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FIRM RESPONSES TO AGGREGATE SHOCKS AND IMPLICATIONS FOR WORKERS: LESSONS FROM THE PANDEMIC

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ABSTRACT

Developing countries frequently face aggregate shocks. We use the lens of the pandemic to understand how such shocks propagate through labor markets, from firm responses through to implications for workers. Our analysis uses a panel of Ugandan firms and workers, tracked with high frequency from 2012 to 2022. We find that firms' response to the fall in product demand due to the pandemic was to reduce labor demand and change the skills composition of retained workers. Firms disproportionately laid off skilled employees, namely those that received vocational training (VT) before joining the firm, rather than lay off non-VT workers who instead are trained on-the-job. We calibrate a model of firm profits to assess the relevance of three mechanisms driving such VT-biased responses: laying off non-VT workers is more costly because future hires need to be trained on-the job, non-VT workers are socially tied to firm owners, and the demand for goods/services produced by VT workers falls more than for those produced by non-VT workers. We then study the impacts of firms' VT-biased responses on workers' labor market trajectories, exploiting the fact that we randomly assigned individuals to the offer of VT in 2013. We confirm that VT workers are more likely to be laid off early in the downturn, but document that VT workers remain resilient, with the returns to VT remaining positive over the downturn. The mechanisms driving VT workers resilience are the certifiability of their skills and their greater accumulation of sector-specific experience pre-pandemic, enabling them to switch firms in the same sector later in the downturn. Our findings provide new insights for low-income labor markets on firm responses to aggregate shocks and consequences for workers across the skills distribution.

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A randomized controlled trials registry entry is available at
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1 Introduction

Developing countries frequently find themselves grappling with aggregate shocks – such as those stemming from energy, commodity and currency price shocks, the threat of conflict, or climate change. Understanding how such shocks propagate through labor markets, from firms to workers, is critical for determining the well-being of workers, and prospects for the growth of firms and the economy as a whole. We examine these issues through the lens of the Covid-19 pandemic, which at its peak, led to an estimated loss of 144 million jobs globally, with employment remaining below pre-pandemic levels through to at least 2022 [ILO 2022].

We use this episode to draw broader lessons on mechanisms driving firm responses to aggregate shocks, and implications of job loss during downturns for worker trajectories. We study the issue in a low-income country: Uganda. Labor markets across Sub-Saharan Africa share many characteristics with Uganda's, including a lack of: (i) policies supporting firms during shocks, including credit or subsidies to enable them to hoard labor through a downturn; (ii) social insurance for workers, so their resilience to aggregate shocks is crucial for lifetime welfare. We approach the issue in two stages. We first examine firm responses to the downturn in sales caused by the pandemic, through changes in labor demand and the skills composition of retained employees. We then examine the consequent impacts of these firm responses on workers, focusing on the differential impacts on high-skill trained and lower-skill untrained workers. Understanding varying impacts across the skills distribution is important because the resilience of high-skilled workers is key to preserving the value of human capital, sustaining productive worker-firm matches, and supporting long-run prospects for firm growth.

Throughout our analysis of firm and worker behavior, we pay close attention to delineate mechanisms that are pandemic-specific – such as those related to lockdowns or the health of workers – from more deep rooted mechanisms driving firm and worker behavior, and so more widely relevant for understanding the propagation and consequences of shocks through labor markets in other economic downturns.

Our analysis builds on our earlier work from the same project that utilized pre-pandemic data to study the returns to vocational training [Alfonsi *et al.* 2020], and how training impacted job search [Bandiera *et al.* 2023]. Our core analysis builds on a panel of firms and workers tracked from 2012, exploiting new rounds of high frequency data collected over the pandemic.

On the firm-side, our original study tracked 2000 SMEs in eight study sectors across manufacturing and services over five survey waves pre-pandemic (from 2013 to 2017). Firms in our study sectors operate in welding, motor mechanics, electrical wiring, construction, plumbing, hairdressing, tailoring and catering sectors. These sectors employ about 30% of individuals in Uganda aged 20-30 working outside of agriculture. Two features of these firms are central to understanding their response to aggregate shocks. First, they employ a combination of skilled and unskilled workers. Skilled workers include those with vocational training (VT) received from a vocational training institute. Non-VT workers are instead more likely to be trained on-the-job by firm owners. There is a substantial earnings premium to VT workers, in part because firms capture greater rents from workers trained on-the-job whose skills are not perfectly transferable to other firms. This has implications for the incentives of firms to retain VT and non-VT workers during a downturn. Second, these are multi-product firms. A useful classification of products is between (low-priced) simple and (high-priced) complex products. Simple products tend to be produced by less skilled workers, while complex goods/services use both skilled and unskilled labor. For example, in motor-mechanics, non-VT workers specialize in products like tube repairs, while VT workers engage in advanced services like engine repairs. The incentives for firms to retain VT and non-VT workers during a downturn then depend on the relative falls in product demand across simple and complex goods/services, and the degree of substitutability of the two groups of worker in production.

On the worker-side, our original study tracked 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 certified vocational training courses in 2013, in one of the eight study sectors from which our firm sample is drawn. This enables us to experimentally study how the labor market trajectories of high-skill vocationally trained (VT) workers and non-vocationally trained (non-VT) workers are differentially impacted because of the strategic responses of firms to the pandemic shock.

During the pandemic, surveys to our tracked firms were fielded in 2020 and 2021. Surveys to our tracked workers were fielded in 2020, 2021 and 2022. These surveys were timed around the two lockdowns in Uganda – the first occurred in April/May 2020, and the second in June/July 2021. In both firm and worker surveys, we 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 firm panel allows us to document the dynamic evolution of uncertainty firms faced, their responses in terms of labor demand, and the skills composition of retained and laid off workers. The resulting 10-year worker panel allows us to build a picture of the dynamic evolution of worker skills, employment, earnings, sectoral allocations, search behavior and savings. We know of no comparable data set that enables such an analysis of how any aggregate shock propagates to firms and workers in a developing country setting, while allowing the heterogenous impacts on workers across the skills distribution to be experimentally studied.

Our first set of results exploit the firm-side data to characterize firm owners expectations over the pandemic downturn. Towards the end of 2020: (i) the majority believed the economy would not rebound within six months; (ii) the number of customers and sales for 2020 were expected to be around half their 2019 levels, a large fall in product demand. We next document key margins of response of firms. First, they immediately reduced employment – labor demand fell to 55% of the pre-pandemic level during the first lockdown – an enormous shock in the space of just two months, with labor demand recovering post-lockdown, but to only 70% of the pre-pandemic level by mid-2021. Adjusting on the employment margin is the most natural response of firms to the downturn, in Uganda and other low-income settings because of low firing costs, and a lack of policies or credit availability helping firms to hoard labor over the downturn.

Second, in order to cut costs in the face of falling sales revenue, firm owners did not layoff workers at random. Rather, their response was to lay off more skilled workers – those with vocational training (VT) and the most experience within the firm. Our first key takeaway is that all firms adopted this form of *VT-biased* response to the pandemic downturn. We document this for firms irrespective of their exposure to the pandemic because of their reliance on face-to-face trade for example. Firms in sectors that are more exposed to face-to-face trade do experience larger falls in labor demand, but the composition of workers tilts in a similar VT-biased way for firms irrespective of their exposure.

We document three mechanisms for the use of VT-biased responses, each of which remains relevant in non-pandemic downturns. First, non-VT workers are more likely to be trained on-the-job. Hence relative to VT-workers, laying off non-VT workers entails greater future costs to the firm in terms of having to recruit and train unskilled hires. Second, non-VT workers are more likely to be socially tied to owners, in line with evidence from similar settings on employment as a form of social redistribution [Macchi and Stadler 2024, Swanson 2025]. This provides a second explanation for VT-biased responses, namely the implicit costs of firing a non-VT worker are higher than for a VT worker because of these social ties to firm owners. Third, in these multi-products firms, VT-biased responses can be optimal if the complex (higher priced) products produced by VT workers have a larger fall in product demand during the shock than the simpler (lower priced) products produced by non-VT workers.

Importantly for learning through the lens of the pandemic, these mechanisms are not specific to the pandemic: they remain relevant whenever firms face shocks to sales that lead to them needing to cut costs through reducing labor demand, and having choice over which workers to lay off.

We assess the optimality of firms' VT-biased survival strategies by developing a stylized model of firm profits. We calibrate the model to understand whether the VT-biased strategies pursued were privately optimal for firms relative to a natural counterfactual: following a skills-unbiased strategy that preserves the same ratio of VT to non-VT workers as were present in the firm pre-pandemic. This means that in this counterfactual, fewer VT workers and more non-VT workers are laid off than is actually observed. In this counterfactual we still match the observed fall in labor demand, and only vary the skills composition of retained employees. This exercise provides the second main takeaway from our analysis: firms VT-biased strategies can be rationalized through the existence of all three mechanisms above, although the quantitatively more important mechanisms are that non-VT workers are more costly to fire because firms internalize the future costs of having to train future hires, and social ties between firm owners and non-VT workers make their contemporaneous implicit firing cost higher.

The second part of our analysis builds on these findings to provide a complete analysis of how the pandemic downturn propagated through labor markets. We use the worker-side data to examine how firms' VT-biased strategies differentially impact the subsequent labor market dynamics of VT and non-VT workers, exploiting our field experiment offering certified vocational training to workers in 2013. We consider ATT estimates of outcomes between compliers that take-up vocational training and controls. We find that: (i) in line with firms's VT-biased strategies, VT workers were more exposed to the shock in terms of the loss of employment; (ii) despite this initial greater exposure, VT workers are resilient: they experience a V-shaped recovery in employment and earnings. Using these dynamics to quantify the returns to VT over the pandemic, we find that VT workers spend 61% more time employed in one of our study sectors than controls, and their earnings from wage/self employment are 28% higher. These cumulative impacts are around half those documented for the pre-pandemic period 2013-18, over which VT workers accumulated 117% more experience in one of the study sectors, and their earnings were 59% higher than controls.

Hence our third takeaway is that the returns to vocational training survive, despite such high-skilled workers being more exposed to the shock through firms' VT-biased responses. These returns go beyond measured earnings (as would be included in any IRR calculation) and include building resilience and insuring workers against aggregate shocks even as rapid and severe as the pandemic.

Although our results highlight the resilience of VT workers, they still strongly refute the idea that the pandemic downturn caused labor markets to merely freeze and quickly recover once lockdowns ended. Rather, we document persistent impacts of job loss in downturns in a developing country context. Comparing actual outcomes relative to counterfactual projections based on pre-pandemic trends reveals lasting impacts of the downturn, with VT (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 earnings loss estimates in the displaced workers literature, that typically uses administrative data from high-income settings [Jacobsen *et al.* 1993, Couch and Plaszek 2010, Davis and von Wachter 2011]. This can be due to the fact that as in most developing country contexts, there is a lack of social insurance for displaced workers.

To the extent that earnings reflect individual productivity, the fact that earnings for VT workers remain well below trend suggests productivity losses are large. In line with this, we document: (i) the V-shaped recovery experienced by VT-workers is *not* because they are re-hired by their original employer post-lockdown despite enjoying long tenure with their pre-pandemic employer; (ii) rather, VT workers demonstrate greater mobility across firms in the same sector over the downturn, but have to rebuild firm-specific experience with their new employer; (iii) productivity losses are exacerbated by some VT workers eventually switching into casual employment and many remaining unemployed even as the economy recovered. These outcomes reflect skills downgrading/depreciation, similar to patterns observed for displaced workers in the US and middle-income countries after economic shocks [Huckfeldt 2022, Dix-Carneiro *et al.* 2024].

We then examine generalizable mechanisms driving the returns to VT during the downturn. We build on the fact that on the eve of the pandemic, VT workers had accumulated greater experience in good sectors, different search capital and higher savings. These channels might cause treated and control workers to differ in their resilience. We examine these mechanisms following Hainmueller [2012], reweighting controls to match pre-pandemic covariate moments among compliers.

Our fourth takeaway is that sector-specific experience accumulated pre-pandemic sustains the returns to VT during the downturn. While VT workers were more mobile across firms within the same sector, consistent with certifiable skills facilitating job mobility, their depth of sector-specific experience explains much of their resilience [Topel 1991, Neal 1995, Kletzer 1998]. We find a more limited role for other mechanisms, such as VT workers using different job search strategies to controls during the downturn, or being able to use their higher pre-pandemic savings to build resilience. This reiterates the insurance value of vocational training during downturns operates primarily through labor market channels – the portability of certified skills and the value of sector-specific experience enabling workers to reallocate across firms within a sector – rather than through any financial buffer that higher pre-pandemic earnings of VT workers provides.

Although we focus on mechanisms insightful for responses to aggregate shocks more generally, an obvious pandemic-specific mechanism relates to health impacts on workers determining their trajectories. We establish 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 workers' concerns about health or Covid risks impacted job search behavior or job preferences.

Our work contributes to three literatures. First is work examining labor market impacts of the pandemic [Adams-Prassl *et al.* 2020, Egger *et al.* 2021, 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, focusing on impacts by gender [Alfonsi *et al.* 2023, Chakravorty *et al.* 2023]. We build on these by documenting causal impacts of vocational training on labor market dynamics over the pandemic, providing new insights on the firm-driven mechanisms why high-skilled workers are more exposed initially, and the worker-driven mechanisms enabling them to remain resilient. 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 administrative records on employment. They track workers from 2017 to 2021. Counter to our findings, they report the returns to training disappear – or are even negative – during the pandemic despite there being high returns pre-pandemic. Our results explain their findings, we go deeper in studying mechanisms driving the returns to vocational training in a downturn, and our two-sided data collection helps uncover firm-side responses to the aggregate shock that spark the negative impacts on trained workers to begin with.[1]

Our second contribution speaks to nascent concerns that returns to interventions might vary

[1] Barrera-Osorio *et al.* [2022] suggest three reasons for the returns to training becoming negative, but are unable to distinguish between them: (i) training was relatively short; (ii) service sectors were hardest hit; (iii) workers graduated from training around December 2018, so had little labor market experience pre-pandemic. We make progress on these issues as our workers are assigned to six months training in manufacturing and service sectors, and accumulate more sector-specific experience pre-pandemic – a key mechanism driving trained workers resilience.

due to their interaction with aggregate conditions [Rosenzweig and Udry 2020]. By evaluating the returns to vocational training in good economic times and bad, we document that returns are approximately halved in bad times, but remain positive. However, the mechanisms driving the returns in good times and bad differ – skills certification and job search behavior generate the returns to vocational training in times of economic stability [Alfonsi *et al.* 2020, Bandiera *et al.* 2023]. In contrast, in a downturn we find that while skills certification remains important because it enables workers to switch firms in the same sector, mechanisms such as trained workers' greater accumulation of sector-specific experience is also key to ensuring resilience. Search behavior might play less of a role in bad times because of the speed and uncertainty of downturns.

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, and has considered heterogeneous impacts of job loss by 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 less evidence from poorer countries where high risks of job loss exist [Donovan *et al.* 2023, Gerard *et al.* 2023, Carranza and McKenzie 2024]. Second, our panels of workers and firms allow us to understand how both sides of the labor market interact to drive labor market dynamics for workers across the skills distribution during the pandemic. This highlights that in a low-income setting firms respond using VT-biased strategies, but that vocationally trained workers recover from this exposure given their certifiable skills and accumulation of sector-specific experience. Given the mechanisms documented driving firm and worker behaviors, these lessons from the pandemic are valuable for understanding labor market dynamics in low-income settings in times of economic downturn more generally.

The paper is organized as follows. Section 2 describes our data. Section 3 documents firm responses to the pandemic and underlying mechanisms for VT-biased responses. Section 4 describes the field experiment. Section 5 documents treatment effects of vocational training on labor market outcomes over the pandemic. Sections 6 studies mechanisms sustaining the returns to vocational training. Section 7 draws together our findings from both sides of the labor market to discuss policy implications. The Appendix presents further results and robustness checks.

2 Data and Context

2.1 Data Sources

Firms We draw on firm data 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 selecting those: (i) operating in one of the eight 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. Two other features play a central role in understanding firm responses to shocks and consequences for workers across the skills distribution.

First, firms employ workers with differing skills. Skilled workers include those with vocational training (VT) received from a vocational training institute. Workers can also be trained on-the-job by firm owners, but there remains an earnings premium to VT workers, in part because firms capture greater rents from workers trained on-the-job whose skills are not certified, nor perfectly transferable to other firms. Hence VT and non-VT workers differ in their occupation specific human capital, which can impact labor market dynamics after job loss [Huckfeldt 2022].

Second, these are multi-product firms producing both (low-priced) simple and (high-priced) complex products. Simple products tend to be produced by less skilled workers, while complex goods/services use both skilled and unskilled labor. Figure A1, based on reports from workers in our surveyed firms, shows which products VT and non-VT workers help produce. For the four main sectors in our sample, we show the gap in the share of VT and non-VT workers engaged on each product. For example, in motor-mechanics, non-VT workers specialize in products like tube repairs, while VT workers are engaged in services like engine repairs; in hairdressing firms, non-VT workers are more likely to work on services like basic plaiting, while VT workers provide more difficult services like beauty services.

Workers To establish consequences for workers from how firms responded to 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. For those assigned to treatment, vocational training began in January 2013. 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 60%, with a reliance on casual work rather than wage/self-employment. Average monthly earnings were less than 10% of the Ugandan average in 2012.

2.2 Study Timeline

Panel A of Figure A1 shows the study timeline. Panel B narrows in on the pandemic period, overlaying it with the time series of confirmed Covid-19 cases and lockdown periods. 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. Uganda suffered relatively few cases of Covid-19, indeed the first lockdown was imposed when cases remained close to zero. The crisis can be thought of predominantly an economic shock rather than a health shock – we later document that health outcomes of workers do little to explain their labor market trajectories over the pandemic.[2]

The baseline firm survey took place between Jan-Apr 2013, and firms were tracked over four further surveys pre-pandemic. During the pandemic we ran two waves of (phone) surveys to firms. The baseline worker survey took place between Jun-Sept 2012, and workers were tracked over four further surveys pre-pandemic. During the pandemic we ran three waves of (phone) surveys to workers. For both firm and worker surveys fielded during the pandemic, we asked questions related to three time-frames of recall, enabling us track outcomes with high frequency – spanning just before, during and after lockdowns.

2.3 Firm Characteristics

Column 1 of Table 1 describes our baseline sample of 2300 firms in 2012. The average firm employs three workers, and generates monthly profits of $221. Around a third operate in manufacturing, half are in Kampala, and firms are six years old. Firm owners are in their mid 30s. Finally, we report measures of exposure to 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.

Attrition Only 16% of firms attrit by the fourth follow-up. Attrition rises to 28% in the pandemic surveys, with nearly all of this increase occurring between waves 4 and 5, which spans the last pre-pandemic and first pandemic survey waves. We have close to zero attrition between the pandemic surveys. Column 2 of Table 1 shows the *baseline* characteristics of firms that did not attrit by wave 5, our first pandemic survey. On most margins, at baseline non-attriters have similar characteristics to firms in our original sample. Column 4 shows the characteristics of non-attriting firms as measured in the first pandemic survey, with reference to the first recall period on the eve of the pandemic. By February 2020, non-attriting firms had grown significantly with almost double the number of employees, profits, revenues and customers per week since baseline.[3]

Representativeness By 2020, the firms tracked since baseline are no longer representative of firms in the study sectors. To gauge how positively selected tracked firms are, we exploit the fact that alongside our last pre-pandemic survey in 2017, we 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

[2] Uganda had very limited economic policy responses to support firms and workers during the pandemic. In March 2020, some formal firms were allowed to reschedule social security contributions and delay payments for three months, and in April 2020 a food distribution scheme to aid the 1.5million urban poor was started. In our firm sample, only 6% of firms 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.

[3] Columns 1 to 3 of Table A2 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.

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 (although their wage bill to revenue ratio remains relatively constant). This selection needs to be borne in mind for interpreting firm responses to the pandemic, and consequent impacts on workers. By documenting mechanisms behind firm responses, we can consider whether the mechanisms would be weaker or stronger in other firms. Moreover, tracked workers from our sample have acquired six years of potential experience by 2019, moving up the job ladder also into more positively selected firms.

2.4 Uncertainty and Sales Revenue

A feature of aggregate shocks is heightened uncertainty. Firms are used to facing uncertainty in good times, arising from demand volatility or productivity shocks. However, in the pandemic firm owners faced greater uncertainty, and sometimes along new margins. Table 2 shows how uncertainty of firm owners evolved, comparing their expectations in Oct-Dec 2020 (a few months after the end of the first lockdown) to those in May-July 2021 (just prior to the second lockdown). Panel A examines expectations about the pandemic itself. At the end of 2020: (i) 40% of owners believed the economy would rebound within six months; (ii) 37% of owners believed they were unlikely to re-open following any new lockdown, but this eased by mid 2021 ($p = .000$). Owners were thus pessimistic relative to what actually transpired.

Panel B examines pandemic outcomes relative to pre-pandemic. We first consider changes in demand, as measured by the number of customers/week. At the end of 2020, customer demand had fallen by 53% from the eve of the pandemic, and owners report no recovery in demand by mid 2021 ($p = .584$). At the same time, revenues per customer remained constant – suggesting stability of relative demand across low-price simple and high-price complex products. To see how this fall in product demand translates into expected sales, we asked owners about their expected sales relative to 2019: towards the end of 2020 owners expected sales to be around half the 2019 levels – matching the fall in customer numbers, again suggesting little change in the composition of demand. In mid-2021, despite demand still being lower than pre-pandemic, they were expecting total sales by the end of 2021 to have rebounded.

These sources of uncertainty – both pandemic-specific and those arising from falling product demand as in most downturns – make it difficult for owners to predict firm survival. Absent policies to support them, they have to make choices – predominantly over the number and composition of retained workers – to cut costs in the face of falling sales revenue.

3 Firm Dynamics Over the Pandemic

3.1 Operations, Labor Demand, Revenue and Profits

To begin unpacking how firms responded to the shock, we first consider the dynamics of two outcomes over the downturn: whether firms are operating, and their 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 1 (blue) shows that only 40% of firms remained operating during the first lockdown, followed by a V-shaped recovery with all firms reopening thereafter. A handful were not operating in February 2020, so the series rises slightly above one from late 2020.

We overlay this time series with labor demand in these firms (red line). Employment fell to 55% of pre-pandemic levels during the first lockdown – a near-halving in two months – recovering more slowly than operations, to only 70% by mid-2021. In short, even among positively selected firms tracked through the downturn, firm sizes were persistently reduced. Employment reduction is thus a key margin of adjustment.

To account for sectoral and other firm characteristics driving these dynamics, we consider regression adjusted 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 1).[4]

Table 3 confirms the robustness of the descriptive evidence: Column 1 shows the share of operating firms fell by 53pp in the first lockdown but recovered before the second. Column 2 shows labor demand fell sharply during the first lockdown by 53%, and recovered slowly, remaining 41% lower than pre-pandemic in July 2020 (when the number of firms operating is only 9% lower). On the eve of the second lockdown, labor demand remained 30% lower than in February 2020 – a considerable reduction in average firm size.

To see the financial implications for firms, we consider revenue and profit outcomes. Column 3 shows revenues plummeted by 64% during the first lockdown, and by April 2021 they still remained significantly below what they were on the eve of the pandemic (by 26%). Hence firms needed to quickly cut costs. The combined effect of revenues falling and firms cutting costs as a response resulted in profits falling sharply and then recovering, as shown in Column 4: profits fell by 77% during the first lockdown, but recovered to 45% below pre-pandemic by April 2021. To understand the role of employment responses in driving this recovery in profits, Column 5 examines dynamics

[4] 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.

of the wage bill/revenue ratio: this was 40% on the eve of the pandemic, and fell by 8pp (21%) by April 2021. This highlights how changing the composition of retained workers played a central role in firms' responses to the downturn.

Adjusting on the employment margin is the most natural response of firms to the downturn, in Uganda and similar settings, because of a lack of firing costs arising from contractual or severance pay requirements, a lack of credit being available to firms, and an absence of policies helping firms to hoard labor over the downturn. Nevertheless, through the analysis we examine other adjustment margins such as reducing piece rates for products – something we are able to rule out because labor markets display downward nominal wage rigidity, even in the pandemic downturn [Kaur 2019]. Given workers in our study sectors are paid piece rates for products (not hours worked), reducing employee hours is not an effective way to cut costs. This would be an important margin of adjustment in contexts where workers are salaried employees and their pay is not tied to their output, but is less relevant for low-income contexts and the sectors we study.[5]

3.2 Composition of Workers

To see how the labor demand shock translated into differential impacts across workers, we first document the pre-pandemic skills composition of workers. In the pre-pandemic surveys, we collected employee rosters in each firm. Table 4 uses these rosters to describe the circumstances of employees at firms in the last pre-pandemic survey, distinguishing between workers that have been vocationally trained (VT), and those that have not (non-VT), as reported by workers themselves. Panel A shows that: (i) 26% of workers in our tracked firms have received vocational training sometime in the past; (ii) this classification translates into firm owners perceiving significant productivity differences between the two groups of workers; (iii) firm owners report VT workers to be twice as likely to be skilled as non-VT workers when recruited; (iv) VT workers having significantly longer tenure within firms: the mean (median) tenure is 36 (24) months for VT workers and 24 (13) months for non-VT workers.

In the pandemic surveys, we did not elicit worker rosters from firms, but rather proxy VT status of workers if they have substantial experience in the firm, a reasonable proxy given the large tenure differential between VT and non-VT workers in firms pre-pandemic. This information is available for retained workers, hires and layoffs, enabling us to proxy the share of all workers in the firm that are vocationally trained at the time of each pandemic survey: November 2020 and June 2021. We normalize the series to one in February 2020. This is the third series in Panel A of Figure 1 (orange circles). As firms recovered from the first lockdown, the share of workers employed at the firm with VT fell to 67% of the pre-pandemic share. In short, there was both a fall in

[5] When asked about margins of adjustment, the vast majority of firms report reducing hours, employment or both – less than 10% in either pandemic survey reported reducing their capital stock. We also asked about the introduction of other pandemic-specific practices: the majority report introducing Covid-19 preventative measures (71%), and an increased use of taking customer orders by phone (40%) or the internet (17%).

labor demand and a change in the skills-composition of employees, embodied within a *VT-biased* response to the downturn.

Tenure and VT status are correlated in our setting, and thus we might partly capture tenure-biased rather than VT-biased responses. However, this correlation is itself a consequence of the higher productivity and retention rates of VT workers, making this distinction less sharp in reality. As a check, we explore alternative coding rules for proxying VT workers. We find our preferred approach is relatively conservative, with alternative proxies suggesting the share of VT employees fell by closer to 50%.[6]

3.3 Worker Retention, Hires and Layoffs

We now examine the composition of hires and layoffs. In our pandemic surveys we asked firms to report the share of workers retained, and the composition of hires and layoffs from: (i) March 2020 to November 2020; (ii) December 2020 to June 2021. The results are summarized by period in Panel B of Figure 1. The first bar in each period shows the retention rate of employees within the firm: 48% of employees were retained over the first phase of the downturn – in line with the fall in labor demand; 70% of employees were retained in the second phase. Hence many – but far from all – productive worker-firm matches were preserved over the downturn.

The next bars (red) examine characteristics of laid off workers. 65% of firms laid off workers over the first phase of the downturn. Among layoffs, the majority (80%) were VT-workers (proxied by substantial firm experience). This pattern continues: 50% of firms laid off workers over the second phase of the downturn, and 80% of those laid off were vocationally trained.

The final bars (green) show characteristics of hired of workers. The share of firms hiring workers over both phases of the downturn are lower than those laying off workers (14% and 21% respectively), and attempts to hire were more muted over the first than the second phase – in line with the gradual V-shaped recovery in labor demand. To examine the skills of hires, we proxy whether hires are vocationally trained by combining firms reporting hires that are: (i) experienced in the same sector, or, (ii) with no experience but are vocationally trained. We proxy non-VT hires using firm reports of whether a hire was unskilled. Over the first phase, recruited workers are evenly split between those with and without VT, while during the second phase, around a third of hired workers had ever been vocationally trained.

These results have two implications. First, tenure and skills acquired through vocational

[6] We consider two three alternative approaches. In the first two we change the constructed proxy for VT workers employed during the pandemic (for retained, hired and laid off workers). First, we apply a narrower classification where only workers with substantial experience in the firm are counted as VT – this suggests the share of VT workers in June 2021 was 54% of its level in February 2020. Second, if we apply a broader classification by adding workers with experience in the same sector to be assigned as having VT, the share drops further to 50%. Our third approach ignores descriptors related to VT and focuses only on worker's experience. We define a worker as having long-tenure if their tenure is above the sector median. Applying this to workers we find the share of long tenure workers in the firm in June 2021 is estimated to be 49% of its level in February 2020.

training did not protect workers from job loss, as firms employed something like a VT-biased strategic response to the pandemic shock, with vocationally trained workers being more likely to be laid off. Given many firms entered the pandemic with a mix of VT and non-VT workers, it was feasible for firms to lay off some (but not all) high-experienced and high-skill workers, and keep operating with a smaller group of more inexperienced and differentially skilled employees. Second, there is an opportunity for VT workers to be rehired by firms in our study sectors during the pandemic downturn. Among our tracked firms, some attempted to recruit VT workers over both stages of the pandemic. At the same time, such opportunities are limited: firms were as likely to recruit unskilled workers as skilled workers. We directly examine these worker dynamics later using our worker side data.

3.4 Heterogeneity in VT-biased Responses

To underpin the interpretation of VT-biased firm responses, we explore heterogeneity in firm responses along two margins. The first is pandemic specific: exposure to the shock as proxied by reliance on face-to-face trade. The second is not pandemic specific: whether firms report being financially constrained before the shock. This can matter either because continued access to credit during the downturn can shape firm outcomes [Cooley and Quadrini 2001, Gourinchas *et al.* 2025], or because this captures other forms of underlying firm heterogeneity that matter for behavior and outcomes during a downturn. In our context the latter interpretation is likely more appropriate given that 90% of firms report not applying for any loan over the downturn, and there was very limited government support for firms.[7]

On the first margin, we assume firms in motor-mechanics, catering, hairdressing and tailoring are more exposed due to their greater reliance on customer interactions, while firms in construction, welding, plumbing and electrical wiring are relatively less exposed. Figure A3 shows: (i) more exposed firms are less likely to remain operating during the first lockdown, but then recover, with nearly all firms remaining in operation in June 2021 irrespective of exposure (Panel A); (ii) the labor demand shock is greater for more exposed firms (Panel B). In exposed firms, during the first lockdown, employment levels fall to 25% of pre-pandemic levels, while for firms with less customer interaction, labor demand falls to 55% of the pre-pandemic level ($p = .006$). The recovery in labor demand is also weaker in more exposed firms: by June 2021 employment is 60% of pre-pandemic levels, versus 75% in less exposed firms. Hence the pandemic led to a reallocation of employment opportunities across sectors, as also documented for the US [Cirelli and Gertler 2025].

However, despite differences in the magnitude of the labor demand shock, the share of VT workers fell in similar proportions across both types of firm: by June 2021, the share of VT workers in more exposed firms was 70% of the pre-pandemic share, while in less exposed firms it

[7] A firm is classified as financially constrained if in the last pre-pandemic survey wave owners report they would borrow if all the following conditions improved: simpler loan application procedures, lower collateral requirements, no fear of asset seizure, and a lower interest rate. 64% of firms are classified as constrained.

was 60% ($p = .215$). Panel C shows the composition of worker hires and layoffs over the pandemic. In line with facing a greater labor demand shock, more exposed firms were less likely to retain workers over both phases of the pandemic. However, among those laying off workers, irrespective of their exposure: (i) 80% of those laid off are VT-workers across both phases of the pandemic; (ii) among firms recruiting, VT and non-VT workers were equally likely to be hired over the first lockdown, with VT-hires falling to one third of all hires later in the downturn.

In short, the pandemic led to larger labor demand shocks for more exposed firms, but both more and less exposed firms follow similar VT-biased strategies. This suggests the mechanisms driving VT-biased strategies are independent of the magnitude of the product and labor demand shocks firms faced, an issue we return to when analyzing the optimality of these strategies.

The second dimension of heterogeneity we consider is whether firms report being financially constrained or not. Figure A4 shows this source of heterogeneity plays little role in explaining differences in the likelihood firms operate (Panel A); (ii) more financially constrained firms experience slightly larger falls in labor demand during the first lockdown but both constrained and unconstrained firms recover to have employment at 70% of pre-pandemic levels (Panel B). Irrespective of financial constraints, all firms have similar patterns of worker retention, layoff and hiring over the pandemic (Panel C). In short, we find little evidence that pre-pandemic financial constraints – that might proxy underlying firm heterogeneity that determines a firm's resilience during a downturn – help explain the use of VT-biased survival strategies.

3.5 Mechanisms Driving VT-biased Responses

To pinpoint the mechanisms driving VT-biased responses, we re-visit the circumstances of employees at firms in the last pre-pandemic survey wave, distinguishing between VT and non-VT workers. Panel B of Table 4 details their skills and training. We see that: (i) VT workers are more likely to be able to conduct a typical sector-specific task when recruited (62% vs. 37%, $p = .000$); (ii) non-VT workers are more likely to be trained within the firm (62% vs. 34%, $p = .000$); (iii) on-the-job training is costly, lasting around nine months; (iv) as a result of being trained on-the-job, by the time of the survey, the gap in being able to perform the typical sector-specific task narrows between VT and non-VT workers (87% vs. 76%). This provides a first explanation for why VT workers are more likely to be laid off in response to shocks: as non-VT workers are trained in-house to a greater extent, their human capital is tilted towards firm-specific and less transferable skills, so firms capture higher rents from them. Relative to VT-workers, laying off non-VT workers entails greater future costs in terms of recruiting and training new unskilled hires.

Panel C shows the relationship between workers and firm owners. Non-VT workers are more likely to be socially tied to owners either because: (i) they are a family or friend of the owner ($p = .093$); (ii) they were hired through family/friends of the owner ($p = .002$). This is in line with evidence from similar settings on employment as a form of social redistribution [Macchi and

Stadler 2024, Swanson 2025]. This provides a second explanation for VT-biased strategies: the implicit costs of firing a non-VT worker are higher than for VT workers.

These first two mechanisms both relate to the option value of retaining non-VT workers during a downturn: either because any future replacement is more likely to need to be trained on-the-job, or because of costs of laying off someone the owner is socially connected to.

The remainder of Table 4 examines a third explanation: changes in relative demand for complex and simple products. Panel D details earnings, hours and production differences across workers. VT workers have 43% higher monthly earnings than non-VT workers ($p = .000$), so there is a large earnings premium to VT in these firms. This is not because they work more hours, but because they work on outputs obtaining 31% higher prices. That these are higher-quality outputs is also evidenced by: (i) the value of materials used for the typical product produced by VT workers being 28% higher than for non-VT workers ($p = .003$); (ii) the typical product that VT workers are engaged on takes more time to produce than the typical product that non-VT workers are engaged on ($p = .047$); (iii) VT workers produce 20% fewer of these high-value but time-intensive products each month than the number of low-value products produced by non-VT workers; (iv) the majority of workers are paid piece rates based on the number of units they produce, and VT workers are paid 37% higher piece rates for their output than non-VT workers ($p = .000$).

This provides a third explanation for VT-biased strategies: these responses arise if complex products have a larger fall in demand during the pandemic, and VT and non-VT workers are relatively substitutable in the production of complex goods/services.

All three mechanisms are not specific to the pandemic: they remain relevant whenever firms face shocks that lead to falls in revenues and labor demand. We use our data from the pandemic to draw lessons from this episode for other downturns.

3.6 Optimality of VT-biased Responses

To shed light on the optimality of firm responses and the quantitative relevance of these mechanisms, we develop a stylized model of firm production, costs and profits. We rationalize firm behavior assuming they maximize profits throughout. We calibrate the model to understand whether the VT-biased strategies pursued were privately optimal relative to a counterfactual skills-neutral response to the downturn. We use the comparison between observed and counterfactual profit impacts to establish the combinations of parameter values under which observed VT-biased strategies are consistent with profit maximization, and thus the conditions under which each mechanism is necessary to rationalize firm behavior.

3.6.1 Set-up

Assume a firm has two types of worker: vocationally trained and not vocationally trained. We denote each type as V and U respectively, and the number of each type of worker in the firm as

n_i, $i \in \{V, U\}$. In line with the descriptive evidence (Figure A1, Table 4), firms produce two types of product j: a simple (S) and a complex (C) product. Y_j and p_j denote the output and price of product $j \in \{C, S\}$ respectively. The simple good/service is produced from tasks performed by U workers, while the complex good/service requires a combination of tasks performed by V and U workers. Let Q_i denote the tasks workers of type i conduct. Hence, $Y_S = f(Q_U, n_U)$ and $Y_C = f(Q_V, n_V, Q_U, n_U)$. Workers are paid a piece rate τ_i per task regardless of which final product their tasks contribute to, and b_i are fixed in-kind payments to each worker type (irrespective of their output). The evidence from Table 4 suggests $p_C > p_S$ and $\tau_V > \tau_U$.

Production and Profits We now make explicit that prices, outputs and tasks can vary over periods (pre- and during the pandemic), and make functional form assumptions on production aligned with the evidence. We model production of product type j in period t as:

$$Y_{St} = Q_{Ut} n_{Ut}, \tag{2}$$

$$Y_{Ct} = [\alpha(Q_{Vt} n_{Vt})^{\rho} + (1-\alpha)(Q_{Vt} n_{Ut})^{\rho}]^{\frac{1}{\rho}}. \tag{3}$$

Output of the simple good/service only requires U workers and we assume a linear technology. The complex good/service requires both worker types and we assume that when U workers assist on such products, they conduct basic tasks at the same pace as V workers. Their contribution to output is therefore constrained by the V type's production, Q_V (not Q_U). We assume these inputs combine in a CES production function where α is the distribution parameter, and the elasticity of substitution $\sigma = \frac{1}{1-\rho}$ is a key parameter. As σ falls below one, workers become less substitutable in the production of the complex good/service (and in the limit when $\sigma = 0$, production requires a fixed proportion of VT to non-VT workers as in a Leontief technology), $\sigma = 1$ is the Cobb Douglas case, and as σ increases above one, VT and non-VT workers become more substitutable so the firm can more easily sustain output of the complex good/service by substituting V for U workers. Firm profits in period t are then given by:

$$\Pi_t = [p_{Ct} Y_{Ct} + p_{St} Y_{St}] - \tau_V Q_{Vt} n_{Vt} - \tau_U (Q_{Ut} + Q_{Vt}.\mathbf{1}[n_{Vt} > 0]) n_{Ut} - b_V n_{Vt} - b_U n_{Ut}. \tag{4}$$

As U type workers receive piece rate τ_U per task for both S and C products, their contribution to the wage bill is $\tau_U(Q_{Ut} + Q_{Vt})n_{Ut}$ when C production is active ($n_{Vt} > 0$), and $\tau_U Q_{Ut} n_{Ut}$ otherwise. This per-period profit function is static – it does not incorporate the option value of retaining non-VT workers due to the first two mechanisms described above. We use this fact – that there might exist a wedge between the simulated (static) and true (dynamic) profit function – to quantitatively assess these mechanisms in explaining the use of VT-biased responses versus a counterfactual skills-neutral response to the downturn.

Calibration The model for profits remains highly stylized – assuming, for example, that the firm produces one complex and one simple product. For internal consistency, we rescale the profit function in two steps: (i) we rescale revenue and cost components to ensure the wage bill/revenue ratio matches the 40% figure on the eve of pandemic; (ii) we then rescale the profit function to match the observed fall in profits of 45% from February 2020 to April 2021 – we do so for our preferred parametrization, and apply this normalization to all simulated profit changes, thus preserving all relative comparisons across scenarios and parameter values. The Appendix details the mapping between the model and data used for the calibration.[8]

Simulation As we do not observe the substitutability between VT and non-VT workers in the production of the complex product, we simulate profits under various choices of σ, and set $\alpha = \frac{1}{2}$. We consider $\sigma \in \{.6, 1, 1.4\}$, spanning the typical developing country estimate ($\sigma < 1$) [Teal 2000, Edwards and Behar 2006], the Cobb-Douglas benchmark ($\sigma = 1$), and the typical developed country estimate ($\sigma > 1$). The only parameter that varies across simulations is σ, governing the substitutability of V and U workers in the production of the complex good/service Y_C.[9]

In each simulation, we compare the observed employment mix against a natural counterfactual scenario: laying off V and U workers in proportionate rates – so following a skills-neutral strategy that preserves the same ratio of VT to non-VT workers as were present in the firm pre-pandemic. In this counterfactual: (i) fewer VT workers and more non-VT workers are laid off than is actually observed; (ii) we maintain the same fall in total labor demand as observed in April 2021, only varying the skills composition of retained workers.

3.6.2 Results

Let Π_{Feb20} denote baseline profits on the eve of pandemic in February 2020, calibrated using data on pre-pandemic prices, quantities, and the skills composition of employees. All simulated profit changes from February 2020 through to April 2021 are expressed as percentage changes relative to this baseline: $\%\Delta\Pi = 100 \cdot \frac{\Pi_{\text{Apr21}} - \Pi_{\text{Feb20}}}{\Pi_{\text{Feb20}}}$. We first assume that over the pandemic, demand for the complex and simple products falls equally by 21% (matching the change in demand observed),

[8] On the first adjustment, we introduce a scaling parameter γ that multiplies the wage bill so that the ratio of the re-scaled wage bill to revenues matches the .404 in February 2020, as reported in Column 5 of Table 3: $\gamma = \frac{.404 \times \text{Revenue}}{\tau_V Q_{Vt} n_{Vt} + \tau_U (Q_{Ut} + Q_{Vt} . \mathbf{1}[n_{Vt} > 0]) n_{Ut} + b_V n_{Vt} + b_U n_{Ut}}$. We recalibrate γ for each σ because model revenues, $p_{Ct} Y_{Ct} + p_{St} Y_{St}$, depend on σ through the CES production technology. For the second adjustment, our simulation using the preferred parameterization of $\sigma = .6$ produces a fall in profits of -63% for the VT-biased scenario. The rescaling factor therefore is $45/63 = .714$ and we apply it uniformly to all simulated profit changes.

[9] The literature on the elasticity of substitution between skilled and unskilled workers spans settings. The standard approach recovers σ by regressing the skill premium on relative skill supply and taking the inverse of the coefficient. In developing country settings, σ is often estimated to be less than one. For example, Teal [2000] finds $\sigma \approx 0.3$ for Ghanaian manufacturing, and Edwards and Behar [2006] report $\sigma < 1$ between skill groups in South African manufacturing. Estimates for developed countries are in the range $\sigma \in [1.3, 1.7]$. An influential benchmark is Katz and Murphy [1992], who exploit within-industry variation in the relative supply of college and non-college workers across US industries and recover an elasticity of 1.4.

and that prices for both products rise by 1.2% (matching the price change observed for the typical product). This approach shuts down the third mechanism for VT-biased responses, and allows to focus on the two mechanisms related to the option value of retaining non-VT workers. We later relax this assumption to consider the quantitative importance of the third mechanism related to relative demand shifts between the complex and simple products.

The results are in Panel A of Figure 2. We first consider the most realistic case for developing-country settings where $\sigma = .6$. The first bar shows that following the observed VT-biased response, the calibration assures simulated profits fall by 45%. The next bar shows this would be dominated by the counterfactual skills neutral response in which firms reduced VT and non-VT workers in proportionate terms, as that would have led to a 43.8% fall in profits. The fact that firms are not observed following this counterfactual response suggests there exists a wedge between simulated and true profit functions. Natural candidates to explain this wedge are the first two mechanisms: (i) laying off non-VT workers entails greater future costs of training on-the-job new unskilled hires; (ii) the implicit costs of firing a non-VT worker are higher because they are socially tied to firm owners. A *lower bound* on the combined strength of these mechanisms of 1.2% of pre-pandemic monthly profits, is needed to rationalize the observed change in skills composition of employees. This lower bound could easily be exceeded given the financial and opportunity costs to firm owners of training workers on-the-job, and evidence from similar settings on the financial implications of using employment as a form of social redistribution [Macchi and Stadler 2024, Swanson 2025].[10]

We next consider the Cobb Douglas case for the production of the complex good, $\sigma = 1$. The first bar shows that if firms follow a VT-biased response, simulated profits fall by 43.6%. The next bar shows had firms followed the counterfactual neutral strategy, this would have led to a very small further fall in profits. The final case assumes moderate substitutability between VT and non-VT workers ($\sigma = 1.4$). Relative to the observed VT-biased strategy that generates a 42.9% fall in profits, we simulate the fall in profits would have been larger under the counterfactual skills-neutral response. In this case, because VT and non-VT workers are sufficiently substitutable, the skills composition of employees matters less as workers of either type can sustain output of the complex product. Hence the observed VT-biased survival strategy is privately optimal relative to the counterfactual scenario without having to appeal to either of the first two mechanisms.

We next explore the third mechanism potentially driving firms' VT-biased strategy: that the fall in product demand is disproportionately concentrated on the higher-priced complex product. To do so, we hold constant the overall fall in demand, and reallocate it across complex/simple products. In the pandemic surveys, we did not measure how product demand shifts between goods/services produced by VT and non-VT workers. We therefore consider various scenarios

[10] Macchi and Stadler [2024] use a field experiment with grain-processing firms in Kampala, Uganda to document this mechanism. They find revenue per effective hour worked is understated by 3 to 5%. Swanson [2025] uses a field experiment with urban firms and agricultural employers in Zambia to understand social pressures to hire relatives. He documents that when firms have plausible deniability about the identity of hires, the subsidy required to induce a firm to choose a non-relative over a relative is the equivalent of 4% of monthly profits.

for these relative demand changes. We use a single parameter δ to capture this reallocation of demand across the two products: $\delta = 0$ is the baseline case considered above, when demand for simple and complex products changes in the same proportion. For $\delta < 0$, demand for the complex product is $|\delta|$% lower than in the baseline scenario (where both product demands drop by 21%), while demand for the simple product drops by less than 21% to ensure the drop in total revenue-weighted demand remains unchanged. $\delta > 0$ is the unrealistic case when demand for the complex product falls by less than for the simple product.

Panel B of Figure 2 shows how, for each $\delta \leq 0$, simulated profits across scenarios change with the strength of this mechanism. Focusing on the $\sigma = .6$ case, we see that even when the complex product's demand fall is up to 22% lower than the equal-fall baseline, simulated profits under a VT-biased strategy remain below those under a skills-neutral counterfactual strategy in which VT and non-VT workers are laid off in equal proportions. The gap between the simulated profits under the two scenarios narrows, so the strength of mechanisms related to the option value of retaining non-VT workers weakens for $\delta \in [-.22, 0)$, but all three mechanisms remain relevant for driving firm behavior during the downturn. If demand for the complex good falls by more than 22%, the observed VT-biased strategies can be rationalized without having to appeal to either of the first two mechanisms related to the option value of retaining non-VT workers.

In the $\sigma = 1$ and $\sigma = 1.4$ cases, where VT and non-VT workers are more substitutable in producing the complex good, any greater fall in demand for the complex product is sufficient to rationalize VT-biased responses without having to appeal to the first two mechanisms.

In short, the third mechanism of greater falls in demand for the complex product help rationalize VT-biased strategies. When substitutability is low ($\sigma = .6$), demand for the complex good must fall by at least 22% below the equal-fall baseline for this mechanism alone to fully rationalize VT-biased responses. Smaller shifts in relative demand still contribute by reducing the required strength of the first two mechanisms related to the option value of retaining non-VT workers. When substitutability is higher ($\sigma \geq 1$), even modest relative demand shifts are sufficient to rationalize VT-biased strategies without appealing to other mechanisms.

A final point of note is how these results can be used to infer behaviors among less positively selected firms than those we track into the downturn. First and foremost, those firms might be less likely to survive the downturn. Conditional on survival, one way to speculate on the issue is to consider their potential response if the strength of mechanisms they face varies. Our approach suggests less positively selected firms would have even more VT-biased responses if: (i) their costs of training on-the-job new unskilled workers are higher; (ii) they have stronger social ties between non-VT workers and firm owners, so they face even higher implicit costs of firing such workers; (iii) they have a greater fall in relative demand for their complex product.

4 Workers

4.1 Design of the Experiment

We build on these findings to provide a complete analysis of how the downturn propagated through labor markets. The firm-side evidence establishes that VT-biased strategies were pervasive and privately optimal. We now exploit the other side of our data to trace the consequences for workers' labor market trajectories, using the fact that our original field experiment randomly assigned labor market entrants to certified vocational training in 2013 – creating exogenous variation in VT status precisely along the margin that drove firm layoff decisions.

Treatment The vocational training intervention provided workers six months of sector-specific training in one of the eight study sectors our firm-side sample is drawn from. Training was delivered by formal and well-established vocational training institutes (VTIs). Our intervention partner BRAC covered training costs, at $470 per trainee. Courses were full-time, covered practical skills, and worker attendance was monitored. Upon graduation, trainees received a certificate verifying their skills. As Alfonsi *et al.* [2020] document, in good times there are high returns to having certifiable skills from 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 in the pre-pandemic period is studied in Bandiera *et al.* [2025]. In this paper given our focus on impacts across the skills distribution from firm responses to an aggregate shock more than six years after the interventions took place, we pool both and show the robustness of key results in each treatment arm.

Balance and Attrition Table A1 shows baseline labor market characteristics of workers in each treatment arm. Table A3 shows other background characteristics. In both cases, the samples are well balanced and normalized differences in observables are small.

We consider attrition of workers in two periods: pre-pandemic from baseline until the fourth follow-up, 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 occurring between the last pre-pandemic and first pandemic surveys, when in-person surveys switched to being phone surveys; (iii) there is close to zero further attrition through to our final survey wave; (iv) during the pandemic, controls are 8-9pp more likely to attrit than those offered vocational training. In the Appendix we document that on most margins we find little evidence of heterogeneous attrition between treatment and control groups, either before or during the pandemic (Table A5), and show the robustness of our results to addressing selective attrition on non-observables.

Compliance 65% of workers took-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 A6 shows correlates of take-up. Individuals with lower cognitive ability, lower locus of control, or resident outside Kampala were more likely to take-up the offer. Given we aim to shed light on implications for workers of VT-biased responses to the pandemic downturn by firms, for the worker analysis we mostly consider 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.

4.2 Pre-pandemic Outcomes

To begin to understand the implications for workers of firms' VT-biased responses, we briefly review results from our earlier work [Alfonsi *et al.* 2020] that establishes the pre-pandemic impacts of VT on skills and labor market outcomes. This crystallizes initial differences between VT and non-VT workers before the pandemic shock occurred, supplementing the earlier evidence on this from the firm-side worker rosters. These results are detailed in the Appendix, but to summarize: (i) on a sector-specific skills test we developed, VT workers increase their measurable sector-specific skills by 27% more than controls (or by $.40\sigma$ of test scores); (ii) as validation that these acquired skills are relevant, the task composition of employed workers differs between VT and control workers – highlighting these groups of worker differ in their occupation specific human capital, which can impact labor market dynamics after job loss [Huckfeldt 2022, Braxton and Taska 2023]. In terms of labor market outcomes we document that in our final pre-pandemic survey, VT workers: (i) are 18.1pp more likely to be working in one of the eight study sectors (a 77% increase over controls); (ii) have total monthly earnings 25% higher than controls; (iii) cumulatively across all four pre-pandemic surveys, VT workers accumulate 117% more experience in study sectors, and accumulate 59% higher earnings than controls.

These cumulative differences in earnings (and hence the resources available to workers), and labor market attachment to good sectors (hence the information available to workers), mean that in good times, there are high returns to vocational training in our study context. Moreover, these cumulative differences in resources and knowledge at the outset of the pandemic can play a critical role in determining the resilience of VT workers during the downturn. These are key mechanisms explaining labor market trajectories that we come back to document.

5 Labor Market Dynamics Over the Pandemic

5.1 Estimation

During the pandemic, our worker surveys ran from September 2020 to February 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, spanning just prior to, during, and just after each lockdown. We estimate the following specification by 2SLS in time-frame t from survey waves $L1$, $L2$ and R:

$$y_{ist} = \alpha + \sum_{t=1}^{t=7} \beta_t Trained_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{ist}, \tag{5}$$

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. The regression estimates from (5) are reported in Table A7. To establish the constancy of the impact of VT on outcomes over the pandemic, 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 $L1$, are the same as in the recovery wave R, $\beta_1 = \beta_7$. 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 VT and control workers.

5.2 Employment

Figure 3 shows unconditional differences tracing the returns to VT over seven time-frames of the downturn, along four extensive margins of employment between compliers and controls reweighted for their compliance probability. As a point of comparison we also show the outcome from the last pre-pandemic survey wave.

Panel A examines whether individuals are employed. Pre-downturn, both vocational trainees and controls have employment rates close to 85%. During the first lockdown, employment rates for controls drop to 45%. The corresponding regression specification in Table A7 shows employment rates drop even more for VT workers, who are 13.4pp less likely to still be in employment ($p = .006$). Hence VT workers are in proportionate terms, hit harder by the shock – in line with firms' VT-biased strategies.

After the end of the first lockdown, employment rates of VT 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 VT workers. However, the recovery is incomplete (so $\overline{y}_7 < \overline{y}_1$): in February 2022, employment rates remained

16pp lower for VT workers than on the eve of pandemic in February 2020 ($p = .000$).

Panel B focuses on whether VT and controls are employed in one of the study sectors – as a marker of working in a productive sector and gaining valuable labor market experience. On this margin we see pronounced differences between the groups through the pandemic. As Table A7 shows, on the eve of the pandemic VT workers were 22pp more likely to be employed in a study sector ($p = .000$). They maintain this advantage over controls, except during the lockdowns. After each lockdown, VT workers quickly regain employment in the study sectors, so by February 2022, they were 17pp more likely than controls to be employed in a study sector ($p = .000$).

The remaining Panels of Figure 3 examine employment types. Panel C confirms that the differential employment dynamics between VT and control workers are driven by wage/self-employment, and this is largely driven by wage employment rather than workers shifting into self-employment.[11]

Panel D shows trends in casual work. Controls engage in casual work at higher rates at the outset of the pandemic. This gap is maintained initially but later in the downturn these employment rates almost converge as VT workers shift into casual work. By February 2022, employment rates in casual work are 4pp higher for VT workers than on the eve of the pandemic ($p = .000$). 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.* 2025].

These dynamics mimic the patterns documented earlier using the firm-side data: (i) VT workers' employment declines more in response to the pandemic than that of non-VT workers; (ii) the V-shaped recovery in employment is observed in both data sources suggesting that over the downturn, VT workers were able to find new employment by reallocating across firms.

5.3 Earnings

Job loss during downturns can 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 analysis for earnings outcomes. The underlying regression estimates are in Columns 5 to 7 of Table A7. Earnings display V-shaped and double-dip dynamics similar to employment: VT workers are hit harder by lockdowns, consistent with firms' VT-biased strategies, and recover more quickly between lockdowns, but there is no full recovery in levels, with earnings in February 2022 still below their pre-downturn level.

Panel A of Figure 4 shows the dynamics of total monthly earnings (from all forms of employment). In nearly all time frames VT workers earn more than controls. Again, in proportional terms, VT workers experience larger earnings losses during lockdowns. In line with the earlier results, the first lockdown suppresses earnings more than does the second. Panel B focuses on

[11] More precisely: (i) on the eve of the pandemic, self-employment is far less prevalent than wage employment (27% versus 48%); (ii) VT workers are not more likely to be self-employed than controls; (iii) the differential likelihood of VT and control workers to be self-employed never differs statistically in any time frame of the pandemic.

earnings from wage and self-employment (including zeros). In line with the extensive margin results, VT workers retain significantly higher earnings than controls throughout the downturn. By February 2022, VT workers' monthly earnings from wage/self-employment are 16% higher than for controls, so back to the pre-pandemic differential.

Panel C conditions earnings on wage and self-employment. As in Panel A, in nearly all time frames VT workers have higher earnings than controls. Finally, Panel D shows that earnings from casual work remain higher for controls in the initial phase of the pandemic, but these gaps disappear later in the downturn – in line with the earlier evidence that VT workers eventually downgrade into casual work.

5.4 Cumulative Impacts

To establish the returns to vocational training over the pandemic and compare them to pre-pandemic returns, we calculate the cumulative difference in labor market outcomes over the pandemic between VT and control workers. To do so we estimate the following 2SLS specification for individual i in strata s:

$$\sum_{t=1}^{t=7} y_{ist} = \alpha + \beta Trained_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{is}, \tag{6}$$

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, and we interpolate outcomes over the other 11 using a constant imputation, namely we assume treatment effects remain constant from any given time frame until the month before the next time frame is measured.

The results are in Table 5 where we show the four margins of employment from Figure 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 (6). 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 VT 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 VT 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 downturn – roughly halving in magnitude, but still widening cumulative gaps in labor market outcomes between VT workers and controls. This occurs despite VT workers being exposed to firms' VT-biased strategies, speaking to their resilience during the pandemic.

Extensions and Robustness We present two further sets of results in the Appendix. First, how the returns to VT vary by: (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 being greater shifts into casual work among skilled women relative to skilled men; (ii) the 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 pandemic surveys. This helps ameliorate the concern that the estimated labor market impacts are driven by attrition alone. Moreover, we earlier showed no strong evidence of differential attrition on observables for treatment and controls. The double dip dynamic impacts on employment and earnings margins further alleviate the concern that attrition might drive the results, or that the returns to VT fade out over the pandemic. Nevertheless, in the Appendix we address the issue using multiple approaches.

5.5 Did the Pandemic Reshape Worker-firm Matches?

Even if VT workers remain resilient to VT-biased responses of firms, this still leaves open the broader question of how the pandemic downturn shapes the sustenance of productive work-firm matches formed pre-pandemic. A natural benchmark is that the shock was severe but brief, with labor markets freezing during lockdowns and recovering quickly once restrictions lifted, 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, so even though lockdowns ended, their consequences persisted. We present two results strongly suggesting the latter interpretation, again focusing on using the lens of the pandemic to understand persistent impacts of job loss in downturns in low-income labor markets.

5.5.1 Recovery Relative to a No-pandemic Counterfactual

We first use pre-pandemic data to project the labor market outcomes our cohort would have experienced absent the pandemic, and then contrast these counterfactuals with actual outcomes. Using the five pre-pandemic survey waves (2012-18), we fit a power function to each outcome and project forward to February 2022. We overlay the projected paths with the observed paths in Figure 5, that shows projections for compliers and reweighted controls for employment in our study sectors, and total earnings from wage/self-employment. The power function allows for concave growth in employment and earnings typical of young workers accumulating labor market experience, and fits pre-pandemic trajectories well. These pre-pandemic trends for both VT and control 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 downturn relative to a no-pandemic counterfactual. VT workers are 37% less likely to be employed in one of the study sectors than their projected counterfactual, and their total earnings are 34% below trend. The corresponding shortfalls for controls are 49% and 45%. In proportionate terms, VT workers' shortfalls are smaller, but both groups remain well below the levels they would have reached in the absence of the pandemic.

To the extent that earnings reflect productivity, the fact that earnings for VT workers remain 34% below trend suggests large productivity losses from the destruction of pre-pandemic worker-firm matches. Even though not all workers are displaced, these magnitudes remain at the top end of estimates from the literature on dynamic labor market outcomes for displaced workers using administrative data from high-income settings – that find long run earnings losses between 15% and 30% [Jacobsen *et al.* 1993, Couch and Plaszek 2010, Davis and von Wachter 2011]. The difference to our context can be due to the impacts of job displacement in a fast-moving aggregate shock differing to those arising from episodes of mass layoffs, and the fact that as in most developing country contexts, displaced workers lack social insurance.

5.5.2 Worker Mobility

We now turn to the mechanism behind these persistent losses: the reallocation of workers away from their pre-pandemic employers. If firms' VT-biased layoffs severed productive matches, we should see this show up as: (i) VT workers becoming detached from their pre-pandemic employers (despite enjoying high tenure in those firms); (ii) subsequent movement into other firms in the sector, self-employment, casual work, or unemployment. Table 6 examines these margins.

Firm and Sectoral Reallocations To examine the reallocation of workers across firms and sectors and how this differs between VT 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).[12]

Column 1 of Table 6 shows that among controls who were wage employed before and after the first lockdown, 87% remain employed in the same firm. The ATT estimate shows VT 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 VT workers is not because they are re-hired by the same firm. Despite VT workers enjoying long tenure with their pre-pandemic employer, firms do not try to reverse their initial VT-biased responses by recalling these laid off workers. Rather, as Column 2 shows, VT workers are more likely to transition across firms in the same sector than controls ($p = .001$). The magnitude of this impact is 19pp, four times the rate of such transitions among

[12] 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.

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.

This pattern of mobility of VT workers across firms in the same sector is in line with the firm-side evidence presented earlier on firms' recruitment strategies over the pandemic, where we documented that firms attempted to recruit VT workers during the downturn (Figure 1).

Two features of this labor market help explain the mobility of VT workers across firms in the same sector. First, VT skills are certified by the training institutes, making them observable and transferable across firms in the same sector. Our earlier work documented that in good times, certification is a key channel through which VT increases mobility and shortens unemployment spells [Alfonsi *et al.* 2020]. As a result, workers are more mobile across firms and experience quicker transitions back into employment when unemployed. Table 6 shows this mechanism remains relevant during a downturn. Second, given the widespread use of VT-biased strategies across firms in our study sectors, it might be common knowledge that more 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 [Oyer and Schaefer 2011].

Transitions Out of Wage Employment The second half of Table 6 examines transitions from productive wage employment into other forms of work or unemployment, and how this differs by VT and control workers. We consider individuals that were in wage employment pre-lockdown. We find no differential shift into self-employment, but VT workers are significantly more likely to move into casual work around the second lockdown, consistent with Figure 3. This is a second important route through which VT workers are persistently impacted by the downturn. Finally, Column 7 shows that large flows into unemployment (over 20pp around each lockdown) affect VT and control workers alike: VT does not insulate workers from this extensive-margin shock. What it does is shape how quickly, and into what kind of work, they re-enter the labor market.

6 Mechanisms Driving the Resilience of VT Workers

What explains VT workers' resilience? We distinguish between mechanisms that operate through the labor market – workers' accumulated sector-specific experience, certified skills, and history of good jobs and good matches – and mechanisms that operate through financial buffers or differential behavior during the shock, such as savings, job search, and health. We examine these explanations by considering whether the ATT estimates of cumulative treatment effects of VT shrink if we reweight controls to have the same distribution of relevant characteristics as compliers. We follow the approach of Hainmueller [2012] where the control group data is reweighted to match pre-pandemic covariate moments among compliers.[13]

[13]To account for attrition and sources of worker heterogeneity that potentially correlate with the reweighting covariate, when reweighting for continuous covariates we first regress the covariate on worker characteristics (either

6.1 Labor Market Attachment

Between 2013 and the eve of the pandemic, VT workers accumulate greater labor market attachment than controls. To get a sense of the differential accumulation of sector-specific experience, Figure A5 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 for controls: each row corresponds to the sector the worker was vocationally trained in (or desired to be trained in), the columns show the share of months spent in each sector. VT workers spent between 25% (plumbing) and 89% (construction) of all working months employed in their sector of training. The off diagonal entries show that VT workers spend almost no time working in other study sectors beyond the one in which they have been trained. 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 months employed in the sector in which they would have like to be trained. In short, VT workers have greater experience working in the good sectors in which they were trained, accumulating more sector-specific skills. These more productive work histories mean trained workers also acquire different search capital, and accumulate more savings than controls. All these margins might lead treated and control workers to differ in their resilience to the pandemic downturn.

The results of the reweighting exercise linked to these mechanisms are in Table 7.[14]

Sector-specific Experience Panel A shows ATT impacts on each cumulative labor market outcome. In Panel B we reweight controls to match the (residualized) cumulative labor market experience of compliers 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 eliminated. The accumulation of sector-specific skills thus matters for resilience [Topel 1991, Neal 1995, Kletzer 1998]. For earnings, after accounting for sector-specific experiences, Column 5 shows the cumulative impact of VT is only slightly affected – falling from 17% to 16%. Column 6 shows a larger adjustment for earnings from wage/self-employment: the cumulative impact of VT falls from 28% to 21%.[15]

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.

[14] 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.

[15] Our finding thus help explain the result in Barrera-Osorio *et al.* [2022] that the returns to VT disappeared during the pandemic partly because in their study context, workers graduated from training courses in December 2018, and so had little labor market experience pre-pandemic.

Experience of Good Jobs To separate out experience in good sectors from experience in good jobs as driving resilience, Panel C of Table 7 repeats the exercise with an alternative measure of labor market attachment: labor market experience in wage/self-employment – irrespective of sector – from baseline to the last pre-pandemic survey. On the extensive margin, Column 2 shows such labor market attachment only explains around half the cumulative impacts of VT over the downturn, 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, 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 VT workers are on average in higher quality matches that pay well, their earnings are more likely to fall following job loss [Schmeider *et al.* 2023]. To distinguish this from the accumulation of sector-specific skills, we proxy good worker-firm matches using the average duration of employment spells from baseline to the last pre-pandemic survey, and reweight controls to match this among compliers. Panel D shows the resulting cumulative impacts of VT: while the baseline estimate suggested treated workers spent 61% more time in good sectors, the reweighted estimate reduces to 44%. On the total earnings margin in Column 5, the cumulative impacts of VT on total earnings fall from 17% to 12%. Hence the returns to VT narrow on the earnings margin when accounting for a history of good matches, but the returns to VT in terms of attachment to good sectors are still primarily driven by the accumulation of sector-specific skills.

6.2 Savings, Search and Health

Savings A consequence of treated workers accumulating more labor market experience and earnings pre-pandemic is that they also enter the downturn with more savings. This can impact their ability to finance costly search behaviors and remain resilient for firms' VT-biased strategies. To explore this mechanism, we consider how our ATT estimates of cumulative treatment effects change if we reweight controls to have the same residualized distribution of savings as complier treated workers as measured in our last pre-pandemic survey. The result in Panel E of Table 7 shows the cumulative impacts on working in the study sectors remain almost unchanged from the baseline estimates (61% vs. 60%), as do the cumulative impacts on total earnings (17% vs. 16%).

Search Behavior In good economic times, treated and control workers differ in their search behaviors [Bandiera *et al.* 2023]. VT workers search more intensively and direct their search towards higher quality firms. As a result, they accumulate different search capital by the eve of the pandemic. Hence, the resilience of VT workers might reflect their continued use of different search behaviors. In the pandemic surveys we asked individuals about search effort and whether they directed their search towards particular sectors, firms or locations. We find little evidence that VT and control workers differ in search behavior along either margin (Table A8).[16]

An implication relates to the generalizability of returns to interventions as aggregate conditions vary [Rosenzweig and Udry 2020]. By evaluating the returns to vocational training in good times [Alfonsi *et al.* 2020, Bandiera *et al.* 2023] and during a downturn, we show that although returns to VT are sustained over both periods, the underlying mechanisms differ. Certification and job search behavior are key to generating returns to VT 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. The accumulation of sector-specific skills is also key to ensuring the resilience of VT workers, while the speed of the downturn likely makes search strategies relatively ineffective.

Health and Other Experiences The mechanisms documented driving firms' VT-biased strategies and VT workers resilience are not specific to the pandemic, but have wider implications for how downturns propagate through labor markets in developing countries. However, one obvious pandemic-specific feature of the downturn relates to potential health impacts on workers determining their labor market trajectories. 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. This is unsurprising given Covid-19 case rates were relatively low in Uganda. Finally, we present results exploring the possibility that treated and control workers experienced the pandemic differently on other margins.

7 Discussion

Developing countries often grapple with fast-moving aggregate shocks: current shockwaves reverberating around the developing world from energy price shocks being a sobering example. Understanding how such shocks propagate through labor markets is critical for determining individual well-being and future economic prospects. We exploit a 10-year panel of firms and workers to use the lens of the pandemic downturn to provide novel insights on how aggregate shocks impact firm and worker behaviors, paying close attention to how firm responses vary across the skills distribution of workers, and the consequent heterogeneous impacts across workers.

We show that in the face of falling demand and a need to cut costs, the key response of firms

[16] On search intensity, treated and control workers do not differ over the pandemic in terms of whether they are searching for work. Conditional on actively searching, they also do not differ on how many days they spend searching, the number of applications they send, or job offers received. On whether workers strategically revise the value of employment they attach to different sectors or firms and so engage in directed search, treated and control workers also do not differ in terms of whether they report searching for work in the eight study sectors, in formal firms, in the informal sector and in Kampala (Columns 5 to 8 of Table A8).

was to lay off more skilled and experienced workers, corresponding to workers that had received vocational training (VT). The mechanisms for this are that firms internalize the fact that if they lay off non-VT workers, they will need to recruit and train new hires on-the-job later as demand recovers, that the costs of firing non-VT workers are higher because they are more socially tied to owners, and the demand for complex products produced by VT workers falls by relatively more during the downturn than for simpler products produced by non-VT workers. However, despite being more exposed to the shock, VT workers remain resilient over the downturn because of their certified skills and pre-pandemic accumulation of sector-specific skills.

Our findings contrast instructively with the pandemic labor market experience in high-income countries, where high-wage workers experienced small and relatively transient employment losses. The differences that help explain this divergence are that: (i) high-income countries introduced policies incentivizing firms to hoard labor [Guerrero *et al.* 2022, Giupponi and Landais 2023, Gourinchas *et al.* 2025], while no such intervention existed in Uganda; (ii) firms in high-income settings had greater availability of credit to help smooth input adjustments, and also face higher firing costs than firms in Uganda and other low-income countries; (iii) transitions to remote-work are largely irrelevant in our study sectors. These comparisons suggest VT-biased responses may be a more general feature of how firms in developing countries respond to aggregate shocks, and how aggregate shocks propagate from firms through to workers for the majority of the global workforce.

We draw a number of policy implications relevant for low-income settings from our findings.

One set of implications stem from our results on worker behaviors and mechanisms driving resilience. First, in our earlier work we documented the IRR to vocational training to be 30% in the pre-pandemic steady state [Alfonsi *et al.* 2020]. The returns to VT remain positive even during a severe downturn – although the earnings premium halves during the pandemic, once we factor in the utility gains from the insurance value of VT, the returns to vocational training over good times and bad remain high. The resilience that skills interventions build might be in contrast to other anti-poverty interventions whose returns could dissipate during aggregate shocks. Moreover, because the insurance value of vocational training operates through labor market mechanisms – certified skills and sector-specific experience enabling workers to reallocate across firms within a sector – rather than through any financial buffer that higher pre-pandemic earnings and savings of VT workers provide, cash transfers or savings programs alone may be insufficient to replicate the resilience that vocational training provides.[17]

Another set of implications stem from our results on firm behaviors and mechanisms driving VT-biased responses to the downturn. The first is the distinction between the private optimality

[17] 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 [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 that provide certified skills, 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.

of VT-biased strategies for individual firms and their social efficiency. Our calibration exercise establishes conditions under which VT-biased responses are optimal for firms. However: (i) the loss of productive worker-firm matches may reduce aggregate output even as individual firms recover; (ii) the persistent earnings losses we document for both VT and control workers suggest much of the social costs of the downturn are born by workers across the skills distribution. While our results suggest that even without social insurance, certified vocational training can help workers remain resilient, they still underscore the high demand for social insurance in low-income settings.

By studying both sides of the labor market to analyze how aggregate shocks propagate from firm responses to implications for workers' trajectories, our findings emphasize potentially high returns to policies to help firms avoid VT-biased strategies and hoard the most productive labor in economic downturns. In fast moving aggregate shocks, extending such policies to low-income settings may be, on the margin, more effective than targeting workers directly, given it is hard for policymakers to observe the skills of workers, and it is firms' strategies that trigger job loss among skilled workers – the spark that sets off a long shadow of consequences for employment, productive worker-firm matches, growth, and trajectories of economic development.

A Appendix

A.1 Calibration

To map the model to the data, we construct proxies for prices, quantities, compensation, and workforce composition by combining information from the last pre-pandemic employee roster with firm-level outcomes and worker flows observed during the pandemic. February 2020 is treated as the pre-pandemic baseline, and April 2021 as the final post-pandemic period.

Prices We proxy worker-type-specific output prices using information on the price of a typical product. From the employee roster in Wave 4, we observe the average price of the product most frequently worked on by vocationally trained (V) and non-VT (U) workers, denoted p_{V4} and p_{U4}. At the firm level, we observe the price of a typical product produced by the firm in Waves 4 to 6. We thus compute firm-level price deflators. The February 2020 deflator is $\Delta_{\text{Feb20}} = \frac{p_{2020}^{\text{firm}}}{p_4^{\text{firm}}}$, and the April 2021 deflator is $\Delta_{\text{Apr21}} = \frac{p_{2021}^{\text{firm}}}{p_4^{\text{firm}}}$. We impute worker-type-specific prices by assuming prices of the product most frequently worked on by VT and non-VT workers evolve proportionally to the firm's typical product price: $p_{Vt} = p_{V4} \cdot \Delta_t$, $p_{Ut} = p_{U4} \cdot \Delta_t$, $t \in \{\text{Feb 2020, Apr 2021}\}$.

Quantities Quantities are proxied using firm revenue, assuming revenues reflect output per worker. From the employee roster in Wave 4, we compute baseline monthly quantities produced per worker by dividing earnings by piece rates: giving Q_{V4} and Q_{U4}. We compute average firm-level revenue growth relative to wave 4 for February 2020 and April 2021, based on revenues reported in the pandemic firm surveys. Quantities in each period are obtained by scaling baseline type-specific quantities by the corresponding revenue growth factor: $Q_{i,\text{Feb20}} = Q_{i,4}\left(1 + g^{\text{rev}}_{\text{Feb20}}\right)$, $Q_{i,\text{Apr21}} = Q_{i,4}\left(1 + g^{\text{rev}}_{\text{Apr21}}\right)$, $i \in \{V, U\}$.

Piece Rates and In-Kind Payments From the employee roster in Wave 4, we observe piece-rate earnings and in-kind compensation for VT and non-VT workers. We compute average piece rates τ_V, τ_U and in-kind payments b_V, b_U by worker type. We assume these are held fixed across periods. To justify this, we draw on data from Alfonsi *et al.* [2023] that was collected over the pandemic from graduates of VTIs in Uganda. Using a comparable sample of workers, we find 90% of vocationally trained workers report no reductions in piece rates during the pandemic. These labor markets therefore display downward nominal wage rigidity, even in the pandemic downturn, in line with evidence documented in other low-income contexts [Kaur 2019].[18]

Workforce Composition We proxy the workforce composition by combining the pre-pandemic baseline with observed worker flows in each pandemic survey. Using the employee roster in wave 4, we compute each firm's pre-pandemic share of VT workers, denoted $\widehat{\text{share}}_{V4}$. We assume this remains constant until February 2020, and construct the level of baseline VT employment at this time as $\hat{n}_{V\text{Feb20}} = \widehat{\text{share}}_{V4} \times N_{\text{Feb20}}$ where N_{Feb20} is total employment at the firm in February 2020. Non-VT employment is obtained residually as $\hat{n}_{U\text{Feb20}} = N_{\text{Feb20}} - \hat{n}_{V\text{Feb20}}$.

We then update the workforce composition in two steps. First, we proxy VT employment in December 2020 using hires and separations reported between March and November 2020. Second, taking the December 2020 proxy as the new baseline, we use flows between December 2020 and June 2021 to proxy workforce composition in April 2021. As described in the main text, we do not directly observe whether hired or separated workers were vocationally trained. Instead, for hires, we observe whether the last hire (wave 5) or the majority of hires (wave 6) had: (i) experience in the same sector, (ii) experience in another sector, (iii) no experience but vocational training, or (iv) was unskilled. We classify hires as VT if they have (i) experience in the same sector or (iii) no experience but vocational training. For separations, we observe whether the last separation (wave 5) or the majority of separations (wave 6) involved workers with (i) substantial experience in the firm, (ii) experience in the sector, or (iii) unskilled workers. We classify separations as VT only if they involved workers with (i) substantial experience in the firm.

We then update VT employment according to: $\hat{n}_{Vt+1} = \hat{n}_{Vt} + \text{Hires}_t \cdot \mathbf{1}\{\text{hire is VT}\} - \text{Separations}_t \cdot \mathbf{1}\{\text{separation is VT}\}$. If a firm reports no hires and no separations, we assume workforce composition is unchanged. If hires or separations are positive but the corresponding skill information

[18] Exploring other responses related to compensation we find that: (i) 95% of 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 owners reporting allowing employees more flexibility in hours at work ($p = .001$).

is missing, we set the proxy to missing. Finally, we impose feasibility bounds by restricting VT employment to lie between zero and total firm employment in each period.

A.2 Worker Attrition

To examine whether there is differential attrition between treated and control groups, 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 considered are those that could affect behaviors and 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. Table A5 shows that on most margins and survey waves we find little evidence of heterogeneous attrition, 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 among the treated group until survey wave 4 (2018). Table A9 re-examines baseline balance among non-attriters by survey waves 4 to 7. In line with little selective attrition, among non-attriters we find no significant differences between treatment and control groups on each outcome.

A.3 Pre-pandemic Outcomes

We briefly review results from our earlier work [Alfonsi *et al.* 2020] that establishes the pre-pandemic impacts of VT on skills and labor market outcomes. To do so, we estimate the following ITT specification for outcome y_{isw} for worker i in strata s in survey wave w:

$$y_{isw} = \alpha + \beta VT_i + \gamma y_{is0} + \delta \mathbf{x}_{is0} + \lambda_s + u_{isw}, \tag{7}$$

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.[19]

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

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

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 A10 reports the results. Panel A reports the ITT estimates $\widehat{\beta}$ from (7), and Panel B reports ATT estimates.[20]

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 A10 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 .28σ 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 .41σ of test scores).[21]

Tasks To validate that acquired skills are relevant to our study sectors, we analyze worker tasks from 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). For task j in sector k, we construct the share of workers reporting performing task j, for compliers and controls. Figure A6 graphs the difference in these shares by task and sector. Focusing on the four main study sectors, we observe a clear divergence: some tasks are performed more by VT workers (right side of each panel), others more by controls (left side). In three of four sectors, a Chi-squared test rejects the null that task composition is the same between groups. This reinforces the notion that VT and non-VT workers specialize in different products within these multi-product firms.[22]

[20] 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).

[21] 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.

[22] 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/. 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.

Labor Market Outcomes We consider labor market outcomes in the final pre-pandemic survey, measured from March to July 2018, around 55 months after workers graduate from vocational training. In Panel A of Table A10, 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 VT translates into cumulative impacts on outcomes across all pre-pandemic surveys (waves 1 to 4). In these surveys we asked workers to recall their labor market outcomes over 12 months, so can construct a panel of employment spells and earnings histories, based either on monthly or quarterly recall data depending on the outcome and wave. Columns 5 and 6 in Panel A show those offered vocational training: (i) accumulate 83% more work experience in one of the study sectors; (ii) accumulate 42% higher earnings than controls. From Panel B we see that VT workers: (i) accumulate 117% more experience working in one of the study sectors; (ii) accumulate 59% higher earnings than controls.[23]

A.4 Heterogeneity

Gender A major lesson from the pandemic, across high- and low-income settings, was the gendered nature of impacts of lockdowns. Women's labor force participation was more affected because: (i) the sectors they work in are more exposed to social distancing restrictions [Alon *et al.* 2022]; (ii) the unequal distribution of housework and care duties [Adams-Prassl *et al.* 2020]. This is especially relevant in Uganda, where schools remained closed for an extended period. The first set of results in Table A12 thus consider how the returns to VT over the pandemic vary by gender. In Panels A and B we see the cumulative ATT effects of VT are generally larger for women. The most striking contrast is in shifts into casual work: vocationally trained men are 26% less likely to shift into casual work, while vocationally trained women are 40% *more* likely to do so compared to controls. This is in line with findings from Alfonsi *et al.* [2023] in urban Uganda, and those of Chakravorty *et al.* [2023] in rural India. These divergent shifts lead to no earnings gains from casual work for VT women, but a 37% drop for VT men.[24]

[23] Table A11 confirms that on all but one dimension of pre-pandemic outcome, there are no statistically significant differences between workers with and without match offers.

[24] 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 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].

Desired Sector of Training Workers who initially wanted vocational training in manufacturing sectors may differ in unobserved ways from those who preferred service sectors. Given desired sector of work correlates highly to the actual sector treated workers are trained in, this also proxies for whether the individual spends most of their working life in manufacturing or services. 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, VT workers spend 28% less time in casual work, in line with our baseline results. Both sets of VT workers retain a large advantage over controls in terms of total earnings and earnings from wage/self-employment.

Region of Residence To explore whether locations help explain the returns to VT over the pandemic, we reweight controls to have the same region of residence of 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%), and total earnings impacts shift only slightly (17% vs. 18%).

Matching We next consider whether cumulative treatment effects differ between those offered vocational training and those additionally offered matching. Panels F and G show slightly larger employment effects for those offered only VT, while earnings impacts from wage/self-employment are marginally higher among those also offered matching.

A.5 Robustness to Attrition

We address concerns about attrition using multiple approaches following Blattman *et al.* [2020] and using data from the three pandemic survey waves (i.e. waves 5, 6 and 7). Results are in Table A13, where each row corresponds to our key cumulative outcomes. For reference, Column 1 shows our baseline ATT effects over the pandemic. Column 2 shows results to be very similar when we drop the controls ($\mathbf{x}_{is0}$). Column 3 shows that using inverse probability weighting (IPW) to correct for selective attrition also produces minimal changes in results.[25]

In the remaining Columns we test robustness to unobserved selection using bounding exercises in the spirit of Manski bounds. Column 4 replaces all missing values in both compliers and controls with the average outcome for non-attriters in the control group. This effectively assumes compliers

[25] 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.

who attrit are negatively selected, but control attriters are not. As is intuitive, the ATT estimates are slightly lower than in Column 1, but remain positive and significant.

In Column 5, we assign control group attriters an outcome .1SD above the mean for control non-attriters, and complier attriters are assigned an outcome .1SD below the same mean. This assumes positive selection in controls and negative selection in 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 scenario, while ATT estimates on earnings remain positive but are no longer significant. Column 6 reverses this assumption, assigning complier attriters .1 SD above and control attriters .1 SD below the control mean. Under this imputation, treatment effects are similar to Column 1 and remain 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, results from these bounding exercises show our findings are robust to plausible degrees of selective attrition on unobservables. This reinforces the earlier direct evidence of no selective attrition on unobservables over time between treated and control groups.

A.6 Health and Other Experiences of the Pandemic

One obvious pandemic-specific feature of the downturn relates to potential health impacts on workers determining their labor market trajectories. We consider whether health interacts with labor market outcomes, and whether these interactions differ between VT and control workers. Table A14 first considers 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.[26]

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

[26] 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).

of economic recovery. At the outset of the pandemic, 27% of controls expected the economy to rebound within six months (Column 7) and 66% expected a rebound within a year. We see no differences in these expectations between treated and control workers.

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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	.341	.330	[.393]	.402	[.000]		
	(.278)	(.277)		(.280)			
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)					

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. 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, Customers and Sales Revenues Over the Pandemic

p-values of test of equality in brackets

		Firm Survey Wave 5 Recall	Firm Survey Wave 5	Firm Survey Wave 6	[p-value]	[p-value]
		March 2020	Oct-Dec 2020	May-Jul 2021		
		(1)	(2)	(3)	(1) = (2)	(2) = (3)
A. Pandemic						
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. Pandemic Period Relative to Pre-pandemic						
Number of customers per week		29.8	14.0	13.2	[.000]	[.584]
		(58.7)	(30.7)	(31.0)		
Monthly revenues per customer		95.9	132	103	[.167]	[.335]
		(432)	(566)	(527)		
Expected total sales throughout 2020 (2021) as a percentage of 2019 sales			.504	1.09		[.000]
			(.484)	(1.38)		

Notes: All data comes from the fifth and sixth rounds of firm-side surveys. We report mean outcomes (with standard deviations in parentheses for continuous variables). The last two Columns report p-values in brackets on tests of equality of means across the survey periods. Panel A presents firms' expectations about the pandemic. Sums might not report to one as firms could also respond 'don't know'. In Panel B, we report the number of customers that placed orders or made purchases at the firm in the last week. In Wave 5, we ask firm owners to recall the number of customers in the last week or in the month of March 2020 (the month before the first lockdown) and the number of customers in the week/month preceding the survey, which ran from Oct-Dec 2020. For firms in retail sectors (hairdressing, tailoring, catering, motor-mechanics), we ask firm owners for the number of customers in the last week in March 2020 and in the last week before the survey. For firms in non-retail sectors (construction, welding, plumbing, electrical wiring), we ask firm owners for the number of customers in March 2020 and in the last month before the survey. We harmonize these by dividing the number of customers in the last month by four to get the average number of customers per week. In Wave 6, which ran from May-Jul 2021, we asked firm owners to recall the number of customers in the last week or in the month of May 2021, depending on whether the firm operates in a retail sector. As in Wave 5, we harmonize these questions to get the average number of customers per week for all sectors. In the second row of Panel B, we winsorize the top 1% of monthly revenues per customer. In the third row, we report the expected total sales in 2020 as a share of 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.

Table 3: Firm Dynamics Over the Pandemic

OLS Panel regression coefficients, robust standard errors in parentheses

	Operating	Number of Employees	Revenues	Profits	Wage Bill / Revenue
	(1)	(2)	(3)	(4)	(5)
February 2020		reference period			
April 2020 (during first lockdown)	-.529***	-2.93***	-515***	-188***	-.058**
	(.017)	(.591)	(65.1)	(25.7)	(.026)
July 2020	-.088***	-2.29***	-425***	-148***	-.021
	(.015)	(.392)	(56.9)	(16.1)	(.018)
November 2020	.051***	-1.17***	-87.7	-66.1***	-.066***
	(.013)	(.391)	(63.0)	(16.8)	(.016)
February 2021	.033**	-1.94***	-187***	-111***	-.067***
	(.013)	(.367)	(66.1)	(16.1)	(.016)
April 2021	.023*	-1.69***	-205***	-109***	-.084***
	(.014)	(.449)	(67.0)	(16.4)	(.016)
Mean in February 2020	.869	5.58	803	244	.404
April 2020 = April 2021 [p-value]	[.000]	[.033]	[.000]	[.001]	[.318]
Baseline firm characteristics	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	6577	5006	4502	4502	2859

Notes: All data comes from the fifth and sixth round of firm-side surveys. OLS estimates are shown with robust standard errors in parentheses. In Column 5 the wage bill is calculated as the monthly earnings of the average worker multiplied by the number of employees. 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, and winsorized at the top 1%. 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: Worker Roster, Pre-Pandemic

Wave 4 Firm Survey (Mar-Jul 2017)
Means, standard deviations in parentheses
p-values of test of equality in square brackets

	VT Worker (1)	Not VT Worker (2)	[p-value] (1) = (2)
A. Worker Composition			
Share of type	26.1%	73.9%	
Owner assessed productivity of worker (0-10) at time of survey	7.87	6.79	[.000]
	(1.78)	(2.46)	
Skilled when recruited	.705	.347	[.000]
Tenure (months)	35.8	23.5	[.000]
	(34.7)	(29.3)	
B. Skills and Training			
Able to perform typical task when recruited	.616	.368	[.000]
Received on-the-job training (at the firm)	.340	.618	[.000]
	(.474)	(.486)	
Length of on-the-job training (months)	8.50	9.15	[.091]
	(6.60)	(6.03)	
Able to perform typical task at time of survey (2017)	.869	.755	[.000]
C. Relationship to Owner			
Relationship to owner: family/friend	.362	.392	[.093]
Hired through family/friends	.335	.396	[.002]
D. Earnings, Hours and Production			
Total earnings last month	105	73.3	[.000]
	(115)	(92.9)	
Total hours worked per month	266	263	[.295]
	(79.5)	(71.5)	
Price charged for typical product	61.9	47.2	[.000]
	(108)	(90.0)	
Value of materials used for typical product	33.5	26.2	[.003]
	(61.8)	(52.1)	
Hours spent working on the product the employee most works on	5.79	4.89	[.047]
	(15.2)	(7.39)	
Number of typical items produced last month	25.7	32.8	[.026]
	(42.8)	(61.5)	
Paid piece-rate only or piece-rate + in-kind	.760	.644	[.000]
	(.428)	(.479)	
Price received by employee for typical product (piece-rate)	9.22	6.72	[.000]
	(12.3)	(10.0)	

Notes: All data come from the employee roster in the last pre-pandemic firm-side survey (fourth wave), conducted in Mar-Jul 2017. Workers are split into vocationally trained (VT) workers (Column 1) and non-vocationally trained workers (Column 2), based on whether the worker ever received vocational training. The table reports worker-level mean outcomes, with standard deviations in parentheses for continuous variables. Column 3 reports the p-value of a t-test of equality of means between VT and non-VT workers. Panel A reports the owner's 0-10 assessment of how productive each employee is relative to all workers the owner knows in the same sector, an indicator for whether the worker was skilled when recruited (reported by the firm owner), and how long the worker has been employed at the firm (in months). Panel B reports an indicator for whether the worker is able to independently perform a sector-specific task at the time of recruitment, and at the time of the survey, and an indicator for whether the worker has received on-the-job training at the firm. The sector-specific task was defined by the research team in collaboration with BRAC as a typical task common to all firms within the same sector. Panel C reports indicators for whether the owner describes the relationship to the worker as family/friend, as family, and whether the worker was hired through family or friends. Panel D reports total earnings in the last month (cash plus in-kind) and total hours worked per month. Total hours worked per month are calculated as reported hours worked per day multiplied by days worked per week and then by four. For each worker, the owner reports the typical product the employee works on and provides information on the production and pricing of that product. The price charged for the typical product and the value of materials used for that product are conditional on the worker working in production; the number of typical items produced last month, and the price received by the employee for the typical product (the piece-rate), are all conditional on the worker being paid and working in production. The number of typical items produced last month is calculated at the worker level as total earnings last month / piece rate for the typical product, where total earnings includes both cash and in-kind earnings. All monetary variables (earnings and prices) are winsorized at the top 1% (in-kind and money earnings are winsorized separately before they are summed for total earnings) and 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: 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 6: Worker Mobility During the Pandemic Downturn

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 7: Mechanisms Driving the Resilience of VT Workers

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

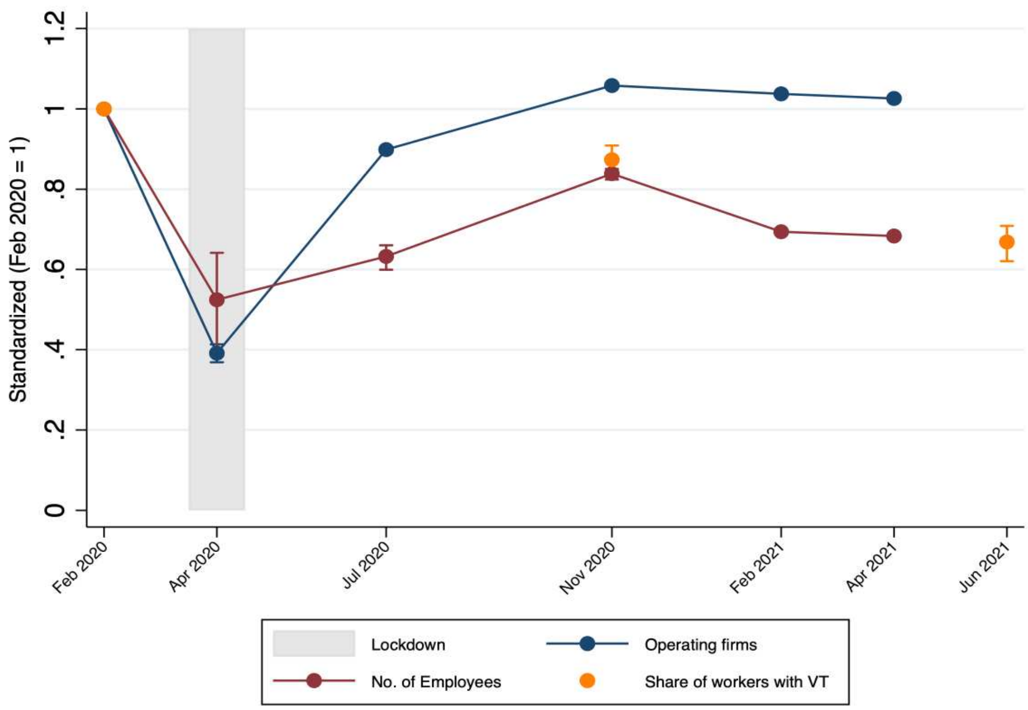
ATT estimates

	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

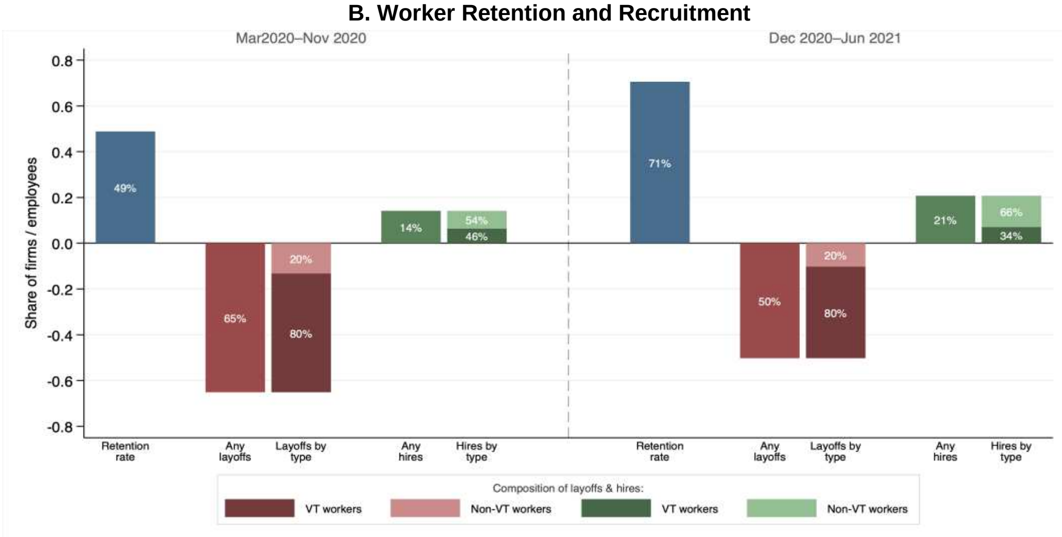
Notes: Each panel reports interpolated estimates of cumulative outcomes covering the 25 months between February 2020 and February 2022, from the 14 months of outcome data collected over the 7 time-frames of pandemic surveys. We use a constant imputation method, so assuming each outcome remains constant 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 the estimation sample using entropy balancing so that the distribution of the residualized reweighting variable among controls matches that of compliers. These entropy balancing weights are applied to the IV regression. The reweighted control mean continues to use the probability of compliance weights in all panels. 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 1: Firm Outcomes Over the Pandemic

A. Operating, Labor Demand and Share of Vocationally Trained Employees



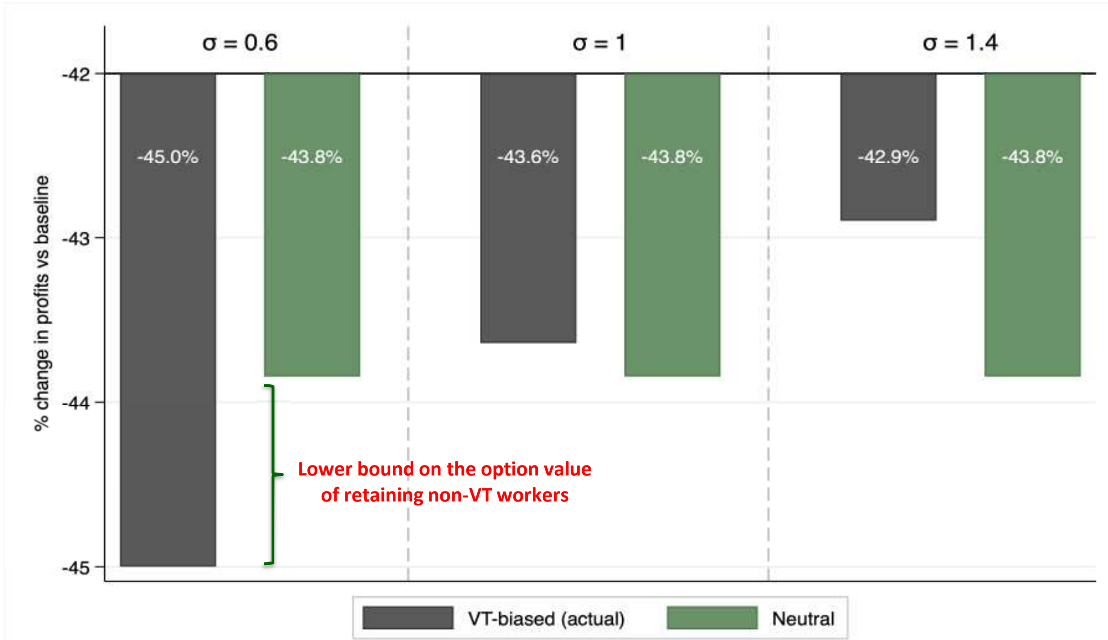
B. Worker Retention and Recruitment



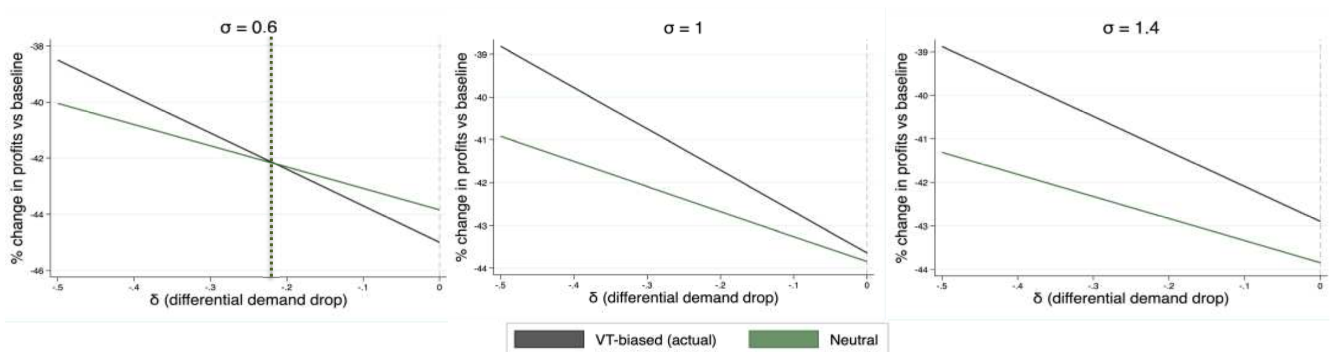
Notes: All data comes from the fifth and sixth rounds of firm-side surveys, fielded in October-December 2020 and May-July 2021, respectively. In Panel A, the gray shaded region corresponds to the first COVID-19 lockdown. All outcomes are normalized to 1 at their February 2020 levels; confidence intervals are similarly normalized by dividing each bound by its February 2020 value. The panel reports (i) the share of firms operating (blue), (ii) the average number of employees in operating firms (red), and (iii) the share of workers in operating firms with vocational training (orange). The proxied share of workers with vocational training is only available in February 2020, November 2020, and June 2021, given that the skill level of hires and separations was only asked in these time frames. Panel B reports firms' retention and recruitment behavior over two reference periods: March-November 2020 and December 2020-June 2021. For each period, we report the share of employees still employed at the firm, whether the firm laid off any workers, and whether it recruited any new workers. We also report the skill composition of layoffs and hires, conditional on the firm reporting any layoffs or recruitment in the relevant period. Layoff bars are shown as negative to reflect employment losses. Labels inside split bars show the share of workers with VT and without VT conditional on the firm having any layoffs or hires. Both panels proxy vocational training status. The proxy is computed using the workforce composition at Wave 4 (2017), updated with the hires and separations reported during the pandemic. In the fifth survey wave, owners reported the skill level of most workers laid off between March–November 2020 and the skill level of the most recent new worker hired. In the sixth survey wave, owners reported the skill level of most workers laid off and most workers hired between December 2020–June 2021. For layoffs, skill categories were: (i) substantial experience in the firm, (ii) experience in the sector, (iii) experience in another sector, (iv) no experience but vocational training, and (v) unskilled. For hires, skill categories were: (i) experience in the sector, (ii) experience in another sector, (iii) no experience but vocational training, and (iv) unskilled. We code VT = 1 for layoffs if most laid-off workers had either substantial experience in the firm or no experience but vocational training. We code VT = 1 for hires if the most recent hire (Wave 5) or most hires (Wave 6) had either experience in the sector or no experience but vocational training. Panels A and B report 95% confidence intervals.

Figure 2: Simulated Profits

A. Fall in Profits Under Alternative Firm Strategies

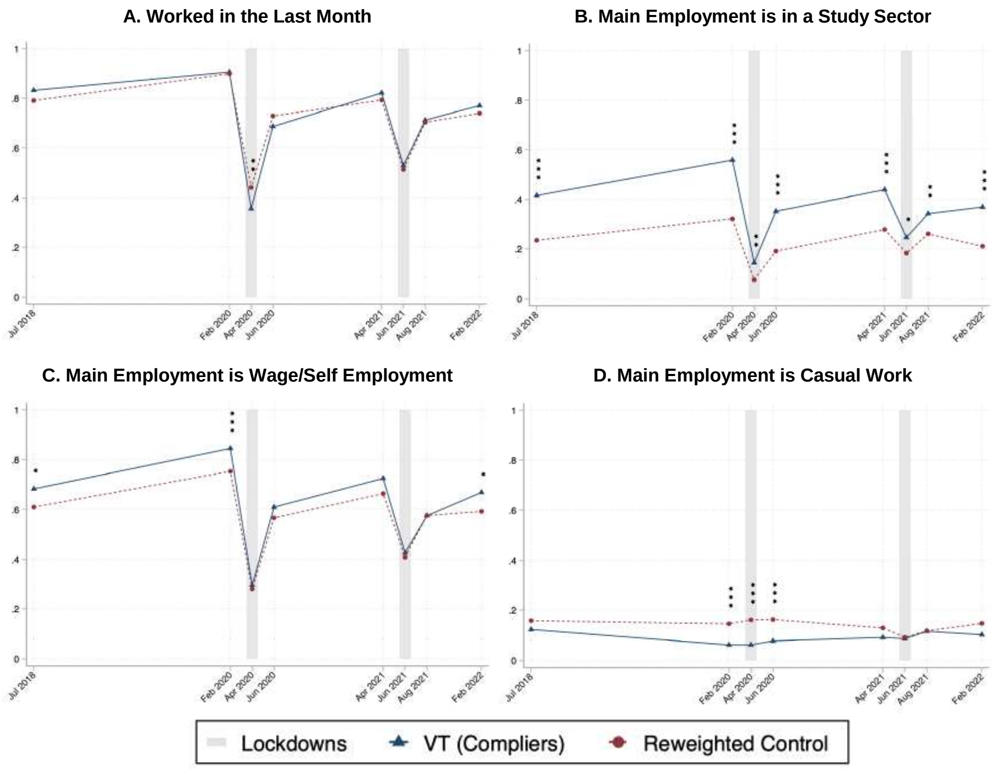


B. Fall in Profits Under Alternative Firm Strategies and Changes in the Relative Composition of Product Demand



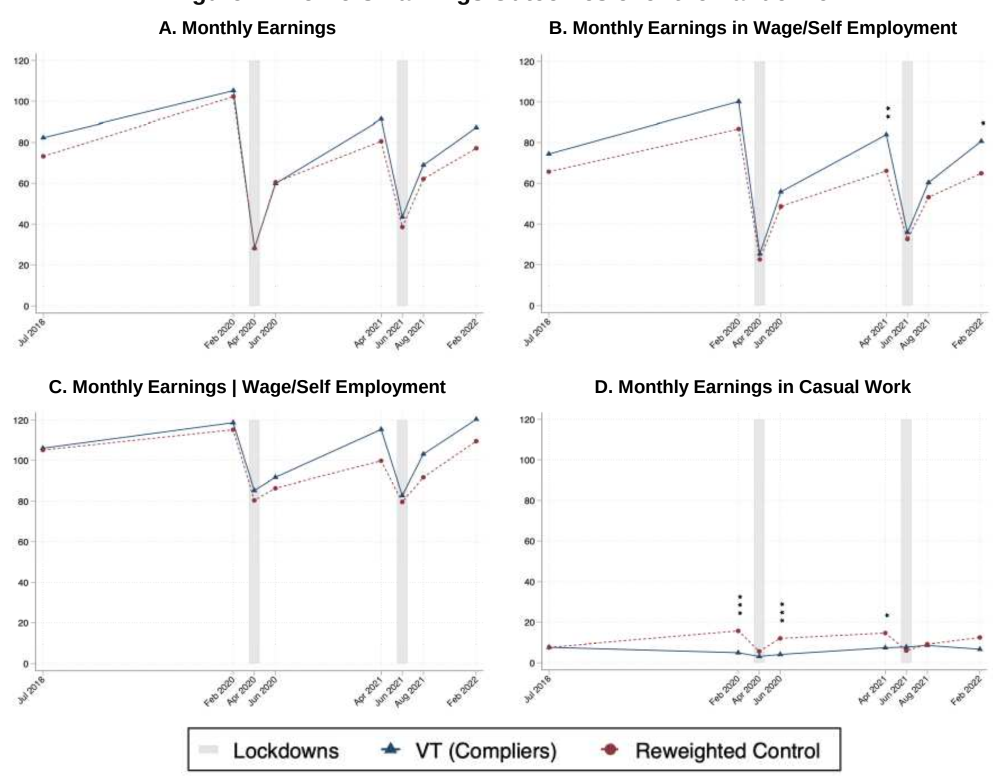
Notes: : Panel A reports simulated percentage changes in firm profits between the pre-pandemic baseline (February 2020) and April 2021, relative to pre-pandemic profits, under alternative assumptions about the skill composition of workers retained during the pandemic. In all simulations, post-pandemic prices and quantities are held fixed at their observed (proxied) post-pandemic values and total employment is held constant; only the skill mix of retained employees varies. In the VT-biased (actual) scenario, we use the proxied post-pandemic composition of employees, reflecting a disproportionate reduction in employment of vocationally trained workers. In the Neutral scenario, we assume the firm reduces employment of vocationally trained and non-vocationally trained workers in equal proportions. Each panel varies the elasticity of substitution between vocationally trained and non-vocationally trained workers in the CES production function for the complex product. Panel B reports the simulated percentage changes in firm profits relative to the pre-pandemic baseline, but under differential demand shifts between the complex and simple products.At $\delta = 0$, both products experience the same demand conditions as in Panel A. Moving left along the x-axis ($\delta < 0$) redistributes the demand drop toward the complex product: the complex product quantity is scaled by $(1+\delta)$ relative to its baseline post-pandemic level, while the simple product quantity is scaled upward to keep the overall demand constant. The figures show only the range $\delta \in [-0.5, 0]$, i.e. only the case in which the complex product absorbs a larger share of the total demand drop.

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.

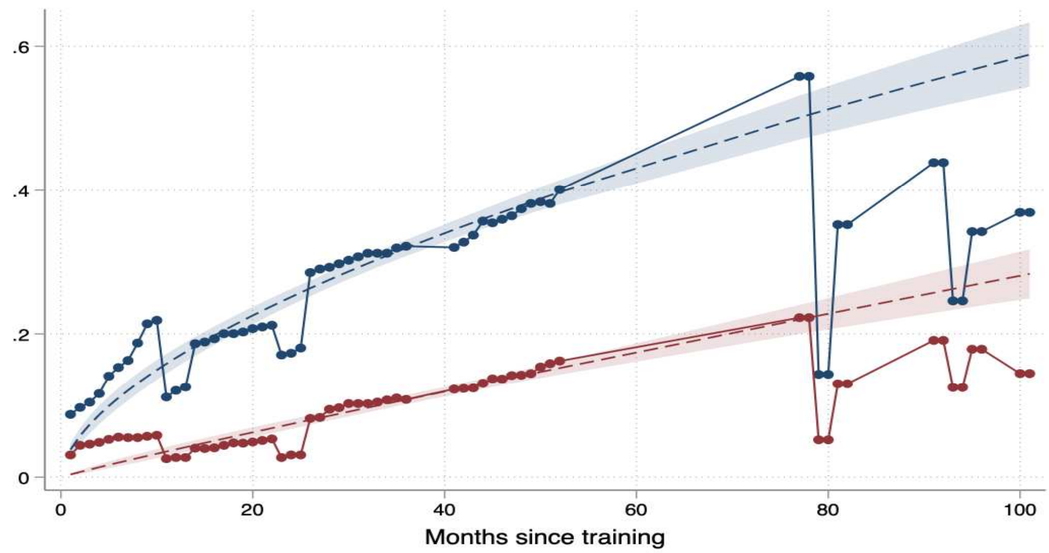
Figure 4: Workers Earnings Outcomes over the Pandemic



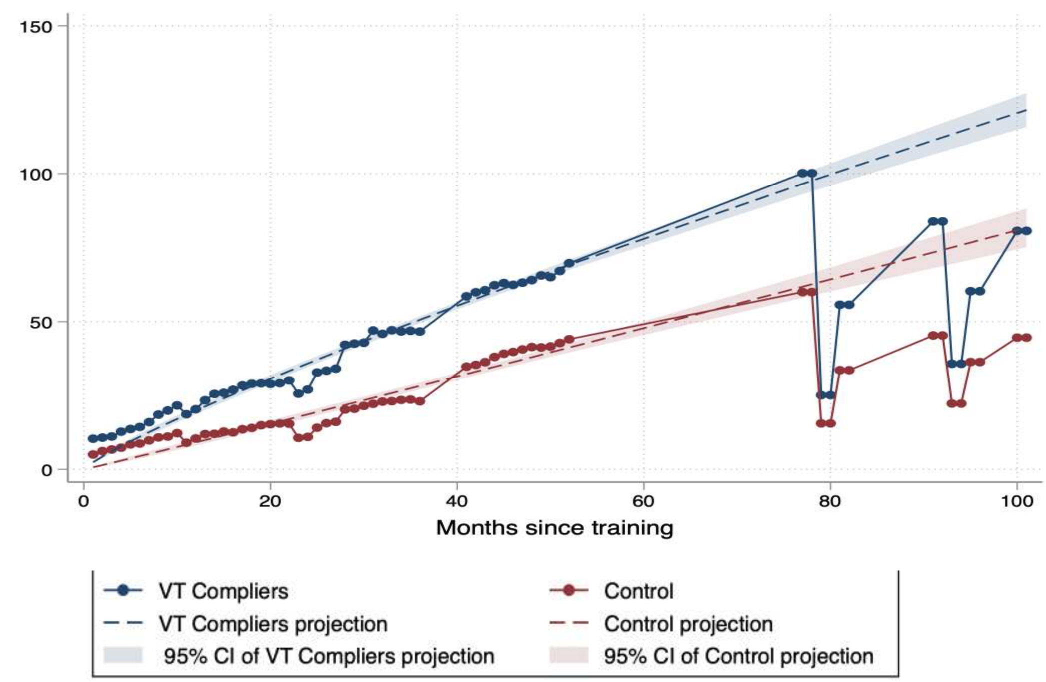
Notes: In each Panel we compare mean outcomes for compliers to the offer of vocational training to controls, where controls are reweighted by their probability of compliance. The first data point corresponds to Wave 4 conducted in 2018 before the pandemic survey waves. The stars in each time frame report 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: No-pandemic Counterfactual

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.

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: Firm Attrition

OLS regression coefficients, robust standard errors in parentheses

	Outcome: Firm missing in		
	2017 (Wave 4)	2020 (Wave 5)	2021 (Wave 6)
	(1)	(2)	(3)
Number of employees	.004	-.006	-.005
	(.004)	(.005)	(.004)
Log monthly profits (USD)	-.033***	.027**	.019
	(.010)	(.012)	(.011)
Manufacturing	-.021	-.009	-.038*
	(.018)	(.022)	(.022)
In Kampala	.149***	-.028	-.054***
	(.016)	(.021)	(.020)
Firm age	-.006***	-.005***	-.005***
	(.002)	(.002)	.002
Female owner	-.044**	.035	.021
	(.018)	(.021)	(.021)
Owner age	.002	.001	-.001
	(.001)	(.001)	(.001)
Mean outcome	.157	.284	.272
Test of joint significance of firm characteristics [p-value]	.000	.025	.000
R-squared	.058	.081	.103
Number of observations (firms)	1860	1860	1860

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 is missing in survey waves 4, 5, and 6, respectively. A firm is recorded as missing in a given wave if it cannot be located, or if the firm owner is recorded as deceased, mentally ill, or having moved abroad. Attrition is wave-specific: firms that were missing in one wave could re-enter the sample in subsequent waves. Firms that were not approached in a given wave are coded as non-missing, and we additionally control for a dummy indicating whether the firm was not approached. The covariates included in all Columns are collected at baseline. 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. 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 A3: 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	
		(.063)	(.015)	(.006)	(.010)	(.004)	(.006)	(.015)	
Control	448	20.1	.596	.028	.103	.011	.042	.562	
		(.100)	(.023)	(.008)	(.015)	(.005)	(.010)	(.024)	
Offered Vocational Training	692	20.0	.548	.044	.126	.023	.035	.563	{.682}
		(.080)	(.019)	(.008)	(.013)	(.006)	(.007)	(.019)	
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 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: 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 A7: 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 instrument actual take-up with the random offer of training, using a 2SLS framework. The regression includes time-period dummies and interactions between each period dummy and treatment status. By including all interaction terms simultaneously, each coefficient reflects the conditional ATT for that period, net of the treatment effect in other periods. That is, the estimate isolates the marginal impact of treatment in each period relative to controls and controlling for other time periods. 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 A8: 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 A9: 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 A10: 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 in which main activity was in any of the eight sectors	Monthly earnings from wage/self-employment (USD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITT						
Offered Vocational Training	.225***	5.98***	.121***	13.0**	5.03***	528***
	(.042)	(2.12)	(.040)	(6.55)	(.874)	(132)
Panel B: ATT						
Vocationally Trained	.319***	8.49***	.181***	18.6**	6.90***	760***
	(.056)	(2.87)	(.058)	(9.19)	(1.15)	(185)
Control mean (SD)	.663	30.7 (21.3)	.240	72.0 (75.0)	5.99	1263
Reweighted control mean (SD)	.664	30.9 (21.4)	.235	73.2 (75.9)	5.90	1281
Number of observations	755	755	1008	935	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 and 6, 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 A11: 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			
	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 in which main activity was in any of the eight sectors	Monthly earnings from wage/self-employment (USD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITT						
T1: Offered Vocational Training	.234***	5.01**	.125***	11.7*	5.43***	420***
	(.044)	(2.20)	(.042)	(6.99)	(1.05)	(156)
T2: Offered Vocational Training + Matched	.205***	7.92***	.115**	15.1*	4.50***	670***
	(.049)	(2.64)	(.047)	(7.88)	(1.09)	(176)
Panel B: ATT						
T1: Vocationally Trained	.314***	6.72**	.180***	16.1*	7.05***	578***
	(.054)	(2.80)	(.059)	(9.37)	(1.30)	(205)
T2: Vocationally Trained + Matched	.329***	12.8***	.185**	23.6*	6.67***	1046***
	(.073)	(4.07)	(.073)	(12.1)	(1.57)	(269)
p-value: T1=T2 (ATT)	[.759]	[.060]	[.931]	[.457]	[.819]	[.104]
Control mean (SD)	.663	30.7 (21.3)	.240	72.0	5.99	1263
Reweighted control mean (SD)	.664	30.9 (21.4)	.235	73.2	5.90	1281
Number of observations	755	755	1008	935	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 and 6, 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 A12: 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 + Matched	-.324	2.91**	-.020	-.442	353	477**	-123*
	(.944)	(1.30)	(1.13)	(.749)	(235)	(237)	(74.2)
Reweighted control mean	17.9	5.64	14.3	3.52	1577	1272	305
Implied Treatment Effect (%)	***-1.81%***	***51.6%***	***-.140%***	***-12.6%***	***22.4%***	***37.5%***	***-40.3%***
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. In all panels, the reweighted control mean reweights control group observations by their probability of compliance. The implied treatment effect is calculated dividing the ATT by the reweighted control mean. Panels A and B split the sample by gender (dummy). Panels C and D split the sample by desired sector of training at baseline (dummy). The reweighted control mean within each subgroup continues to apply the probability of compliance weights. Panel E additionally reweights controls using entropy balancing so that the distribution of region of residence among controls matches that of compliers. Entropy balancing is used rather than sample splitting to exploit the multi-valued categories of region of residence. Panels F and F present results separately by treatment arm, again applying the probability of compliance weights to the control group mean. 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 A13: 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 A14: Worker Health and Search Behavior

ATT estimates, robust standard errors in parentheses

	Pre-pandemic health in 2016		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.

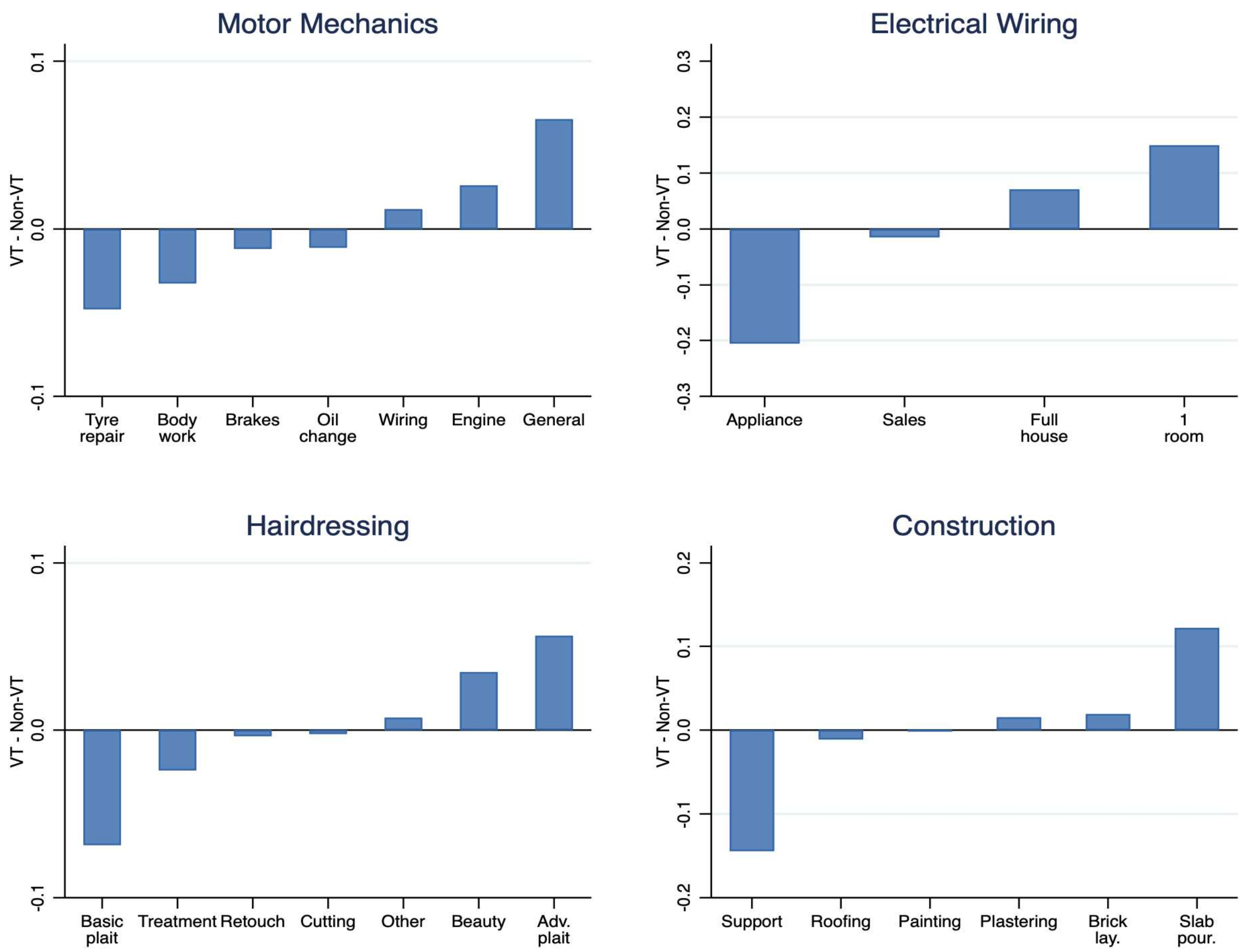
Table A15: Experiences of the Pandemic

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: Differential Engagement of VT and non-VT Workers in the Production of Goods/Services, by Sector



Notes: Data are from the Wave 4 firm-side follow-up survey. For each employee listed in the firm's employee roster, the survey asks the firm owner to identify the product/service the employee works on most often. Responses are recorded as free text by the enumerator. We clean these free-text responses and classify them into within-sector product subcategories by matching similar descriptions. Each bar shows the difference in the share of VT workers in that subcategory minus the share of non-VT workers (i.e. the share of all VT workers whose main product falls in that subcategory, minus the corresponding share of non-VT workers). Bars are sorted from most negative (non-VT workers over-represented) to most positive (VT workers over-represented). Workers with missing VT status are excluded.

Figure A2-A: Timeline of Surveys

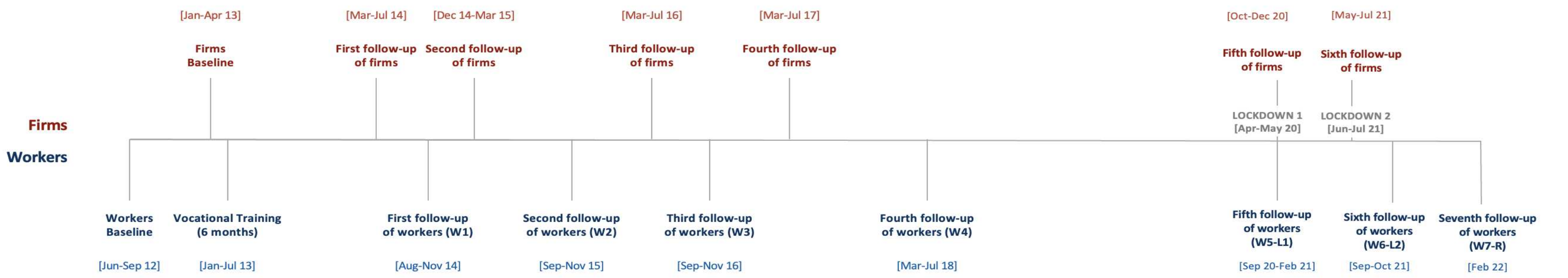
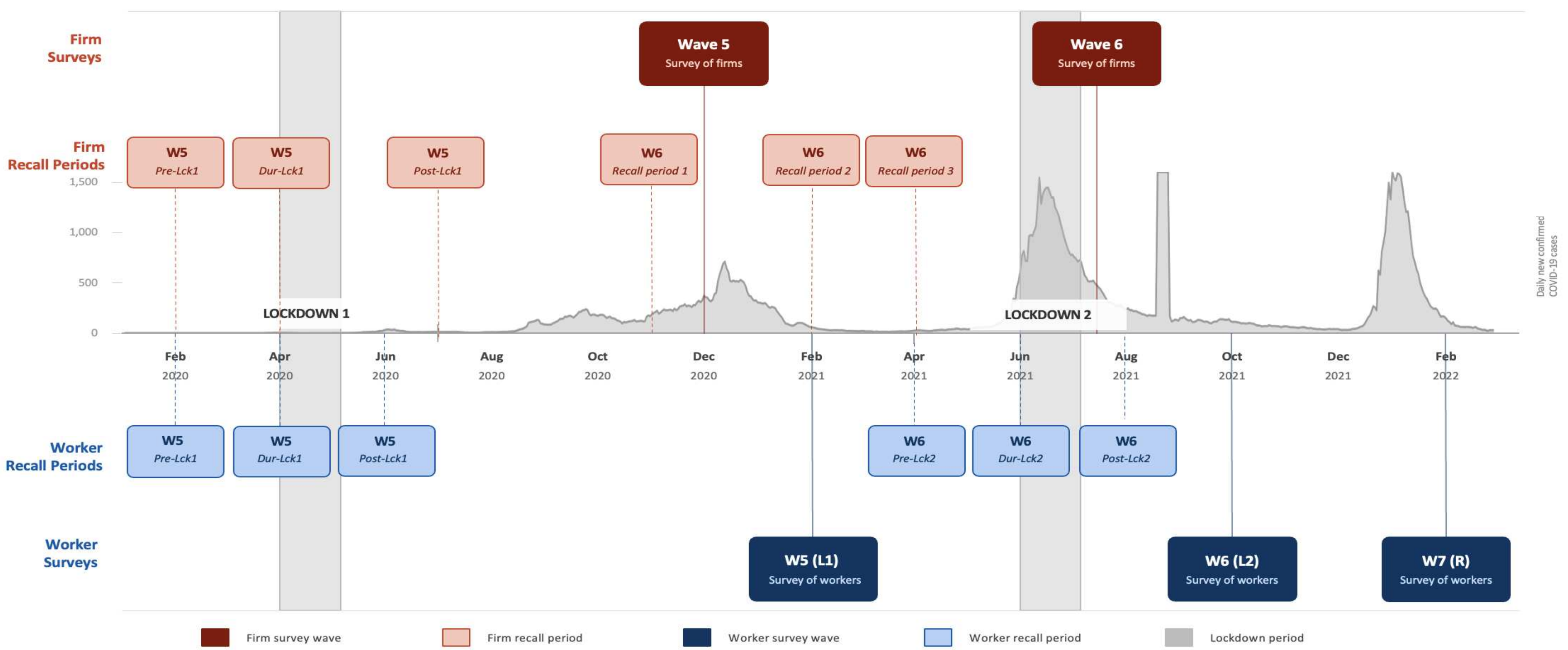
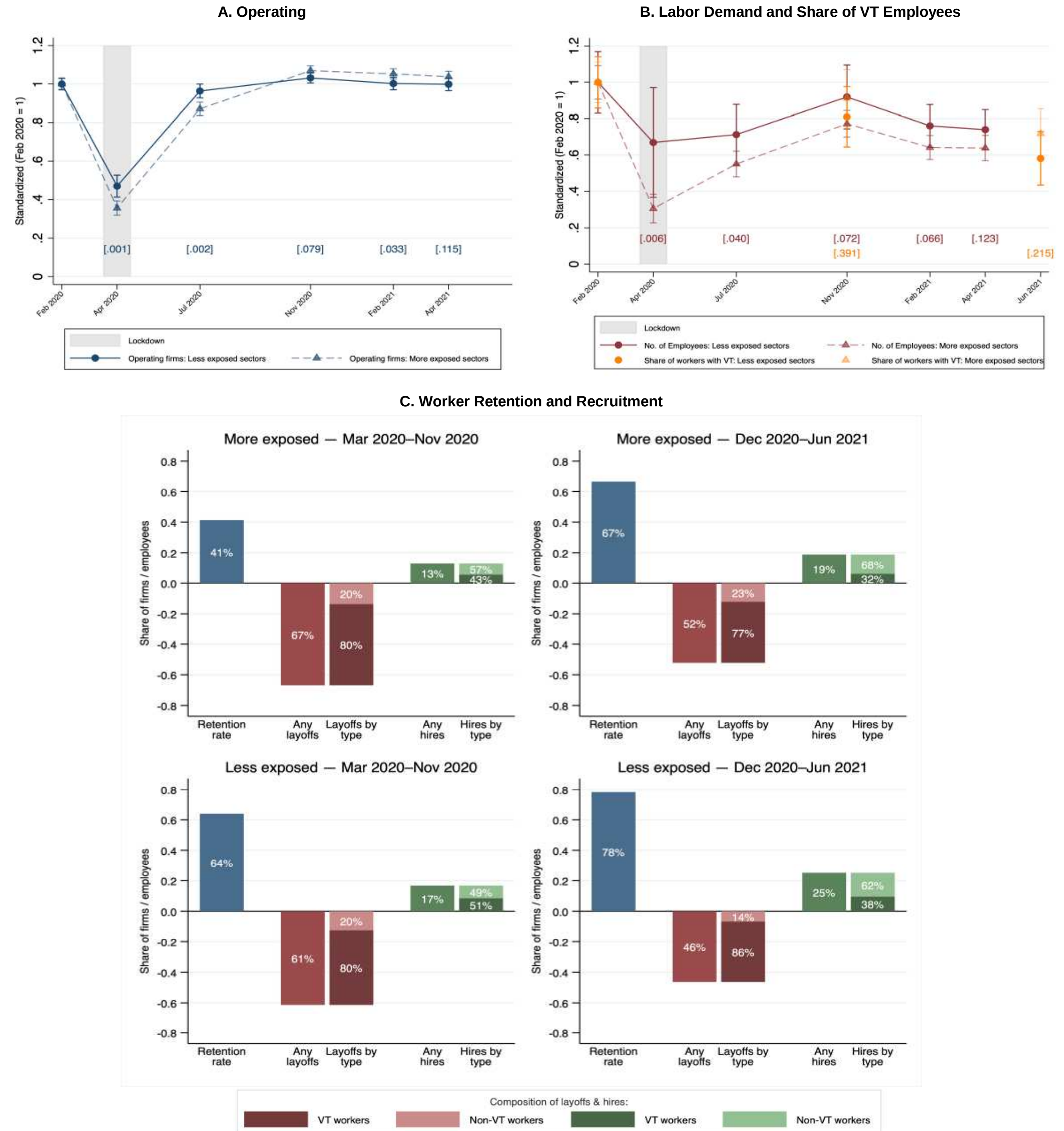


Figure A2-B: Surveys, Confirmed Covid-19 Cases and Lockdowns



Notes: Panel A shows the timeline of all firm and worker surveys conducted between 2012 and 2022. Panel B zooms in on the COVID-19 pandemic period. Survey waves included recall periods, where respondents were asked retrospectively about periods before, during, and after each lockdown, plotted against daily new confirmed COVID-19 cases in Uganda (source: Our World in Data, https://ourworldindata.org/covid-cases). The apparent spike in August 2021 reflects a one-day reporting backlog (19 August 2021); the series is capped at its 99th percentile for display. Regarding policy responses: Uganda confirmed its first COVID-19 case on 21 March 2020. Lockdown 1 (April–May 2020) involved closure of non-essential activities, a ban on public gatherings and transport, school closures, and a nationwide curfew. Restrictions were progressively lifted from May 2020, with non-essential businesses reopening in July 2020 and schools reopening in February 2021. Lockdown 2 (June–July 2021) was less stringent: schools and universities closed and transport between districts was banned, but most businesses and restaurants remained open. By early July 2021, transport was permitted across districts and activity largely resumed. Schools reopened and the curfew was lifted in January 2022.

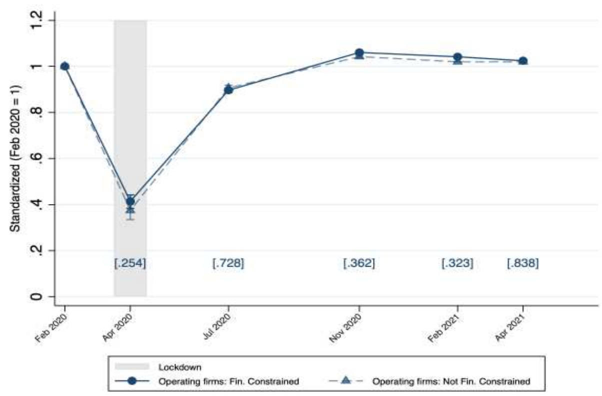
Figure A3: Firm Dynamics Over the Pandemic by Exposure to Face-to-Face Customer Interactions



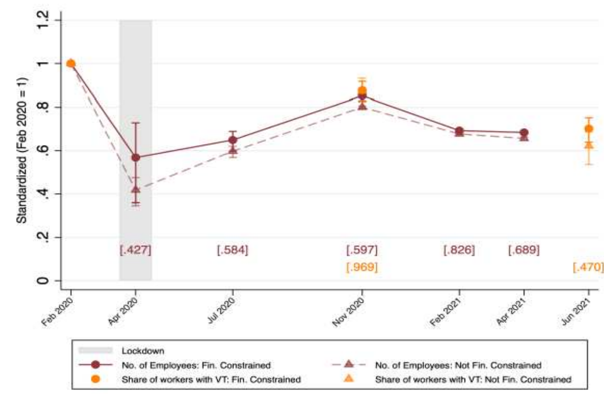
Notes: All data comes from the fifth and sixth rounds of firm-side surveys, fielded in October-December 2020 and May-July 2021, respectively. All panels split firms into two groups: those operating in sectors more exposed to lockdown restrictions due to more frequent customer interactions, and those in sectors less exposed. In Panels A and B, the solid lines correspond to firms in sectors with low frequency of customer interactions — plumbing, electricity, construction, and welding — which were therefore less exposed to lockdown restrictions. Dashed lines correspond to firms in sectors with high frequency of customer interactions — catering, tailoring, hairdressing, and motor-mechanics — which were therefore more exposed to lockdown restrictions. In Panels A and B, the gray shaded region corresponds to the first COVID-19 lockdown. All outcomes are normalized to 1 at their February 2020 levels; confidence intervals are similarly normalized by dividing each bound by its February 2020 value. Panel A reports the share of firms operating (blue). Panel B reports the average number of employees in operating firms (red), and the share of workers in operating firms with vocational training (orange). The proxied share of workers with vocational training is only available in February 2020, November 2020, and June 2021, given that the skill level of hires and separations was only asked in these time frames. Panel C reports firms' retention and recruitment behavior over two reference periods: March-November 2020 and December 2020-June 2021. For each period, we report the share of employees still employed at the firm, whether the firm laid off any workers, and whether it recruited any new workers. We also report the skill composition of layoffs and hires, conditional on the firm reporting any layoffs or recruitment in the relevant period. Layoff bars are shown as negative to reflect employment losses. Labels inside split bars show the share of workers with VT and without VT conditional on the firm having any layoffs or hires. Both panels proxy vocational training status. The proxy is computed using the workforce composition at Wave 4 (2017), updated with the hires and separations reported during the pandemic. In the fifth survey wave, owners reported the skill level of most workers laid off between March–November 2020 and the skill level of the most recent new worker hired. In the sixth survey wave, owners reported the skill level of most workers laid off and most workers hired between December 2020–June 2021. For layoffs, skill categories were: (i) substantial experience in the firm, (ii) experience in the sector, (iii) experience in another sector, (iv) no experience but vocational training, and (v) unskilled. For hires, skill categories were: (i) experience in the sector, (ii) experience in another sector, (iii) no experience but vocational training, and (iv) unskilled. We code VT = 1 for layoffs if most laid-off workers had either substantial experience in the firm or no experience but vocational training. We code VT = 1 for hires if the most recent hire (Wave 5) or most hires (Wave 6) had either experience in the sector or no experience but vocational training.

Figure A4: Firm Dynamics Over the Pandemic by Financial Constraints

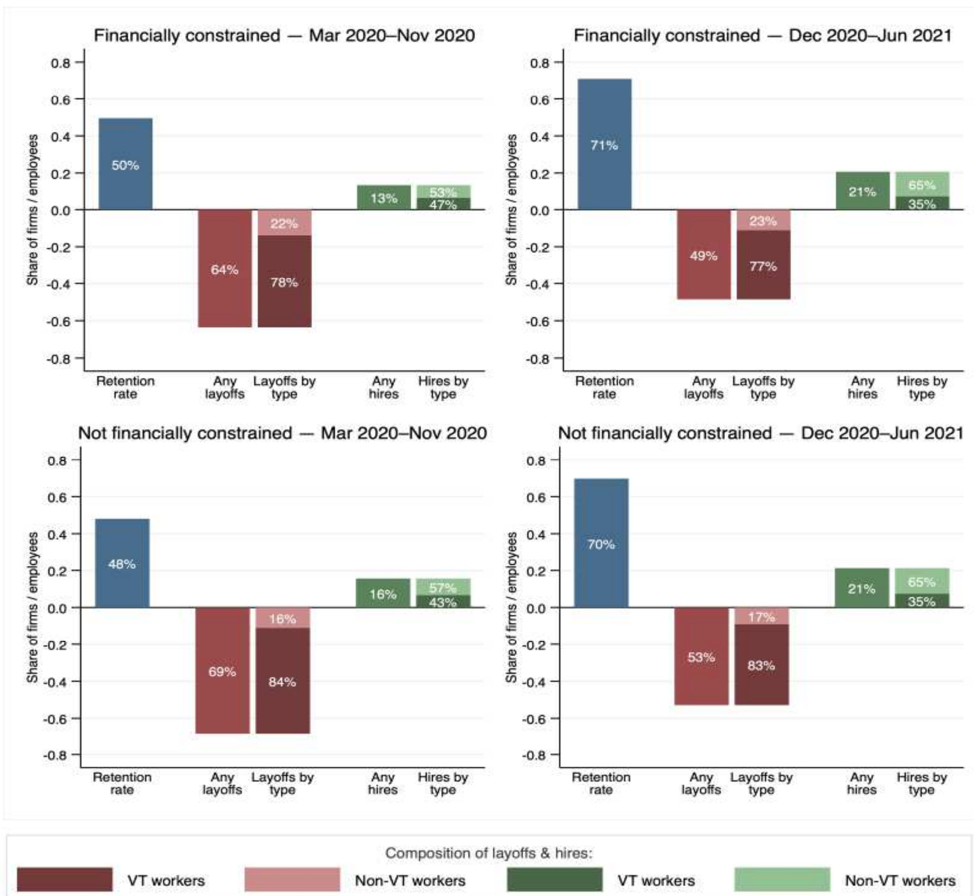
A. Operating



B. Labor Demand and Share of VT Employees



C. Worker Retention and Recruitment



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 panels split firms by whether they were financially constrained pre-pandemic. A firm is classified as financially constrained if, in Wave 4 (2017), the owner reported that they would borrow if all of the following borrowing conditions improved: a simpler loan application, lower collateral requirements, no fear of asset seizure, and a lower interest rate. 64% of firms were classified as financially constrained in 2017. In Panels A and B, the gray shaded region corresponds to the first COVID-19 lockdown. All outcomes are normalized to 1 at their February 2020 levels; confidence intervals are similarly normalized by dividing each bound by its February 2020 value. Panel A reports the share of firms operating (blue). Panel B reports the average number of employees in operating firms (red), and the share of workers in operating firms with vocational training (orange). The proxied share of workers with vocational training is only available in February 2020, November 2020, and June 2021, given that the skill level of hires and separations was only asked in these time frames. Panel C reports firms' retention and recruitment behavior over two reference periods: March-November 2020 and December 2020-June 2021. For each period, we report the share of employees still employed at the firm, whether the firm laid off any workers, and whether it recruited any new workers. We also report the skill composition of layoffs and hires, conditional on the firm reporting any layoffs or recruitment in the relevant period. Layoff bars are shown as negative to reflect employment losses. Labels inside split bars show the share of workers with VT and without VT conditional on the firm having any layoffs or hires. Both panels proxy vocational training status. The proxy is computed using the workforce composition at Wave 4 (2017), updated with the hires and separations reported during the pandemic. In the fifth survey wave, owners reported the skill level of most workers laid off between March–November 2020 and the skill level of the most recent new worker hired. In the sixth survey wave, owners reported the skill level of most workers laid off and most workers hired between December 2020–June 2021. For layoffs, skill categories were: (i) substantial experience in the firm, (ii) experience in the sector, (iii) experience in another sector, (iv) no experience but vocational training, and (v) unskilled. For hires, skill categories were: (i) experience in the sector, (ii) experience in another sector, (iii) no experience but vocational training, and (iv) unskilled. We code VT = 1 for layoffs if most laid-off workers had either substantial experience in the firm or no experience but vocational training. We code VT = 1 for hires if the most recent hire (Wave 5) or most hires (Wave 6) had either experience in the sector or no experience but vocational training.

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

VT COMPLIERS

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

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

CONTROLS

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

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

Study Sectors

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

Other Sectors

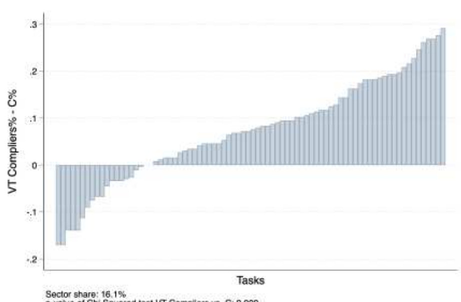
BOD	BODA BODA / TAXI DRIVER
RET	RETAIL SHOP WORKER
FAC	FACTORY WORK
STR	STREET FOOD MAKING AND VENDING
EDU	EDUCATION / TEACHER
MAN	OTHER MANUFACTURING
OFF	OFFICE WORK
OWN	OWNER OF RETAIL SHOP
OTH	OTHER
OTS	OTHER SERVICES
CLE	CLEANER / HOUSEKEEPER

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

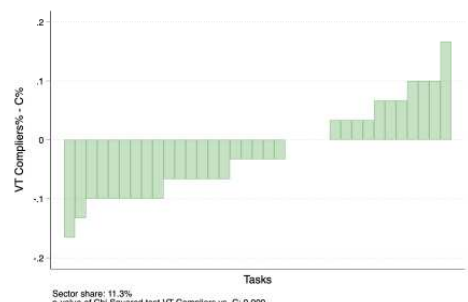
Figure A6: Tasks Performed by Vocationally Trained and Control Workers

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

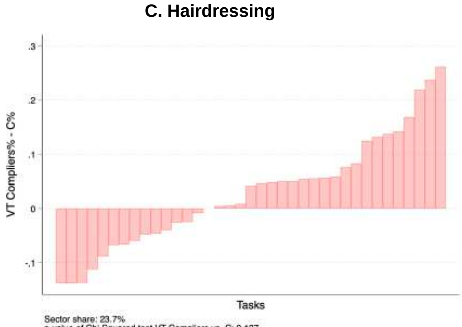
A. Motor Mechanics



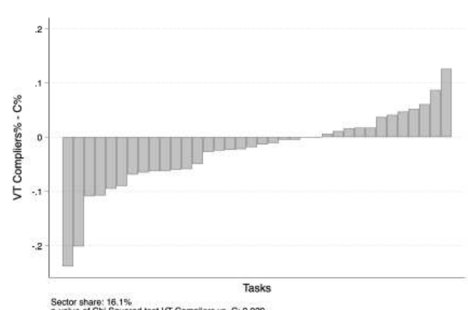
B. Electrical Wiring



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.