8-9pp more likely to attrit than those offered vocational training. In the Appendix we further document that on most margins and survey waves we find little evidence of heterogeneous attrition between treatment and control groups, either before or during the pandemic (Tables A5, A6). We later show the robustness of our results to alternative approaches addressing selective attrition on non-observables.[8]

Compliance 65% of workers take-up the offer of vocational training. The VTIs were paid half the training fee at the start and half after the worker completed the training, resulting in a 95% completion rate conditional on enrolment. Table A7 shows correlates of take-up. Individuals with lower cognitive ability, lower locus of control, or resident outside Kampala are more likely to take-up the offer. Given our focus is on the returns to training over the pandemic, our analysis mostly considers ATT estimates, so the differential impact between compliers taking-up vocational training relative to controls. Whenever we present descriptive statistics on controls, we reweight their outcomes to account for their likelihood to comply based on the results from Table A7.

4.3 Pre-pandemic Outcomes

To begin to understand the implications for workers of firm's FIFO strategies and hence differential treatment of skilled and unskilled workers, we first need to establish the impacts of vocational training on pre-pandemic labor market outcomes, as documented in our earlier work using data from this project [Alfonsi *et al.* 2020]. We briefly review those results as they make clear that trained workers are those with more skills, higher earnings and greater labor market attachment, and so more exposed to firms FIFO strategies during the first phase of the pandemic. We establish pre-pandemic impacts, we use OLS to estimate the following ITT specification for outcome y_{isw} for worker i in strata s in survey wave w:

$$y_{isw} = \alpha + \beta VT_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{isw}, \tag{2}$$

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.[9]

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

[9] All regressions control for the training implementation round and dummies for the month of interview. We control for the following baseline characteristics: desired sector of training, marital status, whether they have children, whether they are in work, and whether they score above median on the cognitive test score. For each

Sector-Specific Skills We measure skills using a sector-specific skills test developed in conjunction with skills assessors of written and practical occupational tests in Uganda. Each test comprises seven questions (multiple choice and more complex questions). Workers had 20 minutes to complete the test, and we convert answers into a 0-100 score. The test was given to all workers (including controls) at third follow-up, measuring persistent skills accumulation. There is no differential attrition by treatment into the test. Table 5 reports the results. Panel A reports the ITT estimates $\widehat{\beta}$ from (2), and Panel B reports ATT estimates.[10]

Before administering the test, we asked workers whether they had *any* skills relevant for the study sectors. The dependent variable in Column 1 of Table 5 is a dummy equal to one if the worker reported having skills for any sector. As reported at the foot of the Table, 66% of controls report having skills relevant for some sector, and reassuringly this rises to close to 100% for those offered vocational training, as measured three years post-intervention. All workers who reported having sectoral skills took the test: others were assigned a score of 11 assuming they would answer the test at random. Column 2 shows workers offered training significantly increase their measurable sector-specific skills by 19% (or $.28\sigma$ of test scores). Columns 1 and 2 in Panel B show that among those taking up vocational training, nearly all report having some sector-specific skills, and their skill measure is 23% higher than controls when we reweight for their compliance probability (or $.41\sigma$ of test scores). Figure A2 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.[11]

Tasks To validate that these acquired skills are relevant to our study sectors, the Appendix presents additional analysis considering tasks workers conduct at work, showing that the task composition of employed workers differs between trained and control workers. This highlights that on the eve of the pandemic, these groups of worker differed in their occupation specific human capital, which can impact labor market dynamics after job loss [Huckfeldt 2022, Braxton and Taska 2023] – an issue we return to when studying whether and how the returns to training endure through the pandemic.

covariate we also include a dummy for whether it is missing at baseline.

[10] 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).

[11] 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.

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 5, 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 training in times of economic stability.

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

5 Labor Market Outcomes Over the Pandemic

5.1 Estimation

As Figure 1 describes, during the pandemic, our worker 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. We estimate the following specification by 2SLS in time-frame t from survey waves $L1$, $L2$ and R:[13]

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

[12] Table A8 confirms that on all but one dimension of pre-pandemic outcome, there are no statistically significant differences between workers with and without match offers.

[13] 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].

where $Trained_i$ indicates whether worker i took up the offer of vocational training. We instrument $Trained_i$ 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 training 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 (3) are reported in Table A9. To establish the constancy of the *impact of training* on outcomes over the pandemic, in Table A9 we report the p-value on a test of whether treatment effects on the eve of the pandemic in the first time frame in wave $L1$ (February/March 2020), are the same as in wave R (February 2022), when the economy is recovering, H_0: $\beta_1 = \beta_7$. To establish whether workers fully recover in the *level* of outcomes over the pandemic, we report the p-value on a test of whether $\overline{y}_1 = \overline{y}_7$ for skilled and unskilled workers.

5.2 Employment

Motivated by the literature showing that following job loss, re-employment probabilities can 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 3 shows unconditional differences in each time frame for four outcomes along this margin between compliers and reweighted controls. As a point of comparison we also show the outcome from the final pre-pandemic survey wave 4. The x-axis is scaled to match the periods covered and the gray shaded regions refer to each lockdown.

Panel A examines whether individuals are employed. Pre-lockdown 1, both vocational trainees and controls have employment rates close to 85% – reflecting that when the pandemic struck they were prime age workers with six years of potential experience and high labor market attachment. During the first lockdown, employment rates for unskilled workers drop to 45%. The corresponding regression specification in Table A9 shows that employment rates drop even more for trained workers, who are 13.4pp less likely to still be in employment ($p = .006$). Hence trained workers are in proportionate terms, hit harder by the shock going into the first lockdown – in line with firms' FIFO strategies.

After the end of lockdown 1, employment rates of trained and control workers follow similar trajectories, with both dipping again during the second lockdown. The 'double dip' exactly matches the timing of lockdowns, with the severity of the impacts for the first lockdown being greater than for the second, in line with the first being more stringently enforced. Comparing levels of outcomes around each lockdown, we observe a V-shaped recovery in employment outcomes for both groups, with the depth of the V-shaped employment shock being greater for trained workers. However, the recovery is incomplete (so $\overline{y}_7 < \overline{y}_1$): in February 2022, employment rates remained 16pp lower for trained workers than on the eve of pandemic in February 2020 ($p = .000$).

Panel B focuses on whether trained and control workers are employed in one of the study sectors – as a marker of working in a more productive sector, and gaining valuable labor market experience. On this margin we see pronounced differences between the groups through the pandemic. As Table A9 shows, on the eve of the pandemic trained workers were 22pp more likely to be employed in a study sector ($p = .000$). They maintain this advantage over controls throughout, except during the lockdowns. After each lockdown, trained workers recover more quickly in regaining employment in the study sectors. In February 2022 trained workers were 17pp more likely than controls to be employed in a study sector ($p = .000$). However, neither set of workers recover in levels: in February 2022 employment rates in study sectors remain 19pp lower for trained workers than on the eve of pandemic in February 2020 ($p = .000$).

The remaining Panels examine employment types. Panel C confirms that the differential employment dynamics between trained and control workers are driven by wage/self-employment, and this is itself largely driven by wage employment rather than workers shifting into self-employment.[14]

Panel D shows trends in casual work. To begin with, we note that control workers engage in casual work at higher rates at the outset of the pandemic. This gap is maintained over the first lockdown with employment rates in casual work being significantly higher for controls around the first lockdown. However, by the time of the second lockdown these employment rates almost converge as trained workers shift into casual work at later stages of the pandemic. By the end of the pandemic in February 2022, employment rates in casual work are 4pp higher for trained workers than on the eve of pandemic in February 2020 ($p = .000$). This kind of downgrading and switch into casual work has been documented for US workers in response to job loss [Huckfeldt 2022], and in response to trade shocks in middle-income contexts [Dix-Carneiro *et al.* 2024].[15]

On all employment margins, we cannot reject that the ATT effects are the same in the first and last time-frames of the pandemic, as shown in Table A9. Hence the magnitudes of treatment effects of training on these outcomes remain the same at the end of the pandemic as at its start.

Comparison to Employment Dynamics in Firms The employment dynamics show for workers largely mimic the broad patterns of what we documented from the firm-side perspective. On wage employment dynamics, employment rates of trained and control workers fall further in the first lockdown than among workers in firms in our study sectors, but the V-shaped recovery

[14] 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, trained workers are not more likely to be self-employed than controls; (iii) the differential likelihood of trained and control workers to be self-employed never differs statistically in any time frame of the pandemic, including during the first or second lockdowns.

[15] In the Appendix we present results examining whether the patterns align with worker expectations of earnings conditional on wage employment. These confirm that through the pandemic, trained workers have higher minimum 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, this suggests trained workers retain higher reservation wages than controls throughout the pandemic for wage employment. Hence any shift into causal work is not driven by a fall in their reservation wage.

is similar in both data sources. This suggests that over the course of the pandemic, trained workers are able to reallocate across firms in the same sector – something suggested earlier from the firm-side data and that we explore in more detail below using the worker-side data.

5.3 Earnings

Job loss can lead to permanently lower earnings – the 'scarring effects' of recessions [Ruhm 1991, Jakobsen *et al.* 1993, Davis and von Wachter 2011]. We examine this in Figure 4 where we repeat the earlier analysis for earnings outcomes. The underlying regression estimates are shown in Columns 5 to 7 in Table A9. These follow very similar V-shaped and double dip dynamics for employment outcomes, with trained workers more severely impacted by lockdowns – in line with firms' FIFO strategies, recovering more quickly between lockdowns, but there is no full recovery in levels with earnings in February 2022 remaining below what they were on the eve of pandemic.

Panel A of Figure 4 shows the dynamics of total monthly earnings (from all forms of employment). In nearly all time frames trained workers have higher monthly earnings than controls. It is again the case that in the depth of each lockdown, the gap in total earnings between trained and control workers approaches zero, so that in proportionate terms, trained workers have larger earnings losses during lockdowns. In line with the earlier results, the first lockdown suppresses earnings more than does the second. Finally, we continue to find that the recovery in levels is far from complete by the end of our study period. In February 2022 total earnings remain 17% lower for trained workers than they were on the eve of the pandemic ($p = .026$), with the corresponding figure for control workers being 24% ($p = .000$).

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

Panel C conditions earnings on wage and self-employment. As in Panel A we see that over the pandemic, in nearly all time frames trained workers have higher earnings than controls. Moreover, this is a margin of outcome for which there is a full recovery in levels by February 2022 for both groups of worker. Finally, Panel D shows that earnings from casual work remain higher for control workers just pre- and post the first lockdown, but these earnings gaps disappear around the second lockdown – in line with the earlier evidence that trained workers downgrade and shift into casual work around the second lockdown.

Comparison to Earnings Dynamics in Firms We can compare these earnings dynamics with the earnings dynamics for workers who *remain employed* in study-sector firms documented earlier in Figure 2. Trained workers are more severely impacted by lockdowns – in line with firms' FIFO strategies, but thereafter a sharp divergence emerges in the earnings dynamics of retained workers in the firm-side data and those of treated workers. Earnings conditional on employment for treated workers recover in a V-shaped pattern, while earnings for workers who remain employed in study-sector firms show an L-shaped pattern because of the FIFO strategies of firms. This again suggests that over the course of the pandemic, trained and control workers are able to reallocate across firms in the same sector – something we explore below.

5.4 Cumulative Impacts

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

$$\sum_{t=1}^{t=7} y_{ist} = \alpha + \beta Trained_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{ist}, \tag{4}$$

where we again instrument $Trained_i$ 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 6 where we show the four margins of employment from Figure 3 (Columns 1 to 4) and three of the earnings margins from Figure 4 (Columns 5 to 7). For each outcome we show the ATT effect from (4). In the lower part of the table we show the implied cumulative treatment effect. Focusing on those margins where the ATT estimate differs significantly from zero we see that over the pandemic trained workers spend 61% more time employed in one of our study sectors, and their earnings from wage/self-employment are 28% higher. These cumulative impacts are around half of those we documented for the pre-pandemic period, over which trained workers accumulated 117% more experience in one of the study sectors, and their earnings from wage/self-employment were 59% higher earnings than controls.

The key takeaway is that the returns to skills acquired through vocational training survive the pandemic – roughly halving in magnitude, but still widening cumulative gaps in labor market outcomes between trained and control workers. These cumulative impacts are quantitatively important, despite trained workers being hit harder by the first lockdown – due to firms' FIFO strategies. This speaks to their resilience during the pandemic.

Extensions and Robustness We present two further sets of results in the Appendix. First, we estimate how the returns to training 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.

Second, we address concerns over attrition. We earlier documented that although attrition rises between our last pre-pandemic survey in 2018 and our first pandemic survey, attrition is near zero across the three waves of pandemic surveys. This helps ameliorate the concern that the estimated dynamic labor market impacts are driven by attrition alone. Moreover, we earlier showed no strong evidence of differential attrition by treatment and control based on observables. The double dip dynamic impacts documented on both employment and earnings margins further help ameliorate the concern that attrition might drive the impacts, or that there is any steady fade out of the return to skills over the pandemic. Nevertheless, in the Appendix we address concerns related to attrition using multiple approaches following [Blattman *et al.* 2020].

5.5 Did Labor Markets Just Freeze?

Even if trained workers remain resilient to FIFO strategies of firms and the returns to training survive the pandemic, this still leaves open the broader question of the overall impacts of the pandemic on worker outcomes and the sustenance of productive work-firm matches formed pre-pandemic. At one extreme, the shock might be severe but brief: the pandemic caused the labor market to freeze in time, but it recovered quickly upon reopening – as documented for prime age workers in the US [Chetty *et al.* 2024]. The other view is that the pandemic caused persistent losses to workers in part because of the loss of productive work-firm matches. We present two sets of results that strongly suggest the latter interpretation.

5.5.1 Post-pandemic Recovery

One way to benchmark workers' recovery from the pandemic is to use pre-pandemic data to project labor market outcomes in a counterfactual absent the pandemic, and then contrast projected and actual outcomes in February 2022. Figure A4 shows projections for compliers and reweighted controls for two key outcomes: employment in one of our study sectors, and total earnings from wage/self-employment. Using data across the first five survey waves, we use a power function to project the path labor market outcomes would have taken. We overlay these with the actual paths of each outcome. Pre-pandemic labor market trends for both skilled and unskilled workers were upward, unlike the flat or declining trends during the pandemic shown in Figures 3 and 4. The resulting gaps between projected and actual outcomes imply lasting impacts of the pandemic: (i) trained (control) workers' likelihood to be employed in one of the study sectors is 37% (49%) below trend; (ii) trained (control) workers have total earnings that are 34% (45%) below trend. These magnitudes are at the top end of estimates from the literature on dynamic labor market outcomes for displaced workers typically using administrative data from high-income settings – these find long run earnings losses between 15% and 30% [Jacobsen *et al.* 1993, Couch and Plaszek 2010, Davis and von Wachter 2011].

This raises the wider issue of whether there are productivity losses from the destruction of pre-pandemic worker-firm matches? To the extent that earnings reflect individual productivity in our study sectors, then the fact that earnings for skilled workers remain 34% below trend in a counterfactual absent the pandemic, suggests, all else equal, productivity losses are large.

5.5.2 Worker Mobility

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

Firm and Sectoral Reallocations To examine the reallocation of workers across firms and sectors and how this differs between treated and control workers, we focus on the time frames before and after each lockdown and restrict the sample to those in wage employment before *and* after each lockdown (so in time frames 1 and 3, or in time frames 4 and 6). We then examine whether, pre- and post-lockdown, they report working: (i) at the same firm; (ii) in a different firm but in the same sector; (iii) in a different sector (and hence a different firm).[16]

Column 1 of Table 7 shows that among controls who were wage employed before and after the first lockdown, 87% remain employed in the same firm. The ATT estimate shows trained 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 for trained workers is not because they are re-hired by the same firm – the FIFO strategy of firms persists and does not cause them to immediately recall workers initially laid off. Rather, as Column 2 shows, trained 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.

Two labor market features can help explain the mobility of trained workers across firms in the same sector. First, in our earlier work examining returns to training pre-pandemic, we documented that in good times returns are partly generated because skills acquired through vocational training are certified [Alfonsi *et al.* 2020]. As a result, workers are more mobile: they experience quicker transitions back into employment when unemployed. The results in Table 7 can be interpreted as

[16] 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.

showing this mechanism remains relevant during the pandemic. Second, given the widespread use of FIFO strategies across firms in our study sectors, it might be common knowledge across firms that the most skilled or experienced workers are being laid off first. This information can aid the re-employment of such workers at other firms later during the pandemic [Gibbons and Katz 1991, Oyer and Schaefer 2011, Carrington and Fallick 2014] – consistent with the result in Column 2.[17]

The finding raises the issue of what kinds of firms (in the same sector) do trained workers reallocate to? These firms could be: (i) of the kind represented in our firm survey; (ii) larger firms; (iii) firms that started in the pandemic. Our data is not well suited to distinguish these cases because our pandemic worker surveys do not have data on the size of firm workers were employed at. However, we can illuminate the issue by comparing the distribution of earnings in our tracked firms during the pandemic to the distribution of earnings of complier and control workers at the same moment in time. This comparison is shown in Figure A5 for three prominent sectors: motor-mechanics, hairdressing and construction. For hairdressing and construction sectors, given the overlap in earnings distributions, trained workers might move to the firms similar to those in our firm survey. This appears less likely for trained workers in the motor mechanics sector, where the bulk of the earnings distributions do not overlap, suggesting those workers might have transitioned to larger employers than those sampled in our firm-side surveys.

Transitions Out of Wage Employment The second half of Table 7 examines transitions from productive wage employment into other forms of work or unemployment, and how this differs by treated and control workers. We consider individuals that were in wage employment pre-lockdown. We find: (i) no evidence that trained workers transition into self-employment at a differential rate than controls; (ii) trained workers are significantly more likely to switch into causal work around the second lockdown – in line with the evidence in Figure 3. This is a second important route through which persistent effects of the pandemic exist for high-skill workers. Finally, Column 7 shows there are large flows into unemployment – over 20pp – around each lockdown. Although this is not different between treated and control workers, it remains true that for workers of prime working age when the pandemic struck, their labor market trajectories worsened with persistent consequences for them and a loss of human capital utilization for the economy as a whole.

[17] 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: trained 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.

6 Mechanisms

We now drill down to understand why trained workers remained resilient to the pandemic. We consider mechanisms relating to the differential labor market attachment on the eve of the pandemic of treated and control workers, their differential accumulated search capital, or their differential health and other experiences of the pandemic.

6.1 Labor Market Attachment

Between 2013 and the eve of the pandemic, trained workers accumulate greater labor market attachment than controls. To get a sense of the differential accumulation of sector-specific experience, Figure 5 shows the share of months workers spent in any given sector pre-pandemic. The top panel shows this for compliers and the lower panel shows the same information for controls: 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, trained workers spent between 25% (plumbing) and 89% (construction) of all working months employed in their sector of training. The off diagonal entries show that workers trained in one sector spend almost no time in the other study sectors. Rather, as the final Column shows, when not working in their sector of training they spend time in other occupations, often related to the retail sector or as taxi drivers. In contrast, controls spent between 0% (plumbing, welding) and 29% (construction) of all working months employed in the sector in which they would have like to be trained.

In short, trained workers have greater experience working in the good sectors in which they were trained, so accumulate greater sector-specific skills. They also have greater experience of good jobs in both wage and self-employment, irrespective of their sector of training. These more productive work histories mean trained workers also acquire different search capital, and they accumulate more savings than controls. All these margins might lead treated and control workers to differ in their resilience to the pandemic, following trained workers greater exposure to initial job loss through FIFO strategies of firms.

We examine this set of explanations by considering whether the ATT estimates of cumulative treatment effects of training shrink if we reweight controls to have the same distribution of characteristics as compliers, as measured in the last pre-pandemic survey wave. We follow the approach of Hainmueller [2012] to create balanced samples where the control group data is reweighted to match pre-pandemic covariate moments among compliers. To account for differential attrition and other background sources of worker heterogeneity that potentially correlate with the reweighting covariate, when reweighting for continuous covariates we first regress the covariate on a set of worker characteristics (either measured at baseline or that are time invariant, and that can also predict attrition). We then split the distribution of residuals into deciles and use this to reweight controls so the distribution of residual deciles corresponds to that of the compliers. Non-compliers are not reweighted in this exercise. The results are in Table 8.[18]

Sector-specific Experience In Panel A we show the baseline ATT impacts on each cumulative labor market outcome. In Panel B we reweight controls to match the (residualized) cumulative labor market experience compliers have in the eight study sectors pre-pandemic. On the extensive margin, Column 2 shows the impact on the cumulative experience over the pandemic in these study sectors is explained by this margin of labor market attachment: the reweighted ATT estimate is not statistically different from zero and the implied cumulative impact is reduced entirely. This builds on the earlier finding that the composition of tasks that treated and control workers conduct within firms in the same sector differ significantly (Figure A3) and suggests the accumulation of sector-specific skills matters for resilience to job loss [Topel 1991, Neal 1995, Kletzer 1998]. This is so for retaining attachment to good sectors. For earnings, after accounting for sector-specific experiences, in Column 5 we see that the cumulative impact of training on total earnings are only slightly affected – falling 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: the estimated cumulative impact of training then falls from 28% to 21%.[19]

Experience of Good Jobs To separate out experience in good sectors from experience in good jobs, Panel C of Table 8 repeats the exercise with an alternative measure of labor market attachment: labor market experience in wage/self-employment – irrespective of sector – from baseline to the last pre-pandemic survey. On the extensive margin, Column 2 shows such labor market attachment only explains around half the subsequent cumulative impacts of training over the pandemic, so is less important than sector-specific experience. On the margin of total earnings, Column 5 shows that pre-pandemic experience of good jobs explains around half the cumulative impact of training, so more than the effect of sector-specific experience.

Experience of Good Matches Labor market attachment might also capture workers' experience of good matches with employers [Kletzer 1998]. For example, if trained workers are on average in higher quality matches that pay well, then earnings are more likely to fall following job loss [Schmeider *et al.* 2023]. To distinguish this explanation from the accumulation of sector-specific skills, we proxy good worker-firm matches using the average duration of employment spells (in months) from baseline to the last pre-pandemic survey, and then reweight controls to match this among compliers. Panel D shows the resulting cumulative impacts of training: while the baseline

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

[19] Our finding thus support the claim in Barrera-Osorio *et al.* [2022] that the returns to training disappeared during the pandemic partly because their sample of workers graduated from training courses in December 2018, and so had little labor market experience pre-pandemic.

estimate suggested treated workers spent 61% more time over the pandemic in good sectors, accounting for this form of pre-pandemic experience, the reweighted estimate reduces to 44%. On the total earnings margin in Column 5, the cumulative impacts of training on total earnings fall from 17% to 12%. Hence the returns to training narrow on the earnings margin when accounting for a history of good matches, but the returns to training in terms of attachment to good sectors are far more driven by the accumulation of sector-specific skills. Hence it is exactly the same characteristic – the accumulation of sector-specific skills – that leads workers to be targeted in firms' FIFO strategies and that enables them to recover from such layoffs.

Savings A consequence of treated workers accumulating more labor market experience and earnings pre-pandemic, is that they also enter the pandemic with more savings. This can impact their ability to weather economic uncertainty and help finance costly search behaviors [Lentz and Tranaes 2005]. 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 8 shows that the cumulative impacts on working in the eight study sectors remain almost unchanged from the baseline estimates (61% vs. 60%), as do the cumulative impacts on total earnings (17% vs. 16%). Moreover, reweighting for savings also does not explain the non-shift into casual work.

6.2 Search Behavior

Our earlier work showed that in good economic times, search behaviors of trained and control workers differ [Bandiera *et al.* 2023]. Trained workers search more intensively and direct their search towards higher quality firms. All this leads trained workers to have accumulated different search capital on the eve of the pandemic. Hence differences in outcomes over the pandemic between workers might be due to their continued use of different search behaviors – as has been documented in high-income settings [Hensvik *et al.* 2021]. In the pandemic surveys we asked individuals about search effort and whether they were directing their search towards particular sectors, firms or locations. We find little evidence that trained and control workers differ in their search behavior along either margin (Table A13). The one exception is that in the final survey wave R as the economy recovers, trained workers are significantly more likely to report directing their search towards firms in the eight study sectors ($p = .039$) – in line with the earlier evidence that in later stages of the pandemic, firms do try to recruit workers with experience in the same sector (Table 4), and that workers are switch across firms in the same sector (Table 7).[20]

[20] On search intensity, they do not differ over the pandemic in terms of whether they are searching for work. Conditional on actively searching, treated and control 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

An implication of this set of results relates to the generalizability of evaluations 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.* 2023] and during a crisis, we show that although returns to vocational training are sustained over both periods, the mechanism by which this is so differs. In good times search behaviors differ between treated and control workers and this is a key mechanism generating returns, while in the pandemic crisis, this mechanism is far more muted – likely because the speed and severity of the pandemic mean that returns to search effort and alternative strategies are far more uncertain.

6.3 Health and Other Experiences of the Pandemic

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

7 Discussion

While the speed and severity of viral outbreaks is often acute [Altig *et al.* 2020], developing countries often grapple with other fast-moving aggregate crisis that heighten uncertainty in short spaces of time – currency/commodity price fluctuations, or the threat of conflict or trade frictions are other examples where economic uncertainty can rise rapidly. Understanding how the increased uncertainty from such aggregate shocks is transmitted through labor markets is critical for determining individual well-being and future economic prospects of the economy as a whole. We exploit a 10-year panel of firms and workers, using the lens of the pandemic, to provide three fundamental and novel insights on the labor market impacts of heightened uncertainty arising from this aggregate shock.

First, a key survival strategy for firms at the outset of the shock was to lay off more skilled and experienced workers because they have the highest earnings – a first-in-first-out strategy that we validate was successful in enabling firms to return to pre-pandemic levels of revenues and profits as the economy recovered. Second, despite being more exposed to the shock, high-skill trained workers remain resilient in their labor market outcomes over the crisis. In short, the returns to training survive through the pandemic. Third, key mechanisms for this resilience are that trained

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).

workers have certified skills and have accumulated more sector-specific skills pre-pandemic: both mechanisms enabling them to switch across firms in the same sector during the pandemic.

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

Upskilling and Aggregate Shocks Once we factor in trained workers' resilience to aggregate shocks in bad economic times, the returns to training are even higher than documented in our earlier work evaluating the intervention during good economic times alone – 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 trained workers have 28% higher earnings from wage/self-employment than control workers (Table 6), while pre-pandemic this earnings gain was 59% (Table 5). However, this does not value the utility gains from the insurance provided: trained workers remain resilient to shocks as severe, rapid and uncertain as the Covid-19 pandemic, even if they are initially more exposed to the shock because of firms' FIFO survival strategies.

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 in Uganda, it enabled individuals to build sector-specific human capital, and we selected workers into the evaluation based on their willingness to undertake training rather than take-up other short-term labor market opportunities.

Mechanisms in Good Times and Bad Our results show the mechanisms driving the returns to training differ in good times and bad. This speaks directly to wider concerns over returns to interventions interacting 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 during times of economic stability [Alfonsi *et al.* 2020, Bandiera *et al.* 2023]. Over the pandemic we find that certification remains critical because it allows trained workers to switch firms in the same sector after being laid off as part of firms' FIFO strategies. The accumulation of sector-specific skills is also key to ensuring the resilience of trained workers. These findings speak to the concern that if training programs are overly job-specific, the skills provided may hinder workers adaptation to shocks [Acemoglu and Autor 2011, Deming and Noray 2020]. We find this not to be the case because of the multiple mechanisms through which skills build resilience to 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 mechanisms we uncover driving the returns to training. Other mechanisms – such as trained workers accumulating more savings or different job search capital – might be more relevant to how they cope with more gradual economic downturns.

Policy We extend a mature empirical literature on the dynamics of displaced workers which is almost exclusively based in high- or middle-income settings, to a low-income setting in sub Saharan Africa, where the highest risks of job loss actually exist [Donovan *et al.* 2023, Carranza and McKenzie 2024]. Our analysis provides new insights for labor market policy in developing countries. First, absent formal safety nets, it is in such settings that the demand for social insurance is high, and indeed such worker-targeted policies are now beginning to be implemented in the developed world [Gerard *et al.* 2023]. Our results suggest that even absent social insurance, the provision of certified vocational training can enable workers to remain resilient to fast moving aggregate shocks – although the persistent impacts on earnings and employment for even high-skilled individuals suggests there still remains a valuable role for insuring workers.

However, our results reveal a deeper insight, on the potentially high returns to firm-side policies to help firms avoid FIFO strategies and hoard productive labor in times of aggregate crisis. Such policies are widespread in middle- and high-income countries and shown to be effective in response to fast moving aggregate shocks such as the pandemic [Guerrero *et al.* 2022, Giupponi and Landais 2023, Gourinchas *et al.* 2025]. Extending such policies to the lowest income settings might be even more effective than policies targeting workers given it is firms' FIFO strategies that trigger job loss among skilled and experienced workers, and it is this spark that leaves the economy vulnerable to the long run loss of productive worker-firm matches, casting a long shadow of consequences for the growth of firms and the economy as a whole.

A Appendix

A.1 Worker Attrition

We consider differential attrition between treated and control groups. To do so we re-estimate the correlates of attrition between baseline and waves 4 to 7, further including interactions between baseline characteristics and treatment. The baseline characteristics we consider are those that could affect behaviors and labor market outcomes during the pandemic: whether the worker reports having any sector-specific skills, their cognitive skills, their perceived locus of control, gender, their desired sector of training, whether they reside in Kampala, and whether they were employed at baseline. The results in Table A5 show that on most margins and survey waves we find little evidence of heterogeneous attrition between treated and control groups, either before or during the pandemic. However, those with any sector-specific skills and resident in Kampala at baseline are significantly less likely to be tracked until survey wave 4.

Table A6 re-examines balance of baseline labor market outcomes of non-attriters by survey waves 4 to 7. 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.

A.2 Tasks

To validate that acquired skills are relevant to our study sectors, we consider tasks workers conduct at work. We measure tasks in the third follow-up survey. For each sector, we construct a list of 30 to 40 worker tasks (based on the O*NET task list).[21] For any given task j in sector k, we construct the share of workers reporting performing task j, separately for compliers and controls. Figure A3 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. In each sector we see a divergence from the zero line in the differences in these shares: within a sector, there are some tasks performed relatively more by vocationally trained workers (at the right hand side of each panel), and others performed relatively more by controls (at the left hand side of each panel). In three of the four sectors, a Chi-squared test rejects the null that the task composition of workers is the same between vocationally trained and control workers.[22]

A.3 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 A10 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, trained workers have significantly higher beliefs than controls. In wave $L1$ the magnitude of the effect is 1.27 (on a 0-10 scale), a 27% increase over controls (reweighted for compliance probability). This divergence in beliefs more than doubles between trained and control workers later in the pandemic. Columns 2 to 4 show treatment effects on expected earnings if workers could transition into their most preferred study sector job. Among compliers, in each pandemic survey, they significantly revise upwards their minimum expected earnings, their maximum expected earnings are revised upwards by a greater extent, and their average expected

[21] 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/

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

earnings shift forward. We again observe a divergence in beliefs along this margin between trained and control workers later in the pandemic: the gap in expected earnings is twice in magnitude in wave R relative to that in wave $L1$.

A.4 Heterogeneity

Gender A major lesson from the pandemic, across high- and low-income settings, was the gendered nature of impacts of lockdowns because: (i) women's labor force participation was more affected because the sectors they engage in are more sensitive to social distancing [Alon *et al.* 2022]; (ii) the unequal distribution of housework and care duties [Adams-Prassl *et al.* 2020]. That might be especially relevant in the Ugandan setting where schools were locked down for a long period. The first set of results in Table A11 thus consider how the returns to training over the pandemic vary by gender. In Panels A and B we see that the cumulative ATT effects of training are larger for women on many margins. The most striking contrast is in shifts into casual work. Among men, we see that trained workers are 26% less likely to shift into casual work. However, among women, trained workers are 40% *more* likely to shift into casual work than controls. This is exactly in line with the findings of Alfonsi *et al.* [2023] in the context of urban Uganda, and those of Chakravorty *et al.* [2023] in rural India. We find these differential shifts into casual work lead to earnings from casual work for trained women to rise slightly relative to control women, while they fall for trained 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 trained women.[23]

Desired Sector of Training Workers who originally desired to be trained in one of the manufacturing sectors in which we offered vocational training might differ in other unobserved ways from those who desired to work in one of the service sectors in which we offered training – say because the latter has more face-to-face trade taking place that could also be differentially impacted during the pandemic. Given desired sector of work correlates highly to the sector treated workers are actually trained in, this also proxies for whether the individual spends most of their working life in manufacturing or service sectors. In Panels C and D we see that extensive margin impacts are similar across those who desired to work in manufacturing and services. The most notable divergence again occurs with respect to shifts into casual work. For those who preferred to work in manufacturing, trained workers spend 28% less time in casual work, in line with our baseline results. In contrast, among those that preferred to work in services, trained workers spend 15%

[23] Alfonsi *et al.* [2023] track 700 young urban vocational trainees in Uganda – these graduated from similar VTIs and followed similar sector-specific courses as in our work. Chakravorty *et al.* [2023] study the dynamic labor market outcomes for 2000 vocational trainees in India, focusing on a sample of rural youth. Our results by gender are also in line with the evidence on differential impacts of job loss across genders in high-income settings, 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.* 2024].

more time in casual work. Both sets of trained workers retain a large advantage over the pandemic to controls in terms of total earnings and earnings from wage/self-employment.

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

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

A.5 Robustness to Attrition

We address concerns related to attrition using multiple approaches following Blattman *et al.* [2020] and using the sample through the three pandemic survey waves (i.e. waves 5, 6 and 7). The results are shown in Table A12 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.[24]

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

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

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

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

A.6 Health and Other Experiences of the Pandemic

We consider the role of health interacting with labor market outcomes, and whether these interactions differ between trained and control workers. In Table A14 we first consider self-reported health in the third worker survey wave (2016). Pre-pandemic, we find no difference in treated and control workers' reported health status (Columns 1 and 2). We then examine health and search behaviors over the pandemic. Across all time periods, we find no evidence of differential behaviors between treated and control workers.[25]

Workers might experience the pandemic differently in other ways. Columns 1 to 3 of Table A15 focus on experiences of lockdown. In Column 1 we see that treated workers are 14pp *more* likely to report that during the first lockdown, everything was completely shut down except for essentials (relative to 69% of controls reporting this). In Columns 2 and 3 we asked about difficulties experienced during each lockdown. The responses from controls in waves $L1$ and $L2$ are in line with the notion that the second lockdown was less strict. We find no difference in reports of the severity of each lockdown from treated and control workers in terms of getting to food markets, but treated workers are 7.6pp more likely to report difficulty in being able to buy food during the first lockdown. Columns 4 to 6 ask about coping strategies. We see no differences between workers in terms of them reporting having to reduce the number or size of meals, having to sell assets, or moving in the period prior to the survey. Finally, we examine whether workers differ in

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

their expectations of economic recovery. At the outset of the pandemic, 27% of control workers expected the economy to rebound within six months (Column 7) and 66% of them expected it to rebound within a year. We see no differences in these expectations between treated and control workers. This contrasts sharply to the differential expectations of these groups of workers about their own labor market outcomes (Table A10).

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Table 1: Firm Characteristics

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

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

Notes: All data comes from the firm-side surveys or the second census of firms conducted in 2017. Column 1 reports firm outcomes at baseline, for firms operating in one of the eight study sectors. Column 2 reports firm outcomes at baseline for those firms that do not attrit by the first pandemic firm survey, or fifth survey overall. Column 3 reports the p-value of the t-test comparing the means in Columns 1 and 2. Column 4 reports outcomes for non-attriting firms in the first pandemic firm survey, or firth survey overall. Column 5 reports the p-value of the t-test comparing the means in Columns 1 and 4. Column 6 reports outcomes for firms in the 2017 firm census, for firms operating in one of the eight study sectors. Column 7 reports the percentile of data from the Census of firms that the wave 5 non-attriters outcomes, as measured at survey wave 5. In Panel D, outcomes are measured at the first follow-up. The number of customers per week is the number of customers that made purchases at the firm in the last week, while the maximum number of customers in a good week is the maximum number of customers the firm typically has in a week when demand is particularly high. Our firmside surveys ask firms to list and answer questions about a maximum of five firms with whom they interact/communicate. In Panel D, the number of social or business ties to other firms is the number of firms that surveyed firms then list as part of their network. The number of supply chain ties is the number of the firms within the network that sell/buy inputs from the surveyed firm. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 2: Uncertainty Faced by Firms

p-values of test of equality in brackets

		Firm Survey Wave 5	Firm Survey Wave 6	[p-value]
		Oct-Dec 2020	May-Jul 2021	
		(1)	(2)	(1) = (2)
A. Pandemic				
Expect a new lockdown within six months	*Likely*	.180		
	Neither	.198		
	Unlikely	.580		
Expect the economy to rebound within six months	*Likely*	.399	.334	[.003]
	Neither	.161	.129	[.046]
	Unlikely	.429	.508	[.001]
If there were another lockdown, expect to reopen after	*Likely*	.430	.580	[.000]
	Neither	.170	.110	[.000]
	Unlikely	.370	.290	[.000]
B. Labor Demand and Sales				
Expected firm size in the next six months as a percentage of current size		1.36	1.24	[.019]
		(1.12)	(.972)	
Expected sales as a percentage of 2019 sales		.504	1.09	[.000]
		(.484)	(1.38)	
C. Supply Chains				
Share of suppliers that closed down since lockdown (compared to pre-pandemic)		.106	-	
		(.253)		
Share of suppliers at time of survey that are "new" (never supplied by them before)		.056	.054	[.769]
		(.171)	(.155)	

Notes: All data comes from the fifth and sixth rounds of firm-side surveys, fielded in October-December 2020 and May-July 2021 respectively. Column 3 reports a test of the equality of means of outcomes across the survey waves. Panel A presents firms' expectations about the pandemic. Sums might not report to one as firms could also respond 'don't know'. Panel B reports expectations of firm owners regarding firm size and sales. In Wave 5 and Wave 6, firm owners are asked to estimate the number of workers they expect to hire in the next six months, and the number of workers they expect to layoff. These expectations are collected at the time of the survey (Oct-Dec 2020 for Wave 6 and May-Jul 2021 for Wave 6). To compute the expected firm size six months from the time of the survey, we use the latest available data on firm size, which corresponds to Jul-Aug 2020 for Wave 5 and Apr-May 2021 for Wave 6, and we add the expected number of hires and subtract the expected number of fires. In Panel B, we report the expected firm size as a percentage of firm size at the time of the survey. For Wave 5, this means we express expected firm size six months ahead as a percentage of firm size reported in Jul-Aug 2020. For Wave 6, expected firm size is expressed as a percentage of firm size in Apr-May 2021. Expected sales in 2020 and 2021 are expressed as a percentage of the firm's 2019 sales. In Wave 5 (Oct-Dec 2020), firms were directly asked to estimate their expected 2020 annual sales as a percentage of their 2019 sales. In Wave 6 (May-Jul 2021), firms were asked to report their expected sales for 2021. To ensure comparability across waves, we compute expected 2021 sales as a percentage of the actual 2019 sales reported in Wave 5. Panel C shows disruptions to firms' supply chains over the course of the pandemic. The first row reports the share of suppliers that had closed down or relocated since the first lockdown, as a share of the suppliers the firm was buying inputs from prior to the lockdown. This question was only asked in Wave 5 (Oct-Dec 2020), so the data refer to changes observed during the first lockdown period. The second row reports the share of suppliers at the time of the survey that are "new", i.e. suppliers from whom the firm was not buying inputs from before the first lockdown (Wave 5) or before the second lockdown (Wave 6). This measure is available in both waves and captures changes in supplier composition relative to each lockdown.

Table 3: Firm Dynamics Over the Pandemic

OLS Panel regression coefficients, robust standard errors in parentheses

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

Notes: All data comes from the fifth and sixth round of firm-side surveys. OLS estimates are shown with robust standard errors in parentheses. All specifications control for the following baseline firm characteristics: a dummy for whether it operates in a manufacturing sector, age, whether the owner is female, the owner's age, and a dummy for whether the firm is in Kampala. To account for missing firm variables at baseline, we set the missing values equal to zero and include a dummy for whether the variable was missing at baseline. At the foot of each Column we report a test of the equality of coefficients between the April 2020 and April 2021 time frames. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 4: Retention and Recruitment of Workers

p-values of test of equality in square brackets

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

Notes: All data comes from the fifth and sixth round of firm-side surveys. The sample covers firms in the eight study sectors. In Panel C, outcomes are conditional on the firm having tried to recruit new workers in the indicated period. In Column 3, we report the test of the equality of means between March 2020-November 2020 and December 2020-June 2021. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table 5: Labor Market Outcomes Pre-pandemic

ITT and ATT estimates, robust standard errors in parentheses

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

Notes: Panel A reports OLS ITT estimates, while Panel B reports 2SLS ATT estimates, where robust standard errors are in parentheses. The outcome in Column 1 is a dummy for whether the individual reports having any sector-specific skills, measured at third follow-up. The outcome in Column 2 is a sector-specific skill test score (which ranges from 0 to 100), administered in the third follow-up. The skills test assesses worker skills in the sector of training for treated workers or in the most preferred sector of training for controls. For those who report having no sector-specific skills, we assume they answer the test at random and so obtain a score of 11. In Columns 3 and 4, the dependent variables are labor market outcomes in 2018 (Wave 4). In Columns 5, 6, and 7, the outcomes are cumulative labor market outcomes from the first to the fourth follow-up, among a balanced panel of workers tracked over that period. At the foot of each column we report the mean (standard deviation) for each outcome among controls, and the reweighted mean (standard deviation) for each outcome among controls, where we reweight observations by their probability of compliance. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. In Columns 1 to 4 we also control for survey month. In Column 4, we control for the dependent variable at baseline, setting the missing values equal to 0 and including a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 6: 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)	(.430)	(.274)	(81.1)	(80.2)	(34.9)
Interpolated effects over 25 months							
Constant imputation	-.271	3.41***	.419	-.752	262*	358**	-95.7
	(.704)	(.918)	(.825)	(.532)	(153)	(151)	(67.7)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.51%***	***60.5%***	***2.93%***	***-21.4%***	***16.6%***	***28.1%***	***-31.4%***
Number of observations	708	607	708	708	683	683	683

Notes: The top Panel reports 2SLS ATT estimates, where robust standard errors are in parentheses. The lower panel reports interpolated estimates covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time-frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time frames. The reweighted control mean reweights observations by their probability of compliance. The Implied Treatment Effect is calculated dividing the ATT by the reweighted control mean. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age and, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table 7: Worker Mobility During the Pandemic

ATT panel regression coefficients, robust standard errors in parentheses

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

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

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

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

Table 8: Mechanisms

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

ATT estimates

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

Notes: Each panel reports interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time-frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time-frames. The reweighted control mean reweights observations by their probability of compliance. The implied treatment effect is calculated dividing the ATT by the reweighted control mean. In Panels B to E, we reweight Controls such that the distribution of the residualized reweighting variable is equivalent to that of compliers. When reweighting for continuous covariates, we first regress the covariate on worker characteristics (that are either measured at baseline or are time invariant) and then split the distribution of residuals into deciles -- using this to reweight controls so the distribution of residual deciles corresponds to that of the compliers. Non-compliers are not reweighted. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Figure 1A: Timeline of Surveys

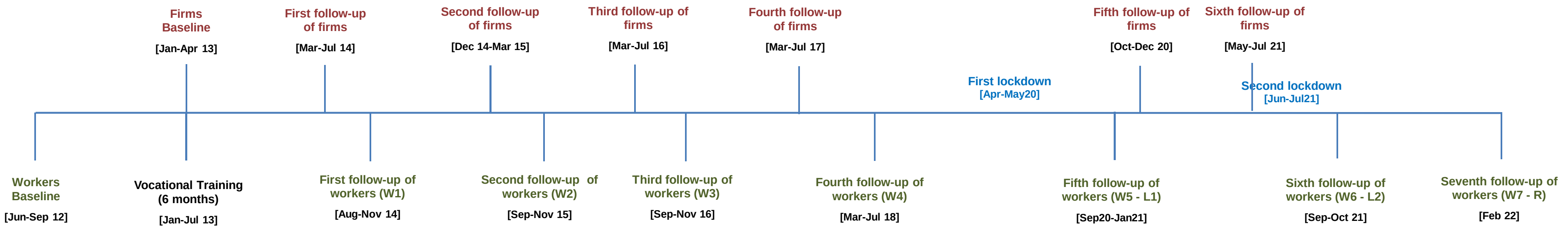
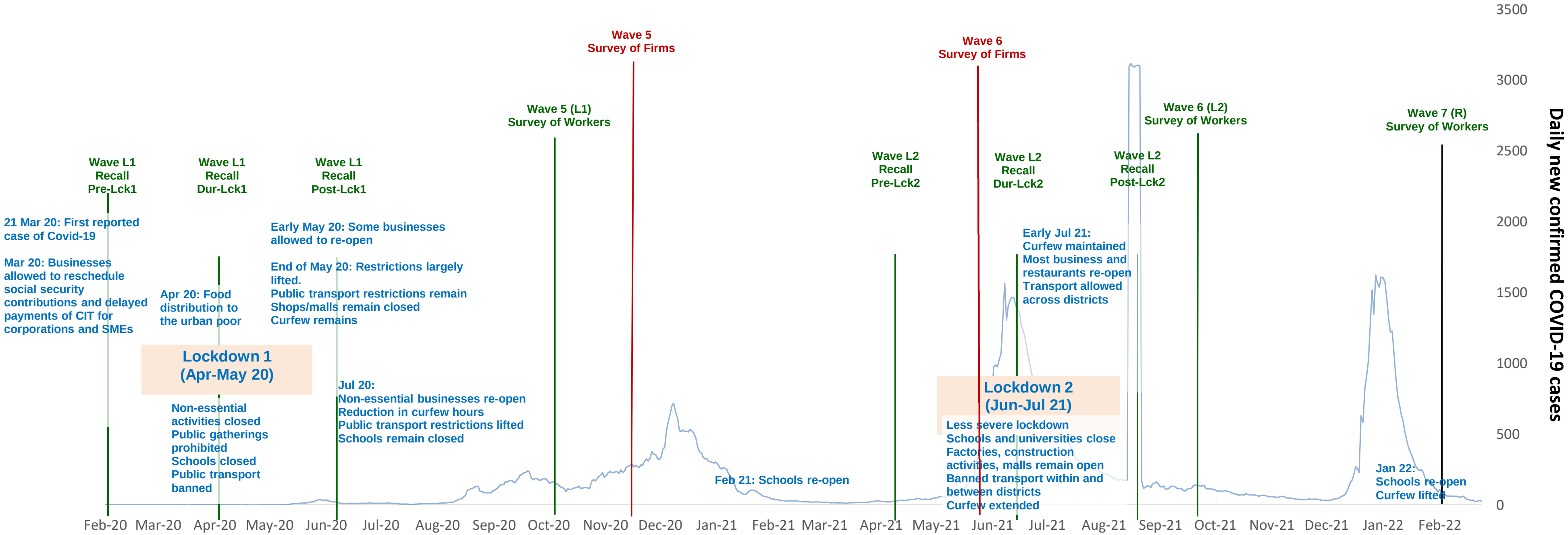
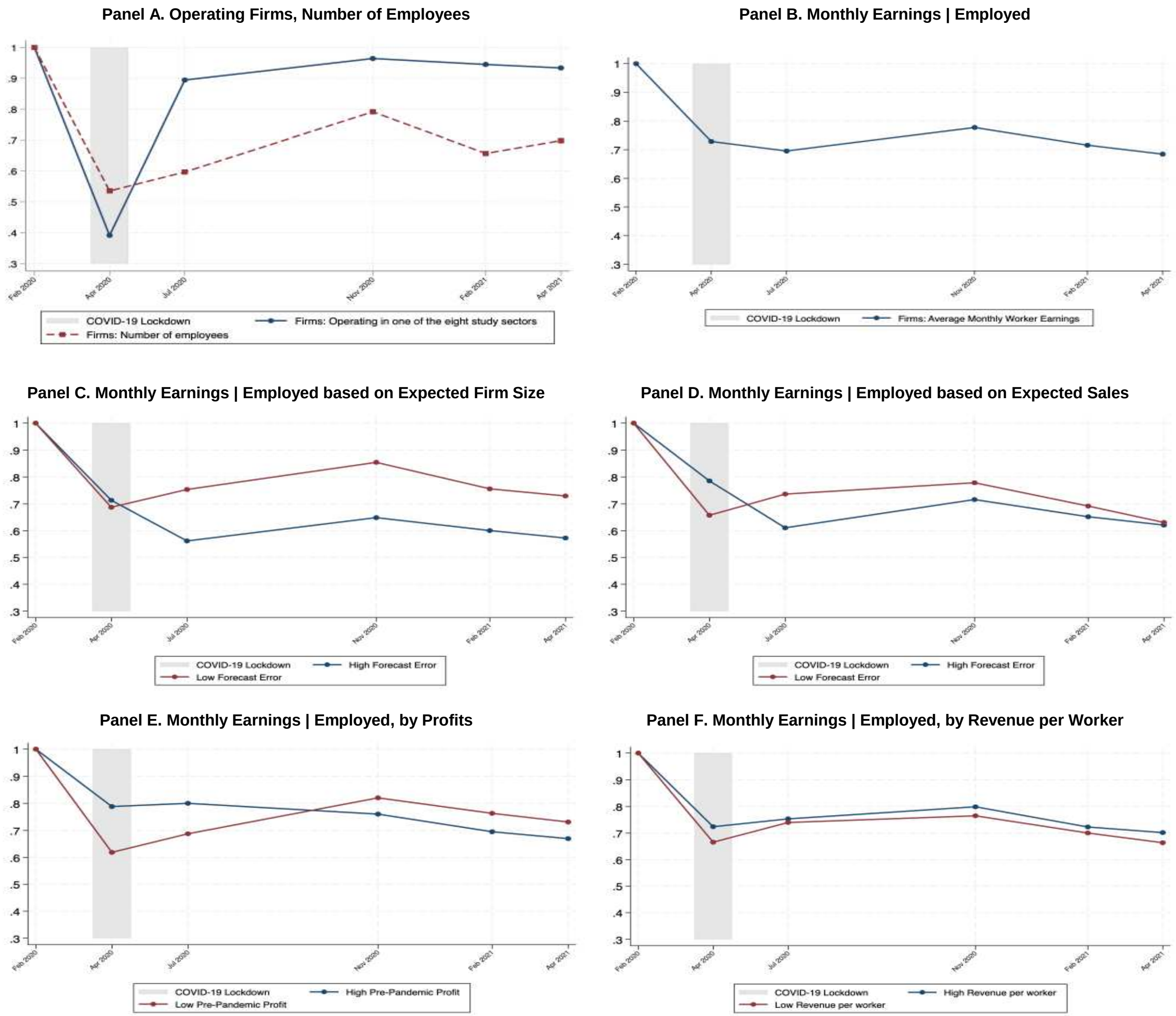


Figure 1B: Surveys, Confirmed Covid-19 Cases and Policy Responses



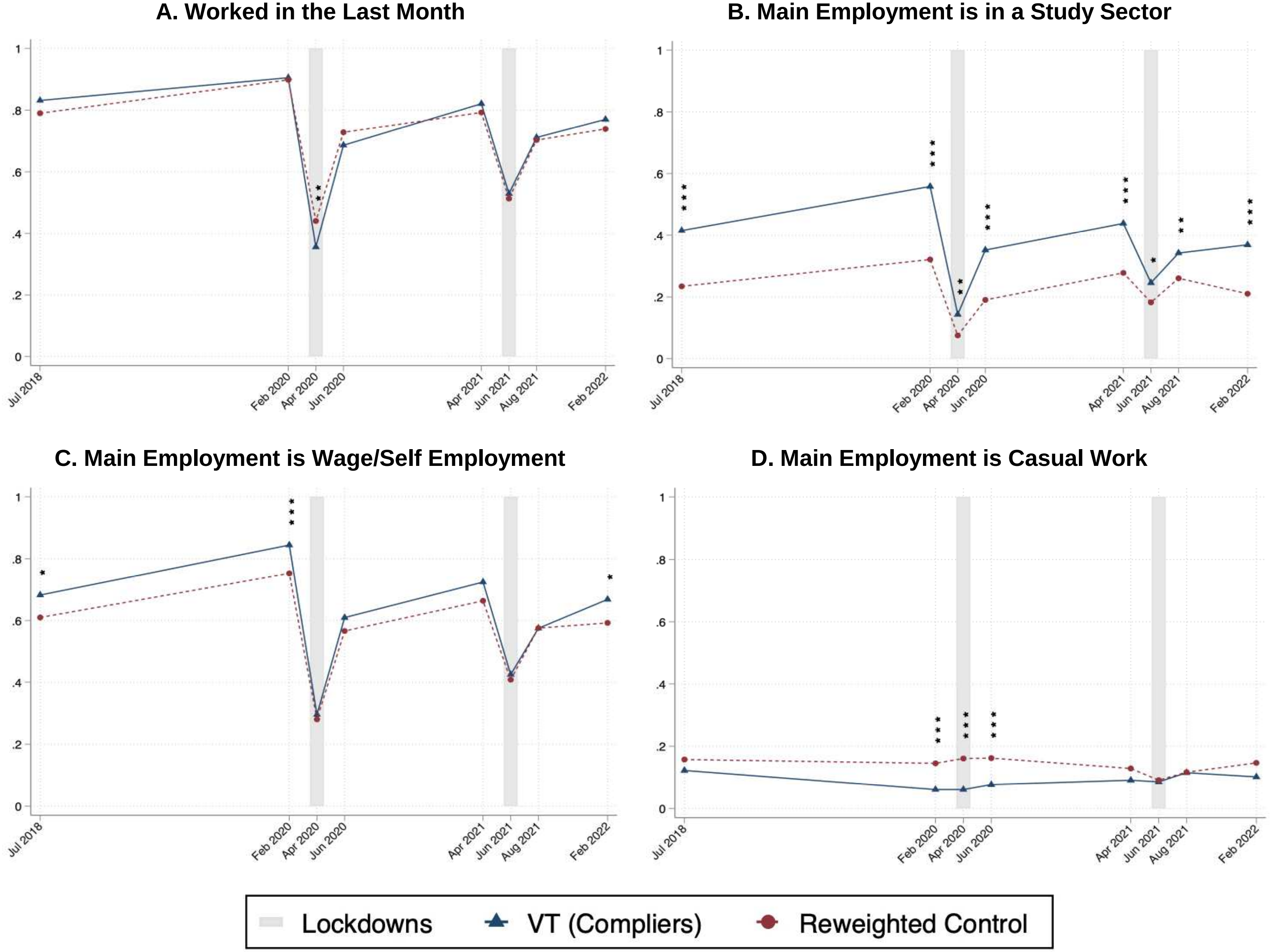
Source for Covid Cases Time Series: Our World in Data [https://ourworldindata.org/covid-cases]

Figure 2: Firm Outcomes Over the Pandemic



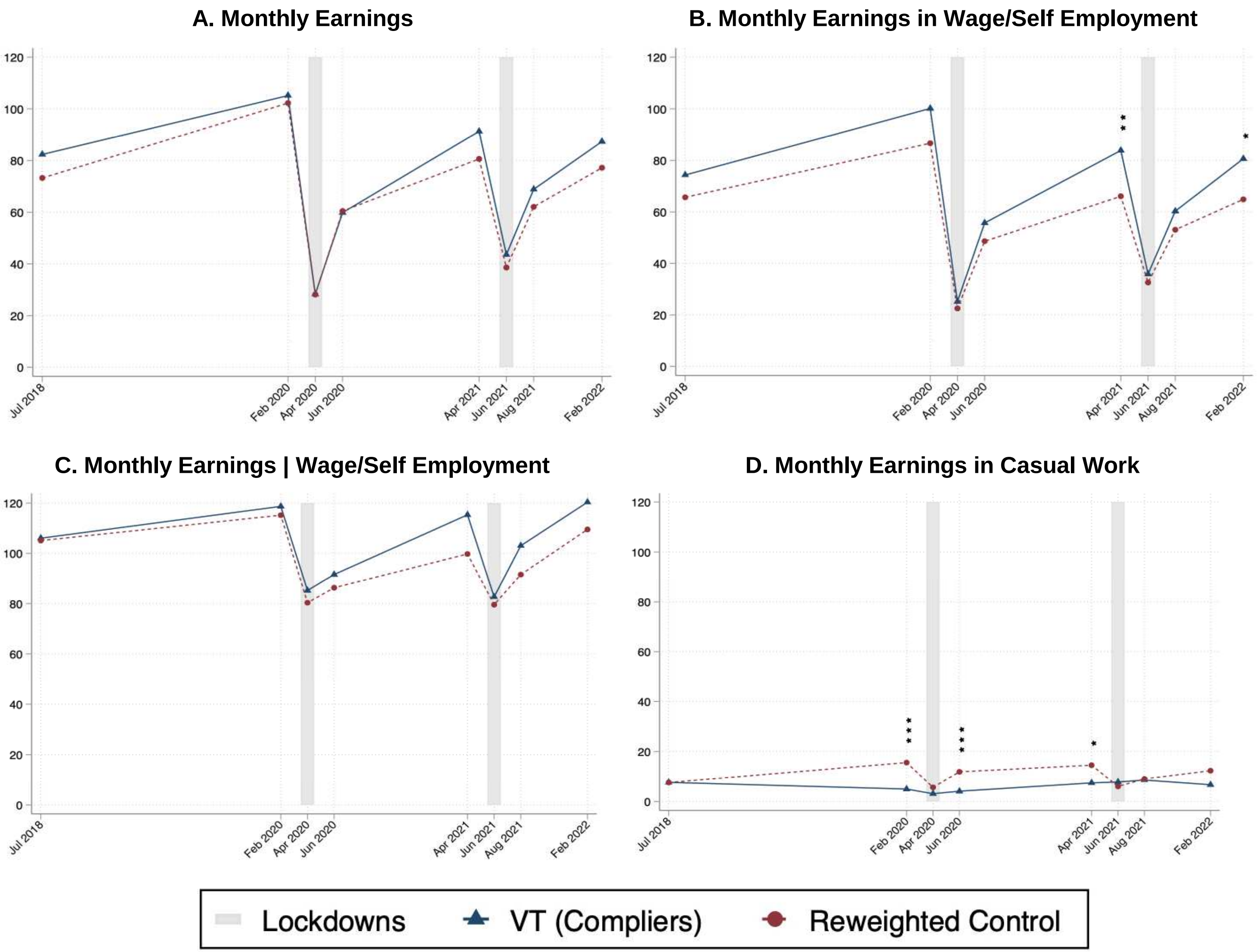
Notes: All data comes from the fifth and sixth rounds of firm-side surveys, fielded in October-December 2020 and May-July 2021 respectively. All firm outcomes are normalized to one at their February 2020 levels. The gray shaded region corresponds to the first COVID-19 lockdown. Panel A shows the share of firms operating and the number of employees at these firms, relative to February 2020. Panel B shows average monthly earnings per workers among operating firms (so among workers retained by operating firms). In Panel C, firms are split into high- and low-forecast errors based on their expectation of firm size. The forecast error is calculated as the absolute percentage difference between the expected and actual firm size [(expected – actual)/expected]. Expected firm size is constructed using data from Wave 5 (collected in Oct-Dec 2020). It is defined as the number of workers reported at the latest available point prior to the survey (Jul-Aug 2020), plus the number of workers the firm expects to hire over the following six months, minus the number it expects to lay off. The actual firm size is then observed in Wave 6 (collected in May-Jul 2021). Firms are classified as high or low forecast error based on whether their absolute forecast error falls above or below the median of this distribution. In Panel D, firms are split into high- and low-forecast errors based on their forecast accuracy for sales. This is constructed by first measuring the absolute percentage difference [(expected – actual)/expected] between expected sales for 2020 (reported in Wave 5) and actual 2020 sales (reported in Wave 6), and then splitting firms into above/below the median of this distribution. In Panels E and F firms are split by above/below median profits and revenues per worker respectively (within sector), as reported in the last pre-pandemic survey wave (Mar-Jul 2017). All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics.

Figure 3: Workers Employment Outcomes over the Pandemic



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

Figure 4: Workers Earnings Outcomes over the Pandemic



Notes: In each Panel we compare mean outcomes for compliers to the offer of vocational training to controls, where controls are reweighted by their probability of compliance. The first data point corresponds to Wave 4 conducted in 2018 before the pandemic survey waves. The stars in each time frame report the significance of these unconditional differences in each period. The gray shaded regions correspond to the first and second lockdowns. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Figure 5: 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.

Table A1: Baseline Balance on Labor Market Histories

Means, standard deviation in parentheses

p-value on t-test of equality of means with control group in brackets, P-value on F-tests in braces

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

Notes: Data is from the baseline worker survey. Columns 1 to 6 report the mean of each worker outcome, and the standard deviation for continuous outcomes. The reported p-values are derived from an OLS regression of the outcome of interest on a treatment dummy of whether the worker was offered vocational training, randomization strata dummies and a dummy for the implementation round. Robust standard errors are reported throughout. Column 7 reports the p-value from F-Tests of joint significance of all regressors from an OLS regression where the dependent variable is a dummy taking the value of zero if the worker is assigned to the Control group, and one for workers assigned to the corresponding treatment group and the independent variables are the variables in Columns 1 to 5 (the variable in Column 6 is dropped as it is missing for individuals who were not wage or self-employed in the month prior the survey). Robust standard errors are calculated. In Column 4, casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing, slashing compounds, and any type of agricultural labor such as farming, animal rearing, fishing, and agricultural day labor. In Column 5, workers who report doing no work in the month prior the survey have a value of zero for total earnings. The top 1% of earnings values are trimmed. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table A2: Baseline Balance on Worker Characteristics

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

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

Notes: Data is from the baseline worker survey. Columns 2 to 8 report the mean value of each worker characteristic, derived from an OLS regression of the characteristic of interest on a treatment dummy. All regressions include strata dummies and a dummy for the implementation round. The excluded (comparison) group in these regressions is the Control group. Robust standard errors are reported in parentheses throughout. The variable in Column 8 is a dummy equal to one if the applicant scored at the median or above on a cognitive test administered with the baseline survey. The test consisted of six literacy and six numeracy questions. Column 9 reports the p-values from F-Tests of joint significance of all the regressors from an OLS regression where the dependent variable is a dummy variable taking the value zero if the worker is assigned to the Control group and taking value one for workers assigned the offer of vocational training, and the independent variables are the variables in Columns 2 to 8. Robust standard errors are also calculated in these regressions. The p-values reported in the last row are from the F-test of joint significance of the treatment dummies in each Column regression where the sample includes all workers.

Table A3: Attrition and Survival of Firms

OLS regression coefficients, robust standard errors in parentheses

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

Notes: All data is from the firm side surveys. OLS estimates are shown with robust standard errors in parentheses. The outcome in Columns 1, 2 and 3 are whether the firm attrits between baseline and survey waves 4, 5 and 6 respectively. Firm owners can attrit at each survey wave 4, 5, and 6 either because they cannot be located, or are recorded as deceased, mentally ill, or having moved abroad. The outcome in Column 4 is whether the firm survives until firm survey wave 6, conditional on being open in the last pre-pandemic survey wave (Wave 4) and on not attriting in either wave 5 or wave 6. The covariates included in all Columns are collected at baseline, and we additionally control for a dummy for firms that were not approached at all. To account for the missing variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. In Column 4 the number of customers per week is the number of customers that made purchases at the firm in the last week, as collected at the first follow-up. Our firm survey also asked firms to list and answer questions about a maximum of five firms with whom they interact/communicate. The number of supply chain ties is the number of the firms within the network that sell/buy inputs from the surveyed firm. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A4: Worker Attrition

OLS regression coefficients, robust standard errors in parentheses

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

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

Table A5: Heterogeneous Worker Attrition

OLS regression, p-values reported

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

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

Table A6: Baseline Balance for Non Attriters, by Survey Wave

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

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

Notes: Data is from the baseline worker survey. Columns 1 to 6 report the mean of each worker characteristic, where standard errors are derived from an OLS regression of the characteristic of interest on dummy variables for the treatment groups. All regressions include strata dummies and a dummy for the implementation round. The comparison group in these regressions is control workers. Robust standard errors are reported throughout. In Column 4, casual work includes any work conducted in the following occupations where workers are hired on a daily basis: loading and unloading trucks, transporting goods on bicycles, fetching water, land fencing and slashing compounds. Casual work also includes any type of agricultural labor such as farming, animal rearing, fishing, and agricultural day labor. In Column 5, workers who report doing no work in the month prior the survey (or only doing casual or unpaid work) have a value of zero for total earnings. The top 1% of earnings values are trimmed. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Table A7: Compliance

OLS regression coefficients, robust standard errors in parentheses

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

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

Table A8: Labor Market Outcomes Pre Covid-19 by Matching Intervention

ITT and ATT estimates, robust standard errors in parentheses

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

Notes: Panel A reports OLS ITT estimates, while Panel B reports 2SLS ATT estimates, where robust standard errors are in parentheses. The outcome in Column 1 is a dummy for whether the individual reports having any sector-specific skills, measured at the third follow-up. The outcome in Column 2 is a sector-specific skill test score (that ranges from 0 to 100), administered in the third follow-up. The sector relates to the sector of training for treated workers or the most preferred sector of training for controls. In Columns 3 and 4, the dependent variables are labor market outcomes in 2018 (Wave 4). In Columns 5, 6, and 7 the outcomes are cumulative labor market outcomes from the first to the fourth follow-up, among a balanced panel of workers tracked over that period. At the foot of each column we report the mean (standard deviation) for each outcome among controls, and the reweighted mean (standard deviation) for each outcome among controls, where we reweight observations by their probability of compliance. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. In Columns 1 to 4 we also control for survey month. In Column 4, we control for the dependent variable at baseline, setting the missing values equal to 0 and including a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A9: Labor Market Outcomes Over the Pandemic

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

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

Notes: We report 2SLS ATT estimates, where robust standard errors are in parentheses. At the foot of each column, we report the reweighted mean (standard deviation) for each outcome among controls, where we reweight observations by their probability of compliance. In all specifications we control for randomization strata, implementation round, survey month, period fixed effects, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. At the foot of each Column, we also report the p-value from an F-test of the joint significance of the seven interactions reported in the table. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A10: 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 A11: 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%***
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	-.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%***
Number of observations	708	607	708	708	683	683	683

Notes: Each panel reports interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant between time frames not questioned about. The reweighted control mean reweights observations by their probability of compliance. The implied treatment effect is calculated dividing the ATT by the reweighted control mean. In Panels B-E, we reweight Controls such that the distribution of the reweighting variable is equivalent to that of compliers. Non-compliers are not reweighted in this exercise. In Panel C the preferred training sector being manufacturing is if the sector of interest reported at baseline was either motor-mechanics, plumbing, construction, welding, or electrical work. In Panel D the preferred training sector being services is if the sector of interest reported at baseline was either hairdressing, tailoring or catering. In all specifications we control for randomization strata, implementation round, the desired sector at application, and the following worker characteristics at baseline: age, dummies for whether the worker is married, has any children, is employed, and whether they have a higher than median cognitive test score. To account for the missing demographic variables at baseline, we set the missing values equal to 0 and include a dummy for whether the variable was missing at baseline. The top 1% of earnings values are trimmed. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD. ***denotes significance at the 1% level, ** at the 5% level, * at the 10% level.

Table A12: Robustness to Attrition

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

ATT estimates, robust standard errors in parentheses

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

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

Table A13: Search Behavior Over the Pandemic

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

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

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

Table A14: Health

ATT estimates, robust standard errors in parentheses

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

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

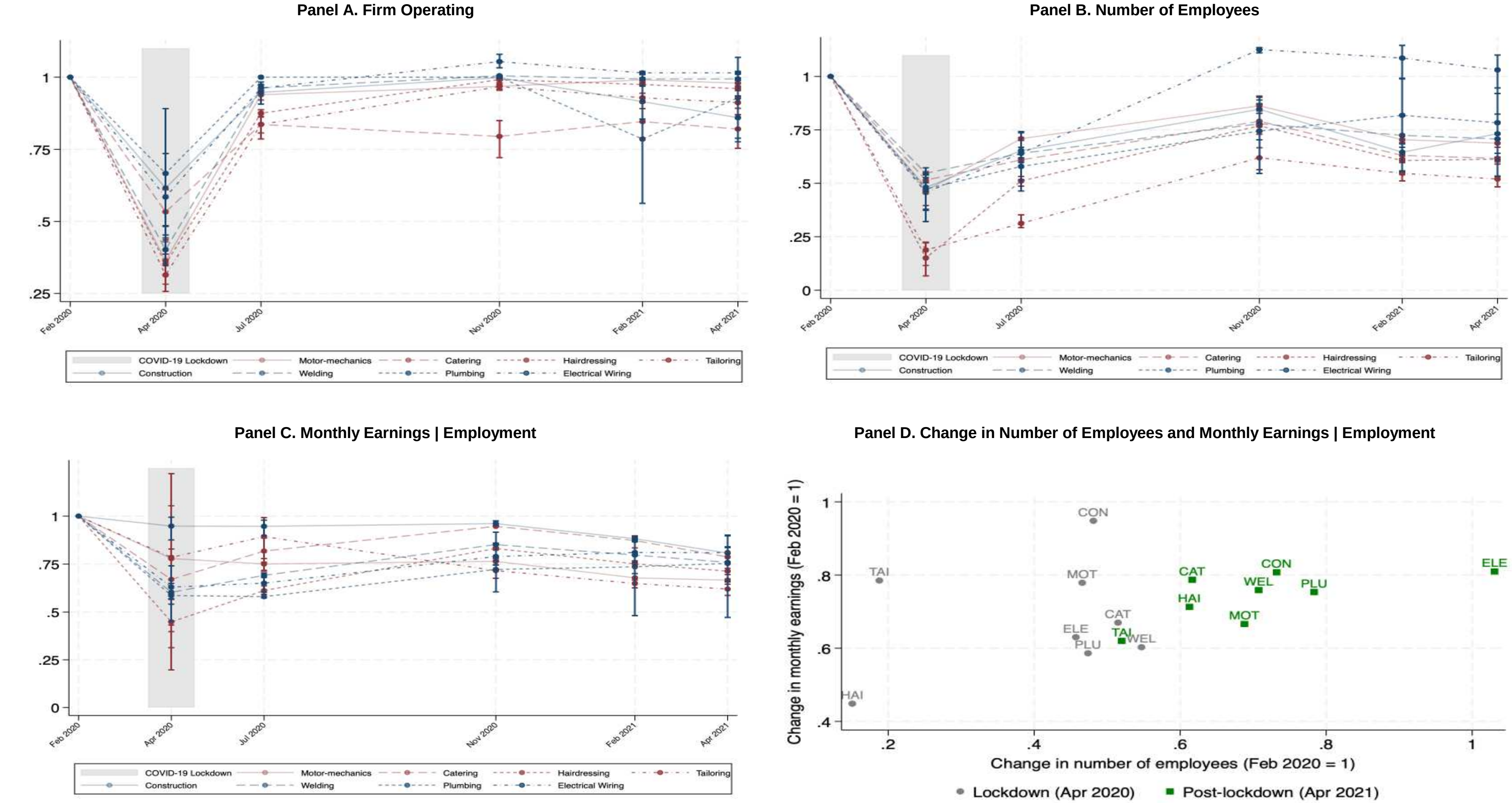
Table A15: Experiences of the Pandemic

Regression coefficients (ATT), robust standard errors in parentheses

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

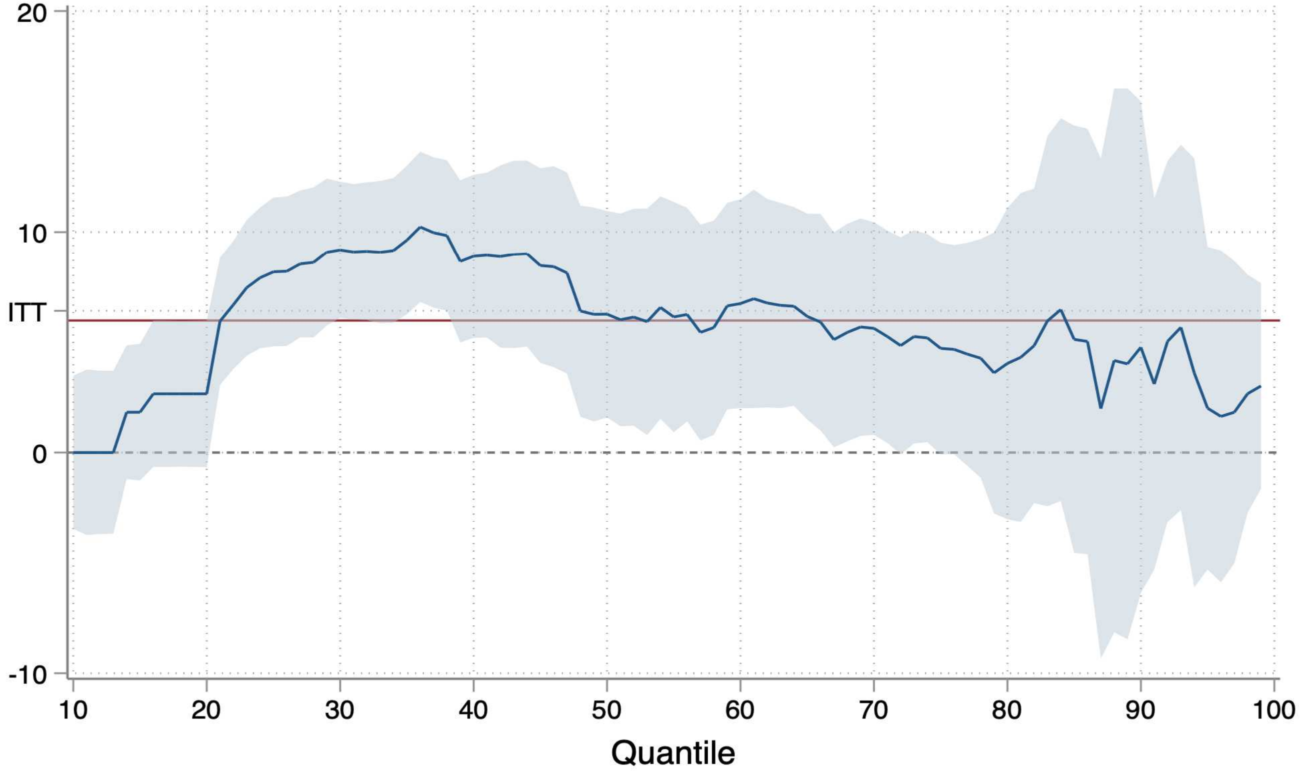
Notes: The data is from the fifth, sixth and seventh worker follow-up surveys. Survey wave 5 (L1) was conducted between September 2020 and January 2021 and spans the first lockdown, while survey wave 6 (L2) was conducted between September 2021 and October 2021 and spans the second lockdown. We report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

Figure A1: Firm Dynamics Over the Pandemic, by Sector



Notes: All data comes from the fifth and sixth rounds of firm-side surveys, fielded in October-December 2020 and May-July 2021 respectively. The gray shaded region corresponds to the first COVID-19 lockdown. All firm outcomes are normalized to one at their February 2020 levels. 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 operating). Panel C shows average monthly earnings per workers among operating firms (so among workers retained by operating firms). Panel D plots the relative change in average firm employment and average monthly earnings per retained employee, with each point corresponding to a sector in April 2020 (first lockdown) and April 2021 (post-lockdown period). In Panels A to C, 95% confidence intervals are reported. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics.

Figure A2: QTE on Sector-Specific Skills

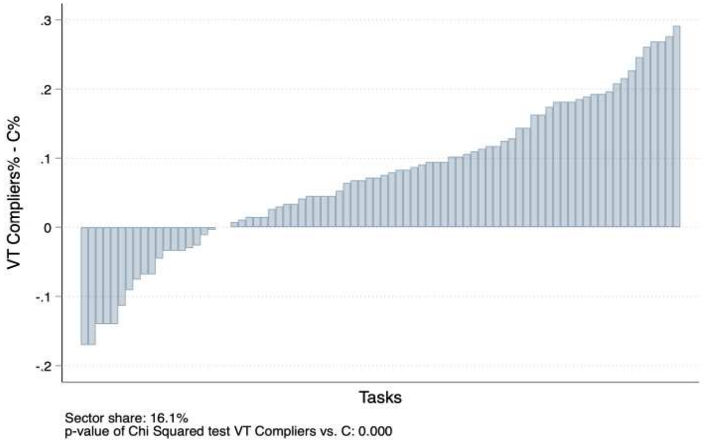


Notes: The Figure reports quantile treatment effect estimates of the offer of training on the sector-specific skills test score (which ranges from 0 to 100) and 95% confidence intervals. The tests were administered in the third follow-up. The sector relates to the sector of training for treated workers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

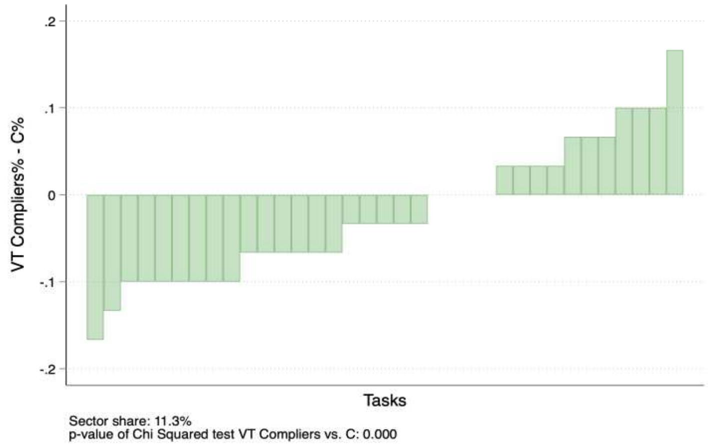
Figure A3: Tasks Performed by Vocationally Trained and Control Workers

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

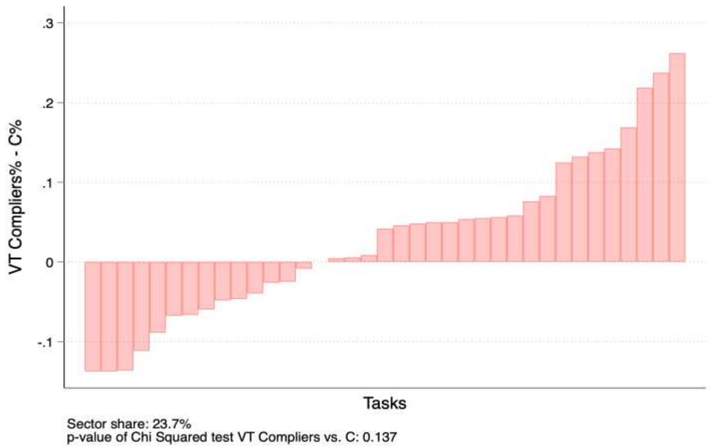
A. Motor Mechanics



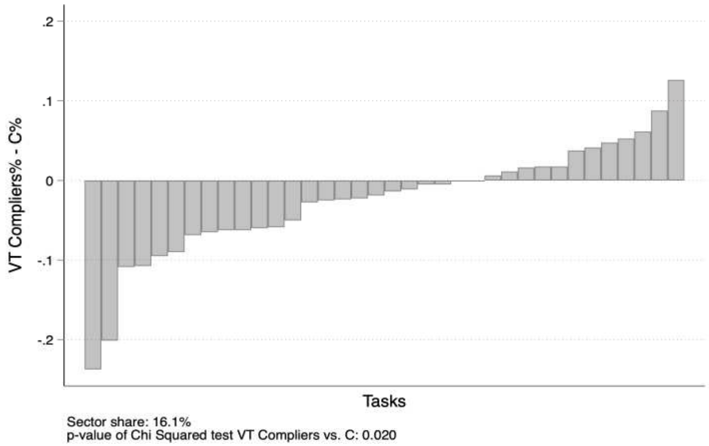
B. Electrical Wiring



C. Hairdressing



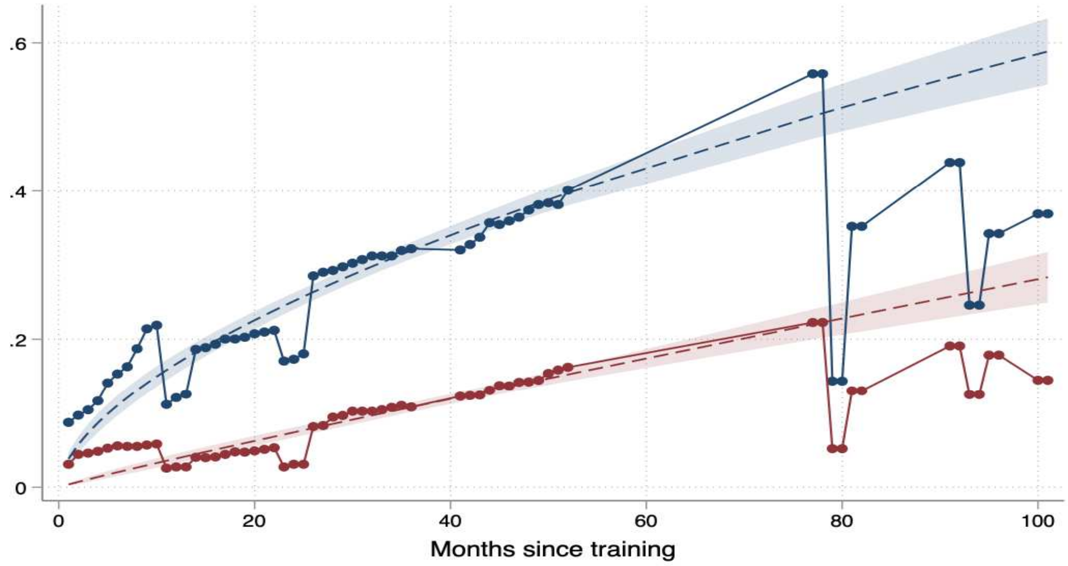
D. Construction



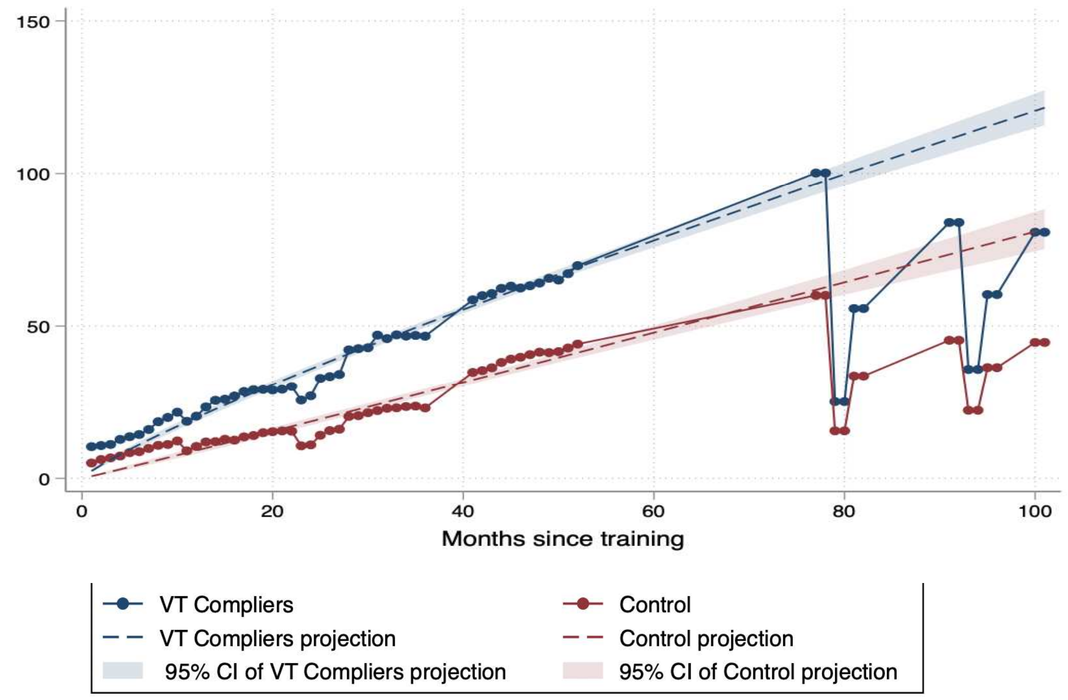
Notes: In the third worker follow-up survey we compiled a sector-specific list of tasks that workers in each sector are expected to be able to perform. We ask respondents whether they can perform each task, for the sector in which they are employed. Each bar in the graph represents a different task.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

Figure A4: Projected Outcomes in Counterfactual Absent Covid-19

A. Main Employment is in a Study Sector

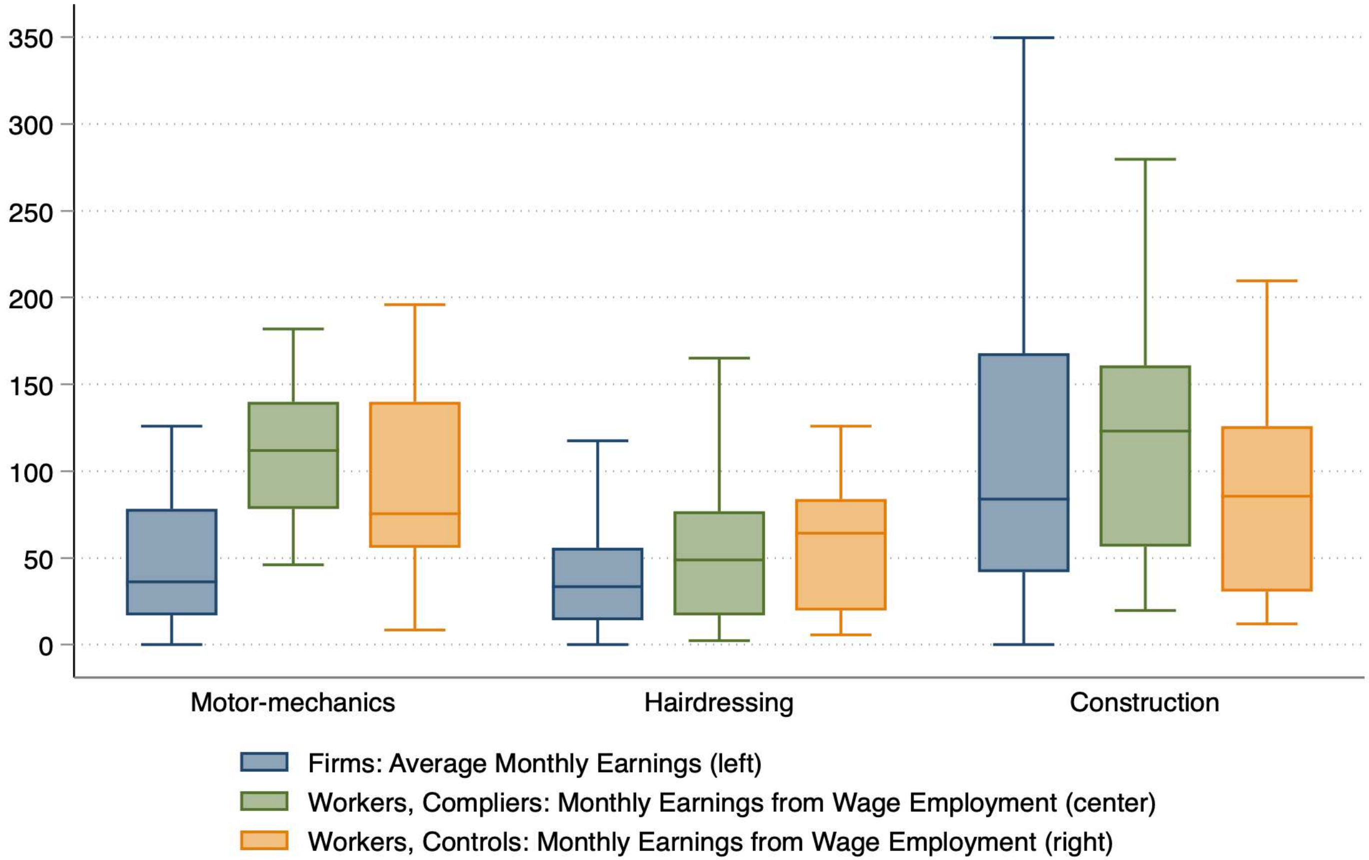


B. Monthly Earnings from Wage/Self Employment



Notes: The projections use data from all worker surveys. Monthly data was collected from waves 1 to 4. From survey wave L1 (2020) onward, respondents were asked to recall information about the last month's activity. For the pandemic survey waves, we interpolate outcomes for missing months. We plot trends and projections for compliers and controls, where controls are reweighted for their probability of compliance, and 95% confidence intervals of the projections are shown. The projections were estimated with a power function using data up until the last pre-pandemic period. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.

Figure A5: Monthly Earnings in Wage Employment, by Sector



Notes: Panel D overlays the monthly earnings of the average worker in our firms' sample at April 2021 (sixth follow-up of the firm-side surveys) with the monthly earnings of our workers' sample in the same period (sixth follow-up of the worker-side surveys). Outliers beyond the interquartile range are winsorized. The firm sample is restricted to firms who were open and operating in April 2021 in three sectors: motor-mechanics, hairdressing, and construction. The workers' sample is restricted to VT compliers and controls who were wage employed in one of the three sectors in April 2021. All monetary variables are deflated at August 2012 prices, using the monthly consumer price index published by the Uganda Bureau of Statistics. Deflated monetary amounts are then converted into August 2012 USD.