Title
Essays in Development and Labor Economics

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Publication Date
2023

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by

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A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Agricultural and Resource Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Jeremy Magruder, Chair
Professor Edward Miguel
Professor Elisabeth Sadoulet

Spring 2023

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Abstract

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The dissertation examines barriers to youth employment and to female labor market participation as well as factors that perpetuate occupational gender segregation in urban labor markets in low-income settings. Chapter 1 presents the results of a randomized mentorship program, "Meet Your Future," that improved participants' labor market outcomes by providing information about entry-level jobs and labor market dynamics. To study whether personalized mentorship by experienced workers improve young job seekers' labor market trajectories, I designed and randomized "Meet Your Future", a mentorship program which assisted a subset of 1,112 vocational students during their school-to- work transitions in urban Uganda, where youth unemployment is high. The program improved participants' labor market outcomes. Relative to the control, mentored students were 27% more likely to work three months after graduation; after one year, they earned 18% more. Call transcripts from mentorship sessions and survey data reveal that mentorship primarily improved outcomes through information about entry level jobs and labor market dynamics, and not through job referrals, information about specific vacancies, or through building search capital. Consistent with this finding, mentored students revise downward their overly optimistic beliefs about starting wages and revise upward beliefs about the returns to experience. As a result, they lower their reservation wages and turn down fewer job offers. The results emphasizes the role of distorted beliefs among job seekers in prolonging youth unemployment and proposes a cost effective and scalable policy with an estimated internal rate of return of 300%. In the second chapter of this dissertation, I move to investigate whether hiring processes themselves can disadvantage women and consequently explain part of the gender wage gap and the occupational segregation documented in many labor markets across the world. Specifically, I look at referrals, a significant factor in hiring decisions and one of the primary ways to land a job. I designed and conducted a correspondence experiment to examine how referrals by firm employees may perpetuate occupational gender segregation among Uganda's skilled workers. We start by presenting pairs of gender-differing profiles of potential candidates to workers in a wide range of industries, and ask who they would refer to their firm for an internship we

subsidize. We randomize the gender of the high-experience profile to elicit discriminatory preferences while mitigating endogeneity in network formation. To validate the findings in this anonymous setting we subsequently offer participants the possibility to use their networks as referral choice sets, the lifelike setting. We further randomize the disclosure of the referral source's name to the employer. We document three facts. First, discrimination in referrals exists against both genders and is correlated with subjects' gender and the gender dominance of their sector; however, discrimination against the non-stereotypical gender is more prevalent in male-dominated sectors. Second, the intrinsic preferences of employees are a significant driver of their discrimination in referrals, which, in general, do not simply reflect passthrough from employers' preferences. Thirdly, when the referral is private, subjects in male dominated sectors are more likely to refer women, indicating that beliefs regarding employer preferences are a significant driver of pro-male bias in these sectors. In the last chapter I investigate gender disparities in the effect of COVID-19 on the labor market outcomes of skilled Ugandan workers. Leveraging a high-frequency panel dataset, my coauthors and I find that the lockdowns imposed in Uganda reduced employment by 69% for women and by 45% for men, generating a previously nonexistent gender gap of 20 p.p. Eighteen months after the onset of the pandemic, the gap persisted: while men quickly recovered their pre-pandemic career trajectories, 10% of the previously employed women remained jobless and another 35% remained occasionally employed. Additionally, the lockdowns shifted female workers from wage-employment to self-employment, relocated them into agriculture and other unskilled sectors misaligned with their skill sets, and widened the gender pay gap. Pre-pandemic sorting of women into economic sectors subject to the strongest restrictions and childcare responsibilities induced by schools' prolonged closure only explain up to 65% of the employment gap.

To my grandfather, who taught me the value of hard work and kindness.

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Acknowledgments

I owe a tremendous debt of gratitude to my advisor, Jeremy Magruder. Jeremy, your support throughout this journey has exceeded all expectations. You have encouraged me to pursue my own research ideas and supported me at every step of the way. Your willingness to listen, even when my ideas were partially formed (basically all the time), has been invaluable. You helped me transform them into sensible research questions and agendas, and it is mostly thanks to you I have a dissertation I feel proud of.

I am also indebted to my committee members, Ted Miguel and Elisabeth Sadoulet. Ted, I consider myself incredibly lucky to have had you as an advisor, professor, and co-author. You have been a constant source of inspiration and motivation in all these roles. Your enthusiasm for policy-relevant and academically-oriented research has been infectious. Whenever I found myself questioning why we do what we do, I would think about the impact your work has had, and my deepest uncertainties would dissipate.

Betty, I am filled with gratitude for all you have done for me over the years. In particular, when I transitioned from the classes to the unstructured years of research, your advice on how to avoid getting lost was crucial to stay on track. Your voice would echo in my mind when I was about to procrastinate, or when I had to convince myself that establishing a routine and continuing to push was necessary. Good research, as you taught me, requires time.

Next, I would like to thank Imran Rasul and Tommaso Monacelli, my mentors since the very beginning. Imran, when I started working with you nine years ago, during what would today be called a pre-doc, I knew little about how that experience would shape my life, or how our collaboration would grow into work that spanned multiple years and multiple projects. I am grateful for the guidance you have provided throughout the years, proud of the accomplishments we have achieved together, and excited for what comes next.

Tommaso, you have been a guiding light since my first year of undergrad, and now a dear friend. At the outset of my undergraduate studies, I was drawn to the arts and humanities, and my academic pursuits reflected this. However, after attending several macroeconomics classes, I began to question whether my chosen field was the right path for me. I came to you for advice, and our conversation changed everything. You illuminated the far-reaching potential of the discipline. and the ways in which its insights could be applied to address global challenges. Your passion for the subject was contagious. I was immediately drawn to its rigor and intellectual depth. From that moment on, I knew I wanted to pursue a career in economics.

I am also thankful for the many Berkeley faculty and staff who have provided invaluable feedback at various moments during graduate school. I owe a special thanks to David Card for pushing me outside of my comfort zone and encouraging me to engage with complex, thought-provoking questions. I am also deeply grateful to Chris Walters for his patience in rephrasing complex concepts multiple times as I stared at him and repeated, ”I don’t understand what you just said,” over and over again. I would also like to express my gratitude to Carmen Karahalios and Diana Lazo, who have been a constant source of help and kindness when navigating Berkeley's administrative systems and requirements.

I am also thankful for the wonderful friends and colleagues I have met along the way. Mary Namubiru has been a wonderful co-author and an inspiring and friend of nine years. Ottavia Veroux and Pedro De Souza helped me carrying out my research in essential ways. I also cannot imagine what graduate school would have been like without my brilliant friend Gaia, my cheerful friend Maya, my passionate friend Hadar, my kind friend Jenöya, and my sweet friend Tati. There are also, of course, the Italians, the Chileans, and the Dev Bros, who added inspiration and fun to the journey.

Beyond the research world, I am grateful for the support of my lifelong friends. Bianca, Dani, Fra Bianchi, Lara, The Rachel, Pinchets, Terenzi, and Zee: thank you for being there to celebrate my successes, but also for reminding me to enjoy life outside of research and to not take myself too seriously. And Lodo: you wholeheartedly believed in me without any reservations, for which I will always be grateful. Last but not least, I am thankful for my family. Your unconditional love is the driving force behind everything I do.

Chapter 1

Meet Your Future: Experimental Evidence on the Labor Market Effects of Mentors

This chapter is coauthored with Mary Namubiru and Sara Spaziani.

1.1 Introduction

Globally, youth unemployment is a major policy concern. Nowhere is this challenge more pronounced than in Africa. The continent, home to one-fifth of the world's first-time job seekers, suffers from youth unemployment and underemployment rates as high as 60% (UN World Population Prospects, 2019; AfDB, 2018). The current trends in fertility are an aggravating factor: by 2050, one-third of the world's new labor market entrants will be seeking employment in Africa (Bandiera et al., 2022b). Because of its implications on individual well-being and country-wide economic growth, getting young Africans into work is a top priority for every government on the continent.

The most common policy response to the youth unemployment challenge has been to invest in skills training programs to boost the employability of workers and resolve skills gaps (McKenzie, 2017). While these programs have proven effective for promoting employment in a few contexts (Alfonsi et al., 2020; Maitra and Mani, 2017), their job placement rates are often low, resulting in a mass of untapped talent (Bandiera et al., 2022a). One explanation for low placement rates is that supply-side information frictions may be a particularly significant barrier to entry for youth in low-income settings (Abebe et al., 2021b; Donovan et al., 2022; Banerjee and Sequeira, 2022). Young job seekers often lack knowledge on many aspects of the job search process, such as how to identify job openings, how to apply for jobs, and how to prepare for interviews (Jensen, 2012; Beam, 2016; Groh et al., 2016; Abel et al., 2019; Abebe et al., 2021a; Carranza et al., 2022; Bassi and Nansamba, 2022). This limited information is commonly accompanied by unduly optimistic expectations. Young job seekers frequently hold overly optimistic views of their work prospects, turn away accessible jobs in favor of greater opportunities that frequently do not materialize, and end up in voluntary unemployment (Groh et al., 2016; Abebe et al., 2021b; Banerjee and Chiplunkar, 2022; Bandiera et al., 2022a).

In an attempt to correct the overly optimistic beliefs of job seekers, Jones and Santos (2022) and Chakravorty et al. (2021) rolled out targeted information interventions to university graduates in Mozambique and vocational students in India. In the first study, job seekers did not correct their beliefs. Conversely, in the second study job seekers did change beliefs, but this resulted in an increase in program dropouts. In addition, four ongoing studies indirectly facilitate job seekers' learning about the job market. With the exception of Abebe et al. (2021b), who find that a job fair is beneficial to low-educated workers exactly by facilitating the adjustment of erroneous beliefs, in the other studies, treated job seekers do not attain greater employment rates or wages. Instead, they report a decline in employment, a decline in job quality, and an overall sense of despondency (Kelley et al., 2021; Banerjee and Sequeira, 2022; Bandiera et al., 2022a).

In this paper, we propose a low-cost and scalable way of providing tailored and credible information to young job seekers in low-income settings, capable of rectifying their overly optimistic beliefs without leading to discouragement. We design and administer a mentorship program, which we call Meet Your Future (MYF), that connects soon-to-be graduates of vocational training institutes (VTIs) with successful young workers for personalized mentorship sessions.

The program draws on the interdisciplinary literature on messenger effects, which shows that people are more likely to act on information delivered by a messenger with similar characteristics to themselves (Durantini et al., 2006; Dolan et al., 2012) and the literature on job referrals, which emphasizes the importance of connections in informal labor markets. We sought to apply these insights into the program's design, by pairing students with professionals with whom they would likely identify and feel comfortable seeking guidance. We selected our mentors from recent graduates of the same VTIs and vocations, a population that is both relatable for the students and holds pertinent information on local labor market conditions.

Our goal was to assist young job seekers in forming realistic expectations of jobs available in the labor market, enhance their grasp of the search process, and improve their initial match quality and, by extension, their career trajectory. Students consistently displayed a high level of engagement in response to this approach, demonstrating its high potential as a solution to the information gaps that lead to unemployment among these newly trained job seekers.

We evaluate the impacts of MYF using a randomized control trial. Specifically, we conduct an experiment with 1,112 vocational students poised to make the school-to-work transition in three urban labor markets in Central and Eastern Uganda. Our primary method of data collection consists of deploying innovative questionnaires directed at both students and mentors. Specifically, we build a three-year panel of students consisting of six rounds of data collection beginning two years prior and following one year after the students' graduation. We also build a two-year panel of mentors consisting of four rounds of data collection, three prior to the MYF roll-out and one after. Additionally, we collect a post-intervention survey from students and mentors to measure immediate reactions. High-frequency data collection around the time of the intervention allows us to evaluate the nature of each student-mentor engagement and the lessons learned by all parties. In a novel dimensional measurement, we capture voice recordings of the first interaction between students and mentors, allowing us to assess not only the content of these engagements but also attributes that are often difficult to codify or are subject to measurement error, such as enthusiasm and curiosity. We collect this wealth of information to examine the inner workings of such mentorship links.

Similar to recent literature, our paper replicates the finding of striking overoptimism regarding entry level pay in our setting. 94% of the students overestimate their first-job earnings.[1] On average, first-job realized earnings were just 14% of students' prior expectations. When their expectations are compared to their realized earnings one year later, the proportion rises to ∼65%, indicating that optimism about wages is prevalent, but especially pertinent to their first job, as students fail to account for the reality that many will be unpaid or low-paid. Likewise, only 21% of students claim they would accept an unpaid job as their first job, while the realized share of unpaid first jobs in the cohort is 52%. Relatedly, we highlight a novel fact: not only are new entrants overly optimistic about their starting salaries, but they also have a limited grasp of job-to-job transition probabilities, returns to experience, and salary growth potential. Most crucially, students undervalue initial unpaid employment spells, failing to see that they are frequently stepping stones to securing better employment and earnings down the line.

In this context, our study stands out from the existing literature in several ways. First, our program proved particularly successful in boosting employment outcomes. Access to mentors mitigates information frictions and improves labor market outcomes. We identify large positive impacts on employment three months after the school-to-work transition. Labor market participation is 27% higher for treated students; these students obtain their first jobs faster and are 33% more likely to use and advance their gained skills through vocational education. In addition, these accelerated first employment spells allow students to climb the career ladder more rapidly. One year after the intervention, the earnings of treated students are 18% higher than those of control students. We estimate the IRR of this intervention to be in the order of 300%.

Second, we leverage our data on conversation topics to explore why our program was successful. Based on the literature on supply-side frictions and the content of the audio recordings, we suggest four plausible mechanisms driving the effects of the intervention on labor market outcomes: job referrals, search tips, information about entry level conditions, and encouragement. Through the lens of an expanded McCall 1970 search model that accommodates subjective beliefs, we derive testable predictions for each of the four mechanisms underlying the effectiveness of mentors. To map the conversational material to our four mechanisms, we evaluate transcripts of the coaching sessions as well as supplementary

[1] The panel structure of our data allows us to compare each student's expected earnings with their realized earnings.

data characterizing the students' key takeaways. We find that mentorship acted as a particularly salient information treatment: students revised downward their unduly optimistic assumptions about their first job and improved their understanding of early employment's significance in determining career prospects. In response, they reduce their reservation wages and decline fewer job offers. Contrary to earlier empirical and theoretical studies, we do not identify direct job referrals or stronger search abilities as viable routes for the observed treatment effects.

To confirm that the two primary mechanisms via which MYF impacts job search behavior and labor market outcomes are learning about the entry level market conditions and learning that conditions do get better with time, we leverage a second randomization built into the research design, namely that of students to mentors. We accomplish this by analyzing the effect of each topic of conversation on labor market outcomes. At first, we use Empirical Bayes tools to estimate the mentor-level heterogeneity. The large estimates of bias-corrected variance indicate that some mentors are more effective than others. To understand the determinants of this heterogeneity and to confirm our previous result, we employ an Instrumental Variables approach, capitalizing on the random assignment of students to mentors: the most effective mentors are those providing mentees with information about entry level conditions and encouragement. We also use our research design to estimate the degree of heterogeneity among mentors and predict their value added using their demographic characteristics as well as policy-relevant program characteristics, such as the number of mentees each mentor is assigned to.

Last, to determine whether simultaneously relaxing liquidity constraints would amplify the effects of mentorship, we unconditionally provided the sum of 40,000 UGX ($\sim$ \$12) to a random subset of MYF Program participants, with the recommendation that they use the money to finance their job search or engage with their mentors. Contrary to our expectations, the cash transfer had no differential impact on short run labor market outcomes but attenuated the effects of the MYF program on labor market outcomes after one year. While the additional cash had no effect on the frequency and level of engagement of the student-mentor conversations, it prompted the mentors to provide more actionable search tips, which crowded out information about wage-growth potential and encouragement. Students assigned to the MYF+Cash treatment were consistently more likely to discuss actionable search tips with their mentors and to report search tips as their main takeaway. Once again, this finding confirms that students who learned about entry level market conditions, market dynamics, and wage-growth opportunities benefited the most from the program.

Taken together, our results demonstrate that access to mentors improves labor market outcomes: facilitating interactions that rectify young job seekers' overly optimistic beliefs while credibly preventing discouragement can spur career development. Furthermore, the study's results highlight the role of unwarranted beliefs, in reducing earnings and career progression.

This paper contributes to four strands of literature. First, the extensive literature on the effects of active labor market strategies as a means to decrease youth unemployment in low-income areas. Two sub-strands of this literature are closely related to our work:

(i) a series of studies investigating ways of reducing information and search frictions to which we contribute by proposing a low-cost and scalable method of delivering trustworthy and individualized information to job seekers preparing to move from school to work;[2] (ii) a series of studies evaluating the effectiveness of vocational education. Across low- and middle-income countries, subsidies for vocational education are one of the leading policy responses to promote upskilling and employability and reduce youth unemployment. These programs have proven effective for generating productive human capital and promoting employment in some contexts (Alfonsi et al., 2020; Maitra and Mani, 2017) but not everywhere.[3] Moreover, even when (certified) skills raise the likelihood of regular employment, overall job placement rates are low, resulting in underutilized talent (Bandiera et al., 2022a). We examine the student population transitioning from the vocational education system to the labor market. This is a crucial transition with long-lasting effects on the future career paths of the students. By analyzing the content of the conversations between students and their mentors, we identify the labor market frictions that prevail among young and skilled job seekers in urban labor markets in Uganda. In addition, we provide an effective and scalable policy solution, capable of generating tailored support at a low cost, thereby enhancing the efficacy of vocational training programs.

Secondly, the paper contributes to the literature on mentorship programs. Over the past decade, these programs have become increasingly widespread. They are often institutionalized by schools and universities in high-income settings to improve the academic achievements of at-risk adolescents. As a result, the mentorship literature focuses on programs that typically involve adolescents and attempt to improve high school graduation, college enrollment, and minimize risky behaviors (Rodríguez-Planas, 2012; Falk et al., 2020). Instead, this body of literature seldom focuses on job seekers or workers, and, to our knowledge, never in low-income countries. Such studies demonstrate that mentorship has a moderately beneficial impact overall. However, due to the cross-sectional, non-experimental nature common to most of these papers, it is unknown whether significant correlations between mentorship and outcomes demonstrate a causal effect. In addition, remarkably little is known about how exactly a mentor operates and what aspects of a mentor are beneficial in terms of labor market outcomes. Our contribution to the mentorship literature is twofold. First, we rigorously evaluate the effectiveness of such a program in a high-stake setting in a low-income country, thereby filling a gap in the existing evidence. We show that these programs have great potential in contexts characterized by a high degree of labor market informality and a high reliance on connections to navigate the labor market. Second, through close observation of the mentor-mentee interactions, intensive data collection effort and the random assignment to mentors, we develop a framework to analyze and test what is useful, making ours one of the first studies to "open the black box" of the underlying mechanisms of such mentorship

[2] Abel et al. (2019); Altmann et al. (2018); Banerjee and Sequeira (2021); Beam (2016); Beam et al. (2016); Behaghel et al. (2014); Belot et al. (2019); Bruhn et al. (2018); Carranza et al. (2022); Cottier et al. (2018); Dammert et al. (2015); Jensen (2012).

[3] See the meta-analyses of Blattman and Annan (2016); McKenzie (2017) and Card et al. (2018) for studies on impacts of training programs in low-income settings).

relationships.

Thirdly, this paper contributes to the literature on behavioral job search; this is a nascent and fast-growing literature that studies how job seekers' misperceptions about their own prospects delay their exit from unemployment and career progression. Recent survey data from high-income countries reveals considerable overconfidence among job seekers regarding their labor market prospects (Spinnewijn, 2015; Mueller et al., 2021; Potter et al., 2017). Ongoing research in low-income settings documents similar findings and warns that distorted beliefs can dampen the effectiveness of active labor market policies (Abebe et al., 2021b; Kelley et al., 2021; Chakravorty et al., 2021; Bandiera et al., 2022a; Banerjee and Sequeira, 2022; Jones and Santos, 2022). Two previous attempts at correcting job seekers' overly optimistic beliefs are Jones and Santos (2022) and Chakravorty et al. (2021), who rolled out targeted information interventions to university graduates in Mozambique and vocational students in India. The first study finds that public information provision as shared via SMS has no impact on employment outcomes, as optimistic expectations are barely affected. In the second study, information sharing that leads to a correction in beliefs also reduces the accumulation of human capital after overly optimistic students leave the program. Additionally, four ongoing studies indirectly pushed natural learning to occur faster than it normally would. With the exception of Abebe et al. (2021b) treated job seekers do not achieve higher employment rates or wages in either of these studies. In Kelley et al. (2021) job seekers have high expectations when they join a job portal. Because the job offers are subpar, we observe voluntary unemployment as job seekers hold out for better opportunities. In Banerjee and Sequeira (2022) the intervention, a job search subsidy, reduces search expenses, pushing job searchers to search more intensively. When jobs fail to materialize immediately, they become increasingly impatient and redirect their search towards low paying jobs closer to home. Similarly, in Bandiera et al. (2022a) workers assigned a match offer respond to a lower-than-expected callback rate by revising down their beliefs over their own job prospects, directing their search to lower quality jobs, searching less, and becoming discouraged. We present, to the best of our knowledge, the first successful debiasing method that does not lead to discouragement.

Last, the paper contributes to the literature on social networks and labor markets by providing experimental evidence on one of the mechanisms by which networks may produce surplus: belief correction. The role of social networks as a determinant of labor market outcomes has a long history in economics, beginning with Granovetter (1973)'s demonstration of the significance of social ties, particularly weak ties, in finding a job. From the job seekers' perspective, the traditional theory of networks posits that they utilize networks to reduce search costs by relying on their ties to get connected to employment possibilities: a network connection is therefore a link facilitator that connects you to a firm, a person, or a vacancy (Calvó-Armengol and Jackson, 2004; Mortensen and Vishwanath, 1994; Ioannides and Loury, 2004; Topa, 2001). Empirically, a vast literature has established that networks affect labor market outcomes (Bayer et al., 2008; Beaman, 2012; Magruder, 2010; Munshi, 2003). However, endogenous group membership and limited data availability make it often difficult to understand the channels via which networks operate and what about a network member is useful. With Meet Your Future, we exogenously generate weak ties between young job seekers entering the labor market and successful workers in their sector of training. Under these lenses, we demonstrate that weak ties are beneficial for employment, but contrary to what classic network theory would anticipate, the primary mechanism by which they exert their influence is neither job referral nor link-to-job formation. Rather, it is the combination of encouragement and knowledge about entry level labor market conditions that influences job seekers' perceptions and search behavior, eventually placing them on steeper job ladders.

The rest of the paper is organized as follows: Section 2 provides context for the labor market under study. Section 3 describes the randomized controlled trial and the Meet Your Future program in detail. Section 4 describes the MYF program's impacts on labor market outcomes and dynamics. Section 5 proposes a model of job search with subjective beliefs, produces testable predictions regarding the mechanisms underlying mentors' effectiveness, and tests them. Section 6 presents IRR estimates. Section 7 concludes.

1.2 Context

1.2.1 The Ugandan Labor Market

We study three urban labor markets in Central and Eastern Uganda. Like many others across Sub-Saharan Africa, they are characterized by high rates of youth underemployment, job turnover, and job separation (Donovan et al., 2022). Most youths fail to climb the job ladder—their employment is characterized by transience and informality. The relative magnitudes of the supply and demand-side imbalances are unclear. Firms may be unable to recruit workers who satisfy their needs. Simultaneously, workers may be overly optimistic about their job prospects; the frequency of their failures to obtain their ideal employment may lead them to indefinite withdrawal from the job market (Bandiera et al., 2022a). These labor market characteristics hold even for relatively skilled job seekers; although training and credentials raise the propensity for stable employment, the market still does not clear for such individuals (UNHS, 2018).

Given the structure of the Ugandan population pyramid and continued challenges to growth, it is of fundamental importance that matching frictions do not inhibit the efficient allocation of skilled workers across the few available good jobs. These frictions create casual occupation traps and permanent labor market detachment. Their unfortunate consequence is human capital wastage.

1.2.2 Study Population

Vocational Training Institutes: To strengthen the country's productivity, the Ugandan government implemented a decennial strategic plan in the early 2000s aimed at its vocational education sector. Today, as in many other East African economies, the vocational sector is well established in the country; vocational training is a common route through which workers acquire skills and firm owners are familiar with recruiting VTI graduates. Numerous NGOs working in Uganda support or run VTIs to promote the transition of secondary scholars from disadvantaged backgrounds into practical tertiary training. While effective at generating productive human capital (Alfonsi et al., 2020), most VTIs do not provide career services upon and following graduation.

Our sample comprises vocational students about to enter the labor market. Specifically, we surveyed the 2019 cohort of students enrolled in the National Certificate Program at five VTIs across Eastern and Central Uganda.[4] The National Certificate is a two-year-long program aimed at instructing students in a specific occupation. The certificate includes theoretical and practical classes. It provides a certification of skill fluency with national validity. The 1,112 students in our sample are trained in 13 specific skills: motor-mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical engineering, carpentry, machining and fitting, teaching/early childhood development, agriculture, accounting and secretarial studies. As shown in Table A.3.5, these sectors constitute a source of stable employment for young workers in Uganda: they collectively employ about 16% of workers aged 20–30, a percentage that more than doubles if we exclude young Ugandans involved exclusively in agriculture. We present students' distribution across fields of study and treatment arms in Table A.3.5. Our sample is representative of the population of Ugandan youth enrolled in practical tertiary training.[5] It arguably represents a labor market segment with the potential to become among the most productive workers in the country.

Students/Job seekers Table 1.1 reports students' baseline characteristics: they are on average 20 years old, 40% are female, the majority are single and they are largely of Christian faith. The sample is relatively heterogeneous in terms of socioeconomic background—the distribution of households' assets and urbanity is wide. Their household of origin's main source of income is divided between subsistence agriculture (32%), commercial agriculture (15%), wage employment (33%), and a family business (19%). About 50% of the students worked before the treatment roll-out almost exclusively in casual occupations.

Mentors These are 158 individuals who we identified as being "successful," by which we mean that they held stable employment with an average tenure of 3 years. We connected these workers to randomly selected students during their labor market transition. We assigned each mentor to between one and five treated students randomly by strata, where the strata are VTI of attendance and occupation. Table A.3.6 reports mentors' demographics and job history. They are 25 years old on average, and 41% are female. One of our goals when designing the MYF program was to generate "realistic" connections. For this reason, we decided to match on VTI–course of study duals. We also restricted our sample of mentors to recent graduates. We wanted to connect students with successful workers to whom they could relate and feel comfortable enough to reach out to for help or advice. The aim was to

[4]We selected VTIs with a long-standing history of collaboration with BRAC Uganda, our implementing partner. BRAC pre-selected VTIs based on their reputation, infrastructure, equipment, teachers' educational attainment, and teacher-to-student ratio.

[5]There is no shortage of VTIs in Uganda; as in other low-income contexts, there are concerns over a long left tail of low quality training providers existing in equilibrium.

allow the mentor-student conversations to flow naturally. We settled on mentors that graduated 2 to 5 years prior to the student's job market entry.[6] These individuals have substantive experience in the labor market without being too senior relative to current students. We also sought to minimize the probability of excessive recall bias. Another reason for the VTI match was to encourage a sense of community between partners to motivate both parties. The VTIs do not systematically track their graduates. They also do not keep organized and updated records of their contacts. To identify successful alumni, we collected and digitized hard copies of phone contacts.[7] Out of 1,368 previous students, we successfully contacted and surveyed 714.[8] After excluding the alumni that did not provide their availability to participate in the MYF program as well as those with no work experience in the occupation of training, we assigned a score to a set of relevant characteristics. We selected the alumni who scored the highest. The characteristics we considered were: (i) accessibility and phone ownership, (ii) labor market history, (iii) school performance, and (iv) soft skills.[9]

1.2.3 The School-to-Work Transition and Associated Frictions

To gather information about how the school-to-work transition generally evolves, we combined focus group discussions with over 200 participants. These participants were drawn from VTIs' managers, teachers, current students, and alumni.

In Uganda, the worker-firm matching process is largely informal: in the sample of skilled workers from which we drew our "future you" only 2% found their first job via a posted offer. Another 61% did so through friends or family; the rest found their first employment via walk-ins. No one registered at employment centers, indicating the absence of a robust system of public employment services in the country.[10] The high degree of labor market informality and the lack of digital platforms make information acquisition more costly. This has consequences for match quality. These features suggest that the creation of a connection to a successful worker is a promising intervention.

Similar to findings in other contexts, we document distorted beliefs among the entire cohort of students over their future labor market prospects. In Panel A of Figure 1.3 we document a striking optimism bias among job seekers with respect to entry level jobs and specifically with respect to the mean wage distribution of offers. This upward bias held throughout their entire VTI training: expected first-job salaries at baseline were much higher

[6]We avoided the cohort with one year of labor market experience as they overlapped with our student sample. In our sample, in only 3% of cases did the mentor and the student previously interact.

[7]One example of the digitized material is shown in Figure A.3.5.

[8]We attribute the attrition from the initial sample of 1,368 alumni contacts to the quality of the information which was collected by the VTIs at the time of each student's graduation. Due to the written nature and manual entry of the records, the digitization process was not only prone to error, but much of the data was not recent as telephone SIM cards were required to be registered in 2016. This prompted many Ugandans to change their phone numbers.

[9]For more details on the selection process, see Appendix A.3.4.

[10]Similar shares emerge if we look at the broader population of both skilled and unskilled job seekers (WBG, 2019), showing that network connections are crucial in multiple labor market segments.

than realized average salaries. On average, students realized earnings at first job were just 14% of their prior expectations. When compared to realized earnings after one year, the share raises to ∼65%, suggesting that optimism is pervasive and not only relevant to their first spell.[11] We track students' expectations over job offer arrival rates and the distribution of expected earnings. We did so at the start of their programs, a year into their program, and twice in their second year.[12] This finding contributes to the emerging evidence from other low-income settings (Banerjee and Sequeira, 2022; Bandiera et al., 2022a) as well as high-income ones (Spinnewijn, 2015; Mueller et al., 2021) that labor market entrants are too optimistic about their labor market prospects.

In addition, we document a new fact: new entrants are not only too optimistic about their starting wages. They also have a poor sense of labor market dynamics and wage-growth opportunities. Panel B of Figure 1.3 shows the expected and actual transition matrices of employment pathways from three months to one year after the school-to-work transition. In comparing the two, we learn that: (i) students undervalue unpaid (or negatively paid) initial job spells, which they consider as likely to lead to stable wage employment as an initial spell of unemployment; (ii) underestimate the risk related to being unemployed at three months after graduation; (iii) underestimate the overall unemployment prevalence at one year.

Taken together, we interpret this as evidence of overoptimism regarding entry level wages and a general lack of comprehension regarding the process of acquiring a stable wage position. These beliefs are consistent with a model of thin labor markets, in which young job seekers are primarily exposed to people with jobs, but less frequently to starting salaries. If students' beliefs lead them to target jobs that are beyond their reasonable reach, they may have reservation wages that are too high for prevailing labor market conditions. The same holds true if they underestimate the future value of a low paying first job. These "unicorns"—entry level positions that are well-paid and have ample opportunity for internal promotion—are simply not the median outcome for VTI graduates. Although 84% of students believe that their first position will be a permanent position, the vast majority of them find initial employment as apprentices or temporary workers. Learning by doing in the job market, particularly in a low-income and credit constrained context such as Uganda, contributes to human capital destruction.

With a similar population, Bandiera et al. (2022a) found that an initial bad signal through experimentally generated minimal callbacks contributed to excessive downward revisions in individual job market prospects. Job seekers who experienced such a negative shock

[11] Similar patterns occur if we compare students' expectations to mentors' realizations (Figure A.2.2), an exercise that helps rule out a Covid-19 specific effect.

[12] We elicited expected time to first employment and expected earnings at first employment. Their evolution is mapped at four points (5 for the treatment group): baseline, midline 1, midline 2, midline 3, and, for the treatment group, the Post Interaction Survey. We provided monetary incentives that rewarded prediction accuracy in two out of four of the pre-treatment elicitations. To elicit expected earnings, we followed Alfonsi et al. (2020). We asked individuals for their minimum and maximum expected earnings if offered a job in their sector of training right after graduation. We asked them the likelihood their earnings would lie above the midpoint of the two and fitted a triangular distribution to measure their expected earnings.

proceeded to search less intensively over lower quality firms with persistent negative effects on employment outcomes six years later. This is consistent with our finding that control students are more likely to become discouraged. But our treatment crucially differs from Bandiera et al. (2022a) —interaction with successful alumni ameliorates the discouraging effects of the information treatment and leads to greater labor market grit. Greater tenacity pays persistent dividends toward students' career trajectories.

1.3 The Experiment

To study the effect of mentorship on job seekers' performance, we designed Meet Your Future, a program in which graduates about to enter the labor market are matched to successful workers for one-on-one career mentorship sessions. The implementation capacity of our local partner, BRAC Uganda and our long standing collaboration with partner VTIs' management allowed for the randomization of 1,112 students into the program.

1.3.1 Randomization and Treatment Details

The randomization was private, that is, only the research team was privy to the process. We assigned all students in the 2019 cohort to three randomly selected groups and to treatment eligibility as follows: 30% were assigned to the Meet Your Future Program (T1) and 30% were assigned to the Meet Your Future Program with Cash (T2). The remaining 40% were a pure control.[13] The randomization we performed was stratified at the student level. In Appendix A.3.5 we describe how we choose the "strata variables", the set of variables for which we stratify, and the "balance variables", the set of variables for which we require no imbalance. We set specific imbalance goals to make the re-randomization process as transparent as possible. All strata and balance variables are included in all treatment regressions. In all our choices, we followed the principles highlighted in Bruhn and McKenzie (2009) and Athey and Imbens (2017). The identification strategy for our RCT relies on the assumption that within each strata, treatment and control students do not differ on average in all observable and unobservable characteristics. To support this hypothesis, we check for balance across treatment arms over observable characteristics likely to correlate with the outcomes of interest. The experimental design is balanced in nearly all the variables of interest, as shown in Table 1.1. We have low attrition: if we consider attritors those not found at neither endline 1 nor endline 2 we record an attrition rate of 9%. By survey wave we have a 16% attrition at endline 1 and 18% at endline 2, a rate that after three years is satisfactory and in line with the literature. In Appendix A.3.2 we describe correlates of student attrition, confirm thet attrition is uncorrelated to treatment, and show that there is no evidence of

[13] To design our intervention and refine each survey tool and protocol, we piloted a small-scale version of the program with 30 students and 10 mentors from a sixth VTI (not part of the intervention) between October and December 2020. All pilot participants completed the program and provided highly positive feedback about its usefulness.

differential attrition across treatment and control based on observable characteristics (Table A.3.2). For these reasons we do not correct for attrition in our main regression specifications.

The Meet Your Future Program We connect students randomly assigned to receive this treatment with "the future you", a successful worker who graduated from their same course of study.[14] As part of the program, we facilitated three phone conversations, which we refer to as mentorship sessions 1, 2, and 3. During these sessions, students had the chance to ask questions as well as share their doubts, fears, and dreams. These interactions were unrestricted: no specific topic coverage is required. Each student-mentor pair was free to discuss what they find most interesting and useful for the student's transition from the education system into the labor market. In this way, the mentorship is tailored to each student's specific needs and resembles the many forms that real life interactions with a network member can take. The first mentorship session (MS1) took place approximately one month before graduation. It is a conference call between the student, the mentor, and the enumerator who initiates and records the conversation. Treated students learn about the existence of the MYF program from the enumerator during this first session. Following the initial introductions, the enumerator remains silent, listens to the conversation, and compiles an observational survey (the Artificial Survey – more details available in Table A.3.1) to identify the topics covered as well as to characterize the form of the conversation. Immediately following MS1, we administered a brief post-intervention survey to the students to record their main takeaways from their first interaction with the mentor. The second (MS2) and third mentorship sessions (MS3) took place two weeks prior to and two weeks following graduation (Figure A.3.1). These were initiated by the mentor and were private conversations between the mentor and the student. Mentors were required to send a text after the completion of each of these sessions to confirm they took place. We double checked this information with the students during endline 1. Students and mentors are free to interact beyond these three sessions. In such cases, mentors were required to take notes of the frequency, duration, content, and means (in person, phone call, video-call, WhatsApp messages, SMS, email, etc.) of any additional interaction that took place over the two-month duration of the program.

Mentors attended a one-day training led by the research team prior to the start of the program. During training, mentors learned their responsibilities as program ambassadors and were guided through the various ways they could assist students with their transition into the workforce. They were also reminded that they can interact with the students as many times as they wish, and that during their interactions with each student they are free to discuss whatever they think would be most useful for that student to learn. To thank them for their two-month long program participation, conditional upon the completion of the three mentorship sessions with all students and a short check-in survey, we provided mentors with ∼\$40 as well as reimbursements of airtime incurred to make the phone calls.

[14]When pairing students with mentors, we also aimed to maximize the same-VTI match. In 16% of cases, we were unable to find a match on VTI due to a lack of available graduates. In such instances, students were paired with successful graduates from the VTI nearest to their own.

Their facilitation did not depend on students' success in the labor market.

To test whether simultaneously relaxing liquidity constraints would compound the effects of an exogenous network expansion, we unconditionally provided a random subset of the MYF Program participants 40,000 UGX (∼$12) through mobile money upon graduation. The cash transfer is unconditional. However, students are recommended to use such funding for their job search and we require them to report to BRAC how they spent the cash. Against our expectations, the cash transfer proved largely ineffective. In Appendix A.3.7 we report a detailed description of the cash transfer take-up. We benchmark the amount provided and show all the results separately for the students who received the transfer. For the vast part of the analysis, we pool T1 and T2 and refer to the effects as those of the MYF program.

1.3.2 Program Take-up and Participants Engagement

On the extensive margin, the take up rate was high: 91% of the students assigned to the MYF program corresponded with their assigned mentor at least once.[15] The intensive margin reflects the substance of these connections: the average number of interactions over three months was 2.6, with an average duration of 51 minutes each. At one year, the average number of interactions rose to 7.8 with an average duration of 28 minutes (due to an increase in instant messaging). 66% of students-mentor pairs interacted more than the three times dictated by the program and, conditional of having ever connected, 45% of mentor-mentee pairs were still in touch a year after the MYF rollout (Figure A.2.1). The average total amount of interaction time between students and mentors is 3.2 hours.[16] Recognizing that multiple hours of relationship-building per session might over-tax the commitment of the mentor or mentee, we advocated for the first conversation to last around 1 hour while leaving all the subsequent interactions unrestricted (Raposa et al., 2019).

From students, we collected self-reported measures of engagement, identification, transportation, and perceived usefulness. From the enumerators' observations of student-mentor conversations, we were also able to assess the conversation's ease and engagement. We observe high satisfaction rates across all indicators and student-mentor pairs.[17] Similarly, the identification and transportation indices we built adapting Banerjee et al. (2019) were high.

We validate these findings by utilizing our exclusive data source, the audio recordings of the mentorship sessions. First, we physically transcribed 512 audio recordings and translated

[15]In Table A.3.4 we show that non-compliers (57 students) are no different at baseline on observables. We were unable to reach 26 randomly selected students, and hence their participation decisions are unknown. Mentors failed to contact an additional 37 students while the balance of four students were not interested in participation. In brief, almost the totality of students offered the program took it up. Noncompliance almost exclusively comes from the inability to get in touch with some students.

[16]This is hence a comparatively light touch mentorship program; a meta-analysis of mentorship programs found an average length of 6.8 hours across 55 mentorship interventions.

[17]Between 85% and 95% of treated students agreed or strongly agreed with the following statements: "You felt at ease asking questions and talking about personal issues with your mentor"; "The mentor seemed to care about your personal experience"; "Speaking with the mentor made you comfortable, as if you were with a friend"; The mentor seems prone to provide help".

their content when necessary.[18] Figure A.3.8 provides an illustration of a data point, namely a conversation. Typically, the missing audio recordings were absent because the recording quality was insufficient for transcription or because the recording was lost. After tokenizing the conversations by sentence and cleaning the sentences [19], we carry out both sentiment and content analysis (see section 1.3.3 for details on the procedures).

For sentiment analysis, we rely on VADER, a widely used model for text sentiment analysis sensitive to polarity (positive/negative) (Hutto and Gilbert, 2014)[20] Sentiment analysis reveals that the conversations were perceived as neutral or positive by all participants with an even higher positive sentiment from students (Figure A.3.9, Panel A). We report the mentor to student speaking time ratio in Panel B. Its distribution is consistent with a conversation mainly led by the mentor who is transferring salient content to students. At the same time, every student is actively engaged. This is reflected in the average number of questions asked by students to mentors (3.6). In none of the mentorship sessions were zero questions asked from student to mentor. Finally, when at endline 2 we asked the students what they believed future cohorts of students should be charged for participating in case of scale up, their average answer was 24,000 UGX. This is around half of the current program cost per student, which, as we describe in section 1.6, likely gets substantially cheaper after the first three years of roll-out.

To conclude our analysis of the engagement levels, we explore when strong links are more likely to form between mentor and mentee, where we define strong links as the pairs with interactions beyond the designed scope of MYF. For this purpose, we analyze data dyadically, that is, we consider both the characteristics of the student and the mentor in tandem. This allows us to assess whether strong links between students and mentors with similar characteristics (homogamy) are more likely to form than the reverse or whether characteristics of students or mentors—independent of their counterpart—are more strongly associated with strong link formation. We estimate the dyadic regression model introduced by Fafchamps and Gubert (2007). Strong links (SL) in our setting can only be unidirectional, i.e.,$SL_{ij} = SL_{ji}$ for every i and j. The symmetry condition that follows from the unidirectionality allows us to specify the regression as:

$$SL_{ij} = \beta_0 + \beta_1|z_i - z_j| + \beta_2(z_i + z_j) + \gamma|w_{ij}| + u_j \tag{1.1}$$

Where z_i and z_j are characteristics of student i and mentor j thought to influence the likelihood of SL_{ij}, a strong link between them. The coefficient β_1 measures the effect of differences in attributes on SL_{ij} while β_2 captures the effect of the combined level of z_i and

[18]While the majority of conversations were conducted in English, a few contained Luganda or Lusoga segments.

[19]Our data preparation steps were: removing stopwords ('yeah', 'hello', 'ye', 'yes', 'okay', 'ok', etc.); dropping sentences with less than 10 characters; removing greetings: 'good evening', 'have a lovely day'; homogenizing the format of monetary amounts, which included converting shs and UGX (Ugandan Shillings) to USD.

[20]VADER (Valence Aware Dictionary and sEntiment Reasoner) is a lexicon and a parsimonious rule-based sentiment analysis open-sourced tool.

z_j on SL_{ij}. We cluster standard errors at the mentor level as the dependency structure is only partial: dyads that share any common member are allowed to be correlated with one another. However, only one side of the pair can be correlated (i.e., the mentor).[21] We perform estimation over the sample of students assigned to the MYF program. On the students' side we include their gender, rural status of their household of origin, scholarship, asset index, Raven's Test and ownership of land. For the mentors, we include gender, rural, scholarship, asset index, and land ownership. For the dyad, we include tribe, VTI, district of origin, and gender. Table A.1.1 reports the results. We observe three primary inhibitors to strong link formation: students and mentors from different VTIs, age gaps, and common socioeconomic position. Although 86% of pairs are of the same gender, we see no statistically significant differences with mixed gender pairs. Limited statistical power prevents us from making conclusive statements.[22]

1.3.3 Interactions Content and Students' Takeaways

What did students and mentors talk about, and what did students learn from their mentors? The combination of the text data from the audio recordings of the mentorship sessions, the observers' data from the Artificial Survey, and the students' self-reported primary takeaway provides an invaluable window into the conversations and a unique opportunity to characterize the intervention. We are thus able to unpack the black box of interactions between mentors and mentees. We posit that mentors can support the students by providing different kinds of support and information, which we classify into four main groups: provision of information on entry conditions (I); provision of tips and guidance for a successful job search (S); job referrals to potential employers (R); encouragement and confidence over a positive future outlook (E).[23] Panel A of Figure 1.4 presents the raw conversation content

[21]Dyadic observations in our setting are not independent since $E[u_{ij}, u_{kj}] \neq 0$ for all j. If students were allowed to interact with multiple mentors then we would have $E[u_{ij}, u_{ik}] \neq 0$ for all i as in Fafchamps and Gubert (2007) and clustering the SE would have not been enough. In our setting, provided that regressors are exogenous, applying OLS with clustered standard errors yields consistent estimates (see Fafchamps and Gubert (2007) for details).

[22]As a robustness check, we perform a similar analysis of strong link predictors following Mullainathan and Spiess (2017) and Liu, 2019 using a LASSO model with weights based on cross-validation. To do so, we first selected characteristics of students, mentors, and of the dyad which we hypothesized as relevant for strong tie formation. We ran t-tests to check which variables were individually relevant in explaining the outcomes, pooling those that had a P-Value larger than .10. Thus, we had two pools of candidate variables: one comprising all the variables we had initially selected and another comprising those that displayed some individual significance in t-tests. Then, we randomly split our sample into a training subsample (70%) and a hold-out subsample (30%). On the training subsample, we ran an OLS with the pool of significant variables, a LASSO with the pool of significant variables, and a LASSO with all the variables pooled. We ran the prediction functions for each algorithm on the hold-out subsample and selected the best predictive algorithm based on the smallest Mean Squared Errors in the hold-out subsample. In this way, the selected predictors for the process outcome were those selected by the best algorithm. Table A.3.12 reports the results. They are consistent with what we learn from the dyadic regression.

[23]Figure A.2.3 contains detailed information on the most recurring components of each category as recorded by the observers, i.e.,enumerators from the research team who were listening to the mentorship

as computed using the text data. To perform topic analysis and detect the conversation's content, we employ an unsupervised learning model. We rely on the state-of-the-art BART Model trained on the Multi-Natural Language Inference (Multi-NLI) dataset. Specifically, we employ a zero-shot sequence classifier developed by Yin et al. (2019) to determine the similarity scores between each of the sentences in an interview and micro-topics representative of the categories we are interested in. While in the zero-shot classification scenario, a classifier is required to work on labels that it is not explicitly trained with. Indeed, we directly make use of a model pre-trained with NLI tasks, so we do not need any labeled data for model training.

Intuitively, the algorithm assigns each sentence of the conversations to one of four cateogories, I, S, E, and R, based on a similarity score to the labels used to define each category. When all four similarity scores fall below a specific threshold (which we manually identified to maximize the accuracy of the splits), a sentence is assigned to the residual category, neutral (see Appendix H for more details on the procedure as well as for an example of conversation and examples of classified sentences). Manual reading of the content categorized as neutral suggests that (1) the threshold is conservative, i.e.,not all the sentences classified by the algorithm as neutral are indeed neutral with respect to the four categories identified; (2) the vast majority of the neutral sentences consist of initial greetings, personal introductions, exchange of phone numbers, resolutions of issues related to the poor network quality in the call, or simply short sentences that are hard to classify, such as "yes, that completely makes sense" etc.; (3) we identified only two recurring topics we are currently disregarding in our analysis: examinations (upcoming for the students and discussed in roughly 5% of the neutral sentences) or Covid-19 general prevention and worry, when the conversation is not linked to the job market. Appendix A.3.8 presents a brief description of the model we use. We refer the reader to the literature on zero-shot text classification for topic modeling and language inference for a more detailed description.[24]

In Panel A of Figure 1.4 each observation is a conversation. In addition, each sentence is weighted according to its word count. Therefore, the figure represents the raw proportions of each conversation devoted to discussing information about entry level jobs, search tips, job referrals, and encouragement. Several things can be deduced from this figure. First, job referrals, including both the mention of current vacancies the mentor is aware of and the promise of future job referrals, were less frequent than we anticipated. Second, all three remaining categories of support were discussed in the majority of conversations, with information about entry level jobs and encouragement having the highest correlation in terms of frequency. While learning about the conversation content is useful to diagnose what was discussed, Panel B of Figure 1.4 tells us what was *learned* by the students. The figure shows the share of students whose main takeaway from the first mentorship session fell into each of the four categories of support. We confirm that job referrals were not the most salient information the students absorbed. Furthermore, we demonstrate that the elasticity

sessions and noting down micro-topics discussed, live.

[24]Melvin et al. (2016); Devlin et al. (2018); Lewis et al. (2019); Yin et al. (2019); Yinhan et al. (2019).

of retention is significantly greater for the encouragement category than for the search tips and information on entry requirements. These considerations will be helpful when analyzing the mechanisms through which the effects occur.

1.4 Results

1.4.1 Estimation

In this section, we document how mentorship influences students' labor market outcomes three months and one year after the school-to-work transition. We estimate both the ITT and the ATE for compliers. The former set of estimates is useful from a policymaker's perspective because it reflects likely binding challenges to scaling-up similar mentorship interventions. We report ATE estimates in Appendix A.3.10.[25] Our ITT estimates are based on the following ANCOVA specification for student i in strata s at endline $t = 1, 2$:

$$Y_{i,s,t} = \beta_0 + \beta_1 T_i + X_i' \delta + \lambda_s + \epsilon_{i,s,t} \tag{1.2}$$

Y_i is the outcome of interest for student i measured at endline 1 or endline 2 (i.e., at 3 or 12 months). T_i is a treatment indicator that equals 1 for students assigned to the MYF Program and 0 for control students. X_i is a vector of balance variables listed in Appendix A.3.5 and individual covariates measured at baseline to improve statistical power (McKenzie, 2012); these covariates were selected from the baseline data on the basis of their ability to predict the primary outcomes.[26] λ_s are strata fixed effects and $\epsilon_{i,s,t}$ is the error term. We cluster errors at the strata level. Estimation is performed over the entire sample of students. The ATE specification instruments treatment assignment with treatment take-up (with the same controls). We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of MYF on the compliers. In our preferred specification, take-up is defined as a dummy equal to 1 if the student spoke with the assigned mentor at least once (Tables A.3.13, A.3.14 and A.3.15). When we define take-up as having completed all three mentorship sessions, the results are stronger in magnitude (A.3.19, A.3.20 and A.3.21). We run an additional iteration for robustness with standard errors bootstrapping at 1,000 replications. β_1 measures the causal effect of being selected for participating to the MYF Program on Y_i under SUTVA.

This will not hold if treatment displaces control students because treated students are relatively more attractive to employers. As we are currently implementing the program in 5 out of 715 accredited VTIs in Central and Eastern Uganda (1270 nation-wide), any advantage for treated students will likely not come at the expense of the control group.[27]

[25] Because of the high compliance rate in the experiment, ATEs and ITTs are extremely similar.

[26] We adapt the post-double-selection approach set forth in Belloni et al. (2014)

[27] As of 2017-2018, the total number of VTIs in the Central and Eastern regions, both formal and informal, accredited either by the DIT (493, of which 383 located in the Central region, and 110 located in the Eastern region) or UBTEB (291, of which 196 in the Central region, and 95 in the Eastern region) or both (69, of

Indeed, treated students represent a small fraction of the cohort of job seekers, and the urban labor markets into which they are most likely transitioning are among the largest in the country (Kampala, Jinja, and Iganga). The scale of the program is unlikely to meaningfully change labor market conditions for control students. SUTVA could also be violated in the case of spillovers. Specifically, spillover effects associated with the sharing of information and job search recommendations between friends in the same VTI. To limit spillovers between treated and control students, our intervention occurred after classes were concluded and students had returned home (most of these VTIs are indeed boarding schools). After the treatment, the students only met once as part of school activities, on the day of the final exams. We are not overly concerned with spillovers as, given our methodology, they are likely to render the estimates conservative. In any case, we mapped the VTIs' friendship networks of each treated and untreated student to rigorously measure them. Specifically, we gathered information on each student's two closest friends in the cohort, regardless of classroom or field of study. In this way, we are able to determine the treatment status of each student's two closest friends as a result of the fact that, for the primary experiment, we constructed a panel data comprising the entire cohort of interest. Appendix A.3.9 has a more extensive examination of the spillover effects and concludes that there is some suggestive evidence of information spillovers, which, if at all, caused our overall estimates to be conservative.

1.4.2 Initial Labor Market Outcomes

Table 1.2 presents ITT estimates of the impacts on labor market outcomes at three months. We begin by looking at the extensive margin: three months after graduation, we identify large impacts on employment. Among treated students, labor market participation is 27% higher - as measured by being out of the labor market (neither searching nor working) and by days worked in the month preceding the survey (Column 1 and 2 respectively). Column 3 shows that treated students are 33% more likely to leverage human capital complementarities accumulated from their vocational education. Here, the outcome variable is the number of hours spent applying newly acquired skills in their occupation of training in the 30 days preceding endline 1. The tasks may have been performed as part of the respondent's work activity, but also informally for a friend, family member, or themselves. To construct this variable, we designed an innovative survey module to track how much time the respondent spent performing each of a set of detailed typical trade-specific tasks; we compiled a list by combining information from focus group discussions with the alumni and resources from the O*NET Program. Column 4 points towards no differences in earnings, while in Column 5 it emerges that these first matches are more stable for treated students: they last 23% longer.

which 50 in the Central region, 19 in the Eastern one) was 715. However, this is likely to be an upper bound: 33 VTIs (8 in the Eastern Region, 25 in the Central region) that appear in a 2015 list do not appear in what seems to be an updated version of such a list in 2017. Additionally, the overlap between UBTEB and DIT-accredited VTI could be refined. These are the results from an exact match. The lower bound is 460 (should all the UBTEB and DIT overlap and should those 33 have closed down).

1.4.3 Transitions and Medium Run Labor Market Outcomes

Because we followed these students for 1 year, we are able to study dynamic responses to the treatment. Table 1.3 reports treatment effects on the transition across job spells as well as employment and earnings at one year. What emerges is that the more numerous and more stable matches treated students landed early on in their search allowed them to ascend the job ladder more quickly; they are more likely to be both retained within the same firm (Column 1) and promoted across firms (Column 2).[28] Put simply, treated students are more likely to transition to a worker-type position following an initial traineeship at three months. In sum, what seems to be happening here is that treated students land more jobs in their training sector: they do not make more than their control counterparts. However, they work more intensively and leverage and build on their technical skills in those jobs. Hence, they stay longer in those jobs and leverage them for superior future employment opportunities. Control students do not take up apprenticeships as fast. They continue searching, and many of them become discouraged, resulting in a 27% greater likelihood of having left the labor market three months after graduation, and subsequent depreciation in human capital. After one year the control students catch up on the participation dimension: treated graduates are now as likely to be employed as control students. However, treated students earn 18% more than control students. As to labor market participation, the effects are sizable and statistically significant at three months (Column 1 in Table 1.2). At one year, the coefficient is positive and relatively large at around one standard deviation, but the lack of power limits our ability to make decisive statements. However, we see that treated students are less likely to be persistently detached even if they were not at the 3 month interval and vice versa.[29] All main results are unaffected by the inclusion of an additional set of controls selected through a double LASSO procedure (Belloni et al., 2014).

1.5 Mechanisms

1.5.1 An Illustrative Model

Through which mechanisms have the mentors improved young job seekers' labor market outcomes? In this section, we present a stylized model to guide the interpretation of our results. Informed by economic theory, the context of our experiment, and the text analysis of the conversations, we identify four potential mechanisms mediating the treatment effects on labor market outcomes described in section 1.4: job referrals, search tips, information about

[28] 82% of those employed at three months are covering a trainee-position. The rest are either wage-employed (12%) or self-employed (5%).These shares are equivalent in treatment and control. At one year, the share of those in a traineeship is 7% hinting to the fact that some of them have either transitioned to higher positions or into unemployment.

[29] They are less likely to have never rejoined if they left at 3 months, and they are less likely to have detached from the labor market at 1 year if they had not detached at 3 months.

entry level jobs, and encouragement. From the framework, we derive testable predictions. The proofs of the propositions listed below are in Online Appendix A.3.6.

Set-up We consider a simple partial equilibrium environment with a utility maximizing job seeker whose behavior follows a reservation wage strategy. We model their dynamic responses to what the MYF provides through the lens of a finite-timed version of the seminal search model from McCall (1970) in which search occurs sequentially. We adapt this model to incorporate subjective beliefs about the labor market following Cortés et al. (2021). Specifically, our representative job seeker has subjective beliefs over the entry wage distribution, $F(w)$, as well as the job ladder, $\omega(w)$, i.e., the transition matrix from wage w at time t to wage w' at time $t + x$. Time t is discrete and job seekers have preferences over consumption given by $u(c) = c$. Job seekers are homogeneous in skill level, i.e., there are no types. We assume that agents are infinitely lived. When they are not working, job seekers earn their value of leisure, b.

Absent the MYF Program, in each period t unemployed job seekers choose whether or not to search for a job, taking into account the i.i.d. cost of search, $c \sim H(c)$. If a job seeker decides to search, they draw a wage offer w_t with probability λ, a random draw from an exogenous probability distribution $F(w) \sim N(\mu, \sigma)$ with associated density $f(w)$. Job seekers decide whether to accept the offer or wait for the next period. If they accept, they receive w_t in t and $w_{t+1} + \omega$ thereafter, where ω represents a fixed experience premium which you mature if in the previous period you accumulated experience (worked, regardless of pay). We simplify the model by requiring that ω becomes zero for a tenure greater than one spell. If they decline the offer, they return to the search decision step. We do not allow for on-the-job search or job destruction.

Biased Beliefs To replicate what we establish experimentally in section 1.2.3, we assume that job seekers do not know how μ, the mean wage offer they will receive, nor ω, the wage evolution given by the experience premium look like.[30] Instead, they form beliefs about μ and act based on a perceived probability distribution $F(\hat{\mu}, \sigma)$ of the entry level wages. Likewise, they form beliefs about $\hat{\omega}$ and act accordingly. We say that the job seekers's beliefs are biased if $\hat{\mu} \neq \mu$ or if $\hat{\omega} \neq \omega$. Job seekers with $\hat{\mu} > \mu$ are optimistic. While we assume that beliefs change over time, we also assume that job seekers are myopic, i.e. when making their decisions, they do so under the assumption that the expected offer is the same forever. This means that they do not incorporate or foresee future learning w.r.t. their current problem (Cortés et al., 2021).

[30]Our framework comprises of distorted beliefs and subsequent learning about the distribution mean of the wage offer distribution at entry. Alternatively, biases in beliefs about one's job search prospects have been modeled as biases in assumptions regarding the arrival rate of job offers λ (Spinnewijn, 2015; Bandiera et al., 2022a). The students in our study appear to have a good grasp of the timing requirements for obtaining a first job. What they fail to account for is the type of position (internship versus temporary or permanent workers) and earnings associated with the first job. Students reported seeking formal, paid employment, despite the likelihood of obtaining such a position for an individual with their age and skill profile being extremely low. Similarly, Banerjee and Sequeira (2022) find that young job seekers in South Africa expect to earn nearly twice the median actual salary of individuals with similar profiles, primarily due to an overestimation of the likelihood of obtaining a high-wage job.

Similarly to what Krueger and Mueller (2016) documented in New Jersey, learning and the subsequent convergence to the true values of μ and ω occur slowly. Persistently, job seekers overestimate their prospects or anchor their reservation wage on their initial beliefs. As a result, we maintain the assumption that reservation wages and search participation will be chosen based on a fixed belief $\hat{\mu}$, i.e.,without considering future changes in the expected offer (Cortés et al., 2021).

Values of Employment and Unemployment In keeping with much of the literature on learning, we assume that job seekers optimize within an expected-utility framework. The value of employment at wage w for some beliefs $\hat{\mu}$ and $\hat{\omega}$ can be solved for explicitly. As we permit wage growth, the value of employment will depend on the beliefs over the job ladder:

$$W(w, \hat{\omega}) = \frac{w + \beta\hat{\omega}}{1-\beta} \tag{1.3}$$

The value of unemployment instead can be written as:

$$\begin{aligned} U(\hat{\mu}, \hat{\omega}) = \int_c \max_{s\in[0,1]} \Bigg(-cs + b + \beta s\lambda \int max\{W(w, \hat{\omega}), U(\hat{\mu}, \hat{\omega})\} dF(w; \hat{\mu}, \sigma, \hat{\omega}) \\ + \beta(1-\lambda s)U(\hat{\mu}, \hat{\omega}) \Bigg) dH(c) \end{aligned} \tag{1.4}$$

and it depends on the job seeker's beliefs because the expectation is taken over the subjective offer distribution $F(w; \hat{\mu}, \sigma, \hat{\omega})$. Given a draw for search costs c, the job seeker must determine whether or not to search. If they choose not to search, they receive no offers, whereas if they search, they face a probability λ of receiving an offer. By comparing the returns to search, to the returns not to search we obain the expression for the value of c that makes a job seeker with beliefs $(\hat{\mu}, \hat{\omega})$ indifferent between searching and not searching, $c^*(\hat{\mu}, \hat{\omega})$ defined as:

$$c^*(\hat{\mu}, \hat{\omega}) = \beta\lambda \int max\{W(w, \hat{\mu}, \hat{\omega}) - U(\hat{\mu}, \hat{\omega}), 0\} dF(w; \hat{\mu}, \sigma, \hat{\omega})$$

The job seekers will search for draws of c such that $c \leq c^*(\hat{\mu}, \hat{\omega})$.

Lastly, the job seeker determines their reservation wage in order to maximize their perceived continuation value at any point during the unemployment spell. We define the reservation wage, $w_R(\hat{\mu}, \hat{\omega})$, as the wage at which the job seeker is indifferent between accepting a job and remaining unemployed. The resulting expression for the reservation wage equals:

$$W(w_R(\hat{\mu}, \hat{\omega}), \sigma, \hat{\mu}, \hat{\omega}) - U(\hat{\mu}, \hat{\omega}) = 0 \tag{1.5}$$

1.5.2 Predictions on MYF

We predict that a mentor, as MYF provides, can affect outcomes in three ways:

1. Directly affect λ, the job offer arrival rate, by providing job referrals, therefore connecting the student to more jobs, or search tips, making the students better at searching; $\lambda \uparrow$.

2. Rectify beliefs over the mean offer distribution of their first job. As we saw in section 1.2.3 students are overly optimistic about the mean wage offer. The mentor can correct overly optimistic beliefs, therefore lowering $\hat{\mu} \downarrow$.[31]

3. Shift beliefs over the future value of the first job by providing encouragement and hope, raising $\hat{\omega} \uparrow$.

We derive predictions on the reservation wage behavior and discouragement behavior, depending on which of these 3 channels are most activated:

> **Proposition 1:** Search tips and job referrals, by increasing the probability of receiving an offer ($\lambda \uparrow$), lead to an increase in the reservation wage ($w_R \uparrow$) and an increase in the cutoff search strategy ($c^*(\hat{\mu}, \hat{\omega}) \uparrow$).

When the rate of offer arrival increases for a job seeker, the job-finding rate increases automatically. As a result, the job seeker becomes more selective and raises their reservation wage.[32]

> **Proposition 2:** Information on entry conditions rectifies optimistic beliefs, ($\hat{\mu} \downarrow$) leading to a decrease in the reservation wage ($w_R \downarrow$) and in the cutoff search strategy ($c^*(\hat{\mu}, \hat{\omega}) \downarrow$).

> **Corollary 1** The size of these effects is larger for overly optimistic job seekers.

By shrinking the *expected* early stream of high wage job offers, the mentor can induce individuals to revise their beliefs downwards. Once self-confidence is sufficiently low (either immediately leading to no search at all or as the search progresses), job seekers become discouraged and give up on searching. This proposition simply requires the reservation wage to be monotonic in belief ($\hat{\mu}$). Deteriorating beliefs reduce the reservation wage. The intuition for this result is straightforward: reductions in the perceived likelihood of obtaining a well-paid job reduce the option value of remaining unemployed—thus making job seekers more willing to accept offers and reducing the reservation wage. A large literature in

[31]The mentor can correct pessimism as well, therefore raising $\hat{\mu}$. However, less than 4% realized a wage at their first job higher than what they expected at baseline. We will therefore talk about more or less optimistic job seekers only.

[32]For this to work, we are implicitly assuming that λ is known to the job seekers. Alternatively, we need to assume that they form correct beliefs over λ, which they also correctly update following the interactions with the mentors. In other words, the students must be aware of the usefulness of the mentors for the increase in the arrival rate to be perceived, and not only actual.

empirical labor economics finds evidence of reservation wages declinining over an unemployment spell because of natural learning (Barnes, 1975; Devine and Kiefer, 1991; Feldstein and Poterba, 1984). However, more recent evidence points towards underreaction in beliefs, slow adjustment (the observed decline in perceived job-finding probabilities is only one-half of the observed decline in actual job-finding rates) and consequent undersearch (Spinnewijn, 2015; Mueller et al., 2021). We confirm this finding in our setting by looking at the unemployed in the control group, who, 3 months after graduation, are still substantially overoptimistic about their prospects. These sticky reservation wages are shifted abruptly by the treatment.

> **Proposition 3:** Encouragement and confidence over a positive future outlook lead to a decrease in the reservation wage ($w_R \downarrow$) and an increase in the cutoff search strategy ($c^*(\hat{\mu}, \hat{\omega}) \uparrow$) by upward shifting beliefs over the future value of the first job, ($\hat{\omega} \uparrow$).

Encouragement prevents students from losing hope and leaving the labor force. Control students' reservation wages and search behavior are consistent with the belief that wages evolve according to a Markov process: under such a set of beliefs, all jobs have the same slope of income growth over time, so it is reasonable for them to focus primarily on the starting wage. Under such process assumption, the starting salary is a sufficient statistic for the present value of career earnings. When mentors inform graduates of heterogeneity in wage dynamics, including the fact that unpaid jobs are more prevalent than expected (information on entry conditions) and that the path from unpaid to paid jobs is steeper than expected, treated students become more willing to accept lower-paying jobs because their future value has now increased. When optimizing their lifetime income, we anticipate that treated graduates who received encouragement will place a greater emphasis on wage growth rather than just starting wages.

Following participation in the MYF program, job seekers' employment outcomes may shift for two distinct reasons. First, an *actual* change in prospects, modelled as an increase in their arrival rates of offers.The first propositions describes how the search behavior of job seekers can change in response to a direct treatment effect on the fundamentals of the search problem (λ). Secondly, a *perceived* change in future prospects. A behavioral mechanism: propositions 2 and 3 describe the shift in job seekers' search behavior in response to a treatment effect on their perception of the search problem. Theoretically, both the reservation wage and the cutoff search strategy can move in either direction, given that each channel exerts opposing forces. Using our survey data, we will now test empirically what seems to be the dominant channel.

1.5.3 Testing the Model's Predictions: Willingness to Accept a Job and Search Behavior

We start by examining the direct impacts the mentorship program had on job seeekers' willingness to accept a job and search behavior. Columns 1 and 2 of Table 1.4 report treatment effects on reservation wages and self-reported willingness to accept an unpaid job as their first job. The results are clear: the treatment substantially lowered the reservation wage by 32% and increased the willingness to accept an unpaid job. These changes translated into changes in search behavior, most notably with respect to job offers acceptance: treated students are 27% less likely to turn down a job offer while looking for their first job. While we did not collect information on the exact wages offered, we asked the reasons for why each rejected offer was turned down. With the caveat that the sample size decreases greatly when we condition on having declined an offer, we find that treated students were much more likely to decline a job offer because it did not provide sufficient learning potential. While the difference is not statistically significant at the standard levels (P-Value .19) the magnitude of the effect is large, suggesting that power may be preventing us from making definitive statements (Table A.1.2). On the contrary, we see no difference in treatment and control when comparing the likelihood of turning down a job offer because of distance to the workplace or any other reason. The heterogeneity panel of Table A.1.5 shows that results on willingness to accept a job and search behavior are driven by the overly optimistic students at baseline.

Next, we discuss search behavior. First, we examine the effect of the treatment on the decision to participate in the labor market by determining whether or not individuals began their job search after receiving training. Column 4 shows that treated students are more likely to initiate a job search. Despite the decline in reservation wages, the overall impact on labor market participation is positive. This finding highlights the significance of the treatment's encouragement component. Similarly, we might explain the positive effect on the willingness to accept an unpaid job as follows: treated students received the "bad news" and internalized it, as indicated by the decline in reservation wage. However, via encouragement and confidence, mentors raise the perceived future value of a low paying job today, thus helping the students adjust to the "bad news" without letting discouragement set in. According to our model, these findings suggest that the positive effects of encouragement on the cutoff search strategy (Proposition 3) outweigh the negative effects described in Proposition 2.

We then test whether treated students improved their search skills following the mentorship sessions, which included a substantial amount of discussion about actionable search tips. To achieve this, we construct an index of search effectiveness that measures the students' conversion rates during the search process. We determine conversion rates based on the total number of applications, interviews, and job offers. The first ratio equals the number of interviews to the number of total applications. The second metric is the ratio of received offers to applications submitted. We observe no effects of the intervention on any search effectiveness dimension. In addition, in Column 5, we rule out variations in one more aspect of search behavior: search intensity as measured by hours per day, days per week, number of applications submitted, and money spent on search.[33]

[33]While our conceptual framework does not include directed search, we can use rich survey data on the search process to rule out changes in search breadth, as measured by the number of search methods employed, the geographical scope of the search, and the number of sectors targeted. Again, we observe no treatment

Finally, in Column 7, we see that conditional on searching for a job, students assigned to a mentor have a 30% shorter initial unemployment spell. This result is particularly important given all the empirical evidence in support of the existence of a declining hazard rate when it comes to unemployment. Long-standing research has demonstrated that the unemployment exit rate falls as the duration of unemployment progresses due to behavioral changes among the unemployed - for example, because discouragement leads to less job search and thus a lower exit rate (Kaitz, 1970). To conclude, treated students do not seem to have searched any differently. Instead, what the treatment changed was their willingness to accept the existing available jobs, all while not dropping out of the labor market. Column 6 indeed shows that treated students were no less likely to decide not to search at all. To sum up, given the shock to beliefs about the wage distribution and job ladder, treated students are more willing to adopt available employment offers rather than increase their search intensity for unicorns. Given the prevailing challenges of the Ugandan labor market, their returns to revise their search intensity or broadness decrease. Rather, they take up a job more quickly, accumulate practical experience, leverage human capital complementarities, build persistence and tenacity, and eventually, get retained (promoted) or transferred to a better job.

Overall, Table 1.4 along with the results on job referrals, shows that the net treatment effect on reservation wages was negative, hinting at the importance of the information on entry level positions as a channel for our results. We conclude that MYF acted as an especially salient information treatment. Mentorship led students to revise downward their overoptimistic beliefs over labor market conditions and revise upward their beliefs in the criticality of initial employment for career trajectories.

While these figures speak to the relative importance of the information and encouragement channels with respect to the search tips and job referral channels, they do not tell us whether the other two were at all relevant. Did mentors provide valuable job referrals and search tips? Was the belief shock so strong that it dominated all others? Or were these channels not activated in a useful manner? To answer these questions, we do three things. First, we go back to our rich survey data. To measure the relevance of the job referral channel, for each work activity we asked the treated students whether they found it through a connection made by the alum. While 7.4% reported receiving or being offered a referral by the alum, only 2.9% actually found their first job through one of them (half of which were direct hires by the alum). To ensure that we were not underestimating job referrals, we compared the names of all businesses where students worked to those where their mentors had worked three to five years earlier (since for every mentor, we have information on their entire labor market history). In Tables A.3.22, A.3.23 and A.3.24 we report treatment effects on the four families of results after dropping the 2.9% students who found their jobs through a mentor job alum referral. The results essentially remain unchanged, showing that the job referral channel in this setting was not the driver of the treatment effects.

Then, we run two validation exercises exploiting two additional randomization features

effect on any index dimension or the overall index.

of the experiment: first, the random assignment to each mentor. Second the randomization to T2, the additional cash transfer.

1.5.4 Mentor Heterogeneity

In this section, we investigate how students' assignment to different mentors, each of whom is capable of conveying a certain type of support more effectively than others, affected their labor market outcomes. Beginning with Empirical Bayes (EB) approaches, we demonstrate the existence of mentor-level heterogeneity of interest. Then, we employ an Instrumental Variable strategy (IV). We posit a particular set of channels for explaining the heterogeneity, and introduce the underlying assumptions under which the approach is valid.

EB: Variation in mentors effectiveness We estimate the extent of the heterogeneity using EB techniques. We begin running the following reduced form regression:

$$Y_{i,j,d} = \sum_j M_{ij}\gamma_j + \lambda_d + \mu_i \tag{1.6}$$

where Y_i is the outcome of interest for student i as described in equation 1.7. λ_d are VTI and course fixed effects. M_{ij} are the 158 mentor indicators. A standard F-test rejects the null of no mentor heterogeneity (P-Values of .00 and .03 for the short run labor market index and the career trajectory index, respectively). Although the overall sample is large, the sample cells are small within each mentor, leading to finite sample bias. Consequently, the $\hat{\gamma}$ obtained via equation 1.6 are going to be overdispersed: even if all the γ were the same and there was no dispersion in mentor effect, we would still have some chance variation across the $\hat{\gamma}$ we get to see. We therefore estimate a bias-corrected variance of the γ to account for excess variance of the estimates due to sampling error. We do so by subtracting the average square standard error from the estimates of the $\hat{\gamma}$'s variance (Kline et al., 2020).[34] Figure 1.6 reports the distribution of the fixed effects as well as the shrinked posterior means for the coefficients, assuming a normal/normal model. While the original estimates are noisy, the posterior distibution is shrinked toward the prior mean on the basis of the signal-to-noise ratio. The bias-corrected variance estimates we obtain are large. Specifically, .47 for the short run index and .45 for the career trajectory index. These are relatively high when compared to the teacher value added literature, where above .2 is considered high dispersion (Angrist et al., 2017). This means that moving up one standard deviation in the distribution of mentors increases the short run index by .47 and the medium run index by .41 of the standard deviation of each respective index: some mentors are significantly more effective than others. We also have a strong signal-to-noise ratio of around .66 for both indexes, indicating that most of the variation we see in mentors' effectiveness is actual signal and not mere noise.
IV: Mentors' types We now posit the particular set of three channels for explaining this

[34] Under the assumption that the estimated standard errors of $\hat{\gamma}$ are reasonably accurate, this variance estimator is unbiased and consistent with a large number of mentors. Kline et al. (2020) have a general framework for the estimation of unbiased variance components under unrestricted heteroskedasticity.

heterogeneity. Our three channels are exactly the three main types of support emerged during the conversations, which map onto the mechanisms proposed in the illustrative model. What we are after is:

$$Y_i = \beta_0 + \beta_1 Info_i + \beta_2 Enc_i + \beta_3 Search_i + X_i'\delta + \epsilon_i \tag{1.7}$$

where Y_i is the outcome of interest for student i. We focus on the four standardized indexes described above.[35] $Info_i, Enc_i$ and $Search_i$ are three indicator variables for whether the mentor provided mainly information on entry conditions, encouragement, or search tips during the first mentorship session, as measured by the students' main takeaway. However, running equation 1.7 would not necessarily give us the causal effects of conversation content on the outcomes of interest. Although different mentors are more likely to provide information vs. encouragement vs. search tips, conversations were non guided.

To overcome the risk of OVB we leverage the randomization to the mentors. This second randomization takes place after the first one (T1, T2 or Control), which implies that each mentor is either assigned all students in T1 or all students in T2 (Figure 1.2). Being randomly assigned to a mentor generates exogenous variation in conversation content. This suggests using the 158 mentor indicators as an instrument for conversation content and studying whether mentors that shift the conversation in certain directions have bigger effects.

The first stage regressions are:

$$Info_{i,j,d} = \sum_j M_{ij}\gamma_{j1} + \lambda_{d1} + \mu_i \tag{1.8}$$

$$Search_{i,j,d} = \sum_j M_{ij}\gamma_{j2} + \lambda_{d2} + u_i \tag{1.9}$$

$$Enc_{i,j,d} = \sum_j M_{ij}\gamma_{j3} + \lambda_{d3} + \tau_i \tag{1.10}$$

where M_{ij} are the 158 mentor indicators and λ_{d1}, λ_{d2} and λ_{d3} the VTI and course duals fixed effects.

The second stage regression is:

$$Y_{i,d} = \beta_0 + \beta_1 \widehat{Info_i} + \beta_2 \widehat{Enc_i} + \beta_3 \widehat{Search}_i + \lambda_d + \epsilon_i \tag{1.11}$$

where Y_i are the same outcomes of interest in equation 1.7. $\widehat{Info_i}$, $\widehat{Enc_i}$ and $\widehat{Search}_i$ are the fitted values from the first stages.

The validity of this strategy relies on two assumptions:

1. Relevance of the instruments. This assumption is violated if the 158 mentor dummies, our instruments, are uncorrelated with the three endogenous variables representing the main conversation content.

[35] In Table A.1.6 we report separate results for all the outcomes in each of the four families: willingness to accept a job, search behavior, short run and career trajectory labor market outcomes

2. Exclusion Restriction: the instruments (mentor assignment) have direct effects on search behavior and labor market outcomes only through the three channels identified (i.e.,whether they are information on entry conditions-types, encouragement-types and search tips-types). This assumption is violated if, for example, there are other conversational contents we are not accounting for that affect the outcomes of interest not through the endogenous regressors. We rule out this possibility below.

Relevance We test for weak identification following Sanderson and Windmeijer (2016)[36] At the bottom of Table 1.6 we report, for each endogenous regressor separately, the P-Value on three first-stage F-statistic for excluded instruments. We reject the null hypothesis of weak identification for all three endogenous regressors. First-stage F-statistics are always between 11 and 27, suggesting finite-sample bias is not an issue. In other words, there is sufficient variation to be exploited in our instruments even after partialling out the predicted value of the other two endogenous variables.

Exclusion Restriction To test the exclusion restriction, we leverage the large number of orthogonality conditions (158 to identify 3 endogenous variables). The resulting 155 overidentifying restrictions generate an overidentification test of the sort widely used with instrumental variable estimators. We conduct the Sargan-Hansen test, where the joint null hypothesis is that the instruments are valid ones. We cannot reject the null for three out of four outcomes of interest, and the fourth one is rejected at marginal significance levels, suggesting that we have identified what mediates the heterogeneity.

Results Table 1.6 and the corresponding Figure 1.7 report the results on the four indexes.[37] We confirm the findings from our main analysis: mentors who provided information about entry level jobs as well as encouragement and confidence that things would get better were the most effective in the short run. In the medium run, the role of encouragement becomes even larger: by ameliorating the discouraging effects of the information on entry level wages, the push to persevere and be patient leads to greater labor market grit. Greater tenacity pays persistent dividends toward students' career trajectories.

Extentions To inform the optimal design of mentorship programs, we explore additional characteristics of the mentors and of the design, a task we are well positioned to undertake. First, we investigate whether the mentor's demographic traits predict their effectiveness. We have a great deal of information on these mentors and we use it. In Figure A.2.4, we summarize the results: wage-employed, high socio-economic status, and enthusiastic mentors are more effective in the long run.

Second, we investigate whether program design and logistical factors can improve effectiveness. We begin by examining the number of mentees. Figure A.2.5 summarizes the findings. It appears that mentors' effectiveness decreases when they are assigned an excessive number of mentees, although we lack sufficient evidence to draw firm conclusions. In the future, we plan to leverage the exogenous variation in the mentorship session-order with respect to the other mentees allocated to the same mentor to examine if exposure to

[36] A modification and improvement of Angrist and Pischke (2009)

[37] Tables A.1.7 and A.1.8 report the results on the single components of each of the four indexes.

a more experienced mentor (one who has already led multiple mentorship sessions) differs from exposure to a first-time mentor.

1.5.5 The Cash transfer

To understand whether simultaneously relaxing liquidity constraints has the potential to magnify the effects of the mentor program, we unconditionally provide 40,000 UGX ($\sim$ \$12) to a random subset of MYF Program participants. We only recommended that they use the money to aid them in their job search or contact the mentors. The additional cash transfer led to no differential impact in the short run (Table A.3.10).[38] Instead, it attenuated the effects at 1 year (Table 1.7). To investigate what caused these patterns, we look at differences in engagement as well as conversation content and students' takeaways. We rule out any significant differences in frequency, timing, engagement level, and duration of interaction between students assigned to MYF only (T1) and students assigned to MYF+Cash (T2). Instead, we see differences in content, both when using text data from the first conversation (search tips are talked about more by mentor-student pairs in T2) and, most importantly, when using data on students' main takeaways (Figure 1.8). These findings point towards a cash transfer stimulating discussion on more actionable search tips. This ultimately crowds out overall encouragement, which was exactly the kind of support that proved useful on average in the medium run.[39]

1.6 Replicability and Cost Effectiveness

Among our most important goals in designing this intervention were replicability and cost effectiveness, given the interest expressed by involved VTIs as well as the BRAC Youth Empowerment Program. For this reason, the intervention is relatively easy and inexpensive to replicate. In its current form, the most challenging step of setting up a program similar to MYF is obtaining the contacts of alumni with two to five years of experience in the labor market, as VTIs are unaccustomed to tracking their alumni. However, once the program is set up, tracking methods are less costly. For instance, students might be systematically asked for updated contact information. VTIs can also make students aware of the mentorship program and enroll them in it prior to graduation. The algorithm proposed to select the mentors is easy to replicate, as it is based on accessible survey and administrative information. Once program administrators have selected the mentors and instructed schools on how to make random matches, the implementation of the intervention is straightforward.

[38] The pre-intervention MDEs we computed for the MYF+Cash treatment were satisfied on all outcomes. However, we expected MYF to have a larger effect than the cash transfer. We were less powered to identify the differential effect of the latter and had therefore pre-specified the pooled sample in advance

[39] Figure A.3.7 shows the dynamics of conversation content at three months and one year: on average, students in T2 received more search tips consistently across time. Earlier on, such search tips crowded out information on entry level conditions; later on, encouragement.

Moreover, institutionalizing the intervention at the school level will make its implementation easier. Indeed, the first interactions between students and mentors will be facilitated by the schools with no need for an enumerator to attend, further reducing the cost of this intervention, which is already relatively low. We estimated a per mentor cost of:$\sim$ \$5 for a half-day training (includes a snack, a face mask, a hand sanitizer, stationary, and a venue); $\sim$ \$15 for airtime (which is the equivalent of 70 hours of talking time) and a $\sim$ \$40 facilitation to thank them for their participation in the mentors' training, the mentor's check-in survey, and the mentoring sessions. Considering that a mentor is connected to an average of 3.9 students, the cost per student is $\sim$ \$15. The per student cost is relatively low and makes up a minute proportion of the fees paid for these programs (\$650 - \$800). Given that the institutions providing need-based scholarships already allocate large amounts of funding to pay for the training of these students, it would behoove them to invest this additional small amount, which is expected to amplify returns by a great deal and make a positive difference in employment outcomes. The costs discussed exclude the administrative costs. While the airtime and training costs are likely to stay the same, we foresee the facilitation being needed only for the first 2-3 years of the program. Once the mentorship program is institutionalized and students who benefited from it during the initial years are themselves asked to be ambassadors, we believe that the monetary compensation will not be needed or could be effectively reduced.

Table 1.8 presents the IRR calculations for all students, assuming a social discount rate of 5% and that the average treatment observed for the medium run income will linger for 15 years (i.e., the treatment will permanently shift subjects' monthly income by 6.15\$). To calculate the opportunity costs for mentors and students, we use the baseline income of students and mentors in May 2021. We overshot the amount of time dedicated to the program to two days. On average, participants dedicated 3.6 hours to the program. To be conservative in our estimates, we consider participating in the program to have demanded more time: the amount of engagement required for the program is therefore an upper bar. To avoid double counting, because mentors were monetarily compensated to make the first three calls, we consider only one day when computing the opportunity cost of mentors, which refers to the interactions on top of the three calls for which we have compensated them. We assume no employment displacement effects.

Panel A shows the per intended beneficiary cost breakdown. The total cost comprises: (1) students' opportunity cost, (2) mentors' opportunity cost for extra interaction, and (3) the program costs (which include the per capita cost for training, airtime, and compensation for mentors). Panel B shows the NPV of 15 years of earnings. The reason for the large benefits-cost ratio and IRR mainly lies in the intervention's small cost (23 dollars per participant). Even considering smaller durations for the medium run effects (10 or 5 years), the IRR remains at 300%. The returns remain positive even under more extreme assumptions and reach the minimum level of 9% only if we assume the maximum student and mentor's income to compute the opportunity costs. Nonetheless, it must be noted that this intervention is delivered to skilled workers who have undergone two years of vocational training. This is a much more expensive program to subsidize, although it likewise yields positive returns

(Alfonsi et al., 2020). We cannot ensure that the same effects and cost- benefit analysis would hold for unskilled workers. Our results show that similar programs can enable policy makers to enhance the effects of vocational training on earnings.

1.7 Conclusions

This chapter introduces a novel, tractable, and generalizable mentorship intervention, Meet Your Future, and assess its ability to boost early career trajectories. In the context of urban labor markets in Uganda, the second-youngest country in the world, we find that MYF improves employment outcomes and human capital complementarities between students' vocational education and sector of employment up to a year later. Mentored students are 27% less likely to have left the labor force three months after graduating from vocational institutes; they obtain their first jobs more quickly and are 33% more likely to utilize human capital complementarities acquired through vocational education. These accelerated first jobs last longer, permit the accumulation of human capital, and ultimately propel treated students up the career ladder faster. After one year, the earnings of treated students are 18% greater than those of the control group.

We attribute these returns to the effectiveness with which credible and approachable mentors communicated information about entry requirements and encouragement. Contrary to our expectations, neither direct job referrals nor the improvement of job seekers' search technology played a role. Students connected to experienced workers for personalized mentoring sessions become more realistic about their initial earnings and less pessimistic about wage growth opportunities and returns to experience. This shift in perception results in 32% lower reservations wages and a greater willingness to accept unpaid work. Indeed, they accept offers more quickly.

In conclusion, we demonstrate that a mentorship program able to provide credible and relevant information to young job seekers improves participants' employment outcomes, career trajectories, and education-career synergies by mitigating overoptimism regarding their initial employment prospects and providing hope for improved future outcomes. Our findings highlight the role of distorted beliefs as an important channel by which information frictions decrease earnings and career advancement. They also emphasize the importance of balancing 'bad news' with hope for better future outcomes in order to prevent *discouragement*, dropout from the labor force, and, particularly among skilled workers, human capital wastage. Finally, the program affordably increases the effectiveness of vocational training programs by a significant margin.

Main Tables

Table 1.1: Baseline Balance on Students Characteristics and Labor Market Outcomes

	Control		Treatment		
	N	Mean	N	Mean	P-value
Panel A: Socio-economic characteristics					
Age	466	19.87	645	19.84	.82
Gender (1=M)	466	.59	645	.60	.86
Christian	466	.83	645	.84	.64
Single	462	.90	642	.89	.33
Has chidren	466	.02	645	.02	.97
Region of origin: central	464	.30	643	.32	.39
Region of origin: eastern	464	.54	643	.51	.40
Region of origin: northern	464	.07	643	.08	.33
Region of origin: western	464	.10	643	.08	.40
Household asset index above mean	458	.42	643	.37	.11
Agricultural household of origin	464	.47	645	.47	.77
Panel B: Labor market history pre MYF					
Ever worked	466	.53	645	.53	.82
Ever worked in training sector	441	.07	614	.08	.39
Monthly earnings (USD)	441	20.62	614	19.49	.63
Has done any casual work	464	.26	645	.25	.75
Has done any wage employment	464	.29	645	.30	.74
Has done any self employment	464	.08	645	.09	.65

Notes: The table reports means and robust standard errors from OLS regressions in parentheses. P-value on T-test of equality of means with the control group in brackets. P-value on F-tests in braces. Data in Panel A is from the baseline survey of students. The following denominations are considered Christian: Anglican, Catholic, Born Again, Pentecostal, Seventh Day Adventist, Protestant, and Masiya. The following denominations are not considered Christian: Muslim, Jehovah's Witness, and Traditional/Tribal denominations. The household index is calculated based on 14 dummy variables regarding the ownership of 14 household assets (boda, car, electricity, computer, flush toilet, fridge, gas, internet, land, mobile phone, private latrine, radio, smartphone with access to internet, TV). The respondent's household of origin is considered agricultural if its main source of income is subsistence or commercial agriculture. Data in Panel B are from the baseline and midline 2 surveys to students, which we use to build updated measures of work experience accumulated before the roll-out of the MYF program. We classified as casual the following occupations: agricultural day labor; (un)loading trucks; transporting goods on bicycle; fetching water; land fencing; slashing someone's compound; and all occupations in which neither principal nor agent had an active working relationship, neither held any contractual obligations toward the other, and the principal requested agent on a need-based basis.

Table 1.2: ITT Estimates: Short Run Labor Market Outcomes

	Short Run				
	Out of the Labor Force (1)	Days Worked Last Month (2)	Hours Practicing Technical Skills (3)	Total Earnings Last Month (4)	First Job Duration (5)
MYF Treatment	-.057***	1.267**	17.234***	1.900	18.469***
	(.019)	(.540)	(5.041)	(2.081)	(5.150)
	[.003]	[.010]	[.002]	[.078]	[.002]
Control Mean	.21	16.15	52.15	11.35	81.18
Treatment Effect (%)	-26.57	7.85	33.05	16.73	22.75
N	934	934	838	933	833

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on primary employment outcomes. These are obtained by ordinary least squares (OLS) estimation of Equation 1.7. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses and q-values in brackets, obtained using the sharpened procedure of Benjamini et al. (2006). For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). In Column 1 the dependent variable is an indicator variable equal to 1 if individuals have not engaged in any work activity in the previous month and have not looked for a job in the previous month. These individuals are predominantly engaged in subsistence farming, casual occupation of sitting at home. In Column 2 the dependent variable is the total number of days worked in either wage- or self-employment in the last month, unconditional of employment status. In Column 3 the outcome variable is the number of hours spent applying newly acquired skills in the occupation of training in the 30 days preceding endline 1. The tasks may have been performed as part of the respondent's work activity, but also informally for a friend, family member, or themselves. To construct this variable, we designed an innovative survey module to track how much time the respondent spent performing each of a set of detailed typical trade-specific tasks a list we compiled by combining information from focus group discussions with the alumni and resources from the O*NET Program. In Column 4 the dependent variable is a measure of total monthly earnings in the main work activity (either a wage- or self-employment spell) in the month prior to the 3 month endline. Individuals reporting no wage employment earnings and no self-employment earnings are assigned a value of zero. The top 1% of earnings value are top-coded at the 99th percentile. All monetary variables are converted into February 2022 USD. In Column 5 the dependent variable is the duration in days of the first work spell after graduation.

Table 1.3: ITT Estimates: Labor Market Trajectory in the Medium Run

	Transitions		**Medium Run**	
	Internship to Job Transition Within Firm (1)	Internship to Job Transition Between Firms (2)	Out of the Labor Force (3)	Total Earnings Last Month (USD) (4)
MYF Treatment	.041**	.076**	-.025	6.149*
	(.019)	(.033)	(.022)	(3.601)
Control Mean	.18	.37	.26	34.84
Treatment Effect (%)	22.87	20.70	-9.46	17.65
N	934	934	916	916

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on match quality and labor market dynamics. These are obtained by ordinary least squares (OLS) estimation of Equation 1.7. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses and q-values in brackets, obtained using the sharpened procedure of Benjamini et al. (2006). For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). In Column 1 the dependent variable is an indicator variable equal to 1 if the respondent was retained after the internship (usually the students are hired as trainee in their first job after graduation). In Column 2 the dependent variable is an indicator equal to one if the respondent transitioned from being an intern/trainee (at three months) to being a worker not in training one year following graduation. In Column 3 the dependent variable is an indicator variable equal to 1 if individuals have not engaged in any work activity in the previous month and have not looked for a job in the previous month. In Column 4 the dependent variable is a measure of total monthly earnings in the main work activity (either a wage- or self-employment spell) in the month prior to the 1 year endline. Individuals reporting no wage employment earnings and no self-employment earnings are assigned a value of zero. The top 1% of earnings value are top-coded at the 99th percentile. All monetary variables are converted into February 2022 USD.

Table 1.4: ITT Estimates: Willingness to Accept a Job and Job Search Behavior

	Willingness to Accept a Job			**Job Search**			**Search Duration**
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Efficacy Index (4)	Search Intensity Index (5)	Started Job Search (6)	Search Duration | Searched (7)
MYF Treatment	-11.581***	.071**	-.057**	-.056	.018	.029**	-8.525**
	(3.357)	(.031)	(.026)	(.059)	(.068)	(.014)	(4.053)
	[.004]	[.052]	[.052]	[.128]	[.293]	[.052]	[.052]
Control Mean	36.76	.54	.21	.04	-.01	.93	28.28
Treatment Effect (%)	-31.50	13.09	-27.24	-157.94	-161.15	3.10	-30.14
N	737	739	745	934	934	934	885

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on willingness to accept a job and job search outcomes. These are obtained by ordinary least squares (OLS) estimation of Equation 1.7. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses and q-values in brackets, obtained using the sharpened procedure of Benjamini et al. (2006). For this table, we use data from baseline, the post-intervention survey and endline 1. For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). In Column 1 the dependent variable is based on a question about the lowest wage the respondent would be willing to accept. In Column 2 the dependent variable measures the willingness to accept an unpaid job as reported by the respondents. In Column 3 the dependent variable is an indicator variable equal to 1 if the respondent has ever rejected a job offer during their first job search spell after graduation. The variable is missing for those who have never searched for a job. The results are unchanged if we condition on having received a job offer. The Index of Search Efficacy in Column 4 is a standardized index of three components: (i) the ratio between the number of interviews and the number of applications; (ii) the ratio between the number of offers received and the number of applications submitted and (iii) the number of CVs dropped during search. This index is only available for students who looked for a job, not for those who tried to start a business as first activity. The Index of Search Intensity in Column 5 is a standardized index of four components: (i) hours per day spent searching/starting up a business; (ii) days per week spent searching/starting up a business (iii) total number of applications submitted and (iv) total savings devoted to job-search/starting up a business. For both indexes we follow Anderson (2008) and account for the covariance structure in the components. We normalize by the standard deviation of the index in the control group to ease interpretation. In Column 6 the dependent variable is an indicator variable equal to 1 if individuals have engaged in any job search following their graduation (and therefore, following the treatment roll-out). In Column 7 the dependent variable measured the length of the first job search spell after graduation, conditional on having started a search. The beginning of the spell is reported by the respondents. The end of the spell is either, the start of the first employment spell, the reported date on which the respondent stopped the search, or the first day of rollout of endline 1.

Table 1.5: Decomposition of the Effects of MYF on Pathways to Employment

	Unemp ↓ Unemp (1)	Unpaid ↓ Unemp (2)	Unpaid ↓ Paid (3)	Paid ↓ Unemp (4)	Paid ↓ Paid (5)
MYF Treatment	-.023	-.024	.056*	.005	.015
	(.016)	(.030)	(.032)	(.024)	(.029)
Control Mean	.07	.24	.26	.12	.22
T Effect (%)	-33.08	-9.84	21.52	3.85	6.89
N	844	844	844	844	844

Notes: This table shows reduced-form estimates of the effects of MYF on various pathways to employment in year 1. There are nine possible pathways, although we only report those with a minimum of 5% of the total number of students (the treatment effects on the pathways we do not report are not statistically different from zero). Each pathway is described by the combination of one of three possible labor market statuses: unemployed; working for a zero or negative wage; working for a positive wage, three months and one year after the intervention. For example, the pathway in column 1 is the sequence of unemployment=1 at three months and unemployment=1 at endline 2. Samples include all students interviewed at both endline 1 and endline 2. Robust standard errors in parentheses.

Table 1.6: 2SLS: Treatment Effects and Mentor Types

	Mechanisms		**Labor Market Outcomes**	
	Search Behavior Index (1)	Willingness to Accept Job Index (2)	Short Run Index (3)	Career Trajectory Index (4)
Entry Conditions	.02	.53***	.28**	.11
	(.12)	(.14)	(.11)	(.12)
Encouragement	-.05	.21**	.25***	.23***
	(.08)	(.10)	(.08)	(.09)
Search Tips	.02	.13	-.02	-.05
	(.11)	(.13)	(.11)	(.12)
Control Mean	-.01	-.18	-.13	-.09
N Mentors	158	158	158	157
N	934	669	933	833
F-Test of joint significance (pval)	0.90	0.00	0.00	0.04
AP Partial F (pval)- Info	.00	.00	.00	.00
AP Partial F (pval)- Encouragement	.00	.00	.00	.00
AP Partial F (pval)- Search Tips	.00	.00	.00	.00
Sargan (pval)	.82	.45	.08	.10

Table 1.7: ITT Estimates: at 3 Months and 1 Year by Treatment Arm

	Transitions		**Medium Run**	
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force at 1 Year (3)	Total Earnings Last Month at 1 Year (4)
T1 (MYF)	.06**	.11**	-.06*	10.84**
	(.02)	(.04)	(.03)	(4.19)
T2 (MYF+Cash)	.02	.01	.01	1.95
	(.03)	(.04)	(.03)	(3.80)
Control Mean	.18	.41	.26	34.84
T1 Effect (%)	32.69	27.38	-22.77	31.10
T2 Effect (%)	13.57	3.10	2.45	5.61
N	934	844	916	916
T1=T2	0.28	0.08	0.12	0.02

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF and the MYF + Cash interventions separately. We do so for the four outcomes for which there are significantly different treatment effects. Below each coefficient estimate, we report the strata-level clustered standard errors. For each outcome, we report the mean outcome for the control group and each treatment effect. At the foot of each column, we also report the P-Value from an F-test of the null hypothesis that the impact of MYF alone is equal to the impact of MYF + Cash. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). For a detailed description of the outcomes, please refer to Table 2, 3 and 4.

Table 1.8: IRR

	All students
Social discount rate	0.05
Remaining expected productive life	15 years
Panel A. External parameters	
Total cost per individual	23.42
· Student opportunity cost (2 days of work)	4.99
· Alum opportunity cost (1 days of work, ext. interaction only)	3.43
· Program costs	15.00
Panel B. Estimated earning benefits	
Extra-earnings in for each month	6.06
NPV change in steady state earnings (from model estimates)	731.64
Benefits/cost ratio	32.24
IRR	3.00
Panel C. Sensitivity	
Sensitivity to different expected remaining productive life of beneficiaries	
Remaining expected productive life = 10 years	3.00
Remaining expected productive life = 5 years	3.00
Sensitivity to different earnings	
Opportunity costs = 90th percentile (9.26+7.29)	2.20
Opportunity costs = double 90th percentile (18.52+14.57)	1.40
Opportunity costs = max (185.19+27.32)	0.25
Opportunity costs = double max (370.37+54.64)	0.09
Sensitivity to different engagements	
5 days of work foregone	1.60
7 days of work foregone	1.20

Main Figures

Figure 1.1: Project Timeline

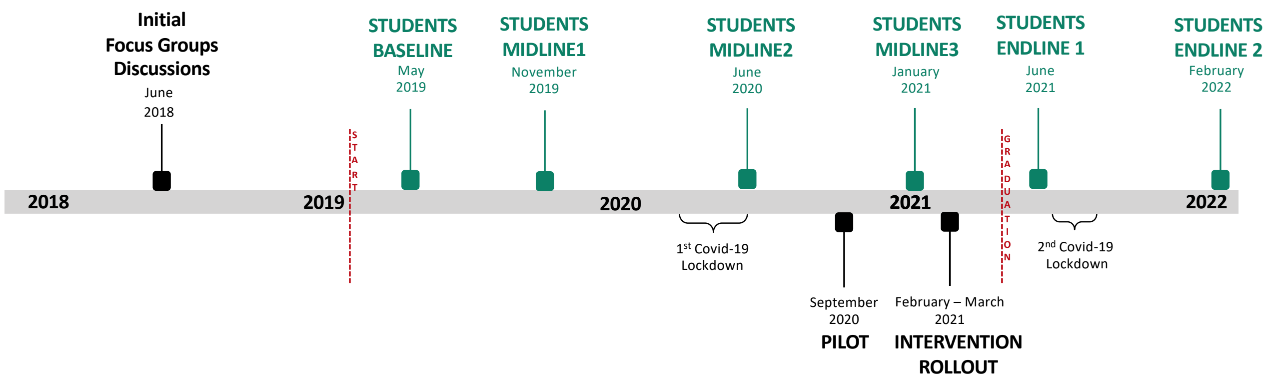


Figure 1.2: Experimental Design

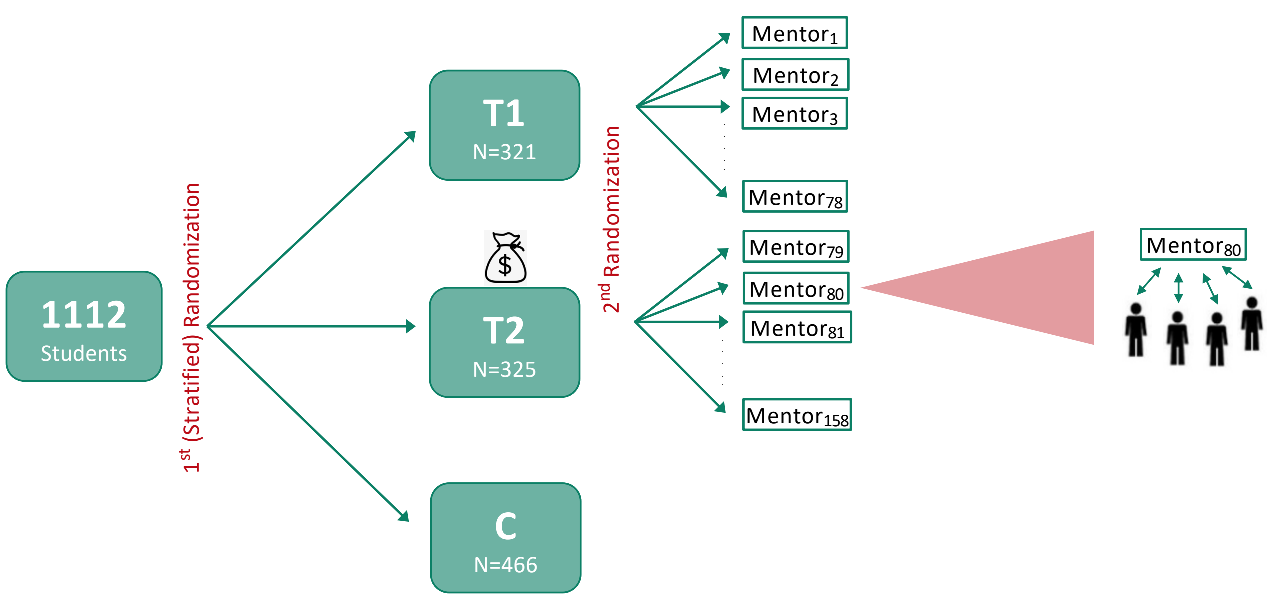
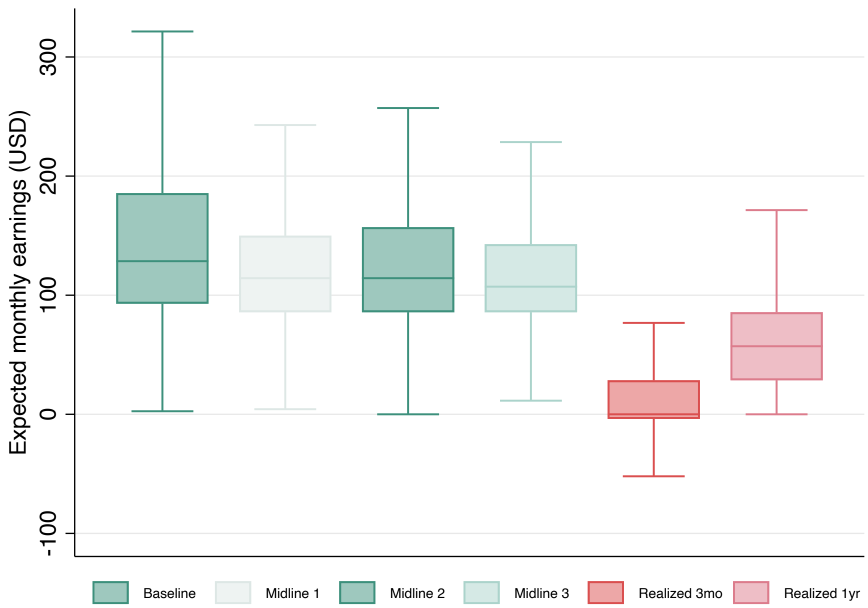


Figure 1.3: Overoptimism

Panel A: Expected and Actual Monthly Earnings | Employment



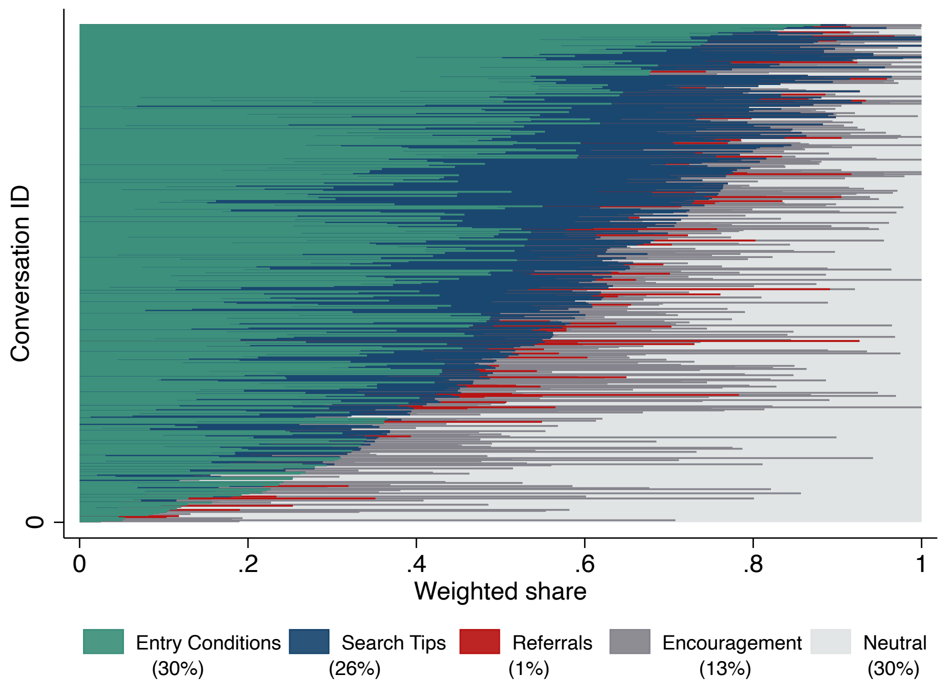
Panel B: Expected and Actual Job Ladders

1 YEAR \ 3 MONTHS	Expected: Paid	Expected: Unpaid	Expected: Unemp	Actual: Paid	Actual: Unpaid	Actual: Unemp
Paid	52%	48%	42%	61%	55%	15%
Unpaid	22%	25%	33%	3%	6%	3%
Unemp	26%	28%	25%	36%	39%	82%

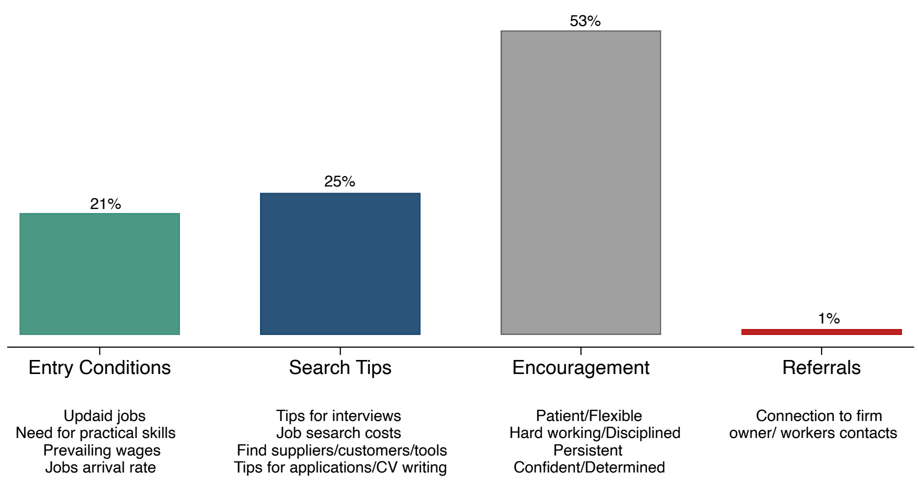
Notes: Panel A shows expected and realized conditional monthly earnings in the control group. In the first four box-and-whisker plots, we plot students' expected monthly earnings at their first job in all four pre-intervention data points. The fifth and sixth plots represent students' actual monthly earnings at their first job as well as at one year, conditional on employment. For this figure, the data comes from the control group exclusively. Each plot shows the 10th, 25th, 50th, 75th, and 90th percentiles of actual/expected earnings distributions. The expected monthly earnings are calculated by taking the reported likelihood thet earnings are above the midpoint of the minimum and maximum, and then fitting a triangular distribution. In Panel B, we report the expected and actual transition matrix from the three-month employment status to the employment status at one year. The unpaid category comprises of workers paying for work (negative wage). The matrix on the left contains information about the *expected* transition shares. Expectations on the transition matrix are not available for the original sample. A similar sample of 55 first and second-year students from a later cohort was surveyed to elicit these expectations. The one on the right contains the *actual* shares as computed in our control group.

Figure 1.4: Conversations Content and Takeaways

Panel A: Coded Conversation Content From Audio Recordings



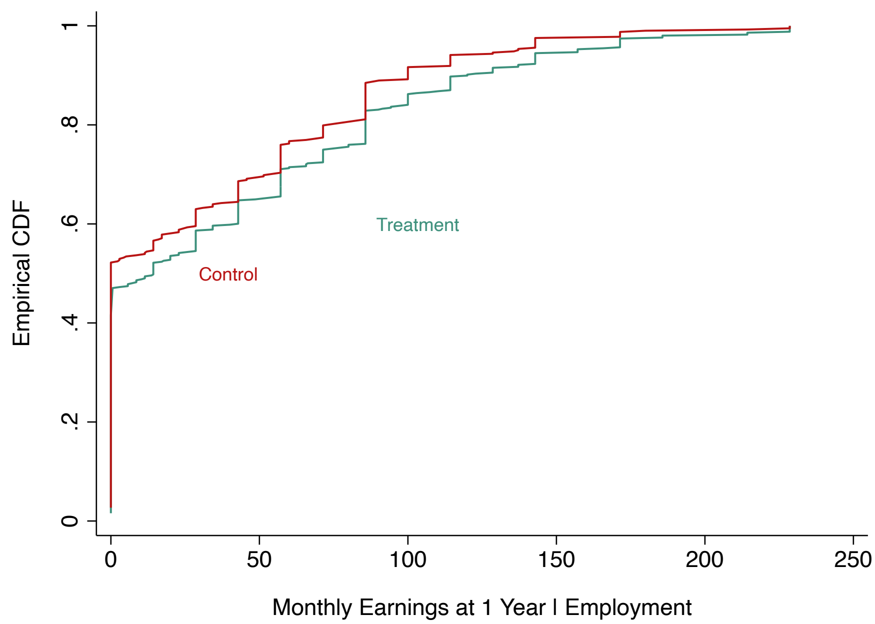
Panel B: Students Main Takeaway



Notes: This figure shows the distribution of the main takeaways students withheld following their conversations with the mentor. We identified three macro-categories of support that mentors can provide to the students: information on entry conditions; search capital; and encouragement. Each bar represents the share of students who reported as their main takeaway something that falls into each macro-category. Below each bar, the most recurrent micro-topic selected by the students is listed.

Figure 1.5: Quantile Treatment Effects on Monthly Earnings at 1 Year

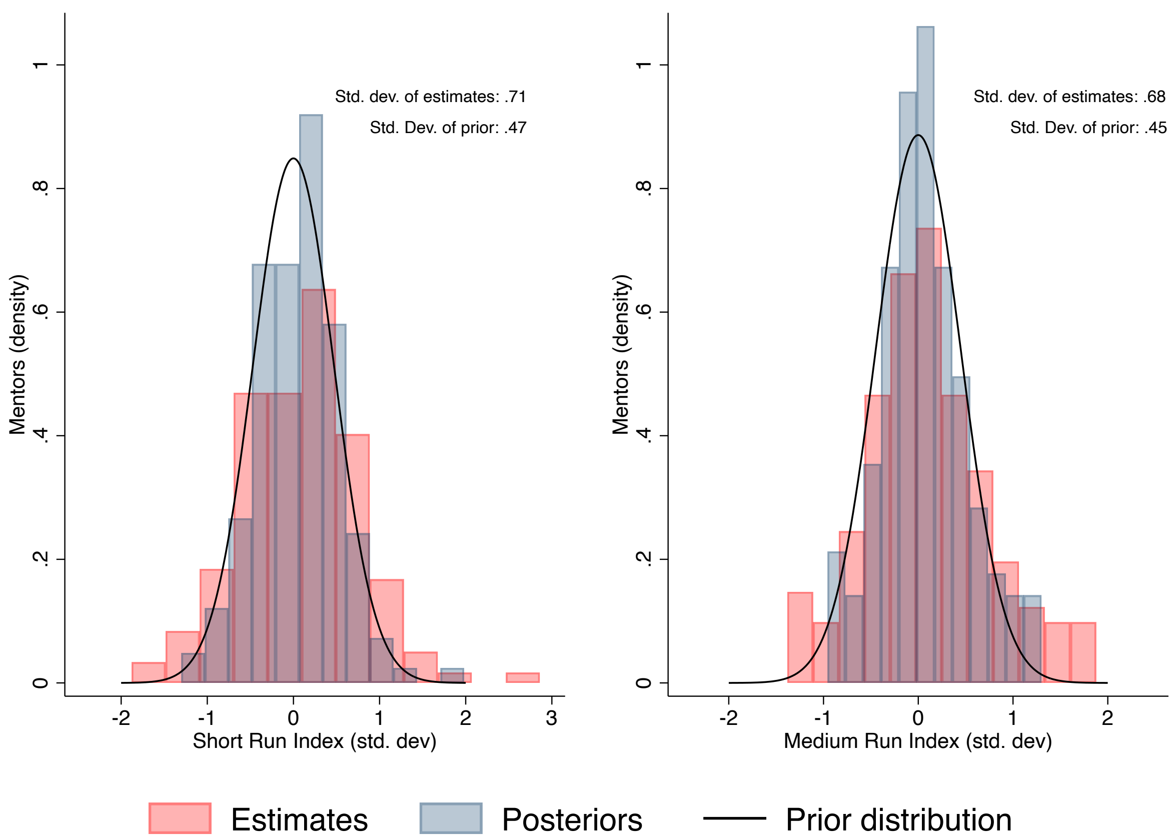
Panel A: Empirical Distributions of Monthly Earnings in Treatment and Control



Panel B: Quantile Treatment Effects of MYF on Monthly Earnings

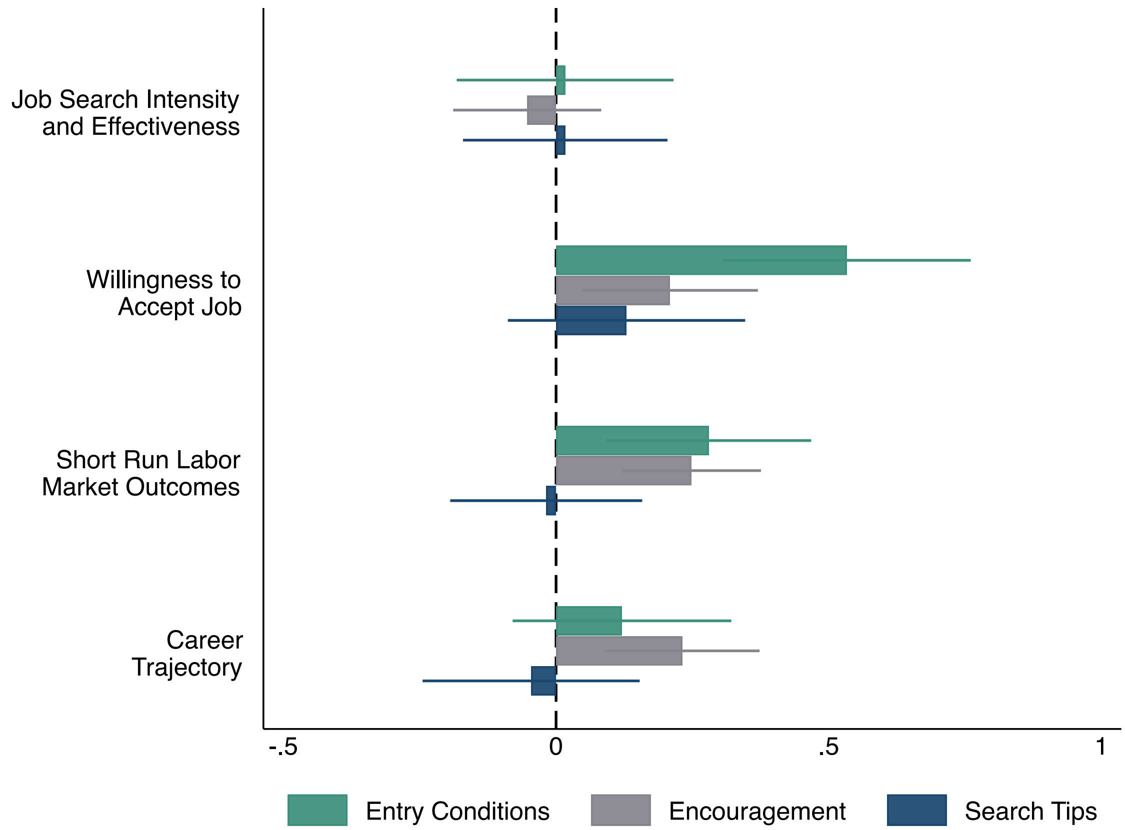
Notes: Panel A shows the empirical distributions of monthly earnings in the MYF treatment and control groups. Earnings are converted into February 2022 USD. Earnings are coded as zero for candidates who were not engaged in any work activity in the month prior to the survey. Earnings below the 42nd percentile are zero. Panel B shows the quantile treatment effects (QTEs) of the MYF treatment on monthly earnings. These are quantile regression estimates of treatment effects on total earnings in the month prior to the survey, with 90% confidence intervals estimated without controlling for any covariates or stratum fixed effects. The sample includes all students from endline 2.

Figure 1.6: Reduced Form Estimates: Biased and Unbiased Mentors Fixed Effects



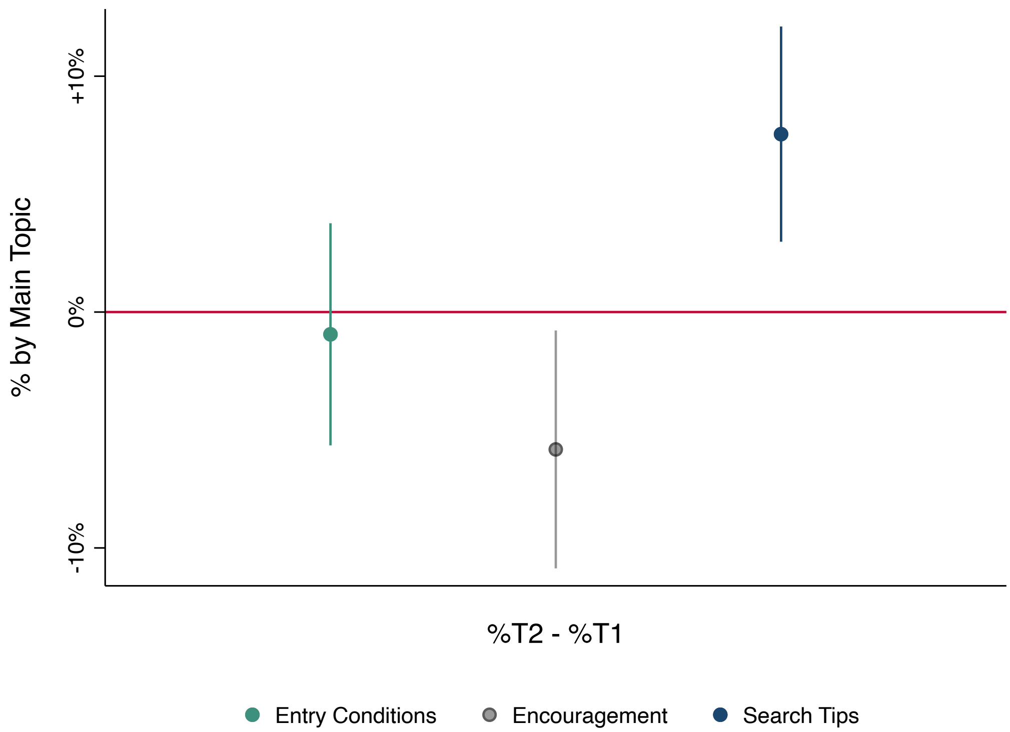
Notes: In this figure we report the biased (estimates) and unbiased (shrunk posteriors) distributions of the mentors fixed effects. We overlay the prior distribution, a normal centered on zero, with the bias-corrected standard deviation.

Figure 1.7: 2SLS: Type of Support Provided and Labor Market Outcomes



Notes: In this figure we report 2SLS regression estimates from equation 1.11. The 158 mentor dummies are used as instruments.

Figure 1.8: Conversation Content by Treatment Arm



Notes: In this figure we report the difference and confidence intervals in shares of conversations by main topic for students in MYF only and students in MYF + Cash (T2). The conversation shares are individual level averages over three conversations: the first conversation (MS1), the last conversation prior to endline 1 and the last conversation prior to endline 2.

Chapter 2

Whom Would You Rather Work With? An Experiment on Gender Discrimination in the Referral System

This chapter is coauthored with Pedro de Souza Ferreira.

2.1 Introduction

Personal connections are an important search and matching channel across different labor markets, formal and informal, in both high- and low-income settings. In the US, about 70% of firms encourage hiring based on referrals and at least half of all jobs are found through informal contacts rather than formal job search (Topa, 2011; Burks et al., 2015). Employee referrals can help firms remedy problems of asymmetric information, providing information on unobservable characteristics to maneuver adverse selection (Rees, 1966; Montgomery, 1991; Kono, 2006) and allowing employers to exploit social networks to tackle moral hazard (Heath, 2018).

Despite improving matching efficiency, the system of employee referral risks penalizing minority groups and reinforcing labor market segregation. On one hand, stereotypes and the threat of workplace harassment limit the labor supply and lead to self-perpetrated segregation. On the other hand, on the demand side, firm owners of businesses in male dominated sectors are, by definition, more likely to be male. The affinity bias – i.e. the tendency to warm up to people like yourself – is likely to perpetuate gender segregation (Milkman et al., 2015). In informal labor markets, where job network is key for landing a job through referrals, biases among employees, and not only firm owners or HR departments, act as an additional barrier to gender equality in access to certain occupations (Beaman et al., 2018).

Employees tend to refer network members with similar characteristics, including gender (Brown et al., 2016). In particular, women are less likely to use informal contacts than men, their contacts tend to be more clustered in certain occupations, and, for them, similar levels of network usage yield lower wages and promotion chances than for men (Topa, 2011). In an experimental setting, Beaman et al. (2018) show that male candidates applying for a job position in a gender-neutral industry are less likely to refer female candidates despite having qualified women in their networks. Additionally, referring decisions might reflect workers' extrinsic preferences, like employers' and consumers' discriminatory behavior, reinforcing existing mechanisms of labor market segregation (Becker, 1957; Bar and Zussman, 2017).

We implemented a correspondence study to investigate the existence of gender discrimination in industries with varying levels of gender segregation and to understand the extent to which such bias is driven by workers' intrinsic preferences and perceptions or by pass-through from employers. In particular, we designed an an incentivized resume rating (IRR) following Kessler et al. (2019). Differently from previous audit studies that submit fake profiles to job openings (handled by either HR departments or firm owners), we investigate the existence of gender biases among employees, the channel through which firm owners often receive candidate referrals. We work in three Ugandan urban labor markets, across a wide range of manufacturing and service jobs which employ 30% of young Ugandans not involved in agriculture. In our setting, labor markets are strongly segregated by gender and the worker-firm matching process is largely informal, with personal connections playing a pivotal role. We show pairs of gender-differing profiles of potential candidates to a sample of 555 successful and educated workers in different industries in Uganda, randomizing the gender of the high-experience candidate. We then ask them to rate the profiles in terms of likability and perceived probability of retention and to choose whom they would refer to a subsidized internship in the company in which they work. With this approach, we can understand how gender discrimination interacts with the sector, the gender, and other features of the respondent when it comes to referring a potential coworker, all while mitigating issues related to the endogeneity of network formation. To validate our findings in such anonymous setting, we introduce the possibility to use their networks as referral choice sets. To gain insight on the drivers of discrimination and, in particular, on the role of pass-through discrimination from employers' preferences, we further cross-randomized the publicity of the referral (whether the employer would know or not the name of the referring employee).

We find that discrimination against the non-stereotypical candidate exists in both male and female-dominated sectors. However, in female-dominated sectors, it is smaller and driven only by likability, whereas in male-dominated sectors, it is stronger and driven by both likability and perceived probability of retention. The smaller perceived probability of retention could reflect both statistical discrimination from workers and pass-through from employers' preferences. In our overall sample of skilled workers, female profiles are 11.1 p.p. less likely to be picked for the internship. Using an alternative specification, this effect is similar in size to having 1.7 fewer months of work experience than the competing candidate, which is sizable considering the low experience usually required for internships. When subjects resort to their own networks, similar preferences are observed even in the lack of differences in availability of a person of the non-stereotypical gender.

We also find that, in male-dominated sectors, the relationship between workers and employers plays an important role in determining the extent of gender discrimination in male-dominated sectors. Making the referrals private made subjects in such sectors 10 p.p. more likely to refer the female candidate rather than the male candidate (a 30% increase). It could be that respondents in male-dominated sectors simply refer more women because the costs of referring a candidate perceived as worse are lower (Beaman and Magruder, 2012), but they are no more likely to refer the low-experience candidate, indicating that relationship with employers is an important driver of pro-male bias in such sectors. The fact that, keeping quality constant, women are ranked as less likely to be retained in male-dominated sectors corroborates this result.

This paper contributes to the thin literature on gender discrimination in referrals by showing that discrimination goes both ways depending on the gender dominance of the segregated sector, but workers in male and female-dominated sectors do so at different strength and for different reasons. Beaman et al. (2018) points out the existence of a pro-male bias in a non-segregated industry. We expand their contribution in two ways, all while mitigating endogeneity in network formation. First, we study gender discrimination in referrals in sectors with varying levels of segregation. Second, we can explore the underpinning channels and discuss the role of extrinsic preferences in referrals. We also uncover that beliefs over employers' preferences plays an important role in referring decisions in male-dominated sectors, aggravating pro-male bias in referrals. With this, we expand the literature on pass-through discrimination, which has already called attention to the pass-through from consumers' preferences onto employers' hiring decisions (Bar and Zussman, 2017). Finally, we contribute to the literature of audit studies and IRR being the first study to investigate gender discrimination in referrals (Hedegaard and Tyran, 2018) and the first to investigate gender discrimination in Africa. Additionally, to the best of our knowledge, all correspondence studies disregard the gender of the person evaluating the profile and how this could interact with the assessment of candidates' profiles.

2.2 Data and Sample

The sample of our study comprises 555 skilled young workers from Central and Eastern Uganda, who the research team has been following since 2020 as part of a different project (Alfonsi et al., 2022). These young adults concluded their post-secondary education at a vocational training institute (VTI) between 2014 and 2019 and hold a National Certificate across a wide array of manufacturing and service jobs. Table 2.1 displays the baseline summary statistics of the sample [1]. The 555 respondents are, on average, 27 years old in 2021, 61% are men, 36% are married, 52% grew up in a rural area, 41% are wage-employed, and, at baseline (2020), had on average 2.8 tenure years active on the labor market. Among those that were wage-employed at baseline, 60.9% found their first job through a personal

[1] The survey, part of a different study, targeted 711 young adults. We successfully surveyed 555 of them. The 150 who attrited (22%) are no different from our respondents, except for gender and belonging to ethnic minority (Table B.3.3, in appendix). However, this is inconsequential for this study and its internal validity since the outcomes of our experiment are measured on the spot.

connection and, in particular, 28.8% found it through a referral from an incumbent employee working in the company of first job (Figure B.3.1).

Our sample is unique for it represents an expression of the country's emerging urban working class. Tables B.3.1 and B.3.2 in the appendix compare summary statistics of our sample with the Ugandan National Household Survey (UNHS). On top of being more educated, our subjects earn 44% more than the average Ugandan population of young workers. Even compared to the sub-sample of Ugandans that attended a VTI, they are more likely to perform non-agriculture occupations, which are more productive (81% against 46% in the overall population and 75% in the population that attended a VTI). Given the crucial role skilled urban workers in low-income settings hold, identifying gender biases in such selected sample builds up evidence on the mechanisms that keep women clustered in the least productive sectors.

Women in our sample are also more economically empowered than the Ugandan average. In the national sample of young workers, 72% of women performed any work (which includes agriculture), but most of them are clustered within agriculture: only 36% performed non-agriculture occupations. On the other hand, our female respondents are as likely to be working as the national average (75%) but they are significantly more likely to work outside agriculture, as 68% reported having worked outside agriculture. Nonetheless, despite being more empowered than the national average, female subjects are still less likely to be employed than male subjects and earn significantly less.

2.3 Experimental Design

Audit studies emerged in the 1960s in the United Kingdom and in the United States parallel to the enactment of anti-discrimination bills and out of the concern of tackling concealed forms of racial discrimination (Gaddis, 2018). Since then, they have become a workhorse in studies of discrimination for their ability to avoid common drawbacks present in alternative techniques, like surveys and regression analyses[2]. Traditionally, audit studies come in two different forms: in-person and correspondence. In in-person audit studies, researchers rely on trained assistants to role-play characters in the field. In correspondence audit studies (also called "resume audit studies"), researchers create hypothetical profiles or applications and present them to subjects through mail or internet. As online applications became more popular in the 2000s, resume audit studies have become the most common type of audit studies and are popular in high-income settings. In the literature of labor economics, they have been widely used to assess employers' preferences and discriminatory behavior on

[2]Surveys that collect self-reported measures of discrimination suffer from experimental demand and fail to generate honest responses. Observational studies based on regression analysis usually confound discrimination with observable and unobservable characteristics (confounders that might differ on the dimension of discrimination being assessed, like level of education and wealth).

various dimensions[3]. In this paper, we focus on gender discrimination in hirings.

To the best of our knowledge, Firth (1982) was the first author to carry out an audit study to investigate gender discrimination in hirings, sending application letters with different gender to job advertisement published in newspapers, and found that men were more likely to get through job screening. Ever since then, many audit studies have investigated the extent of gender discrimination in hiring decisions from employers and HR departments. Some studies pointed out to the existence of gender bias in line with the gender division of sectors in the UK (Riach and Rich, 2006), in Australia (Booth and Leigh, 2010), and in Sweden (Carlsson, 2011). Others also showed that, even if men and women are both discriminated in certain aspects, women tend to be discriminated more (Arceo-Gomez and Campos-Vazquez, 2014; Galarza and Yamada, 2017; Neumark et al., 2019; Campos-Vazquez and Gonzalez, 2020).

Nonetheless, despite their strengths, Kessler et al. (2019) have raised three major concerns regarding audit studies. First, the identification strategy fundamentally relies on the deception of subjects that evaluate the individuals sent by the researcher. Even though institutional review boards usually tolerate the deception involved in audit studies, it is costly for subjects, who waste time pursuing fake profiles, and can also harm real applicants. In the case of labor market audit studies, fake profiles can displace callbacks that would have been otherwise assigned to real jobseekers, who lose the opportunity of being interviewed. Second, in the labor economics literature, audit studies generally fail to disentangle employers' preferences and expectations about the candidate accepting the job. Farber et al. (2019), for instance, did a resume audit study in eight cities in the United States through online applications and found that applicants that were unemployed had higher call-back rates than employed applicants. This, however, does not mean that such employers had a preference for unemployment, but rather that they believed such candidates were more willing to accept a job offer. Third, traditional resume audit studies that rely exclusively on call-back rates lack granular measures of employers' preferences, which prevents them from capturing responses at other points of the distribution of candidate quality.

To maneuver these issues, Kessler et al. (2019) proposed a new methodology, the Incentivized Resume Rating (IRR). Under this new methodology, respondents are shown hypothetical profiles and are asked to rate them on (1) how much they would like to hire the profile and (2) how likely they think the profile is to accept a job with their organization, on a 10-point Likert scale. To make their answers incentive compatible, we told respondents

[3]Race (Bertrand and Mullainathan, 2004; Arceo-Gomez and Campos-Vazquez, 2014; Edo et al., 2019), skin tone (Saeed et al., 2019), castes Banerjee et al. (2009), criminal record (Agan and Starr, 2018), immigration status (Oreopoulos, 2011), sexuality (Weichselbaumer, 2003; Drydakis, 2021), age (Lahey, 2008; Carlsson and Eriksson, 2019), obesity (Rooth, 2009), type of post-secondary institution attended (Deming et al., 2016), and length of unemployment spells (Eriksson and Rooth, 2014). In recent works outside the literature of labor, they have also been used to identify discriminatory behavior based on race among landlords in Airbnb (Edelman et al., 2017) and among Uber drivers (Ge et al., 2020) and to identify price discrimination towards non-coethnics in informal markets (Grossman and Honig, 2017) and towards foreigners Kim and Lopez de Leon (2019).

that their answers would be used to match them to real life candidates according to their preferences. With this approach, we are able to minimize deception and also address the problem of the confounder by disentangling preferences from perceived likability of accepting the offer.

Currently, researchers are progressively replacing traditional audit studies with IRR[4]. Macchi (2020) found that, in Uganda, obesity can act as collateral by signaling wealth in credit markets where there is lack of information. Carranza et al. (2020) investigated how information on job seekers' skills affect hiring decisions and their labor market outcomes in South Africa.

While significantly advancing the potential of audit studies with the proposed solutions, the IRR of Kessler et al. (2019) still leaves a few open points. First, if their subjects believed that only their answer to the question "how much would you like to hire this person" would be taken into account to match them to the ten real CVs but still feared that the profiles shown were not likely to accept the offer, they could still answer this question by embodying both the perception around the quality and the perception about the perceived likelihood of accepting the offer. Also, the lack of comparability between the scales of perceived quality and call-back decisions hinders the interpretation of their result and how they would translate to real life outcomes. In other words, the lack of a question along the lines of "would you hire this candidate?" at the end of the evaluation of each profile prevents the authors to draw a link between the scale of perceived quality (how much would you like to hire) and the actual hiring decision.

When designing our correspondence experiment, we depart from Kessler et al. (2019). Specifically, we adapted their design, which was originally aimed at employers and HR employees, so to suit our focus on employees' referrals. We also attempted to overcome the above mentioned shortfalls by asking employees who they would pick (not only their ratings) and by ensuring our respondents that candidates would certainly accept the job, so to shut down any potential confounders stemming from the perceived probability of acceptance. In our experiment, we offer skilled young workers the opportunity to refer someone for a one-month subsidized internship at the company they work for. The experiment was carried out in the second half of 2021. In the first part of the experiment, we showed to each respondent a pair of gender-differing profiles that matched their sector of specialization but that differed in amount of work experience (6 or 11 months of work experience). Following the work of Kessler et al. (2019), respondents were told that the profiles were hypothetical but based on real-life candidates willing to accept the job. The details of how the hypothetical profiles were built are described in Appendix B.1.

The experiment targeted employees in SMEs. We offered them the opportunity to refer someone to start working as an intern at the company in which they worked. The referred candidate took part in a lottery and, if selected, started working. To increase the stakes of the respondent and to nudge them into considering consider the quality of the candidates, we offered a 100,000 UGX (30 USD) unconditional cash transfer in case the referred candidate

[4]Due to the recency of the work of Kessler et al. (2019), most of works using IRR are still unpublished.

was retained in the company after the one-month internship. To increase power, we also allowed non-wage employed respondents to take part of the experiment, though only in hypothetical terms. These respondents were told that the research team was interested in leveraging their expertise in their sector of specialization and that their answers would help us deliver a set of one-month subsidized internships. They were not offered any amount of money and were not told about any lottery.

After sending each profile through SMS, respondents were asked to rate it in terms of likability and quality, on Likert scales from zero to 10. The timing of the experiment is detailed in Figure 2.1. Specifically, we borrowed questions from Heilman et al. (2004) and asked "How much would you like to work with this candidate?" to measure likability and "What are the chances of this candidate being retained?" to measure quality. We use the question on likelihood of being retained to capture the subject's perception about the quality of the hypothetical profiles. Certainly, the answer to this question may capture characteristics other than candidates' actual competence: on one hand, respondents may weigh the performance of the business where the respondent works and the probability that they are able to retain a new employee (which, in any case, is expected to be the same in both treatment groups), on the other, they may embody the preferences of employers, consumers, or suppliers (pass-through). Conversely, the measure of likability was designed to capture respondents' personal preferences. In our sample, the businesses in which our respondents work have a median size of 3 employees (17 employees, for the subsample of wage-employed). Therefore, they are aware that the new intern has high chances of working with them directly and shall take this into account when rating the candidate in terms of likability. After rating both participants, respondents were required to pick one of the two candidates for the internship.

In the second part of the study, after rating and choosing among the hypothetical profiles, respondents were asked to mention a person they knew (a network member) that would be a good fit for the position we were offering. Then, we asked them to mention someone of the opposite gender of the firstly mentioned network member[5]. After naming each network member, respondents were also asked to rate them in terms of likability and competence. Finally, we asked them whom would they refer to the subsidized internship among the two network members and the two hypothetical profiles. The second part helped us identify to what extent the gender bias observed in the first part is also observed in a setting where respondents have to resort to their actual networks and to what extent the bias is due to the lack of supply of network members of the non-stereotypical group.

In designing the experiment, we addressed the main issues related to audit studies, following the IRR approach proposed by Kessler et al. (2019). First, we further minimize deception by telling respondents that the profiles we show are hypothetical though based in real life candidates[6]. Second, we address the confounding between subjects' interests and their expectations about the candidate shown accepting the job. If subjects believe a

[5]The respondent always had the possibility to say that they did not know anybody in their network that was a good fit.

[6]We still held some deception by making subjects believe that only names had been changed and that

high-experience candidate is not likely to accept the job, they may prefer the low-experience candidate even if they think the other one is more fit. To shut down this channel, we tell all subjects that the candidates underlying the hypothetical profiles are willing to accept a job if offered one. We also tailor our rating questions to embed measures of likability and quality, which are supposed to disentangle the existence of taste-based discrimination and statistical discrimination.

To gain insights on the drivers of discrimination, we performed two cross-randomizations: by gender of the high-quality candidate, to investigate the extent of gender bias, and by privacy of the referral, to disentangle the role of pass-through discrimination from employers' preferences. The two cross randomizations are described in details in sub-sections 3.1 and 3.2 respectively. We hereafter refer to the first one as Main Experiment and the second one as Cross-Randomization. The randomizations were stratified by gender, sector gender dominance (male/female dominated sector)[7], wage employed in baseline of Alfonsi et al. (2022a), and hard to find dummy (which takes value one if the respondent was not found in at least one of the previous three survey rounds).

2.3.1 Main Experiment: Identifying gender bias

We start by looking at whether skilled young workers discriminate by gender when making referrals and if any gender bias occurs on the line of gender sector segregation, as has been found in the literature for developed economies (Riach and Rich, 2006; Booth and Leigh, 2010; Carlsson, 2011). To do so, we randomize the gender of the high-experience profile, splitting the sample into a high experience male group (HEM), who was shown the profiles of the high-experience candidate bearing a male name and the low-experience candidate bearing a female name, and a high experience female group (HEF), who was shown the profiles of the high-experience candidate bearing a female name and the low-experience candidate bearing a male name. Our hypothesis is that gender bias in referrals against female workers exists and, hence, in the HEF, the high-experience candidate will have less chances of being referred than in the HEM. Therefore, our primary outcomes are: probability of referring the high quality candidate, likability of high- and low-experience candidate, and perceived probability of retention of high- and low-experience candidate.

all other characteristics of the profiles (namely gender, education, and work experience) were correct. In any case, the experiment is respectful to subjects' time. In particular, they had the chance to be compensated with money for their choices, by having the chosen candidate starting working in their company and by offering an unconditional cash-transfer reward if the chosen candidate is retained. Concerned with the issues of reputation costs (Rees, 1966) and moral hazard (Heath, 2018) described in the literature, we also make sure subjects are aware of risks and allow them to give up on having their chosen candidate actually starting working with them at any point in the study. If selected in the lottery, we also make sure to disclose information of the matched candidate to the subject, under the matched candidate's consent, before the subject decides to proceed with the candidate working in their company.

[7]Male dominated sectors are: motor-mechanics, plumbing, construction, electrical work, welding, carpentry, agriculture, and machine and fitting. Female-dominated sectors are: food and hospitality, tailoring, hairdressing, teaching, and secretary.

Table 2.1 displays the balance in observable demographic and work characteristics on the non-attrited sample between HEM and HEF. The sample is balanced across all observables, and we can confirm that the assumption of random assignment holds.

2.3.2 Cross-Randomization: Private referrals

Second, we intended to understand whether private rather than public referrals could mitigate the costs of referring female candidates and low-quality candidates. Stratifying by the same strata as in the Main Experiment plus group assignment in the Main Experiment, we cross-randomized the publicity of the referral (whether the business owner would or would not know the name of the respondent who referred the candidate), splitting the sample into public referral and private referral. We expected private referrals to help workers maneuver punishment coming from prejudiced employers against minorities, and to mitigate pass-through discrimination (Becker, 1957; Bar and Zussman, 2017). We also expected that private referrals would change the trade-off that workers face between social benefits and candidate quality when making a referral, as in the model of Beaman and Magruder (2012). In particular, if workers expect employers not to know which worker referred the candidate, they might weigh more the social benefit than the candidate quality, as reputation costs for lower quality candidates would be decreased (Rees, 1966). Therefore, our hypothesis is that private referrals can increase the probability of referring female candidates and the probability of referring low-quality candidates and network members. In this experiment, our primary outcomes are: probability of referring the female hypothetical candidate, probability of referring low quality candidate, likability of male and female candidate, perceived probability of retention of male and female candidate, probability of naming a network member, probability of naming a female network member, probability of referring a network member, probability of referring a female network member, and probability of referring a woman (hypothetical or network).

Tables 2.1 displays the balance in observable demographic and work characteristics on the non-attrited sample between treatment groups. For this experiment, we found some unbalances. By chance, subjects assigned to private referral are younger at the 5 percent level (26.7 years against 27.4 years from subjects assigned to public referral), have less years of activity in the labor market at the 10 percent level (2.60 years against 2.96 years), are more likely to have studied motor-mechanics at the 10 percent level (22% against 17%), and less likely to have studied hairdressing (2% against 4%). There are also unbalances in training at the 10 percent level for carpentry and machine and fitting, but this is not concerning because these are minority in the sample (there are only 3 subjects who did carpentry and 3 who did machine and fitting). However, the unbalance in motor-mechanics and hairdressing is concerning because these are non-trivial shares of our sample (19.5% and 3.1%, respectively) and, despite the stratification in gender dominance, can make the treatment group more prone to have a pro-male gender bias, affecting directly one of our results of interest. Following Bruhn and McKenzie (2009), we control for such four unbalanced variables in regressions estimating the treatment effect of private referrals, expecting that "the remaining unobservables are no more or less likely to be unbalanced". For all these variables, the normalized difference does not exceed the one-quarter standard deviation rule-of-thumb proposed by Imbens and Rubin (2015), under which simple regression models are reliable to remove the biases stemming from imbalance.

2.4 Empirical strategy

In this section, we describe the empirical strategy to identify the existence of gender bias in the setting with gender-differing pairs. We look at subjects' probabilities of picking the high-quality candidate, mimicking traditional audit studies that look at call-back rates. While able to identify the gender bias, this empirical strategy (hereafter called "main identification strategy") does not allow the identification of the quality effect, as the differences in quality are constant in both groups and, as discussed above, account for the difference in 5 months of work experience. To have a benchmark to which compare the size of the gender bias identified, we provide a second empirical strategy ("alternative identification strategy") which allows us to estimate the quality effect, though not causally. The alternative identification strategy, described in Appendix B.2, regards only the primary outcome "probability of referring hypothetical high quality candidate" for the Main Experiment. For all other primary outcomes, we rely solely on the main identification strategy.

Similar to the literature of audit studies that look at call-backs, we elicit the existence of gender preferences among respondents by comparing the referral probability of the high-experience profiles P_h^{refer} in the group where the high-experience profile is a man with the group where the high-experience profile is a woman. If both profiles shown were the same, P^{refer} would be equal to tossing a coin, $E(P^{\text{refer}}) = 0.5$.

If one of the profiles shown has more experience, the probability of referring such profile is expected to be affected by the experience effect:

$$E(P^{\text{refer}}|e) = 0.5 + \beta_1 \text{ExperienceDiff}_q \lesseqqgtr 0.5$$

where $\text{ExperienceDiff}_h = 1$ (if the candidate has higher experience) and $\text{QualityDiff}_l = -1$ (if the candidate has lower experience).

If there is a difference between the genders of both profiles, we would expect that, on top of quality, some sort of gender bias to also affect P^{refer}. Because in our design we always have differential gender pairs and because forcibly $P_h^{\text{refer}} + P_l^{\text{refer}} = 1$, gender affects both profiles:

$$E(P^{\text{refer}}|e, g) = 0.5 + \beta_1 \text{ExperienceDiff}_q + \beta_2 \text{GenderDiff}_g \gtreqqless 0.5$$

where $\text{GenderDiff}_m = -1$ (if the candidate is a man) and $\text{GenderDiff}_f = 1$ (if the candidate is a woman). The coefficient β_2 measures the gender bias in determining respondents' choice. If $\beta_2 > 0$, there is a positive gender bias for female candidates when compared to male candidates. If $\beta_2 < 0$, there is a negative gender bias against female candidates when compared to male candidates.

The identification strategy comes from varying whether the high-experience candidate is a woman or a man, keeping the same differences in experience and the existence of a gender difference in the pair. Denoting the experience difference as equal to 1, the potential outcomes for the groups in which the high-experience candidate is a woman ($P^{\text{refer}}_{h,f}$) and in which the high-experience candidate is a man ($P^{\text{refer}}_{h,m}$) are:

$$\begin{aligned} \text{Group 1 (HEM):} \quad & E(P^{\text{refer}}|E=h, G=f) = 0.5 + \beta_1 + \beta_2 \\ \text{Group 2 (HEF):} \quad & E(P^{\text{refer}}|E=h, G=m) = 0.5 + \beta_1 - \beta_2 \end{aligned}$$

Again, if $\beta_2 > 0$, then the positive bias for woman will make that the high-quality woman is more likely to be picked than the high-quality man. The opposite holds if $\beta_2 < 0$.

The difference between the two groups yields:

$$E(P^{\text{refer}}|E=h, G=m) - E(P^{\text{refer}}|E=h, G=f) = 2\beta_2$$

where $2\beta_2$ is the penalty (or reward) against the female candidate minus the reward (or penalty) for the male candidate. It measures the distance between the gender-differing pair.

It can be causally estimated by regressing:

$$R^H_{i,g} = a + \underbrace{b}_{2\beta_2} \text{Female}_g + \underline{c}\ \underline{S} + \underline{d}\ \underline{X} + \varepsilon_i$$

where $R^H_{i,g}$ is a dummy equal to one if respondent i referred the high-quality candidate (and zero otherwise) and Female_g is a dummy equal to one if the high-quality candidate is female (and zero otherwise). The vector $\underline{S}$ contains the four strata dummies (gender of respondent, gender-dominance of sector, wage-employed in baseline, and dummy for hard to find). The vector $\underline{X}$ refers to sector of specialization and vocational training institute fixed effects. Because, given a sector of specialization, the pair of hypothetical profiles is exactly the same for all subjects, except for gender, controlling for sector of specialization also implies controlling for the type of CVs shown. By controlling for the pairs of profiles shown, we are also able to control for the systematic differences existing between high- and low-quality profiles that extrapolate the 5 months of work experience (firm name and high-school). We include in all specifications the control variables and, following **?**, the strata variables.

We use the above specification for all primary outcomes. In the regressions of the Cross-Randomization, the vector $\underline{S}$ includes the five strata variables for this experiment (gender of respondent, gender-dominance of sector, wage-employed in baseline, dummy for hard to find, and the assignment in the Main Experiment) and all unbalanced variables (age, years of activity in the labor market, dummy for having studied motor-mechanics, and dummy for having studied hairdressing).

2.5 Results

In this section, we present the results for the overall sample. Nonetheless, they hide interesting findings related to the gender dominance of the sectors that we further explore in the following section on hetereogenities.

2.5.1 Main experiment: Identifying gender bias

We begin by looking at gender discrimination in the overall sample. Figure 2.2 shows that, on average, male profiles were more likely to be picked, even though there were as many female profiles as male profiles (with the same quality on average). In total, 55.6% of our subjects picked the male hypothetical profile and 44.3%, the female profile. These numbers are statistically different from 50/50 at the 5 percent level, which is already strong evidence of gender bias against females in our sample. On top of that, we can also see that the high-quality profiles were generally more picked, suggesting that we were successful at flagging the higher quality. In total, 62.5% subjects picked the high-quality profile and 37.5%, the low-quality profile. Comparing the HEM and the HEF, we could see that, when the high-quality hypothetical candidate was a man, 68.5% subjects chose to refer the high-quality, whereas, when it was a woman, 56.7% chose the high-quality profile. In Table 2.2 we confirm this evidence in a regression framework: high-quality profile is 11.1 p.p. less likely to be picked when it bears a male name rather than when it bears a female name, which is significant at the 1 percent level. This shows that subjects in our sample have a significant bias against female candidates. Figure 2.4 illustrates that the effect of having a higher quality is smaller when the high-quality candidate is a woman (right panel) compared to when the high-quality candidate is a man (left panel). However, when we reweigh the sample by gender, this bias decreases and becomes insignificant, indicating that gender is a relevant source of heterogeneity, as explored in Section 6.

Using the alternative specification described in Appendix B.2, we are able to descriptively compute the effect of being high quality. We find that the gender penalty of 11.1 p.p. is similar in magnitude to 34.8% of the estimated effect of having 5 fewer months of work experience than the competing candidate (or, for simplicity, 1.7 months). Considering these are referrals for internship positions, this is a non-negligible figure.

To understand the mechanisms underpinning our subjects' discriminatory behavior, we turn to the rating questions that measure profiles' likability ("how much would you like to work with this person") and perceived probability of retention ("how much likely do you think this person is to be retained"). We expect the answers to these questions to reflect subjects' referral decisions. Figure 2.3 displays the CDF of likability and perceived quality for male and female profiles. It shows that female profiles are, on average, rated worse in terms of likability and perceived probability of retention, with cumulative distributions to the left of those of male profiles.

One could argue that the sizable gender discrimination of 11.1 p.p. could be due to a pass-through of employers' and customers' preferences onto employees' preferences, but our results show that workers' intrinsic preferences also matter. Workers could refer more men because they know business owners and clients prefer male workers (Becker, 1957; Cahuc et al., 2014; Bar and Zussman, 2017). If anything, we expect this to be translated in a smaller perceived probability of retention for female profiles. Indeed, as shown in Column 5 of Table 2.2, the high-quality profile is rated as having less 4.5 p.p. chance of being retained if it bears a female name, which is equivalent to 6.1% of the mean for the male high-quality profiles. However, on top of that, the female high-quality profiles are also rated as 0.31 points less likable (on a scale from 0 to 10), which is equivalent to 4% of the mean for male high-quality profiles, as shown in Column 3 of Table 2.2. This result shows that our respondents regard women as less likely to be retained, but their distaste is also significant. It indicates that their discriminatory behavior is not only due to a pass-through of employers' or customers' preferences, which, if anything, is captured only by the question on perceived likelihood of retention.

2.5.2 Network choices

In the second part, we move away from hypothetical candidates and asked subjects to name two network members of different gender that they believed would be a good fit for the position. Ultimately, we allowed respondents to refer one of them instead of the hypothetical candidate previously selected. Table 2.3 displays the characteristics of the network members that we have collected from subjects. The network members that respondents named are on average 24.8 years old, whereas hypothetical profiles were 22. About 85% have completed post-secondary education (certificate, degree, or university) and they have on average 38.8 months of work experience and 31.3 months of work experience in the sector of specialization of the respondent. It is important to notice that female networks are significantly younger (24.0 against 24.8) and less experienced (33.2 months of work experience and 26.9 months of work experience in the sector of specialization of respondent against 43.4 and 35.0), despite being as educated as male respondents. Out of the pool of network members, 82.5% are friends of our subject and 6% are siblings.

As shown in Table 2.4, 72.3% of our subjects named at least a network member. Conditional on having done so, 38.4% named only a male network member, 24.7% named only a female network member, and 36.9% named both. In total, 253 out of 401 subjects were able to mention the first network but not the second when we probed for someone of different gender. Of these, only 15 had mentioned as first a non-stereotypical worker, while 238 mentioned the first and only network being someone of a stereotypical gender. The low share of respondents able to mention candidates from both genders, even when explicitly asked to do so, makes us believe that the segregation in referrals is also due to limited supply of network members who are non-stereotypical workers in their occupation, precisely because of the segregation of these sectors[8]. However, even when subjects name candidates of both

[8]When probed why they could not think of someone of the opposite gender, 82.2% said they did not

genders, as shown in Table 2.4, they still display a strong pro-male bias on average.

In line with what observed by Beaman et al. (2018), we find that our subjects were less likely to choose female network members for the internship. Rather than reflecting a lack of supply of female acquaintances, our evidence is suggesting of a strong preference for men in our sample. Among those who were able to name two network members, 27.3% referred the female network candidate while almost the double (53.4%) chose the male one. Looking at the heterogeneity in Table 2.4, we can observe that preferences are strongly correlated to gender: women have a mild preference for female network members (referring them 9.4 p.p. more than male network members), while men have a strong preference for male network members (referring them 45.2 p.p. more than female network members). Taken together, our evidence suggests that the same patterns of discrimination observed in the anonymous setting are observed also when subjects have to resort to their networks, even in the lack of shortage of female networks. It could be that these preferences are grounded on the differences in observables described above, but we should highlight that the amount of work experience female network possess on average is more than enough for them to qualify for the internship position offered.

In general, 54.4% of our subjects chose to refer a network member at the end of the experiment and allowing our subjects to choose someone in their network reduced their chances of choosing a woman for the internship. Table 2.5 displays the treatment effects for the primary outcomes. Column 1 display the effect of receiving the high-experience female profile on the probability of referring a woman in the first part, when subjects are shown just hypothetical profiles. Differently from Columns 1 in Table 2.2, these columns display the regression of the variable referring a female candidate (network or hypothetical) on the treatment assignment (receiving high-experience female profile) rather than the regression of referring the high-quality candidate on the treatment assignment. Naturally, being shown a high-quality woman increases the chances of subjects referring a woman in the first exercise by 25.7 p.p., which is significant at the 0.1 percent level. However, when we allow subjects to refer someone in their network, this effect nearly halves, going to 14.2 p.p, as shown in Columns 3 and 4. This indicates that allowing subjects to resort to their network increases pro-male bias, suggesting that the same patterns of discrimination observed for the hypothetical candidates hold for the network members.

On the other hand, showing a high-quality woman rather than a high-quality man did not make our respondents more or less likely to pick a network member rather than one of the hypothetical profile. Column 4 of Table 2.5 show that the gender of the high-experience candidate did not affect the chances of our respondents referring a network member. It could be that respondents simply refer more network members because they have more knowledge about hard-to-observe measures of productivity for their network than for the hypothetical

know any person of the opposite gender that were skilled enough and 9.5% said they simply did not know anyone of the opposite gender that worked in their sector. Only 5.1% said that they thought the job was not appropriate for someone of the opposite gender and a minority gave other reasons, like “I think the employer will not like someone of the opposite gender” (three subjects) and “I do not like working with someone of the opposite gender” (one subject).

candidates (Rees, 1966; Montgomery, 1991; Hensvik and Skans, 2016), but, in the presence of a pro-male bias, we would still expect our subjects to refer less network members when the high-quality profile is a man. As male profiles are perceived as more likely to be retained, we expected our subjects to statistically discriminate women, making inferences on unobservable measures of productivity based on gender, and refer network members more when they are shown a high-quality female profile. Instead, because of the absence of differences in the likelihood of referring a network member across groups, it seems that our subjects aim to extract social benefits from referring a network member (Beaman and Magruder, 2012) and the gender of the hypothetical candidate is inconsequential for this decision[9].

2.5.3 Private referrals

In the first part, when subjects had to choose between the hypothetical profiles, the private referrals do not seem to have changed respondents' attitudes towards female candidates for the whole sample. As shown in Column 1 of Table 2.6[10], private referrals could have increased the probability of referring the female hypothetical candidate by 5.8 p.p., but these effects are not significant for any level. Nonetheless, given the magnitude of the effect (0.13 s.d. to 0.12 s.d.), we believe that the lack of significance could be an issue of power. Private referrals could allow employees to maneuver employers' discriminatory behavior by protecting their own reputation (Rees, 1966). As will be discussed in the next section, when looking at heterogeneous effects, we observe that private referrals were effective in inducing female referrals in male-dominated sectors, but not in female-dominated sectors, indicating that they had a significant effect and that the absence of statistical significance in the whole sample is only due to the averaging of the treatment effect between these two subsamples.

Private referrals also seem not to have affected respondents' attitudes towards low experience candidates. Column 2 shows that private referrals had no effect on the probability of referring the low experience hypothetical candidate, but the magnitude of the coefficients is very small, 1.5 p.p. (or 0.03 s.d.) and 0.7 p.p. (or 0.01 s.d.), which makes us believe that these are precisely estimated zeros. We thought that private referrals would reduce the punishments for a bad referral, but, in our design, subjects have no incentives to prefer more the low-experience hypothetical candidate under private referrals. Being hypothetical candidates, subjects have no social benefit to gain from referring them and concealing the candidate's ties with the subject is pointless. Plus, because we offered an unconditional cash transfer of 30 USD in case the candidate is referred, we believed they would have strong

[9]In short, this is a matter of whether we were powered enough. If the estimates are precise zeros, then the gender of the hypothetical candidate is inconsequential and, even if there is gender bias (so respondents make more positive inferences about unobservable characteristics for men than for women), they have the same chances of referring a network because their main objective in referring a network is to extract social benefits. Instead, if there is a treatment effect and we were not able to precisely estimate it, observable and inferred unobservable information about female profiles sets a lower bar for referring a network member.

[10]The sample size is smaller for Experiment 2 (N=475 instead of N=555) because 80 observations have missing values for at least one of the unbalanced variables for which the regressions are controlled.

incentives to prefer the high-quality candidate with or without the employer knowing that they referred that candidate. Therefore, in the absence of a trade-off between quality and social benefits, private referrals do not seem to hurt the quality of referrals.

As for the mechanisms, we could also observe that private referrals induced our subjects to rate hypothetical candidates better in terms of likability and to rate female hypothetical candidates better in terms of perceived likelihood of retention. As shown in Column 3 of Table 2.6, private referrals make female hypothetical candidates 0.36 points more likable and, as in Column 4, perceived to be 3.9 p.p. more likely to be retained, both of which are significant. Male hypothetical candidates, on the other hand, are also 0.36 points more likable under the private referral than under the public referral, which is significant at the 10 percent level, but perceived to be no more likely to be retained. We believe that these results suggests that, for female candidates, referrals might act as a liability rather than an asset and women coming with a recommendation from other employees may actually be penalized.

In the second part, in line with our hypothesis, private referrals increased the probability that subjects would refer a network candidate rather than a hypothetical candidate. As shown in Column 8 of Table 2.6, private referrals made our subjects 9.2 p.p. more likely to choose a network member rather than a hypothetical candidate, which is significant at the 10 percent level. This shows that private referrals, which conceal the name of the referring worker to the employer (but not to the referred candidate), did change the trade-off that workers face when balancing social benefits when making a referral, as in the model of Beaman and Magruder (2012).

2.6 Heterogeneity analysis

In this section, we explore the heterogeneous effects for the results presented in this section. We first conduct heterogeneity analyses in terms of gender. Then, we zoom into the heterogeneities by gender dominance of the sector.

2.6.1 By gender

As reported in Table 2.7, discrimination against female workers is strongly correlated with gender of respondent, but, while pro-male bias is strong and significant among men, pro-female bias is smaller and insignificant across women. As reported in Column 1, among male subjects, the high-quality candidate has 25.5 p.p. *less* chances of being picked if she is a woman, which is significant at the 0.1 percent level. For female subjects, the profile has 12.1 p.p. *more* chances of being picked if she is a woman, which is significant only at the 10 percent level. Also, as shown in Columns 2 and 3, women tend to rank the high-quality profile slightly better in terms of likability if it is a woman (the coefficient is large but insignificant), while perceiving both male and female high-quality profiles as equally likely to be retained. On the other hand, men tend to rate the high-quality profile significantly worse if it is a woman both in terms of likability and perceived likelihood of retention. In this way, we can argue that both men and women discriminate against the other gender, but men tend to punish female candidates much harder.

2.6.2 By sectorial gender-dominance

As reported in Table 2.8, discrimination against female workers is strongly correlated with gender dominance of the sector. Respondents coming from male-dominated sectors of specialization display a stronger discrimination against women, while respondents from female-dominated sectors of specialization display a positive discrimination for women, both of which are large and significant. In the first part, for subjects in male-dominated sectors, the high-experience hypothetical candidate is 27.0 p.p. less likely to be picked if she is a female, as shown in Columns 1 of Panel C. On the other hand, respondents in female-dominated sectors seem to display a significant, but smaller, preference for female candidates: the high-experience hypothetical profile is 14.6 p.p. more likely to be chosen if it is a female, as shown in Columns 1 of Panel B. Figure 2.6 shows that, in general, subjects tend to refer stereotypical candidates: in both HEM and HEF, subjects in female-dominated sectors tend to refer more the high-quality candidate, but subjects in male-dominated sectors actually prefer more the low-quality profile when it is a male, reflecting the stronger bias from these subjects. This result goes in line with the work of Riach and Rich (2006) and Booth and Leigh (2010), who showed that gender bias is correlated with the gender dominance of the sector, but, contrarily to Carlsson (2011), we found a significant pro-male bias in male dominated sectors. It also expands Beaman et al. (2018) by providing corroborating evidence of pro-male bias in male-dominated sectors and by also showing that there is a significant pro-female bias in female dominated sectors. This finding suggests that debiasing programs aiming to increase female participation in male-dominated sector should also consider that, while being displaced to female-dominated sectors, men would face similar barriers to that of women. Therefore, such programs should also target female-dominated sectors.

In any case, because of the strong correlation between gender and gender dominance of sectors, it could be that these patterns are simply due to respondents' gender. Indeed, 90.8% of subjects in female-dominated sectors are women and 91.7% in male-dominated sectors are men. However, the main driver of respondents' referral choices seems to be the gender-dominance of sector. Table 2.9 displays the main results interacting the treatment assignment with both gender of respondent and gender dominance of sector. The only significant interaction is the one between treatment assignment and female dominated sector, which points to a positive bias of 14.2 p.p. towards women, while the interaction with gender of the respondent is insignificant.

Nonetheless, even though discrimination exists in both types of subjects, the drivers of discrimination seem to be different. Men are perceived to be as likely to be retained as women in female-dominated sectors (though less likable than women), whereas women are perceived to be both less likable and less likely to be retained in male-dominated sectors. On one hand, as shown in Column 3 of Table 2.8, subjects in male-dominated sectors rate the high-quality profile as 7.9 p.p. less likely to be retained if it is a woman, while for subjects in female-dominated sectors this effect is 0.4 p.p. and insignificant at all levels. On the other hand, Column 2 of Table 2.8 shows that subjects in male-dominated sectors rate the high-quality profile as 0.60 points less likable if it is a woman. The same coefficient is not significant for subjects in female-dominated sector, but we cannot reject that the coefficients are different in absolute terms (as shown at the bottom of the table). In fact, Table 2.9, containing the interaction terms with both gender and gender-dominance, shows that perceived likability has more to do with gender of the respondent than with gender-dominance of sector. While the coefficient for the interaction with sectorial gender-dominance is insignificant, the one with gender is positive and shows that women do not like working with men (despite insignificant, the coefficient on likability in Table 2.7 is large in magnitude and is significant hadn't we added the controls). It shows that the perceived likelihood of retention seems to be related to the gender-dominance of the sector, but the likability of the candidate is linked to the gender of the respondent (as the significant coefficient is the interaction of gender). This suggests that, when it comes to likability, intrinsic preferences (embodied in respondents' gender) play a more relevant role, whereas referral decisions and perceived likelihood of retention hinge on the sector of the worker, hinting that pass-through of extrinsic preferences might be an important driver of referrals decision.

When turning to the heterogeneity effects of the private referrals, we could see that private referrals made subjects in male-dominated sectors more likely to refer the hypothetical female candidate, suggesting that a share of the bias against non-stereotypical candidate is also due to employees' relationship with employers. Column 1 of Table 2.10 shows that private referrals made subjects in male-dominated sectors 10.2 p.p. more likely to refer the hypothetical female candidate. It could be that respondents in male-dominated sectors simply refer more women because the costs of referring a candidate perceived as worse are lower Beaman and Magruder (2012), but they are no more likely to refer the low-experience candidate. As shown in Column 2 of Table 2.10, private referrals did not make subjects in male-dominated sectors more willing to pick the low-quality candidates. This indicates that retaliation from employers is an important driver of pro-male bias in such sectors. Such 10.2 p.p. effect of private referrals is also large in magnitude and amounts to an increase of 29% in the chances of referring the female profile.

Additionally, private referrals seem to have increased the probability of referring a network member for subjects in female-dominated sectors, who, in general, tend to refer less their own network. Among those assigned to the control group (public referrals), 57% of subjects in male-dominated sectors referred their own network, while 42% in female-dominated sectors did so. In our sample, the private referrals managed to increase the probability of referring a network member by 14.2 p.p. among subjects in female-dominated sector, which is significant at the 10 percent level, as shown in Column 5 of Table 2.10. Considering that subjects in female-dominated sectors are more likely to know and name female network members, the fact they tend to refer less network members is an additional barrier to female employment. Women are generally disadvantaged in referrals. Beaman et al. (2018) highlight that these exist because men tend to refer less other women, while Brown et al.

(2016) point that homophily in network formation can impose additional barriers in the referral system. Our evidence also shows that, on top of that, women are in disadvantage also because workers that are more likely to name women (those in female-dominated sectors) tend to refer less their own networks. This goes in line with the discussion in Topa (2011), who stress that women are, in general, less likely to use informal contacts than men, which yields higher wages and promotion chances for male workers.

Therefore, subjects in gender-dominated sectors seem to positively discriminate the dominating stereotypical gender, meaning that debiasing programs aiming to increase integration of women in male-dominated sectors should also target female-dominated sectors, as men displaced from male-dominated occupation might face similar discrimination when entering female-dominated sectors. We also find evidence that, even though our respondents say that biases in network referrals are due to homophily in network formation, there are dynamics between employers and employees that increase gender bias in referrals. In particular, we have found that subjects in male-dominated sectors are more willing to refer women if their names are not disclosed to employers and they can, somehow, maneuver punishments coming employers' discriminatory behavior. The fact women are ranked as less likely to be retained in male-dominated sectors corroborates this result. We also find evidence that subjects in female-dominated sectors, who are more likely to know other women, are less likely to refer network members in general, but private referrals make them refer more network members.

2.7 Conclusions

In this chapter I describe a correspondence experiment to study the extent of gender bias in referrals and the extent explained by extrinsic preferences (i.e., pass-through discrimination from employers) and intrinsic preferences of employees. As part of such experiment we showed profiles of hypothetical candidates to workers and asked them to make a referral for a subsidized internship at the company in which they work. We found that female hypothetical profiles are 11.1 p.p. less likely to be referred for a subsidized entry level position in 555 SMEs in Urban labor markets in Uganda. The effects that we document are sizable and comparable to having 1.7 fewer months of work experience than the competing candidate. Allowing respondents to resort to their own networks increases their pro-male bias, suggesting that the evidence found in the anonymous study with the hypothetical candidates only likely offers a lower bound. Rather than being fully driven by employers' and customers' discriminatory behavior (Becker, 1957; Cahuc et al., 2014; Bar and Zussman, 2017), we find evidence that pro-male bias in referrals is also driven by intrinsic discrimination from workers, who generally rate women as less likable to work with.

Gender bias is strongly correlated to the gender of respondents, but this correlation seems to hinge upon the gender division of labor. Subjects in gender-dominated sectors positively discriminate stereotypical candidates. Because this discrimination exists against both genders, debiasing programs aiming to increase integration of women in male-dominated sectors should also target female-dominated sectors, as men displaced from male-dominated occupations will face similar discrimination when entering female-dominated sectors. On top of that, discriminatory behavior from employers seems to be stronger in male-dominated sectors than in female-dominated sectors and is an important driver of referring decisions, as subjects in male-dominated occupations respond positively to private referrals. We found evidence that, even though our respondents say that biases in network referrals are due to homophily in network formation, workers in male-dominated sectors tend to refer less women out of fear of retaliation from employers. Subjects in male-dominated sectors rate female candidates as less likely to be retained and are more willing to refer women if their names are not disclosed to employers and they can, somehow, maneuver punishments coming from employers' discriminatory behavior.

Referrals shape at large the pool of candidates that the employer uses to make their hiring decisions. Taken together, our results show that debiasing programs that intend to reduce gender segregation in the labor market should also target employees, and not only employers. Our findings also suggest that, especially in sectors where men are the majority, debiasing programs aiming to increase referrals to female workers should also target attitudes of employers.

Main Tables and Figures

Figure 2.1: Summary of experiment

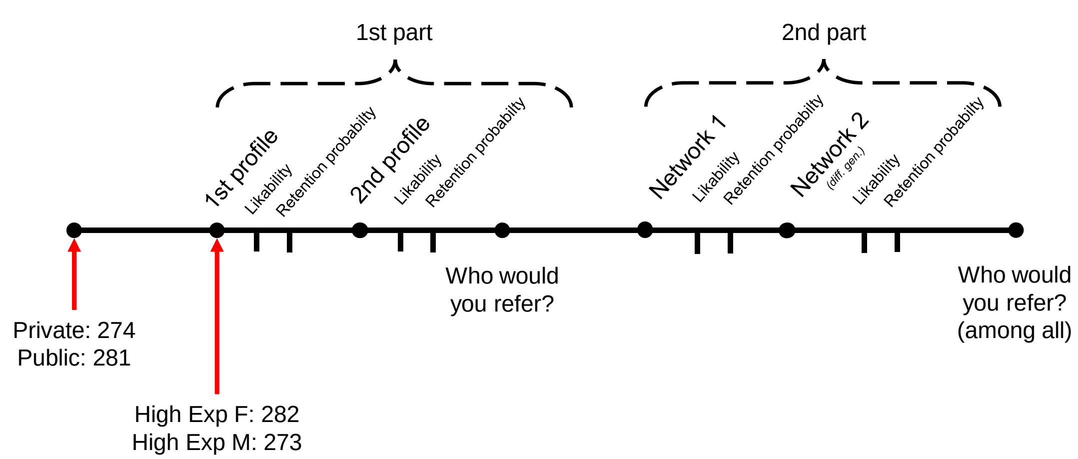


Table 2.1: Balance table

	Total	*Main experiment*			*Privacy of referral*		
	Mean	HEM	HEF	P-value	Public	Private	P-value
Age	27.05	27.03	27.07	0.89	27.39	26.70	0.01
Gender(male=1)	0.61	0.60	0.62	0.76	0.62	0.60	0.56
Married	0.34	0.34	0.34	0.87	0.37	0.31	0.13
Household asset index	0.10	-0.24	0.44	0.17	0.06	0.14	0.87
Scholarship	0.26	0.25	0.26	0.72	0.26	0.25	0.68
Rural	0.52	0.52	0.52	0.97	0.53	0.51	0.76
Years active in the labor market	2.78	2.83	2.74	0.64	2.96	2.60	0.07
Years active in current job	2.28	2.28	2.29	0.98	2.17	2.39	0.22
Business size	42.71	42.73	42.68	1.00	53.48	31.58	0.19
Wage employed	0.41	0.40	0.41	0.77	0.43	0.38	0.29
Motor-mechanics	0.19	0.20	0.19	0.69	0.17	0.22	0.10
Plumbing	0.12	0.14	0.11	0.43	0.12	0.13	0.81
Catering/food service	0.14	0.13	0.15	0.49	0.14	0.15	0.63
Tailoring	0.08	0.07	0.08	0.83	0.07	0.08	0.69
Hairdressing	0.03	0.04	0.02	0.19	0.04	0.02	0.09
Construction	0.06	0.05	0.06	0.66	0.06	0.05	0.64
Electrical work	0.22	0.21	0.24	0.31	0.24	0.21	0.39
Teacher/ECD	0.05	0.07	0.04	0.22	0.05	0.06	0.66

Figure 2.2: Choice of referral, by experience and by gender

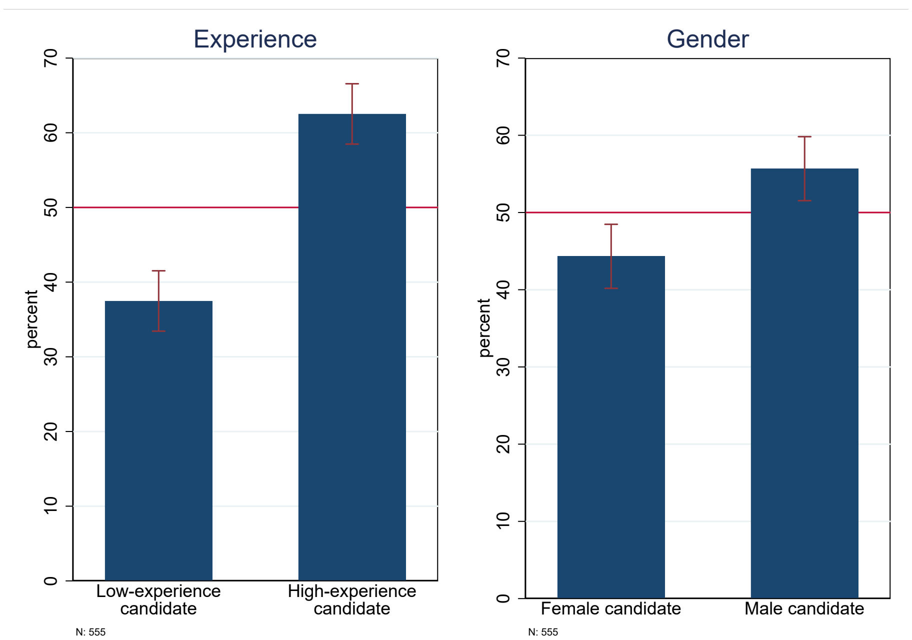


Figure 2.3: Cumulative distribution of likability and perceived probability of being picked

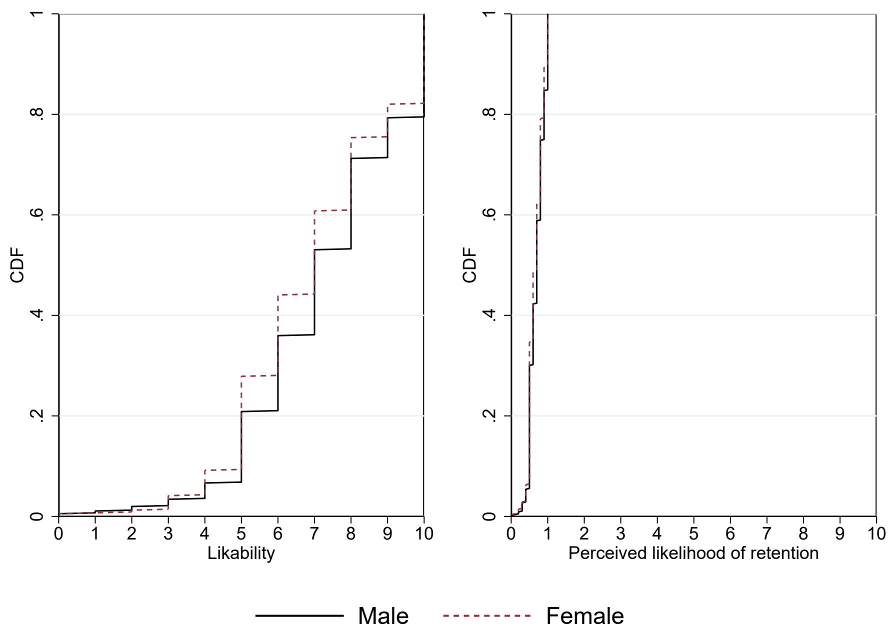


Figure 2.4: Main results: first part (hypothetical profiles)

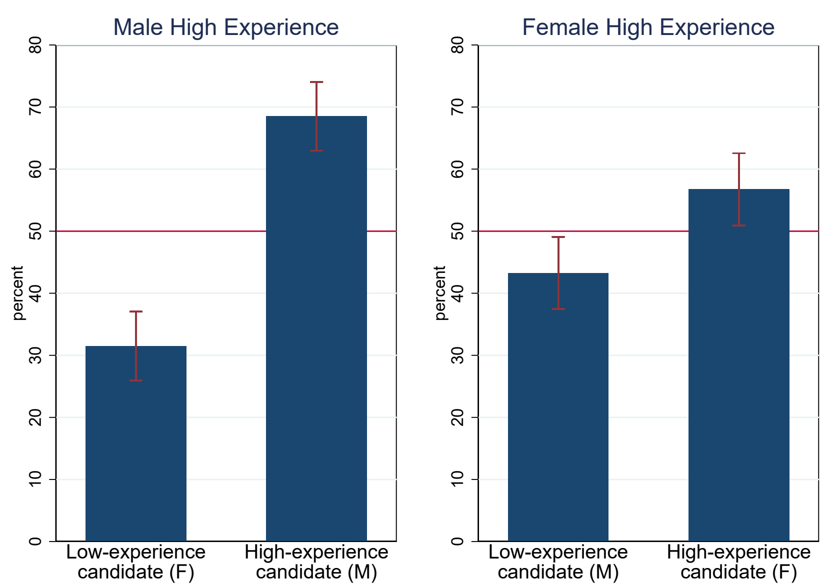


Table 2.2: Main results: first part and mechanisms (hypothetical profiles)

	(1) Probability of selecting Hi Exp	(2) Probability of selecting Hi Exp	(3) Likability of Hi Exp	(4) Likability of Hi Exp	(5) Perceived prob. of retention of Hi Exp	(6) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	-0.111**	-0.070	-0.307+	-0.190	-0.045**	-0.037*
	(0.042)	(0.043)	(0.170)	(0.173)	(0.016)	(0.017)
Control Mean	0.68	0.68	7.62	7.62	0.74	0.74
Control SD	0.47	0.47	1.93	1.93	0.18	0.18
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Weights		Gender		Gender		Gender
Treatment Effect (%)	-16.26	-10.23	-4.03	-2.49	-6.08	-5.03
Treatment Effect (sd)	-0.24	-0.15	-0.16	-0.10	-0.25	-0.20
N	555	555	555	555	555	555

Standard errors in parentheses

Controls include training area and vocational training institute fixed effects.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.3: Characteristics of networks named

Variable	(1) Female N	(1) Female Mean/SE	(2) Male N	(2) Male Mean/SE	(3) Total N	(3) Total Mean/SE	T-test P-value (1)-(2)
Age	245	23.96	297	25.41	542	24.75	0.00***
		(0.22)		(0.25)		(0.17)	
Completed post-secondary education	247	0.86	302	0.85	549	0.85	0.63
		(0.02)		(0.02)		(0.02)	
Mo. of experience	245	33.22	300	43.40	545	38.83	0.00***
		(1.87)		(2.25)		(1.51)	
Mo. of experience in training area	247	26.85	300	34.95	547	31.29	0.00***
		(1.62)		(2.05)		(1.35)	

Notes: The value displayed for t-tests are p-values. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 2.4: Network segregation

	Total	Female respondents	Male respondents
Named a network	72.25	72.69	71.98
Named female network (cond.)	61.60	91.08	42.62
Named male netw. (cond.)	75.31	42.68	96.31
Named both genders (cond.)	36.91	33.76	38.93
Referred female network (cond.)	26.68	50.32	11.48
Referred male network (cond.)	48.63	15.92	69.67
Referred female network (cond. named both)	27.70	39.62	21.05
Referred male network (cond. named both)	53.38	30.19	66.32
Total	555	216	339
Total (cond. named network)	401	157	244
Total (cond. named both)	148	53	95

Cond. refers to conditional on having named a network.

Table 2.5: Main results: second part (with network)

	(1) Referred female 1st part*	(2) Referred female 2nd part**	(3) Mentioned female netw.	(4) Referred network
Hi Exp Candidate female	0.252***	0.143***	0.003	-0.027
	(0.040)	(0.037)	(0.039)	(0.043)
Control Mean	0.32	0.33	0.45	0.56
Control SD	0.47	0.47	0.50	0.50
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Treatment Effect (%)	80.11	42.96	0.65	-4.77
Treatment Effect (sd)	0.54	0.30	0.01	-0.05
N	555	555	555	555

Standard errors in parentheses

Controls include training area and vocational training institute fixed effects.

* Choice among hypothetical profiles only.

** Choice allowing for network member.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.6: Results of cross-randomization (private referrals)

	(1) Referred female candidate	(2) Referred Lo Exp candidate	(3) Likability of female	(4) Perceived prob. of retention of female	(5) Likability of male	(6) Perceived prob. of retention of male	(7) Mentioned netw.	(8) Referred network
Private referral	0.058	0.007	0.360^{+}	0.039^{*}	0.361^{+}	0.016	0.051	0.092^{+}
	(0.045)	(0.046)	(0.191)	(0.019)	(0.198)	(0.019)	(0.042)	(0.048)
Control Mean	0.43	0.38	6.74	0.65	7.09	0.69	0.70	0.51
Control SD	0.50	0.49	1.96	0.19	2.08	0.19	0.46	0.50
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Unbalanced variables FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Effect (%)	13.60	1.90	5.34	6.00	5.10	2.37	7.33	17.98
Treatment Effect (sd)	0.12	0.01	0.18	0.20	0.17	0.08	0.11	0.18
N	475	475	475	475	475	475	475	475

Standard errors in parentheses

Unbalanced variables include: age, years active in the labor market, motor-mechanics and hairdressing FE.

Controls include training area and vocational training institute fixed effects.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2.5: Heterogeneity by gender-dominance of sector: main results

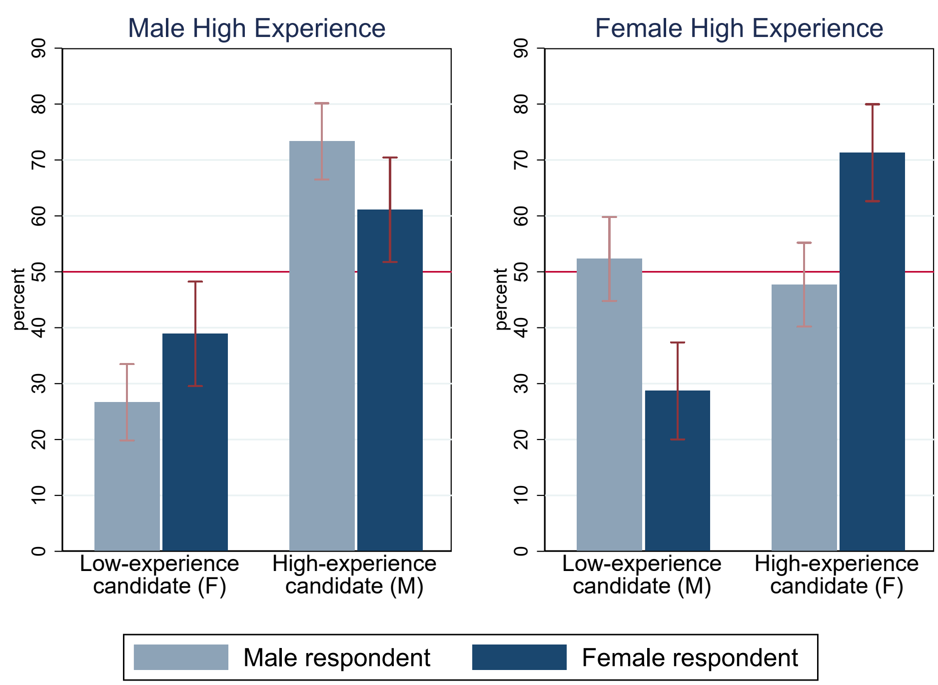


Table 2.7: Heterogeneity by gender: main results

	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Panel A: Full sample			
Hi Exp Candidate female	-0.111**	-0.307+	-0.045**
	(0.042)	(0.170)	(0.016)
Control Mean	0.68	7.62	0.74
Control SD	0.47	1.93	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	-16.26	-4.03	-6.08
Treatment Effect (sd)	-0.24	-0.16	-0.25
N	555	555	555
Panel B: Sample - Female respondent			
	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	0.121+	0.375	-0.005
	(0.070)	(0.277)	(0.028)
Control Mean	0.61	7.47	0.74
Control SD	0.49	1.99	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	19.81	5.03	-0.70
Treatment Effect (sd)	0.25	0.19	-0.03
N	216	216	216
Panel C: Sample - Male respondent			
	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	-0.255***	-0.730**	-0.074***
	(0.052)	(0.221)	(0.020)
Control Mean	0.73	7.72	0.74
Control SD	0.44	1.89	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	-34.84	-9.46	-9.98
Treatment Effect (sd)	-0.58	-0.39	-0.41
N	339	339	339
Difference in absolute effects across subsamples (test Panel B = - Panel C)			
Difference	-0.134	-0.354	-0.079
P-Value	0.248	0.166	0.001

Figure 2.6: Heterogeneity by gender-dominance of sector: main results

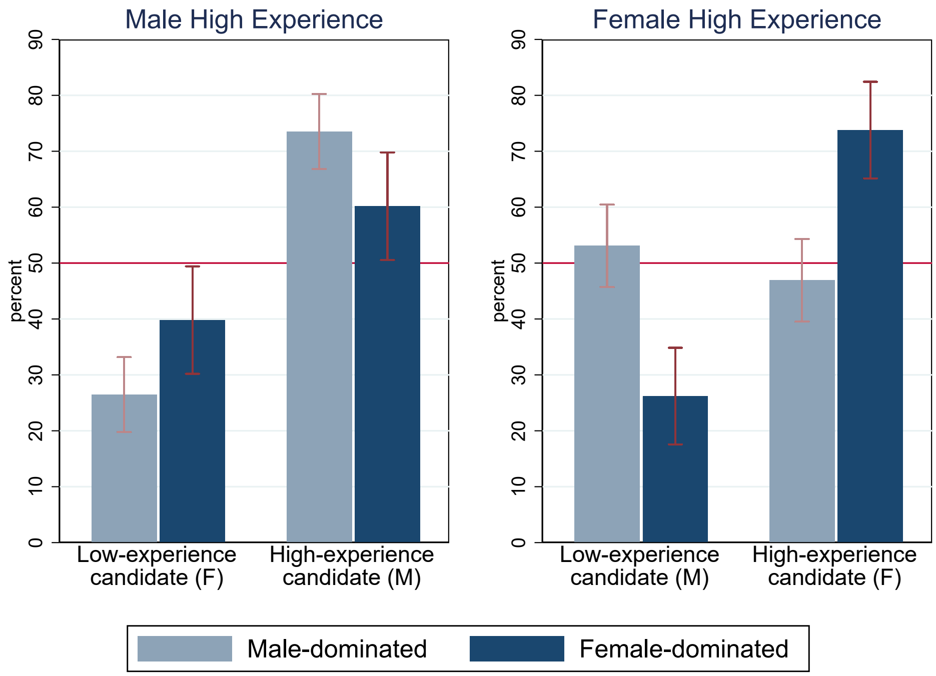


Table 2.8: Heterogeneity by gender-dominance of sector: main results

	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Panel A: Full sample			
Hi Exp Candidate female	-0.117**	-0.311+	-0.048**
	(0.041)	(0.170)	(0.016)
Control Mean	0.68	7.62	0.74
Control SD	0.47	1.93	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	-17.05	-4.08	-6.45
Treatment Effect (sd)	-0.25	-0.16	-0.26
N	555	555	555
Panel B: Sample - Female-dominated sector			
	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	0.146*	0.198	0.004
	(0.066)	(0.282)	(0.026)
Control Mean	0.60	7.44	0.73
Control SD	0.49	2.03	0.19
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	24.33	2.66	0.52
Treatment Effect (sd)	0.30	0.10	0.02
N	206	206	206
Panel C: Sample - Male-dominated sector			
	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	-0.269***	-0.595**	-0.079***
	(0.051)	(0.215)	(0.021)
Control Mean	0.74	7.73	0.75
Control SD	0.44	1.86	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	-36.60	-7.70	-10.51
Treatment Effect (sd)	-0.61	-0.32	-0.43
N	349	349	349
Difference in absolute effects across subsamples (test Panel B = - Panel C)			
Difference	-0.123	-0.397	-0.075
P-Value	0.273	0.224	0.000

Table 2.9: Heterogeneity by gender and gender-dominance of sector: main results

	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	-0.274***	-0.690**	-0.078***
	(0.052)	(0.222)	(0.021)
Female-dominated sector=1	-0.090	-0.317	-0.049
	(0.103)	(0.419)	(0.042)
Female-dominated sector=1 × Hi Exp Candidate female	0.322*	-0.178	0.097
	(0.148)	(0.553)	(0.060)
Female respondent=1	-0.038	-0.043	0.037
	(0.102)	(0.409)	(0.042)
Female respondent=1 × Hi Exp Candidate female=1	0.097	1.148*	-0.014
	(0.147)	(0.547)	(0.061)
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	555	555	555

Standard errors in parentheses

Controls include training area and vocational training institute fixed effects.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.10: Heterogeneity of cross-randomization (private referrals) by gender-dominance of sector

	(1) Referred female candidate	(2) Referred Lo Exp candidate	(3) Likability of female candidate	(4) Perceived prob. of retention of female	(5) Referred network
Panel A: Full sample					
Private referral	0.061	0.013	0.343^{+}	0.038^{*}	0.082^{+}
	(0.044)	(0.046)	(0.189)	(0.018)	(0.047)
Control Mean	0.43	0.38	6.74	0.65	0.51
Control SD	0.50	0.49	1.96	0.19	0.50
Strata FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Treatment Effect (%)	14.29	3.37	5.08	5.83	15.99
Treatment Effect (sd)	0.12	0.03	0.17	0.20	0.16
N	475	475	475	475	475
Panel B: Sample - Female-dominated sector					
	(1) Referred female candidate	(2) Referred Lo Exp candidate	(3) Likability of female candidate	(4) Perceived prob. of retention of female	(5) Referred network
Private referral	-0.011	0.036	0.278	0.026	0.142^{+}
	(0.073)	(0.078)	(0.300)	(0.029)	(0.080)
Control Mean	0.56	0.31	7.16	0.69	0.42
Control SD	0.50	0.47	2.05	0.18	0.50
Strata FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Treatment Effect (%)	-1.96	11.74	3.88	3.80	34.13
Treatment Effect (sd)	-0.02	0.08	0.14	0.14	0.29
N	176	176	176	176	176
Panel C: Sample - Male-dominated sector					
	(1) Referred female candidate	(2) Referred Lo Exp candidate	(3) Likability of female candidate	(4) Perceived prob. of retention of female	(5) Referred network
Private referral	0.102^{+}	-0.005	0.388	0.042^{+}	0.039
	(0.055)	(0.056)	(0.247)	(0.024)	(0.059)
Control Mean	0.35	0.42	6.51	0.63	0.57
Control SD	0.48	0.49	1.88	0.19	0.50
Strata FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Treatment Effect (%)	29.14	-1.09	5.97	6.66	6.86
Treatment Effect (sd)	0.21	-0.01	0.21	0.22	0.08
N	299	299	299	299	299
Difference in absolute effects across subsamples (test Panel B = - Panel C)					
Difference	0.090	0.032	0.666	0.068	0.181
P-Value	0.356	0.609	0.043	0.005	0.001

Chapter 3

Gender Gaps: Back and Here to Stay? Evidence from Skilled Ugandan Workers during COVID-19

This chapter is coauthored with Mary Namubiru and Sara Spaziani.

3.1 Introduction

To curb the spread of COVID-19, governments implemented unprecedented measures to restrict economic activity and individual mobility. Early evidence shows that, all over the world, these restrictions disproportionately affected female workers, who lost their jobs at a greater rate than male ones, and female entrepreneurs, whose businesses saw a disproportionate decline in revenues and workforce.[1] While in the Global North these gendered effects have largely dissipated following the easing of the restrictions (Bluedorn et al., 2021; Lee et al., 2021), it is unclear whether the same holds true in the Global South, where low-capacity countries have mostly been unable to provide targeted support to workers and firms in economic distress and the labor market recovery is slowest (ILO, 2022b). As the integration of female talent in the labor force is a key determinant of GDP growth (Papageorgiou et al., 2018; Hsieh et al., 2019), evaluating how skilled female workers and entrepreneurs in low-income economies have been affected by COVID-19 is crucial for understanding how productivity will fare in these regions once the pandemic subsides.

To make progress on this question, we investigate gender disparities in the effects of two nationwide lockdowns implemented in Uganda on the labor market outcomes of a sample of

[1]Adams-Prassl et al. (2020); Amuedo-Dorantes et al. (2020); Deshpande (2020); Farré et al. (2020); Heggeness (2020); Kristal and Yaish (2020); Andrew et al. (2021); Casale and Posel (2021); Dang and Viet Nguyen (2021); Kikuchi et al. (2021); Landivar et al. (2020); Reichelt et al. (2021); Kugler et al. (2021); Alon et al. (2022); and Casale and Shepherd (2022) find disproportionate effects of the economic restrictions on female workers. Torres et al. (2021); Gulesci et al. (2021); and Alfonsi et al. (2021) focus on entrepreneurs.

714 young, urban, and highly skilled workers and entrepreneurs who, pre-pandemic, received post-secondary vocational education and were employed in a wide range of manufacturing and services sectors. These workers, representing the top 3% of the country's education distribution and characterized by no gender differences in pre-pandemic employment rate and job security, should not be considered as representative of the Ugandan youth, but rather the expression of the emerging urban working class driving the country's structural transformation.

Relying on a unique high-frequency panel dataset spanning from January 2020 to September 2021, we track these workers' labor market outcomes before, during and after the lockdowns, evaluate gender differences in the early responses to the lockdowns and in recovery patterns, and investigate the root causes of the observed trends.

We find that the first lockdown reduced the employment rate by 53 p.p. (69% over the baseline level) among female workers and by 35 p.p. (45%) among male workers, generating an employment gender gap of 20 p.p. Once the restrictions were lifted, male employment rate was back to its pre-pandemic level in six months. Conversely, as 10% of the previously employed women remained jobless and 35% became occasionally employed, female employment rate remained below its pre-pandemic projections. The employment gender gap, further amplified by the second lockdown that once again disproportionately reduced female employment, persisted eighteen months after the onset of the pandemic. We identify three additional gendered responses. First, the disproportionate job losses experienced by female wage-employees resulted in a more pronounced shift towards self-employment. Second, the lockdowns displaced women from the sectors in which they received vocational training and relocated them into agriculture and other unskilled sectors. Third, the earnings gender gap widened. The transition of female workers towards sectors in which they cannot leverage their comparative advantage and experience is likely to induce a disproportionate depreciation of their productive skills. This is especially worrisome when considering the monetary and time investment in vocational education made by these workers.

We investigate two possible determinants of these dynamics identified by the literature: female workers' concentration in economic sectors deemed as non-essential and with higher risk of infection (Alon et al., 2020; Couch, 2020) and the extraordinary childcare responsibilities generated by schools' closures (Del Boca et al., 2020; Couch, 2020; Farré et al., 2020; Hupkau and Petrongolo, 2020; Andrew et al., 2021; Oreffice and Quintana-Domeque, 2021; Sevilla and Smith, 2020; Zamarro and Prados, 2021; Alon et al., 2022). Pre-pandemic, our female respondents were over-represented in sectors subject to the strongest restrictions. Initial closures in these sectors explain 50% of the employment gender gap during the first lockdown; their contribution gradually declines after the restrictions are lifted, but once again rises to 13% during the second lockdown. Moreover, in periods of schools' closure employment declines with the number of school-age children in the household for women but not for men. Childcare responsibilities contribute between 11% and 24% of the employment gender gap in the later stage of the pandemic, following the prolonged school closure. We estimate that, together, gender differences in employment sectors and childcare responsibilities explain up to 65% of the employment gender gap. Consistent with evidence from high- and low- income countries, a considerable share of the gap remains unexplained (Adams-Prassl et al., 2020; Montenovo et al., 2020; Furman et al., 2021; Kugler et al., 2021).

The gender gap in job losses of 20 p.p. we observe is considerably larger than the 2.5-9 p.p. gap documented in other high- and low- income countries for more representative populations (Stantcheva, 2022; Kugler et al., 2021; Alon et al., 2022; Casale and Shepherd, 2022). We identify three drivers of such large and persistent effect. First, our respondents were hit by the pandemic in the earliest, most vulnerable stage of their careers. Several studies consistently find larger job losses (Liang et al., 2022; Montenovo et al., 2020; Kikuchi et al., 2021; Lee et al., 2021; Kugler et al., 2021) and gender differentials (Kristal and Yaish, 2020) among the youth. Second, our respondents were largely employed outside the relatively more sheltered agricultural sector and, given the hands-on nature of their jobs, they were mostly unable to work from home. Third, our respondents could not rely on publicly financed retention schemes, which supported about 50 million jobs across OECD countries (OECD, 2020).

We contribute to the literature on the gendered effects of COVID-19 in three ways. First, with a unique dataset we assembled, we provide an otherwise unavailable look at how the pandemic affected the emerging class of skilled urban workers in a low income-country, for which we find large and persistent gendered effects. This finding expands our understanding of the effects of COVID-19 in the Global South. Evidence from Nigeria shows that gender gaps quickly dissipated in settings characterized by the prevalence of agricultural or other non-farm subsistence activities (Alon et al., 2022). Our results suggest the existence of heterogeneous recovery patterns for different segments of the labor market: even in highly agricultural countries, women employed in manufacturing and services, strongly resembling the workforce of more advanced economies, may never fully recover without targeted support. Consistent with our hypothesis, recent studies report partial recovery and enduring gender gaps in labor market outcomes for the subpopulations of female wage employees across ten low-income countries (Kugler et al., 2021), for female workers in South Africa–a more economically diversified middle-income country (Casale and Shepherd, 2022), and for female return migrants previously employed in urban settings in India (Allard et al., 2022). Overall corroborating our view and concerns, the latest estimates from the Global South confirm that female employment is recovering at a slower pace than male employment, contributing to a growing employment gender gap globally (ILO, 2022a). Second, we provide new insights on how the effects of the pandemic compare between the Global North, where highly educated women were the least affected (Adams-Prassl et al., 2020; Foucault and Galasso, 2020; Lee et al., 2021), and the Global South, where women from some highly educated groups experienced large and persistent effects. Third, while most studies use single or repeated cross-sections and short panels, we leverage one of the longest panel datasets spanning the COVID-19 pandemic. The panel structure of our data, the extended time span it covers, and the availability of pre-pandemic information allow us to monitor labor market trajectories in and out of employment and across sectors, test the persistence of the initial shock for eighteen months, and isolate the specific effects of COVID-19 containment measures from pre-trends.

The findings of this paper indicate that the labor market trajectories of economically empowered women in low-income countries are highly vulnerable to temporary economic shocks. If not pressingly tackled, the labor market disconnection and sectorial misallocation of skilled female workers induced by the COVID-19 pandemic may result in additional barriers to economic growth. Governments, international organizations, and NGOs should prioritize supporting enterprises in female-dominated sectors and women seeking stable employment. Closing the employment gender gap will additionally require identifying the forces behind its unexplained portion, such as employer discrimination or social norms.

3.2 Context

Uganda has 78% of the population aged below 30 (International Youth Foundation, 2011) and a youth underutilization rate of 68% (ILO, 2017). To address the prevailing skills mismatches and workers' underqualification, in 2012 the Ugandan government implemented a decennial strategic plan aimed at reinforcing its vocational education system (EPRC, 2021), which proved effective at generating productive human capital (Alfonsi et al., 2020). Currently representing 4% of the youths, post-secondary vocational graduates have above mean employment rates and earnings.[2] This highly skilled group was projected to grow as further educational and labor market opportunities emerged with the country's sustained economic growth (EPRC, 2021).

The positive economic outlook was, however, undermined by the COVID-19 shock, which contracted the economy to its slowest pace in three decades (World Bank, 2021). To curb the spread of the virus, the government implemented one of Africa's strictest sets of nationwide containment measures. It closed schools on March 18, 2020, and non-essential businesses during a first national lockdown implemented between March 31 and June 2, 2020. The government also imposed travel bans for vehicles and a dusk-to-dawn curfew. While most restrictions for economic activity were lifted in June 2020, schools remained closed until February 2021 when, except for pre-primary schools, they gradually reopened.[3] Amid the fear of a second wave of cases, the government imposed a second, milder, lockdown between June 19 and July 31, 2021. Although most businesses were not shut down, travel limits, a stringent curfew, the suspension of public transportation, and the new school closure lasting until January 2022 hindered once again the fragile economic recovery.

3.3 Data and sample

[2] Authors' elaboration of the latest Uganda National Household Survey from 2016/2017.

[3] Exceptionally, schools reopened in October 2020 for students enrolled in the last year of their education cycle.

3.3.1 The panel dataset

The sample we assembled consists of 714 young and skilled workers who graduated between 2014 and 2019 from five vocational training institutes (VTIs) located in the Central and Eastern regions of Uganda.[4], [5] Given the high technology access and educational attainment of our population, we conducted all surveys by phone. This initial choice allowed us to avoid disruptions in our data collection process once COVID-19 hit and phone interviews became the only tool to collect time-sensitive information. As Figure 3.1 shows, we interviewed our respondents in January, July and December 2020 and in September 2021. In each survey round, we collected detailed current and retrospective information, and obtained five additional data points for each respondent: the time of the first activity after graduation, different for each respondent potentially coinciding with January 2020, March and May 2020, and May and July 2021.[6] Measuring labor market outcomes before, during, and after the two

[4]Like most Ugandan VTIs, none of these five tracked their graduates' career developments nor kept their updated contacts. We therefore collected and digitized schools' hard copies of registries for multiple cohorts of graduates, obtained contacts for 1,368 alumni, and successfully contacted 52% of them. Our sample is not evidently selected with respect to the eligible population: due to the written nature and manual entry of the records, the digitization process was prone to error; additionally, the progressive implementation of the 2013 mandate of the Uganda Communication Commission to register all SIM-cards exogenously pushed many to change their phone numbers. Figure A.3.5 shows an example of the digitized material.

[5]This work was implemented in partnership with BRAC Uganda as a spin-off study of the Meet Your Future Project (Alfonsi et al., 2022b), a randomized control trial connecting vocational students with successful alumni of their schools to facilitate students' transition into the labor market. The respondents of this study represent the pool of alumni from which we selected 158 young professionals who participated to the project as mentors for the students. To make the selection, we elicited the respondents' broad interest in the project and collected detailed information about their demographics, education, and work experience. We do not believe that our respondents manipulated their answer to increase their chances to be selected. First, because the selection was based on merit but also on the goal to recruit mentors for each combination of school and course of study for which we had students. Second, the symbolic compensation and travel reimbursement we promised to selected respondents were likely insufficient to generate misreporting incentives, especially when weighted against the significant time and commitment that mentors put into preparation and actual implementation of the program. Third, because the respondents were not informed about the selection criteria, and hence were in practice unable to manipulate their score. Additionally, given our effort to find male and female mentors in similar fashion, there is no reason to believe that misreporting incentives differed by gender.

[6]If our respondents suffered from recollection bias, we could overstate the autocorrelation between outcomes over time (Godlonton et al., 2018). If so, the existence of a gender gap in our outcomes at the time of measurement may lead us to overestimate the gap in recollected periods. To explain this point, suppose that the employment rate is lower for women than for men at time T. Then, women would be more likely, due to recollection bias, to say they were not employed in T-1; the opposite would be true for men, and we would overstate the employment gender gap in T-1. There are, however, several reasons why we believe recollection bias is limited in our context. First, recollection bias is more pronounced among poor individuals (Das et al., 2012), while our respondents belong to the top tail of the education and income distribution in Uganda. Second, salient events are less subject to recollection bias (Beegle et al., 2012; Das et al., 2012). We structured our questionnaire to clearly identify moments before, during, or after the two nationwide lockdowns, which were disruptive events with tremendous consequences on the lives of our respondents and far beyond. We thus believe that our respondents accurately tracked their labor market outcomes around

lockdowns allows us to evaluate both early responses to the lockdowns and the persistence of the effects eighteen months from the onset of the crisis.

3.3.2 The study population

Our respondents graduated from the National Certificate, a post-secondary education vocational program providing trainees with a nationally accredited skills certificate. They received training in electrical wiring (23%), motor mechanics (19%), food and hospitality (15%), plumbing (12%), tailoring (8%), secretarial and accounting studies (7%), construction (5%), early childhood development (5%), hairdressing (3%), agriculture, welding, carpentry, and machining and fitting (1% or less).

Table 3.1 reports the respondents' baseline characteristics: they are on average 25 years old, they come from all over the country, 36% are married, and 47% have children. Pre-pandemic, 56% of them were paid employees, 21% owned a business, 13% were without an occupation,[7] and a few were enrolled in educational programs or engaged in causal occupations.

Women represent 41% of the sample. Despite being on average 1.5 years younger than men, they are as likely to be married, and live with more school-age children. Crucially, pre-pandemic female workers are as likely as male ones to be employed and hold quality jobs, as indicated by the absence of gender differences in labor market experience, the employment rates in skilled sectors and in the training sector, the self-employment rate, and the probability to work in, or own, a registered firm. Women are also more likely to have a permanent job and less likely to be engaged in casual occupations. These statistics suggest that our female respondents are among the most economically empowered women in Uganda.

3.3.3 Representativeness

The uniqueness of our sample clearly emerges when comparing it to the population of young Ugandan adults in the Uganda National Household Survey from 2016/2017 (UNHS). With 15+ completed years of education, our workers belong to the top 3% of the education distribution for Ugandan youths (Figure C.1.1). Their employment rate in non-agricultural occupations and earnings are 27 p.p. (56%) and $33 (47%) higher than average respectively (Table 3.2). In stark contrast with the average Ugandan youth, largely employed in agriculture or unskilled occupations, 85% of the employed respondents were working in skilled, non-agricultural jobs (Table 3.3).

When comparing our sample to post-secondary VTI graduates from the UNHS, we find smaller differences in socio-economic and labor market characteristics (Table 3.2) and a high sectorial overlap (Table 3.3). Although all differences shrink, they remain significant. This positive selection is plausibly driven by the quality of the VTIs from which our workers

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the lockdowns. Additionally, even if the recollected data points were considered unreliable and dropped from our analysis, all our conclusions would still apply.

[7] In our data we cannot distinguish unemployed and not economically active individuals.

graduated (which were pre-selected by BRAC Uganda based on their reputation, infrastructure, equipment, and teachers' educational attainment) and by the fact that most of our graduates reside in the country's richest urban areas. Accordingly, our findings extend to other top-notch, young and skilled workers in urban Sub-Saharan Africa.

3.3.4 Attrition

We successfully interviewed 714 workers in January 2020, 615 in July 2020 (attrition rate: 14%), 561 in December 2020 (21%) and 561 in September 2021 (21%).[8] Table 3.4 investigates the existence of differential attrition by gender. While we find no evidence of differential probability to reach female and male respondents in the first three survey rounds, female respondents are 6.2 p.p. less likely to be interviewed in the last one. We further investigate this issue by classifying female and male respondents into *Never* and *Ever* Attritors, depending on whether we were able or not to interview them in all the four survey rounds, and comparing their baseline characteristics in Table 3.5. Reassuringly, female Ever and Never Attritors do not significantly differ by any key baseline characteristics, suggesting that our findings are not driven by compositional changes in the female sample correlated with the COVID-19 shock. Male Ever Attritors do not differ from male Never Attritors on wage and self- employment rates nor earnings. However, male Ever Attritors are less likely to be employed in skilled and training sectors (suggesting they are negatively selected, and our estimated gender gaps are an upper bound), but are also more likely to be employed in a permanent job (pushing in the opposite direction). Combined, these findings suggest that male Ever and Never Attritors are not systematically different. Table C.1.1, comparing Ever and Never Attritors after pooling men and women, introduces no new determinants of attrition. In Section 3.4.3 we show that our results remain robust in the balanced panel of Never Attritors, and in 63-100% (depending on the outcome) of the scenarios about attritors' employment we build around the possibility that Ever Attritors are either positively or negatively selected; the results only disappear under implausible assumptions.

3.4 Results

3.4.1 Empirical Strategy

We provide evidence of the emergence and persistence of new gender gaps in the labor market by plotting average employment rate, employment rate in the training sector, employment rate in skilled sectors, and monthly earnings over time for men and women. Formally, we test for the existence of gender gaps by estimating the following equation:

[8] Our attrition rates are aligned with the literature: 15% on average across 91 RCTs published in top economics journals (Ghanem et al., 2020) and 18% in studies surveying youth (Bandiera et al., 2020).

$$Y_{i,t} = \alpha_i + \alpha_t + \sum_{y=Firstjob}^{Sept2021} \beta_y Female_i \times 1_{t=y} + \epsilon_{i,t} \tag{3.1}$$

$Y_{i,t}$ is the outcome measured for respondent i at time t; α_i and α_t are individual and time fixed effects. $Female_i$ is an indicator for female respondents, and $\epsilon_{i,t}$ the error term. Standard errors are robust to heteroskedasticity and clustered at the individual level. The coefficients β_y measure the evolution over time of the gender gap in the outcome variable. They provide a formal test for the absence of gender disparities in the labor market pre-pandemic and for their emergence and persistence during the pandemic. Identification is provided by comparing the outcome between men and women relative to March 2020, our latest pre-pandemic data point, after controlling for time-constant individual characteristics (including those that are unbalanced in Table 3.1) and common shocks across individuals.

3.4.2 The Ugandan *shecession*

Figure 3.2 illustrates the differential impacts of the two lockdowns on male and female employment rates over the course of 2020 and 2021. Panel (a) shows that, prior to the onset of the pandemic, female and male employment levels were nearly identical and constant at around 77%. Consistent with a high fear of infection and the severe restrictions imposed on economic activity, during the first lockdown in May 2020, the employment rate fell by 53 p.p. (69%) for females and 35 p.p. (45%) for males, generating an employment gender gap of 20 p.p. Once the restrictions were lifted, male employment recovered faster than female employment, and by December 2020 it was back to its pre-pandemic level. At that time, female employment was still 8 p.p. (11%) lower than its baseline level. The employment gender gap endured until May 2021, widened to 24 p.p. during the second nationwide lockdown in July 2021, when female workers once again experienced a relatively larger drop in employment, and persisted through September 2021, despite employment levels beginning to recover following the easing of restrictions.

Panels (a) and (b) of Figure 3.3 decompose the effect on overall employment rate into the effects on wage- and self-employment rates respectively. It emerges that the drop in wage-employment is the main driver of the overall effect. One plausible reason is the higher level of compliance to government rules among larger and established firms employing wage labor. Moreover, some wage-employed workers gradually responded to the layoffs by setting up their own activity. This seems especially true among women, who suffered the largest drop in wage-employment. Following job losses, most respondents remained without an occupation, as they did not resume education (panel [c]) nor engaged in casual occupations to make ends meet (panel [d]).

Following the easing of the restrictions, the rebound in employment was driven by both previously employed workers who had lost their jobs re-entering the labor market (panel [a] of Figure 3.4) and initially non-employed workers progressively finding a job, the first following graduation for 57% of them (panel [b]). While the gradual employment of new cohorts of graduates and of other non-employed was symmetrical by gender,[9] the re-entry of previously employed women remained between 10 and 30 p.p. lower than that of men throughout the pandemic. Panel (c) reveals that 80% of the men employed pre-pandemic were still employed in two-thirds of the post-lockdown data points, with a 40% employed throughout the post-lockdown period. Conversely, 10% of the previously employed women remained jobless, and another 35% were employed in half or less of the pandemic periods. The differential re-entry patterns by gender explain the persistence of the employment gender gap for eighteen months.

Additionally, we find that the lockdowns had gendered effects on the employment rate in the training sector, the employment rate in skilled sectors, and earnings. Panel (a) of Figure 3.5 shows that the share of respondents employed in their training sector, approximately 55% for both genders pre-pandemic, dropped by 25 p.p. (45%) for males and by 45 p.p. (82%) for females during the first lockdown in May 2020. While men were back on their pre-pandemic trajectory by December 2020, female employment rate in the training sector remained 20 p.p. below its baseline level for eighteen months. Panel (b) shows that, conditional on employment, the share of men employed in their training sector remained constant throughout the pandemic. Conversely, the share of women employed in their training sector conditional on emplouyment fell by 22 p.p. (31%) during the first lockdown and never recovered. Panel (c) shows that the share of respondents employed in skilled sectors, equal to 65% for both genders pre-pandemic, dropped by 32 p.p. (50%) for men and by 53 p.p. (76%) for women during the first lockdown, generating a previously non-existent gender gap in skilled employment which persisted for eighteen months after the onset of the pandemic. Panel (d) clarifies that the initial drop in male skilled employment is entirely driven by the drop in employment, as the share of men employed in skilled sectors conditional of employment remained constant over time. Conversely, the share of female respondents employed in skilled sectors conditional on employment dropped by 33 p.p. (39%) during the first lockdown and never rebounded in the following eighteen months. Figure C.1.3 illustrates that the reduction in skilled employment was driven by female workers pivoting towards agriculture (although this effect slowly dissipates) and non-agricultural unskilled occupations, where female employment increased by 2 p.p. and 15 p.p. (200%) respectively. Female employment in agriculture and in other unskilled sectors grew disproportionately following the second lockdown as well. Lastly, panel (e) shows that the initial earnings gender gap widened during the pandemic. During the first lockdown in May 2020, earnings fell in a similar fashion for female and male workers. By December 2020, the gender pay gap had tripled, reaching \$69 from a baseline of \$23, and stabilized afterwards. Panel (f), showing the evolution by gender of earnings conditional on employment, reveals that the men who remained employed saw their average earnings decline by \$40 (33%) during the first lockdown. Conversely, female average conditional earnings remained constant, plausibly due to the positive selection of the few women who were still employed in May 2020. However, the number of employed women in May 2020

[9]This dynamic is consistent with the positive association between employment and age found for vocational graduates of both genders in the UNHS (panel [a] of Figure C.1.2).

is so small that the standard errors are too large to make any claims on female earnings and the earning gender gap in this period. By December 2020, the gender pay gap in earnings conditional on employment had widened from \$33 to \$76 (+130). This widening is driven by both higher male and lower female conditional earnings. The former may result from career advancements: for vocational graduates in the UNHS sample each additional year of age is associated to a \$7 increase in monthly earnings (panel [b] of Figure C.1.2); the \$25 increase we document may be driven by the sample positive selection. Panel (b) of Figure C.1.2 suggests that female earnings should have grown too in absence of the pandemic. The observed stagnation may originate from the prolonged inactivity during the lockdown or from the shift to unskilled sectors and into self-employment, but we are not powered enough to draw definitive conclusions.

Table 3.6 reports the β_y coefficients from Equation 3.1, measuring the evolution of the gender gap for each of our main outcomes. Column (1) confirms that, pre-pandemic, there was no gender gap in employment. A 16.6 p.p. gender gap emerged with the first lockdown in May 2020, and grew to 25.5 p.p. in July 2020 despite the easing of the restrictions. By December 2020, the gap had reduced to 8.5 p.p., but it widened again during the second lockdown in July 2021, when it reached 19.4 p.p. Column (2) shows that wage employment contributes 11.8 p.p. (71%) of the new employment gender gap in May 2020 and the total of the gap afterwards. Column (3) shows that self-employment contributes the remaining 4.7 p.p. (29%) of the original employment gap. However, the gap in self-employment had disappeared and switched sign by July 2020, as more and more women set up their businesses following job losses. In September 2021, women were 9.7 p.p. more likely to be self-employed than men. Columns (4) and (5) show the evolution of the gender gap in the employment rate in the training sector, unconditionally and conditional on employment respectively. The former ranges between 14 and 24 p.p. during the pandemic; the latter between 3.6 (insignificant) and 24 p.p. Columns (6) and (7) show the evolution of the gender gap in employment rate in skilled sectors. Unconditionally, the gap in skilled employment ranges between 11 and 25 p.p. during the pandemic; conditional on employment, the gap ranges between 1.7 (insignificant) and 12 p.p. Columns (8) and (9) report the estimates of the earnings gender gap over time, unconditionally ad conditional on gender respectively. Consistent with the graphical evidence, we observe a widening of the gap only in December 2020. The gap in unconditional earnings ranges between \$38 and \$50; conditional on employment, the gap ranges between \$33 and \$49. These findings confirm that the two lockdowns implemented in Uganda had long-lasting gendered consequences on the employment, employment type, sectorial distribution, and earnings of these economically empowered women.

Last, we investigate with t-tests by gender whether the lockdowns had gendered effects on working hours, the need to borrow and to sell assets as a coping strategy, and mental health, and present suggestive evidence of these effects in Table C.1.2. Panel (a) shows that, conditional on employment, female wage employees were 24.6 p.p. more likely than male ones to report they reduced working hours in May 2020. In July 2020, women were still 11.3 p.p. more likely than men to report they were working in a business with reduced hours of operation. Although differences in working hours had dissipated by December

2020, they seemingly reemerged around the second lockdown in July 2021, when women reported working 0.4 marginally insignificant fewer hours per day than men. In panel (b) we investigate our respondents' need to borrow money during the pandemic. We find no gender differences in borrowing initially, but self-employed women were 9.8 p.p. more likely than their male counterparts to borrow money to cope with the second lockdown. Panel (c) shows that men and women were equally likely to sell assets, and panel (d) finds that women were persistently more likely than men to report being anxious because of the pandemic: *fear of infection* and *fear of losing employment* were the main sources of their worsened mental health. This result is in line with Bau et al. (2021), which shows that COVID-19 containment measures induced substantial reductions in female well-being in India.

In sum, tracking the labor market outcomes of a sample of young and skilled Ugandan workers during the COVID-19 pandemic reveals, first, that women suffered from disproportionate job losses. Almost half of the previously employed women failed to stably re-enter employment, driving the persistence of a previously inexistent employment gender gap of 20 p.p. for eighteen months. Second, we find that the disproportionate job losses experienced by female wage-employees resulted in a more pronounced shift towards self-employment. Third, we document a disproportionate displacement of female workers from their training sector towards agriculture and other unskilled sectors in which they can no longer leverage their comparative advantage. Fourth, we observe a widening of the gender pay gap. The sharp and simultaneous decline in female employment during both lockdowns, paired with the strong attachment to the labor market signaled by our female respondents through VTI enrollment, suggest we would almost certainly have not observed these dynamics in the absence of the pandemic. The sectorial misallocation we document may bring to a disproportionate depreciation of women's productive skills accumulated during vocational education. And the endurance of these new gender disparities in the labor market for eighteen months since the onset of the pandemic suggest they will all persist beyond the end of our study period.

3.4.3 Robustness Tests

We test the stability of our findings in several ways. Figures C.1.4, C.1.5, and Table C.1.3 illustrate the emergence and persistence of gender gaps in the main outcomes in the balanced panel of respondents. Together with the overall similarity of Ever and Never Attritors at baseline documented in Table 3.5, this evidence indicates that the observed gaps in labor market outcomes are not driven by compositional changes in the sample over time, but rather reflect true labor market dynamics for our workers. Table C.1.4 reports several bounds to our estimated employment gender gap to investigate its sensitivity to different assumptions about the employment status of attritors, following Horowitz and Manski (2006) and Kling et al. (2007). A considerable gender gap in employment emerges even in the unlikely, lower bound scenario in which all the female attritors and none of the male attritors are employed, although it becomes smaller and insignificant over time. We then test the sensitivity of the gender gaps in employment in the training sector and in skilled sectors under a range of assumptions about attritors' employment and sector. Such gaps emerge in 88% and 63% of the scenarios respectively. The four cases in which the gaps disappear are the most pessimistic scenarios for men and the least pessimistic for women. In these scenarios, all male and female attritors are, respectively, unemployed and employed in the training sector; employed outside the training sector and employed in the training sector or unemployed; employed in an unskilled sector and employed in a skilled sector. The robustness of our findings in most scenarios and the overall similarity between attritors and non-attritors at baseline make us confident that these four cases are the least likely among those considered, and that none of our result is driven by attrition. Then, Figure C.1.6 shows similar employment patterns for different cohorts of women, indicating that fertility choices happening at fixed distance from graduation do not confound our results. Figure C.1.7 also shows that the two lockdowns have similar effects on respondents differing by baseline characteristics other than gender, highlighting the gendered nature of these dynamics and pointing towards a broader generalizability of our findings. Last, Figure C.1.8 shows that our findings are not driven by sector-specific shock, as employment patters remain similar after removing from the sample respondents from one training sector at a time.

3.4.4 Where is the new and persistent employment gap coming from?

3.4.4.1 The role of employment sectors

During the first lockdown, the government suspended economic activity in sectors either deemed non-essential or involving close interactions with clients. We test the hypothesis that the pre-pandemic sorting of women in these sectors contributed to the emergence and persistence of the observed employment gender gap.

In panel (a) of Figure 3.6 we plot the sectors in which our workers were employed pre-pandemic along two dimensions: the share of female workers in each sector and the share of employed workers whose business were closed during the first lockdown in May 2020. The figure shows that economic sectors are highly segregated by gender: sectors such as tailoring, teaching, hairdressing and secretary employ almost only female workers; sectors like motor-mechanics, plumbing, electrical work and construction remain traditionally male-concentrated sectors. The same level of segregation occurs in the Ugandan labor market overall (columns 2 and 3, Table 3.3). Consistent with women's pre-pandemic sorting in sectors subject to the strongest restrictions, we observe a strong positive relationship between the share of businesses closed during the first lockdown and the share of female workers in each sector.

Figure C.1.9 shows that such relationship was still positive in July 2020, despite all restrictions had been lifted. By May 2021 the curve had almost flattened, only to tilt again during the second lockdown in July 2021 even though businesses were not directly prevented from operating. A smaller rebound of labor demand and supply in female-dominated sectors may explain these dynamics. Fear of infection may have pushed customers to postpone the consumption of non-essential services or shift to home production. The lower purchasing power registered among the (mostly) female clients of firms in female-dominated sectors, documented in our study by the lower female earnings as well as in other contexts (Dang and Viet Nguyen, 2021; Martinez-Bravo and Sanz, 2021; Hill and Köhler, 2021; Bau et al., 2021) may have further depressed the demand of female products and services. Moreover, women may have decided not to go back to work when presented with the possibility, due to the close interactions with clients in female-dominated sectors paired with their higher fear for the virus.

To rigorously assess the role of employment sectors over time, we reweight the female sample so that the distribution of female workers across sectors that were severely and mildly hit by initial closures matches that of male workers.[10] Since women were over-represented in severely hit sectors, this procedure assigns large weights to women previously employed in mildly affected sectors. Panel (b) of Figure 3.6 compares actual female and male employment rates with sector-reweighted female employment rate. The latter represents the female employment rate we would observe if, pre-pandemic, women were distributed across severely and mildly hit sectors as men. Sector-reweighted female employment rate is substantially higher than actual female employment rate during the first lockdown, but their distance declines over time. For each pandemic period, Table 3.7 measures the employment gender gap (Panel [a]) and quantifies the share of the gap explained by employment sectors (Panel [b]), calculated as the ratio of the gap between sector-reweighted and actual female employment rates and the gap between male and female employment rates. We concurrently show that this procedure is practically equivalent to calculating the share of the gender gap explained by different endowments using the standard decomposition proposed by Oaxaca (1973) and Blinder (1973) and the respondent's pre-pandemic employment in severely hit sectors as explanatory variable. Initial closures in economic sectors explain 50% of the gap during the first lockdown in May 2020. Their contribution gradually declines following the easing of the restrictions, but rises once again during the second lockdown in July 2021, when it reaches 13%.

Because these economic sectors in Uganda are segregated by gender, there may be other unobserved sectorial characteristics, such as differences in reopening times or in the rebound of labor demand, that account for the residual part of the gap but are inseparable from gender. To test this hypothesis, Figure C.1.10 shows the average employment rate for male and female workers trained in single-gender or mixed-gender sectors. Men and women have the same average employment regardless of the gender composition of their sector, which is evidence against the existence of unobserved sectorial characteristics explaining the gender gap. The gendered employment dynamics we observe may still be driven by the systematic assignment of women and men to different tasks within sectors. We cannot test this hypothesis directly, but the high degree of specialization of our respondents and the absence

[10] We reweight the female sample so that the average of Hit Sector$_i$ matches the male sample average. Hit Sector$_i$ is an indicator equal to one for respondents that pre-pandemic were employed (or trained, if non-employed) in a sector in which more than 50% of our respondents' pre-pandemic businesses were closed during the first lockdown: motor-mechanics, food and hospitality, tailoring, hairdressing, teaching, secretary, and retail. Weights are equal to one for men.

of gender disparities in baseline employment quality point against this supposition.

3.4.4.2 The role of childcare responsibilities

That the availability and cost of childcare affect adult labor supply and business profitability for women is widely documented (Heath, 2017; Delecourt and Fitzpatrick, 2021; Bjorvatn et al., 2022). We therefore investigate the contribution of childcare responsibilities, magnified by the prolonged schools' closure, to the emergence and persistence of the employment gender gap. We use the number of school-age children in the household as a proxy for a respondent's childcare responsibilities. This measure, following Alon et al. (2022), takes into account that our respondents may live with other young family members, such as siblings, cousins, nieces and nephews, on top of their own children. Additionally, as our sample is relatively young and the suspension of pre-primary schooling throughout the study period was especially salient, we define school-age children as children aged three or more. Using this definition, 42% of the respondents live with school-age children. Panel (a) of Figure 3.7 shows female and male average employment rates by the number of school-age children in the household in periods in which schools were open (pre-pandemic) and closed (post-pandemic). Female employment declines with the number of school-age children in the household, but only during schools' closure: the presence of one child reduces female employment by 5 p.p.; additional children further reduce it by 5 p.p. Conversely, male employment does not change with the number of school-age children they live with neither when schools are open nor when they are closed. We formally test for these differences by regressing employment on a constant and indicators for zero (omitted category), one, and two or more school-age children in the household separately for men and women. We report the estimated coefficients in Table C.1.5. Columns (3) and (4) show that differences in employment for men living with and without school-age children are small (0.1—2.8 p.p.) and insignificant regardless of schools' closure. By contrast, columns (9) and (10) show that female employment declines more steeply with the number of school-age children in the household in periods in which schools are closed relative to periods in which they are open. Women living with any number of school-age children are 4.9—5.6 insignificant p.p. less likely to be employed than women with none when schools are open. When schools are closed, relative to women living with no school-age children, women living with one are 5.4 insignificant p.p. less likely to be employed, and women living with two or more are 9.6 significant (at 5% level) p.p. less likely to be employed. The same patterns emerge when using the number of children aged six or more, hence attending primary or secondary schools, as alternative proxy for childcare responsibilities (columns [5], [6], [11], and [12]). When we use the total number of children in the household to consider the possibility that babysitting services for younger kids became inaccessible during the pandemic, we find that female employment declined with childcare responsibilities in a similar way regardless of schools' closure (columns [1], [2], [7], and [8]). This pattern corroborates our hypothesis that the prolonged closure of schools is the main driver of the observed dynamics. Additionally, as a given number of children may reflect different household compositions, we plot in Figure C.1.11 average female and male employment by bins of the ratio of the number of school-age children to the number of adults in the household. Our results remain consistent when incorporating the presence of other adults in the household with whom the respondent may share childcare responsibilities. Overall, this evidence corroborates the self-reported experience of men and women that schools' closure disproportionately limited women's ability to work due to the magnified childcare duties it generated (Panel [e] of Table C.1.2).

To quantify the contribution of childcare responsibilities to the emergence and the persistence of the employment gender gap, we reweight the female sample so that the proportions of respondents with zero, one, and more than one school-age children in the household match those in the male sample. Panel (b) of Figure 3.7 compares female and male actual employment rates with children-reweighted female employment rate. The latter represents the female employment rate we would observe if women lived with the same number of school-age children as men. The figure shows that children-reweighted female employment rate is similar to actual female employment rate in the early stage of the pandemic but becomes higher over time. Panel (c) of Table 3.7 calculates the share of the employment gender gap explained by different childcare responsibilities by gender. This share is obtained, first, by dividing the gap between children-reweighted and actual female employment rates by the gap between male and female employment rates, and, second, as the share of the gender gap explained by different endowments with a Oaxaca-Blinder decomposition in which we use indicators for living with zero, one and two or more school-age children as explanatory variables. The two methods consistently show that different childcare responsibilities have no explanatory power around the first lockdown, but explain between 11 and 24% of the employment gender gap from December 2020 onwards. These estimates would represent a lower bound for the true contribution of childcare responsibilities if our proxy, the number of school-age children in the household, underestimated true responsibilities for women and overestimated them for men. Overall, this evidence points towards initial job losses being mostly unrelated to schools' closure, which contrarily limited females' employment in the longer run.

Consistent with findings from the US (Hansen et al., 2022) and Kenya (Biscaye et al., 2022), we expect female employment to increase following the reopening of schools in January 2022. However, the fact that the Kenyan labor supply response was partly driven by the fall in agricultural child labor, the small portion of the employment gap explained by childcare responsibilities in our sample, and the 15-p.p. employment gap among respondents living with zero school-age children shown in panel (a) of Figure 3.7, together suggest that in our urban context the employment gap will not close following the reopening.

3.4.4.3 The residual employment gender gap

Despite their extensive contribution, neither employment sectors nor childcare responsibilities manage to fully explain the employment gender gap in any period. We thus turn to investigating their joint contribution. As a first approach, we sum in each period the individual contributions of these two factors, reported in Panels (b) and (c) of Table 3.7, whenever they are both positive. The sum gives a sensible estimate of the joint contribution of employment sectors and childcare responsibilities so long as these two factors are independent in the female sample. If women with fewer school-age children were mostly employed in mildly hit sectors pre-pandemic, we would overestimate the share of the gap explained by each factor individually and hence, by taking their sum, their joint contribution. If women with fewer school-age children were mostly employed in severely hit sector, we would underestimate the share of the gap explained by each factor individually and thus their joint contribution. Figure C.1.12 illustrates that employment sectors and childcare responsibilities are independent among women. Panel (a) shows that the distribution of school-age children in the household is almost identical in the original female sample and in the sample of women reweighted by sector; panel (b) that the distribution of respondents in severely and mildly hit sectors is almost identical in the original female sample and in the female sample reweighted by childcare responsibilities. As childcare responsibilities contribute negatively to the employment gender gap in May and July 2020, and as employment sectors contribute negatively to the gap in December 2020 and May 2021, the sum of the independent contributions of childcare responsibilities and employment sectors is only informative in July and September 2021, during and after the second lockdown. Together, childcare responsibilities and employment sectors explain 37% and 21% of the employment gender gap in these periods. Based on this approach, between 50% and 79% of the gap remains unexplained by these two factors.

A second approach to calculating the joint contribution of employment sectors and childcare responsibilities consists in comparing female employment rate to the employment rate of counterfactual female respondents with the most advantageous traits in terms of both employment sectors and childcare responsibilities. Figure 3.8 shows, together with female and male employment rates, the employment rates of women in mildly hit sectors, reweighted to match men's distribution of school-age children in the household, and of women with no school-age children in the household, reweighted to match men's distribution across severely and mildly hit sectors. By relying on the smaller samples of women employed in mildly hit sectors and women with no school-age children in the household, this approach delivers relatively more imprecise estimates. As a result, in some periods we cannot reject the hypothesis that the employment rates of these counterfactual women are equal to both female and male employment rates. Despite the relatively lower power of this analysis, the point estimates suggest that an employment gender gap emerged and endured over time even for these highly advantaged counterfactual women. Panel (d) of Table 3.7 calculates the share of the employment gender gap jointly explained by employment sectors and childcare responsibilities in each period by dividing the difference between the employment rates of children-reweighted women working in mildly hit sectors and women by the employment gender gap (option 1), and by dividing the difference between the employment rates of sector-reweighted women without school-age children and women by the employment gender gap (option 2). Consistent with the large role played by employment sectors during the lockdowns, the employment rate of children-reweighted women working in mildly hit sector lies above female employment rate in May 2020 (lockdown 1), when 65% of the employment gender gap disappears, and in July 2021 (lockdown 2), when 39% disappears. In the remaining periods, the difference between the counterfactual and original female respondents becomes smaller and negligible. And consistent with the larger contribution of childcare responsibilities in the later pandemic periods, the employment rate of sector-reweighted women living with no school-age children lies above female employment rate in December 2020 and in July and September 2021, when 42%, 52% and 25% of the employment gender gap disappears respectively. In the earlier periods, counterfactual and original women behave similarly. We thus estimate that, in each period, between 35% and 100% of the employment gender gap remains unexplained by employment sectors and childcare responsibilities.

To identify additional contributors to the residual employment gender gap, Figure C.1.13 investigates the existence of heterogeneities in the gendered effect of the pandemic on employment rate by several baseline characteristics. Regardless of the dimension by which we split the female and male samples, and despite the lower power due to these additional divisions, we keep observing the same employment dynamics. The absence of heterogeneities by own and household asset ownership suggests that the decline in female employment is not driven by women who could not afford childcare or earning less than their partners. Additionally, there is no heterogeneity by fear of infection. Alternative explanations, then, include women complying more with COVID-19 restrictions (Galasso et al., 2020; Oreffice and Quintana-Domeque, 2021), employers' discrimination in layoffs and hirings, and social norms reducing female attachment to the labor market (Jayachandran, 2020). Understanding the residual forces behind the rise and the persistence of the employment gender gap is essential to design effective countermeasures.

3.5 Conclusions

In this chapter I analyze the gendered labor market effects of the COVID-19 induced restrictions on a sample of young and skilled Ugandan workers and entrepreneurs employed in a wide range of vocational industries. With a unique high-frequency panel dataset spanning from January 2020 to September 2021, together with my coauthors, we identify short- and long- term responses to two lockdowns implemented in Uganda. These restrictions disproportionately reduced female employment, shifted female workers into self-employment and into sectors misaligned with their skill sets, and widened the gender pay gap. While men quickly restored their pre-pandemic labor market trajectories, almost half of the previously employed women found more precarious occupations or became jobless. Together, our findings indicate that hard-earned progress towards women's employment and earnings parity can be set back by temporary shocks. To explain the uneven impact and recovery, we decomposed the employment gender gap to quantify the role of pre-epidemic sorting in severely hit sectors and increased childcare responsibilities due to schools' closure. These factors explain up to 65% of the employment gap; the rest remains unexplained, creating additional barriers to devise effective countermeasures.

Our sample represents a small yet growing share of the Ugandan population. Given the importance of this population for the country's transition into a middle-income economy, the persistence of an employment gap eighteen months from the COVID-19 shock should be of great concern to policymakers. The decline in female skilled employment and the sectorial misallocation induced by the pandemic may slow the country's structural transformation. Given the precarious nature of economic development, Uganda's stakeholders should prioritize policies supporting women seeking to reenter the labor market and provide targeted support for enterprises in sectors with higher female representation.

Main Findings

Figure 3.1: Project Timeline

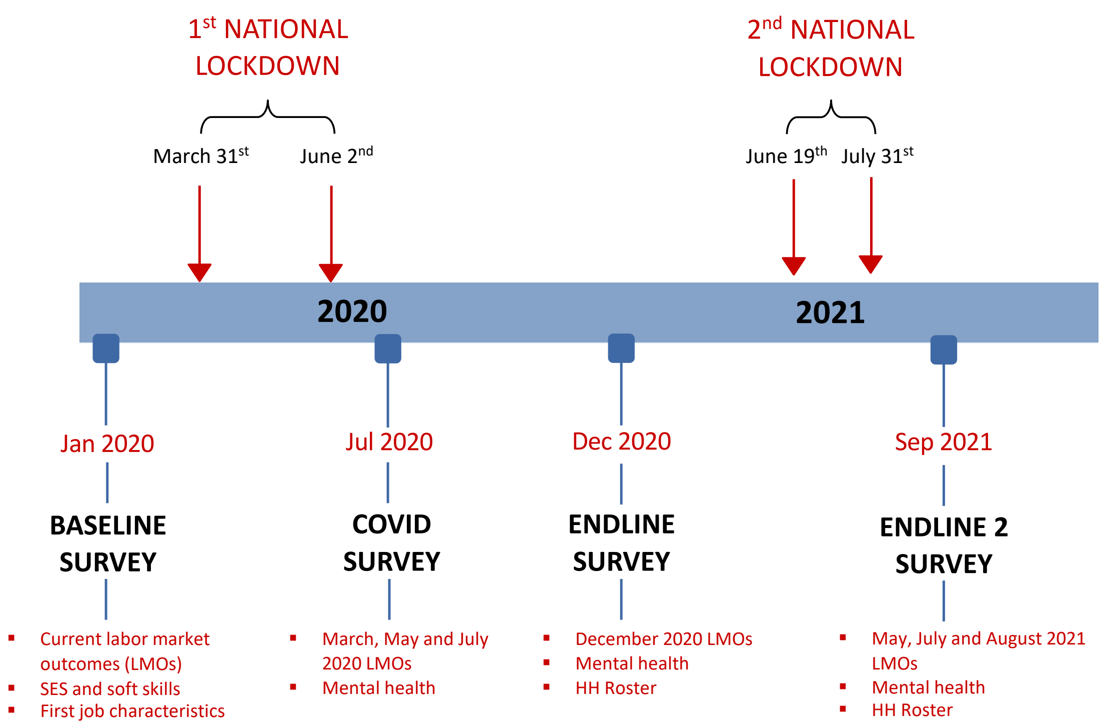


Table 3.1: Baseline Summary Statistics and Balance Table

	All		***Female***		***Male***		
	Mean	SD	Obs	Mean	Obs	Mean	P-value
Panel A: Socio-economic characteristics							
Female	.41	.49	295	1.00	419	.00	.
Age	25.01	3.22	291	24.11	418	25.63	.00
Married	.36	.48	171	.35	232	.37	.68
Has children	.47	.50	218	.51	338	.44	.13
Number of school-age children in the household	.87	1.26	215	1.22	338	.64	.00
Traditional religious denomination	.75	.43	289	.71	414	.77	.07
Ethnic minority	.44	.50	289	.42	414	.45	.48
House of origin: rural	.51	.50	230	.48	332	.53	.27
Region of origin: central	.37	.48	290	.41	415	.34	.05
Region of origin: eastern	.43	.50	290	.40	415	.45	.21
Region of origin: northern	.12	.32	290	.11	415	.12	.61
Region of origin: western	.08	.27	290	.07	415	.09	.49
Caretaker's years of education	10.17	5.18	190	10.63	272	9.85	.11
Agricultural household of origin	.19	.39	286	.20	411	.18	.60
Household of origin asset index	.00	4.95	291	.02	414	-.02	.91
Panel B: Labor market characteristics							
Years since graduation	3.11	2.19	292	2.86	412	3.29	.01
Years employed since graduation	2.74	2.20	225	2.59	324	2.84	.18
Years in current job	2.33	1.75	164	1.98	258	2.55	.00
Wage employed	.56	.50	282	.53	409	.57	.30
Self employed	.21	.41	282	.23	409	.20	.40
Has permanent job	.79	.41	147	.86	224	.74	.00
Works in or owns registered firm	.46	.50	203	.48	302	.45	.54
Employed in Skilled Sector	.65	.48	282	.64	407	.66	.61
Employed in Skilled Sector | Employed	.85	.36	214	.85	316	.85	.86
Employed in Training Sector	.57	.50	282	.54	407	.58	.27
Employed in Training Sector | Employed	.74	.44	214	.71	316	.75	.33
Earnings (USD)	65.96	71.57	178	52.50	253	75.43	.00
Earnings (USD) | Employed	104.52	63.89	110	84.95	162	117.81	.00
Enrolled in further education	.05	.22	282	.05	409	.05	.80
Engaged in casual occupations	.05	.22	282	.03	409	.07	.05
Other non-employed	.13	.34	282	.16	409	.11	.09

Notes: The table reports summary statistics and tests for gender differences in means for a set of socio-economic and labor market characteristics measured at baseline (January 2020). There are few exceptions: the indicator for whether the respondent has children is measured in July 2020; the indicator for whether the respondent is married is measured in December 2020; the number of school-age children in the household is measured in September 2021. School-age children are children aged three or more. The ethnic minority indicator takes value one for respondents who do not belong to the Muganda or Musoga tribes but to one of 35 other tribes. The traditional religious denominations indicator takes value one for respondents belonging to the Anglican, Muslim or Catholic faith. The caretaker education level is calculated as the highest educational level among the two main caretakers the respondent had while growing up. The respondent's household of origin is considered as "agricultural" if its main source of income is subsistence or commercial agriculture. Years employed since graduation are equal to years since graduation minus the respondent's longest unemployment spell since graduation. Wage employed-respondents either have a permanent job or are temporary hires, casual workers, or volunteers. Skilled sectors include motor-mechanics, plumbing, hospitality, hairdressing, construction, electrical work, welding, carpentry, teaching, secretary and accounting, machining and fitting, and a residual skilled category ("Other skilled"). Unskilled sectors include agriculture, retail, and a residual category ("Other unskilled"). "Other skilled" includes the following occupations: painting (walls, buildings), sales and marketing, office work for the government, a company, or a NGO, other business work, IT technician, medical doctor, nurse, police and army, photographer, gardener, banking, veterinary, journalist. "Other unskilled" includes: boda boda/taxi driver, street vendor, street food maker, market vendor, gate keeper/guard, factory work, cleaner/housemaid, transport, printing, driver. Casual occupations include: agricultural day labor, (un)loading trucks, transporting goods on bicycle, fetching water, land fencing, slashing someone's compound, and all occupations in which neither principal nor agent had an active working relationship, neither held any contractual obligations toward the other, and the principal requested agent on a need-based basis. "Other non-employed" includes individuals without an occupation. Within this category, we cannot distinguish the unemployed from not economically active individuals.

Table 3.2: Comparing the Study Sample with Ugandan Youths and Ugandan Young Vocational Graduates

	(1) Mean Young Adults UNHS	(2) Mean VTI Graduates UNHS	(3) Mean Study Sample	(4) Difference (3)-(1)	(5) P-value (3)-(1)	(5) Difference (3)-(2)	(6) P-value (3)-(2)
Full sample							
Female	.410	.410	.413	.000	.999	.000	.998
Age	25.021	25.014	25.008	-.013	.918	-.006	.976
Married	.595	.468	.362	-.229***	.000	-.102***	.003
Completed primary school	.620	1.000	1.000	.380***	.000	-.000	1.000
Completed secondary school	.182	1.000	1.000	.818***	.000	-.000	1.000
Completed vocational training	.051	1.000	1.000	.949***	.000	-.000	1.000
Any work in last 7 days - no Ag	.476	.690	.742	.265***	.000	.052**	.075
Any work in last 7 days - Ag included	.782	.797	.767	-.016	.335	-.030	.251
Monthly earnings (USD) - wage employed	71.174	89.940	104.518	33.377***	.000	14.611**	.024
Female sample							
Age	24.113	24.115	24.113	-.000	1.000	-.001	.997
Married	.671	.561	.351	-.314***	.000	-.204***	.000
Completed primary school	.587	1.000	1.000	.413***	.000	-.000	1.000
Completed secondary school	.142	1.000	1.000	.858***	.000	-.000	1.000
Completed vocational training	.046	1.000	1.000	.954***	.000	-.000	1.000
Any work in last 7 days - no Ag	.328	.617	.745	.415***	.000	.126***	.004
Any work in last 7 days - Ag included	.692	.704	.759	.066**	.013	.054	.204
Monthly earnings (USD) - wage employed	55.318	77.090	84.948	29.532***	.000	7.760	.534
Male sample							
Age	25.632	25.632	25.632	-.000	.999	-.000	.999
Married	.563	.418	.371	-.190***	.000	-.046	.319
Completed primary school	.652	1.000	1.000	.348***	.000	.000	1.000
Completed secondary school	.212	1.000	1.000	.788***	.000	.000	1.000
Completed vocational training	.056	1.000	1.000	.944***	.000	.000	1.000
Any work in last 7 days - no Ag	.585	.746	.741	.155***	.000	-.005	.900
Any work in last 7 days - Ag included	.847	.863	.773	-.075***	.000	-.091***	.005
Monthly earnings (USD) - wage employed	77.622	97.513	117.807	40.185***	.000	20.294***	.008

Notes: The table compares our sample with the population of Ugandan adults aged 18–39 and the subpopulation that completed post-secondary vocational education from the Uganda National Household Survey 2016/2017 (UNHS). The table reports sample means for a set of socio-economic and labor market characteristics, differences in means across the samples, and p-values from the tests that the differences in means are statistically different from zero. The UNHS samples of young adults and VTI graduates are reweighted so that their age and gender distribution matches that of the study sample. The variable "Any work in the last seven days" refers to individuals who worked for pay, run a business, helped out in business or were apprentices in the previous week. In the UNHS, average monthly earnings are available only for wage employed respondents.

Table 3.3: Sector Relevance and Gender Composition Nationwide

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Young Adults UNHS			VTI Graduates UNHS			Study Sample		
	% All	% Female	% Male	% All	% Female	% Male	% All	% Female	% Male
Food and hospitality	.044	.524	.476	.049	.349	.651	.122	.757	.243
Tailoring	.006	.600	.400	.006	.794	.206	.073	.976	.024
Electrical work	.001	.115	.885	.006	.218	.782	.174	.070	.930
Motor-mechanics	.011	.072	.928	.016	.041	.959	.162	.043	.957
Construction	.037	.004	.996	.035	.016	.984	.051	.103	.897
Plumbing	.001	.000	1.000	.003	.000	1.000	.075	.047	.953
Retail	.137	.441	.559	.133	.637	.363	.077	.545	.455
Secretary and accounting	.006	.408	.592	.011	.591	.409	.037	.905	.095
Teaching (pre-primary and primary)	.024	.470	.530	.171	.495	.505	.085	.898	.102
Hairdressing	.013	.425	.575	.019	.593	.407	.031	.889	.111
Agriculture	.528	.444	.556	.158	.320	.680	.030	.235	.765
Machining and fitting	.006	.034	.966	.012	.000	1.000	.007	.250	.750
Other unskilled	.099	.153	.847	.141	.204	.796	.042	.333	.667
Other skilled	.086	.270	.730	.240	.380	.620	.035	.350	.650

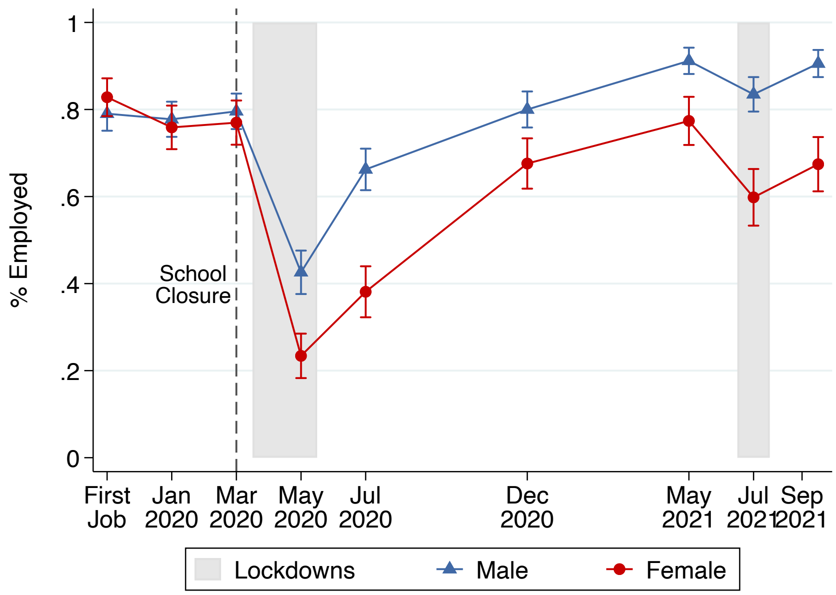
Notes: The table compares our sample (columns [7], [8] and [9]) with the population of Ugandan adults aged 18–39 (columns [1], [2] and [3]) and the subgroup that completed post-secondary vocational education (columns [4], [5] and [6]) from the Uganda National Household Survey 2016/2017 (UNHS). Columns (1), (4) and (7) show the percentage of the considered population employed in each sector of the economy. Columns (2) and (3), (5) and (6), (8) and (9) show the gender composition of the considered population in each sector. The UNHS samples of young adults and VTI graduates are reweighted so that their age and gender distribution matches that of the study sample.

Table 3.4: Attrition Magnitude and Timing by Gender

		(1)		(2)	T-test
		Female		Male	Difference
Variable	N	Mean/SE	N	Mean/SE	(1)-(2)
Interviewed in Jan 2020	295	0.983	419	0.995	-0.012
		(0.008)		(0.003)	
Interviewed in Jul 2020	295	0.851	419	0.869	-0.018
		(0.021)		(0.017)	
Interviewed in Dec 2020	295	0.776	419	0.792	-0.016
		(0.024)		(0.020)	
Interviewed in Sep 2021	295	0.749	419	0.811	-0.062**
		(0.025)		(0.019)	

Notes: The table reports summary statistics and tests for gender differences in means for four indicators summarizing the presence of the respondent in each of the four survey rounds.

Figure 3.2: The Emergence and Persistence of a Gender Gap in Employment After the Lockdowns



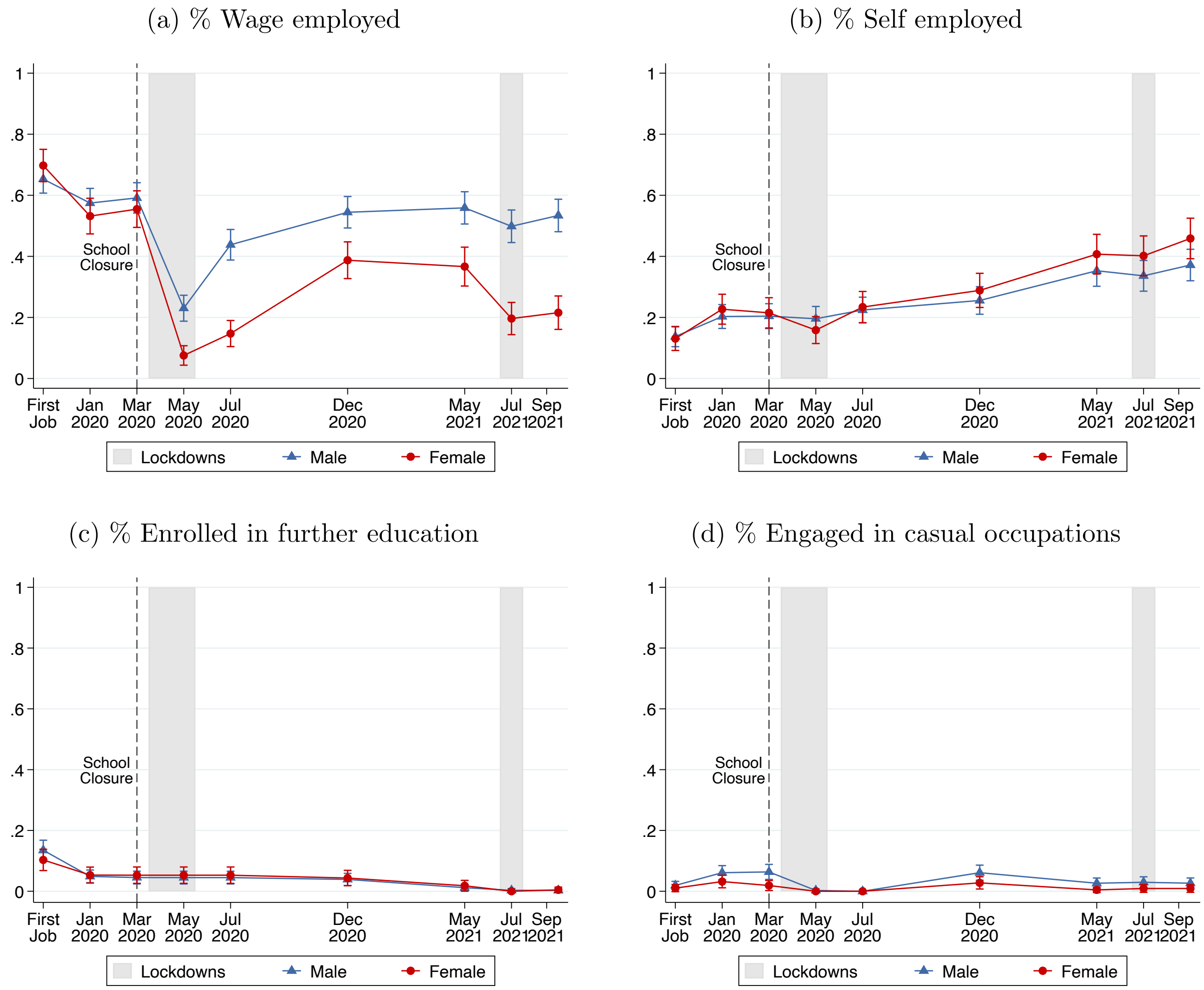
Notes: The figure illustrates the average share of respondents that are employed over time and by gender. At each point in time, a respondent is coded as employed if her main activity is either wage- or self-employment. The first data point refers to the respondents' first activity after completing vocational education. It may coincide with the activity in January 2020 and its start and end date may be different for each respondent. It can be interpreted as the share of individuals who ever worked after completing vocational education. 95% robust confidence intervals are reported.

Table 3.5: Ever and Never Attritors' Baseline Characteristics by Gender: Summary Statistics and Balance Tests

	Male Sample						Female Sample					
	Ever Attritors		*Never Attritors*				*Ever Attritors*		*Never Attritors*			
	Obs	Mean	Obs	Mean	Diff	p-value	Obs	Mean	Obs	Mean	Diff	p-value
Panel A: Socio-economic characteristics												
Age	135	25.793	283	25.555	.238	.489	118	24.059	173	24.150	-.091	.797
Married	41	.439	191	.356	.083	.331	46	.304	125	.368	-.064	.432
Has children	56	.482	282	.436	.046	.531	46	.543	172	.500	.043	.601
Num. school-age children in household	56	.482	282	.674	-.192	.202	44	1.295	171	1.199	.097	.658
Traditional religious denomination	131	.779	283	.770	.008	.851	116	.664	173	.746	-.082	.139
Ethnic minority	131	.427	283	.459	-.032	.544	116	.345	173	.474	-.129**	.028
House of origin: rural	49	.551	283	.527	.025	.751	57	.421	173	.503	-.082	.283
Region of origin: central	134	.425	281	.299	.126**	.013	119	.420	171	.409	.011	.855
Region of origin: eastern	134	.381	281	.484	-.103**	.046	119	.454	171	.368	.085	.148
Region of origin: northern	134	.090	281	.139	-.049	.127	119	.059	171	.146	-.087**	.012
Region of origin: western	134	.104	281	.078	.026	.398	119	.067	171	.076	-.009	.775
Caretaker's years of education	82	8.915	190	10.253	-1.338*	.064	79	10.595	111	10.658	-.063	.932
Agricultural household of origin	129	.225	282	.160	.065	.129	114	.184	172	.203	-.019	.686
Household of origin assets index	131	.421	283	-.219	.640	.143	118	.151	173	-.063	.213	.700
Panel B: Labor market characteristics												
Years since graduation	129	3.481	283	3.201	.279	.230	119	2.588	173	3.052	-.464	.060*
Years employed since graduation	44	2.989	280	2.820	.169	.611	52	2.367	173	2.654	-.287	.403
Years in current job	81	2.765	177	2.452	.313	.264	59	2.153	105	1.886	.267	.304
Wage employed	131	.580	278	.568	.012	.822	112	.491	170	.559	-.068	.267
Self employed	131	.229	278	.187	.042	.337	112	.259	170	.206	.053	.308
Permanent job	73	.849	151	.682	.167***	.004	53	.887	94	.851	.036	.534
Formal firm	100	.490	202	.431	.059	.333	80	.512	123	.455	.057	.428
Employed in Skilled Sector	131	.649	276	.667	-.018	.725	112	.598	170	.671	-.072	.220
Employed in Skilled Sector | Employed	106	.802	210	.876	-.074*	.100	84	.798	130	.877	-.079	.134
Employed in Training Sector	131	.557	276	.598	-.041	.441	112	.500	170	.571	-.071	.247
Employed in Training Sector | Employed	106	.689	210	.786	-.097*	.070	84	.667	130	.746	-.079	.218
Earnings (USD)	83	81.144	170	72.645	8.499	.419	74	56.491	104	49.653	6.838	0.446
Earnings (USD) | Employed	58	116.120	104	118.747	-2.627	.815	46	90.877	64	80.686	10.190	.314
Enrolled in further education	131	.031	278	.058	-.027	.190	112	.036	170	.065	-.029	.263
Engaged in casual occupations	131	.053	278	.072	-.019	.461	112	.018	170	.041	-.023	.240
Other non-employed	131	.107	278	.115	-.008	.804	112	.196	170	.129	.067	.143

Notes: The table reports summary statistics for a set of baseline socio-economic and labor market characteristics separately for "Ever Attritors", (i.e., respondents successfully interviewed in fewer than four survey rounds) and "Never Attritors", (i.e., respondents successfully interviewed in all the four survey rounds) and tests for differences between these two groups in the samples of male and female respondents. See the notes to Table 3.1 for details on how the variables are constructed.

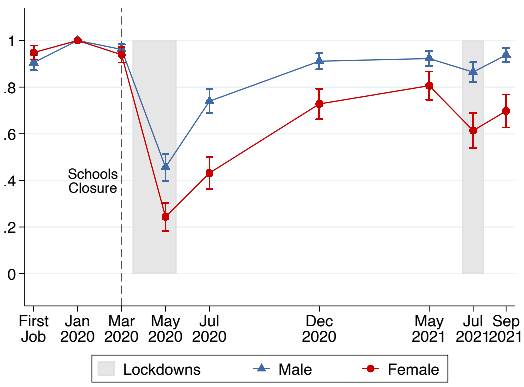
Figure 3.3: The Emergence and Persistence of Gender Disparities in Occupation Type After the Lockdowns



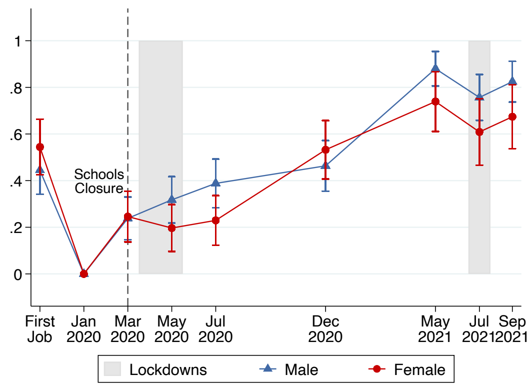
Notes: The figure illustrates the average share of respondents that are wage employed (panel [a]), self-employed (panel [b]), enrolled in educational programs (panel [c]), and engaged in casual occupations (panel [d]) over time and by gender. The first data point refers to the respondents' first activity after completing vocational education. It may coincide with the activity in January 2020 and its start and end date may be different for each respondent. 95% robust confidence intervals are reported.

Figure 3.4: The Drivers of the Recovery in Employment After the Lockdowns

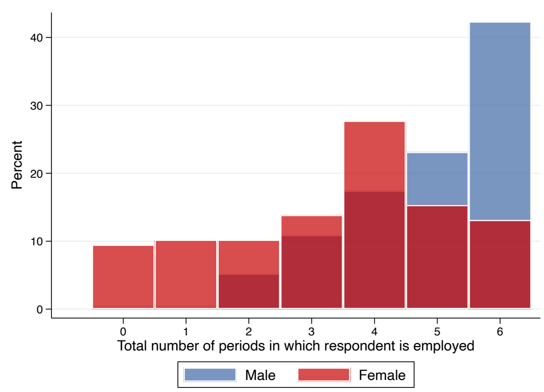
(a) Employed in Jan 2020: % Employed Over Time



(b) Not Employed in Jan 2020: % Employed Over Time



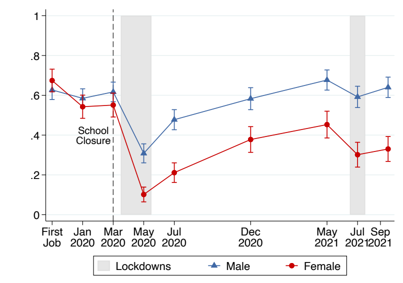
(c) Employment Frequency During the Pandemic Periods



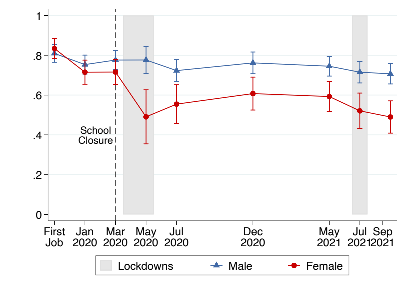
Notes: Panels (a) and (b) illustrate the average employment rate over time and by gender for the 532 respondents who were employed in January 2020 and the 159 respondents who were not employed in January 2020. Employed respondents were either wage- or self-employed. Non-employed respondents were either enrolled in educational programs, or engaged in casual occupations, or without an occupation. The first data point refers to the respondents' first activity after completing vocational education. It may coincide with the activity in January 2020 and its start and end date may be different for each respondent. 95% robust confidence intervals are reported. Panel (c) illustrates the share of female and male respondents employed in zero to six periods between May, July, and December 2020, and May, July, and September 2021. The sample is restricted to Never Attritors that were employed pre-pandemic (January and March 2020).

Figure 3.5: The Emergence and Persistence of Gender Disparities in Employment Quality After the Lockdowns

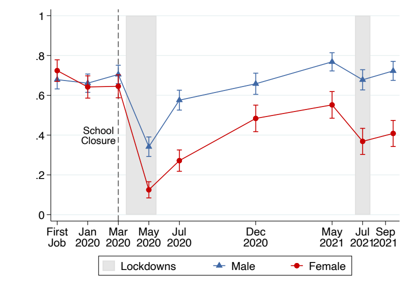
(a) % Employed in Training Sector



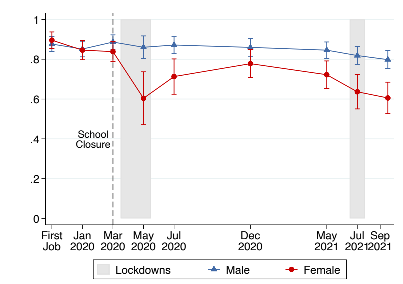
(b) % Employed in Training Sector | Employed



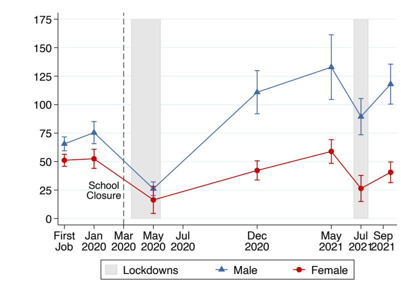
(c) % Employed in Skilled Sector



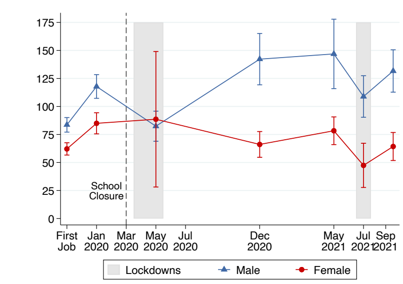
(d) % Employed in Skilled Sector | Employed



(e) Monthly Earnings (USD)



(f) Monthly Earnings (USD) | Employed

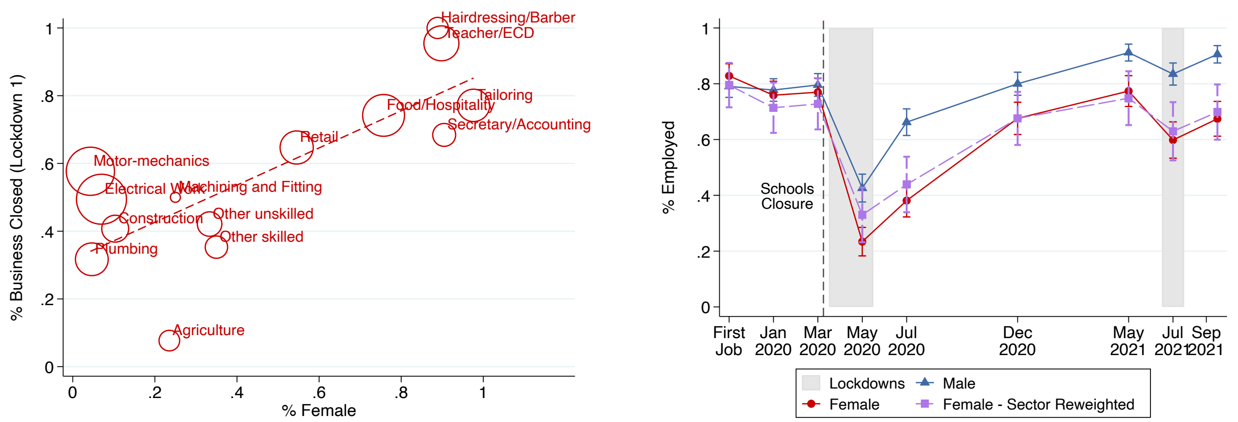


Notes: The figure illustrates the average employment rate in the training sector (upper panels) employment rate in skilled sectors (middle panels) and monthly earnings (lower panels) over time and by gender. See the notes to Table 3.1 for details on how the variables are constructed. In panels (a), (c), and (e), the outcome is set to zero for non-employed respondents, and the average outcome in each point of time is calculated over all respondents found in the corresponding survey round. In panels (b), (d), and (f), the outcome is missing for non-employed respondents, and the average outcome in each point of time is calculated over all the employed respondents. The first data point refers to the respondents' first job after completing vocational education. It may coincide with the job in January 2020 and its start and end date may be different for each respondent. Earnings data were not collected in March and July 2020. In January 2020 and May 2020 respondents placed their earnings in a 15 USD bracket. We use the range midpoint. From December 2020 onwards earnings were asked as a continuous variable. For self-employed workers, the variable measures monthly profits, collected following the same procedure. Results look very similar when we use the range midpoint throughout. 95% robust confidence intervals are reported.

Figure 3.6: The Contribution of Pre-pandemic Employment Sectors to the Employment Gender Gap

(a) Female Concentration in Severely Impacted Economic Sectors

(b) Measuring the Contribution of Employment Sectors



Notes: Panel (a) shows the economic sectors in which our respondents were employed pre-pandemic by the share of female workers hosted before the pandemic and the share of businesses that were closed during the first lockdown in May 2020. Markers are proportional to the number of workers employed in each sector before the pandemic. The slope of the fitted line is 0.55 (standard error: 0.12). See the notes to Table 3.1 for a detail of the occupations included in "Other Skilled" and "Other Unskilled". Panel (b) illustrates average employment rates over time for male, female, and sector-reweighted female respondents. Sector-reweighted female employment rate is equal to female employment rate when weighting the female sample so that the first moment of *Hit Sector*$_i$, an indicator for whether pre-pandemic the respondent was employed in a severely hit sector, matches that in the male sample. Weights are equal to one for male workers. Severely hit sectors are sectors in which more than 50% of the businesses in which our workers were employed pre-pandemic were closed during the first lockdown in May 2020: motor-mechanics, food and hotel, tailoring, hairdressing, teaching, secretary, and retail. 95% confidence intervals are reported.

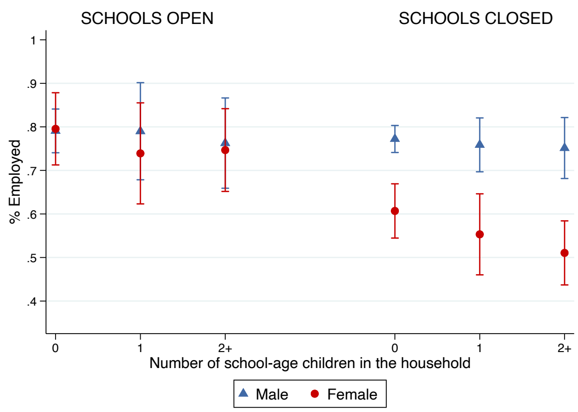
Table 3.6: The Emergence and Persistence of Gender Disparities in the Labor Market After the Lockdowns

Outcome:	(1) % Employed	(2) % Wage-employed	(3) % Self-employed	(4) Employed in Training Sector	(5) Employed in Training Sector | Employed	(6) Employed in Skilled Sector	(7) Employed in Skilled Sector | Employed	(8) Monthly Earnings (USD)	(9) Monthly Earnings (USD) | Employed
Female × First job	0.069*	0.070	-0.001	0.117***	0.070*	0.107***	0.067**	12.067	8.845
	(0.038)	(0.045)	(0.034)	(0.042)	(0.040)	(0.041)	(0.032)	(7.691)	(12.412)
Female × Jan 2020	0.007	-0.015	0.022	0.017	-0.003	0.031	0.027		
	(0.027)	(0.029)	(0.025)	(0.028)	(0.027)	(0.026)	(0.018)		
Female × May 2020 (Lckdn 1)	-0.166***	-0.118***	-0.047**	-0.137***	-0.070***	-0.156***	-0.073**	15.861	30.554
	(0.049)	(0.046)	(0.023)	(0.043)	(0.027)	(0.044)	(0.034)	(9.983)	(34.543)
Female × Jul 2020	-0.255***	-0.254***	-0.001	-0.202***	-0.036	-0.246***	-0.046**		
	(0.045)	(0.042)	(0.012)	(0.037)	(0.026)	(0.042)	(0.021)		
Female × Dec 2020	-0.085**	-0.129***	0.045	-0.131***	-0.050	-0.113***	-0.017	-47.243***	-49.418***
	(0.042)	(0.044)	(0.033)	(0.038)	(0.035)	(0.042)	(0.033)	(11.806)	(18.944)
Female × May 2021	-0.099**	-0.161***	0.062	-0.155***	-0.096*	-0.155***	-0.074*	-48.785***	-35.564**
	(0.048)	(0.052)	(0.046)	(0.051)	(0.053)	(0.049)	(0.044)	(16.567)	(17.608)
Female × Jul 2021 (Lckdn 2)	-0.194***	-0.270***	0.077	-0.215***	-0.111*	-0.244***	-0.121**	-37.791***	-33.051*
	(0.053)	(0.051)	(0.048)	(0.052)	(0.058)	(0.053)	(0.052)	(11.815)	(17.573)
Female × Sep 2021	-0.188***	-0.285***	0.097**	-0.239***	-0.133**	-0.243***	-0.122**	-50.157***	-35.451**
	(0.051)	(0.052)	(0.048)	(0.051)	(0.056)	(0.051)	(0.051)	(12.054)	(16.076)
Observations	5,615	5,615	5,615	5,391	3,764	5,391	3,764	3,772	2,575
R-squared	0.443	0.534	0.581	0.563	0.699	0.538	0.706	0.414	0.485
Mean Dep. Var. Pre-Shock	0.785	0.576	0.209	0.590	0.751	0.680	0.867	65.96	104.5
sd Dep. Var. Pre-Shock	0.411	0.495	0.407	0.492	0.433	0.467	0.340	71.57	63.89

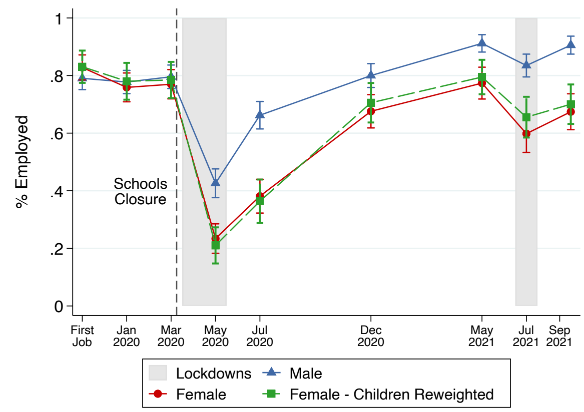
Notes: The table reports the β_y coefficients obtained estimating Equation 3.1 in the full sample. The dependent variables are an indicator for respondents that are employed (column [1]), wage-employed (column [2]), self-employed (column [3]), employed in their trainig sector, unconditional (column [4]) and conditional on employment (column [5]), employed in skilled sectors, unconditional (column [6]) and conditional on employment (column [7]), and monthly earnings in USD, unconditional (column [8]) and conditional on employment (column [9]). See the notes to Table 3.1 and Figure 3.5 for the details about how the variables were built. The coefficient on $Female \times Mar2020$ is normalized to zero for all outcomes except for monthly earnings, in which case the coefficient on $Female \times Jan2020$ is normalized to zero. The table reports the mean and the standard deviation of the dependent variable measured in March 2020 (columns [1]-[7]) and January 2020 (columns [8]-[9]). Standard errors are robust to heteroskedasticity and clustered at the individual level. * p<0.1, ** p<0.05, *** p<0.01.

Figure 3.7: The Contribution of Childcare Responsibilities to the Employment Gender Gap

(a) Gender Gap in Impact of Schools' Closure on Employment



(b) Measuring the Contribution of Childcare Responsibilities



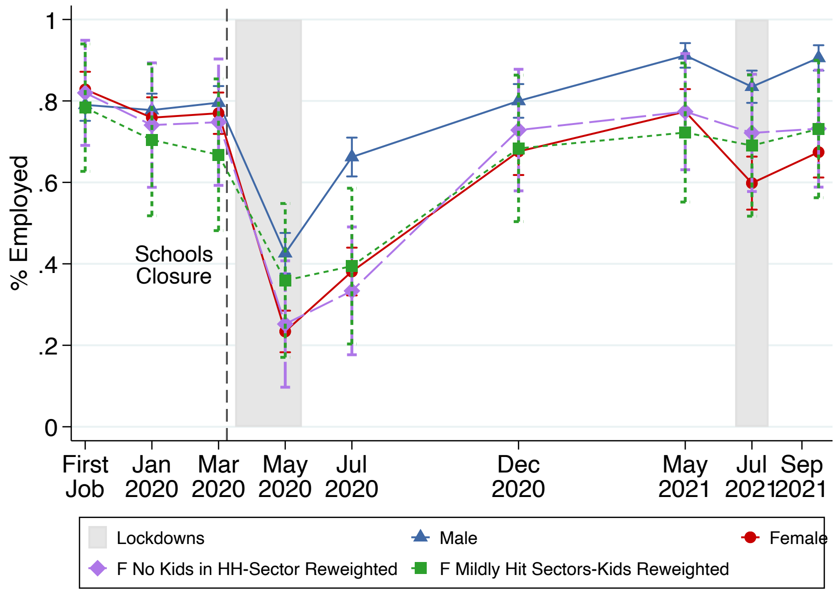
Notes: Panel (a) displays the average employment rate for female and male respondents with zero, one, and two or more school-age children in the household in periods in which schools were open (January and March 2020) and periods in which schools were closed (May, July and December 2020, May, July and September 2021). School-age children are children aged 3 or more. Among women with non-missing information about the number of school-age children in the household, 89 have zero, 47 have one, and 79 have two or more. Among men with non-missing information about the number of school-age children in the household, 229 have zero, 50 have one, and 59 have two or more. Panel (b) illustrates average employment rates over time for male, female, and children-reweighted female respondents. Children-reweighted female employment rate is equal to female employment rate when weighting the female sample so that the proportions of respondent with zero, one, or more than one school-age children in the household match those in the male sample. 95% robust confidence intervals are reported.

Table 3.7: The Contribution of Sectors and Childcare Responsibilities to the Employment Gender Gap

	May 2020	Jul 2020	Dec 2020	May 2021	Jul 2021	Sep 2021
A: Raw Means and Gap						
Male Employment Rate	.426	.662	.800	.912	.835	.906
Female Employment Rate	.234	.381	.676	.774	.598	.674
Raw Gender Gap: Male − Female	.192	.281	.124	.138	.237	.231
B: Individual Role of Sectors						
Sector Reweighted Female Employment Rate	.330	.439	.676	.748	.630	.698
Sector Reweighted Female − Female	.096	.058	-.000	-.025	.031	.024
Female with Male Sectors (Oaxaca) − Female	.100	.060	-.000	-.027	.033	.025
% Raw Gap due to Sectors	50	21	0	-18	13	10
C: Individual Role of Childcare						
Children Reweighted Female Employment Rate	.210	.364	.706	.795	.655	.701
Children Reweighted Female − Female	-.024	-.017	.030	.021	.057	.026
Female with Male Children (Oaxaca) − Female	-.011	-.004	.028	.023	.051	.026
% Raw Gap due to Childcare	-12	-6	24	15	24	11
D: Joint Role of Sectors and Childcare						
Option 1:						
Children Reweighted Female in Mildly Hit Sectors Empl. Rate	.359	.394	.683	.722	.690	.731
Children Reweighted Female in Mildly Hit Sectors − Female	.125	.013	.008	-.051	.092	.057
% Raw Gap due to Sectors and Childcare	65	5	6	-37	39	25
Option 2:						
Sector Reweighted Female w/o Children Empl. Rate	.252	.334	.728	.774	.722	0.732
Sector Reweighted Female w/o Children − Female	.018	-.047	.053	-.000	.123	.058
% Raw Gap due to Sectors and Childcare	10	-17	42	0	52	25

Notes: The table quantifies the part of the employment gender gap due to pre-pandemic employment sectors and childcare responsibilities in each pandemic time. Panel (A) reports average employment rate by gender and the raw gender gap over time. Panel (B) measures the share of the raw gap due to different sectors of employment. It reports, first, female employment rate when reweighting the female sample so that first moment of *Hit Sector*$_i$ matches that in the male sample. *Hit Sector*$_i$ is an indicator for whether pre-pandemic the respondent was employed in a severely hit sector (i.e., a sector in which more than 50% of the businesses in which our workers were employed pre-pandemic were closed during the first lockdown in May 2020: motor-mechanics, food and hotel, tailoring, hairdressing, teaching, secretary, and retail). Second, the panel reports the part of the gender gap explained by different sectors of employment. We calculate it in two ways: 1. as the difference between sector-reweighted and actual female employment rates; 2. using a Oaxaca-Blinder decomposition with *Hit Sector*$_i$ as explanatory variable, and reporting the part of the gap due to different endowments. The share of the gender gap explained by sector is obtained by dividing the explained part of the gap by the raw gap. Panel (C) measures the share of the raw gap due to different childcare responsibilities. It reports, first, female employment when reweighting the female sample so that the proportions of respondents with zero, one, or more than one school-age children in the household match those in the male sample. Second, the panel reports the portion of the gender gap explained by different childcare responsibilities. We calculate it in two ways: 1. as the difference between children-reweighted and actual female employment rates; 2. using a Oaxaca-Blinder decomposition with indicators for whether the respondent has zero, one, or more than one school-age children in the household as explanatory variables, and reporting the part of the gap due to different endowments. The share of the gender gap explained by childcare duties is obtained by dividing the explained part of the gap by the raw gap. Panel (D) measures the part of the raw gap due to different employment sectors and childcare responsibilities jointly, in absolute value and as a share of the raw gender gap. Under option 1, we take the difference between the employment rate of sector-reweighted women with no children and actual female employment rate. Under option 2, we take the difference between the employment rate of children-reweighted women in mildly hit sector (*Hit Sector*$_i = 0$) and actual female employment rate. To obtain the share of the gap explained we divide the part explained by the raw gender gap.

Figure 3.8: The Residual Gender Gap in Employment



The figure illustrates average employment rates over time for male, female, sector-reweighted female respondents with no school-age children, and children-reweighted female respondents that pre-panemic were working in mildly hit sectors. There are 89 women with no school-age children in the household and non-missing data about employment sector. Sector-reweighted employment rate for women with no children is equal to the employment rate of women with no school-age children when weighting them so that the first moment of *Hit Sector*$_i$, an indicator for whether pre-pandemic the respondent was employed in a severely hit sector, matches that in the male sample. Severely hit sectors are sectors in which more than 50% of the businesses in which our workers were employed pre-pandemic were closed during the first lockdown in May 2020: motor-mechanics, food and hotel, tailoring, hairdressing, teaching, secretary, and retail. There are 32 women in mildly hit sectors and with non-missing data about the number of school-age children in the household. Children-reweighted employment rate for women in mildly hit sector is equal to the employment rate of women with *Hit Sector*$_i$=0 when weighting them so that the proportions of respondents with zero, one, and two or more school-age children in the household matches that in the male sample. School-age children are children aged three or more. Weights are equal to one for men. 95% confidence intervals are reported.

Chapter 4

Dissertation Conclusions

This dissertation delved into various labor market frictions in urban areas of low-income countries, with a specific emphasis on the hindrances to youth employment and female labor market participation. I investigated potential solutions to these challenges and evaluated their effectiveness. Chapter 1 proposed and evaluated a program that smooths the transition from school to work and improves youth labor market trajectories, highlighting the importance of distorted beliefs. The findings demonstrate that a mentorship program able to provide credible and relevant information to young job seekers improves participants' employment outcomes, career trajectories, and education-career synergies by mitigating overoptimism regarding their initial employment prospects and providing hope for improved future outcomes. The results highlight the role of distorted beliefs as an important channel by which information frictions decrease earnings and career advancement. Chapter 2 investigated referral-based hiring as another potential channel behind gender disparities in the labor market and the perpetuation of occupational segregation. Referrals shape at large the pool of candidates that the employer uses to make their hiring decisions. The findings suggest that referrals can disadvantage women in the labor market, leading to gender disparities in job opportunities and wages. This research underscores the importance of promoting equal opportunities and emphasizes that debiasing programs intended to reduce gender segregation in the labor market should not only focus on employers but also on employees. Chapter 3 looked at the determinants of gender gaps during COVID-19 and documented how, among skilled Ugandan workers, the gap persisted even 1.5 years after. The results suggest that the pandemic has affected women disproportionately, leading to a widening gender gap in earnings and employment opportunities. This research highlights the need for policies and interventions that address the differential impact of the pandemic on women and aim to mitigate its long-term effects on gender disparities in the labor market. Overall, this dissertation sheds light on the labor market frictions in urban areas of low-income countries and proposes potential solutions to address them. Our findings suggest that mentorship programs can provide credible and relevant information to young job seekers, leading to better employment outcomes and career trajectories. Additionally, we provide evidence that referral-based hiring practices can perpetuate gender disparities in the labor market, emphasizing the need for policies that promote equal opportunities for women. Finally, our research documents the long-term effects of the COVID-19 pandemic on gender disparities in the labor market, highlighting the need for policies and interventions that address the differential impact of the pandemic on women.

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Appendix A

Meet Your Future: Experimental Evidence on the Labor Market Effects of Mentors

A.1 Appendix Tables

Table A.1.1: Strength of the Mentor-Mentee Connection

	Ever Connected (1)	Connected More Than Once (2)	Strong Link (3)
Dyad has same:			
Tribe	-0.18	-0.16	-0.24
	(-0.67)	(-0.57)	(-1.43)
Primary Language	-0.27	0.08	-0.28
	(-0.96)	(0.23)	(-1.33)
District of origin	0.06	0.06	0.38**
	(0.19)	(0.23)	(2.12)
VTI	0.66**	0.67**	0.35
	(1.99)	(2.13)	(1.62)
Gender	-0.35	-0.30	-0.06
	(-0.93)	(-0.73)	(-0.24)
Sum of:			
Age	0.04	0.07*	0.03
	(1.20)	(1.94)	(1.20)
Household Asset Index	-0.14	-0.08	-0.04
	(-1.62)	(-0.91)	(-0.68)
Difference in:			
Age	-0.07*	-0.07*	-0.06*
	(-1.80)	(-1.67)	(-1.84)
Household Asset Index	-0.25*	-0.04	-0.12
	(-1.82)	(-0.31)	(-1.12)
N	603	602	603

Notes: T-statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In this Table we report the estimates from Equation 1.1.

Table A.1.2: ITT Estimates: Reasons Driving the Job Offers Refusals

	No Learning Prospects (1)	Low Wage (2)	Distance (3)
MYF Treatment	.064	-.018	-.082
	(.050)	(.067)	(.075)
Control Mean	.10	.27	.57
Control SD	.30	.45	.50
T Effect (%)	67.55	-6.75	-14.36
N	178	178	178

Notes: In this table, we report treatment and control differences in the main reason behind refusing a job offer. Outcomes are conditional on having refused a job offer, which explains the small sample. Standard errors are robust. In Column 1, the outcome is an indicator variable that takes value one if the respondent refused a job offer because it did not provide enough learning or promotion prospects. In Column 2, the outcome is an indicator variable that takes value one for those respondents who refused a job offer beucase of the wage being too low. In Column 3, the outcome is an indicator variable that takes value one for those respondent who refused job offer because the distance to the job premise was too long. The remaining share of main refusals were classifiable as personal reasons (family reasons, illness, discrimination/harrassment).

Table A.1.3: Quantile Treatment Effect

	Monthly Earnings			
	Q(25)	Q(50)	Q(75)	Q(90)
MYF Treatment	.000	8.571	17.143***	20.000**
	(.)	(5.207)	(6.083)	(9.941)
Control Mean	34.84	34.84	34.84	34.84
N	916	916	916	916

Notes: This table shows the quantile effects of the MYF treatment. The dependent variable is the total labor earnings in the month prior to the survey. These estimates of treatment effects are estimated without controlling for any covariates or strata fixed effects. About 45% of the respondents had zero earnings at endline 2.

Table A.1.4: ITT Estimates: Employment at 1 Year

	Out of the Labor Force (1)	Has Worked Last Month (2)	Days Worked Last Month (3)
MYF Treatment	-.025	.006	.265
	(.271)	(.862)	(.776)
	[1.000]	[1.000]	[1.000]
Control Mean	.26	.56	12.50
Treatment Effect (%)	-9.53	1.15	2.12
N	923	923	923

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on employment outcomes at one year. These are obtained by ordinary least squares (OLS) estimation of Equation 1.7. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses and q-values in brackets, obtained using the sharpened procedure of Benjamini et al. (2006). For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). In Column 1 the dependent variable is an indicator variable equal to 1 if individuals have not engaged in any work activity in the previous month and have not looked for a job in the previous month. These individuals are predominantly engaged in subsistence farming, casual occupation of sitting at home. In Column 2 the dependent variable is an indicator variable equal to 1 if the respondent has engaged in either a wage- or self-employed work activity in the previous month. In Column 3 the dependent variable is the total number of days worked in either wage- or self-employment in the last month, unconditional of employment status.

Table A.1.5: Overoptimistic Students Drive Results on Reservation Wage and Willingness to Accept Unpaid Job

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Duration | Searched (4)
MYF Treatment	-11.58***	.07**	-.06**	-10.58**
	(3.36)	(.03)	(.03)	(4.90)
MYF Treatment				
× Feb expectations above mean	-23.52***	.14**	-.11	-8.06
	(5.99)	(.06)	(.09)	(8.32)
× Feb expectations below mean	1.43	.02	-.06	-5.85
	(3.13)	(.05)	(.06)	(6.53)
Difference	-24.951	.116	-.052	-2.204
P-Value	.000	.131	.545	.835
Control Mean	36.76	.54	.21	33.94
Control SD	48.14	.50	.41	73.45
Treatment Effect (%)	-31.50	13.09	-27.24	-31.17
N	737	739	745	740

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on willingness to accept a job and job search outcomes. We do so for the overall sample (in the top panel) and in two different samples: those with pre-MYF above mean and those with below mean expectations over their earnings prospect. All estimates are obtained by ordinary least squares (OLS) estimation of Equation 1.7 in each subsample. We then report the difference in coefficients and the P-Value of the T-test of equality. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses and q-values in brackets, obtained using the sharpened procedure of Benjamini et al. (2006). For this table, we use data from baseline, the post-intervention survey, and endline 1. For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014).

Table A.1.6: 2SLS Estimates: Summary of Main Findings

	Search Behavior Index (1)	Willingness to Accept Job Index (2)	Short Run Index (3)	Career Trajectory Index (4)
MYF Treatment	.018	.260***	.202***	.139**
	(.083)	(.083)	(.044)	(.064)
Control Mean	-.01	-.18	-.13	-.09
Control SD	1.04	1.09	.96	.98
N	934	669	933	833

Notes: In this table, we report 2SLS regression estimates on four standardized indexes, each for one of the four families of outcomes in our main analysis: willingness to accept a job, search behavior, short run labor market outcomes, and career trajectory.

Table A.1.7: 2SLS Estimates: Type of Support Provided, Job Search and Willingness to Accept a Job

	Job Search			**Willingness to Accept a Job**			**Search Duration**
	Started Job Search (1)	Search Efficacy Index (2)	Search Intensity Index (3)	Reservation Wage (4)	Would accept Unpaid Job (5)	Refused Job Offer | Searched (6)	Search Duration | Started (7)
Entry Conditions	-.04	.07	.05	-21.83***	.13**	-.12**	-4.56
	(.05)	(.11)	(.10)	(5.74)	(.06)	(.05)	(6.66)
Encouragement	.02	-.11	-.01	-11.26***	.09*	-.04	-9.05**
	(.03)	(.08)	(.07)	(4.17)	(.05)	(.03)	(4.54)
Search Tips	.03	-.09	.04	-1.09	-.07	.02	-14.41**
	(.05)	(.11)	(.10)	(5.63)	(.06)	(.05)	(6.32)
Control Mean	.78	.04	-.01	36.76	.54	.21	28.28
N Mentors	158	158	158	158	158	155	155
N	934	934	934	737	739	745	885
F-Test of joint significance (pval)	0.64	0.35	0.93	0.00	0.02	0.10	0.05
AP Partial F (pval)- Info	.00	.00	.00	.00	.00	.00	.00
AP Partial F (pval)- Encouragement	.00	.00	.00	.00	.00	.00	.00
AP Partial F (pval)- Search Tips	.00	.00	.00	.00	.00	.00	.00
Sargan (pval)	.54	.73	.42	.04	.06	.13	.97

Notes: In this table, we report 2SLS regression estimates where 158 mentor dummies are used as IV for the leave-out estimator of the conversation content by mentor. For each outcome, we report the mean outcome for the control group and each treatment effect. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses. The P-Values reported in the last row are from the F-test of joint significance of the treatment-content dummies in each column regression where the sample includes all students.

Table A.1.8: 2SLS Estimates: Type of Support Provided and Labor Market Outcomes

	Short Run Impacts					Transitions		Medium Run Impacts	
	Out of the Labor Force (1)	Days Worked Last Month (2)	Time Practicing Technical Skills (3)	Total Earnings Last Month (4)	First Job Duration (5)	Retained post Internship (6)	Internship to Job Transition (7)	Out of the Labor Force (8)	Total Earnings Last Month (9)
Entry Conditions	-.08*	1.73*	13.92	6.34	17.94	.03	.01	-.02	11.36*
	(.05)	(1.05)	(13.76)	(4.51)	(13.83)	(.05)	(.06)	(.05)	(6.09)
Encouragement	-.07**	1.14	20.84**	3.02	26.44***	.08**	.08*	-.04	8.79**
	(.03)	(.71)	(9.40)	(3.07)	(9.43)	(.03)	(.04)	(.04)	(4.25)
Search Tips	-.01	.10	1.67	-5.54	3.41	-.04	.05	-.00	-2.13
	(.04)	(.99)	(12.97)	(4.23)	(13.02)	(.05)	(.06)	(.05)	(5.92)
Control Mean	.21	16.15	52.66	11.35	78.07	.18	.41	.26	34.84
N Mentors	158	158	158	158	158	158	157	157	157
N	934	934	934	933	929	934	844	923	916
F-Test of joint significance (pval)	0.08	0.22	0.15	0.17	0.04	0.05	0.28	0.72	0.07
AP Partial F (pval)- Info	.00	.00	.00	.00	.00	.00	.00	.00	.00
AP Partial F (pval)- Encouragement	.00	.00	.00	.00	.00	.00	.00	.00	.00
AP Partial F (pval)- Search Tips	.00	.00	.00	.00	.00	.00	.00	.00	.00
Sargan (pval)	.44	.01	.02	.06	.01	.07	.47	.26	.04

Notes: In this table, we report 2SLS regression estimates where 158 mentor dummies are used as IV for the leave-out estimator of the conversation content by mentor. For each outcome, we report the mean outcome for the control group and each treatment effect. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses. The P-Values reported in the last row are from the F-test of joint significance of the treatment-content dummies in each column regression where the sample includes all students.

A.2 Appendix Figures

Figure A.2.1: High Take-Up and Successful Creation of New Ties

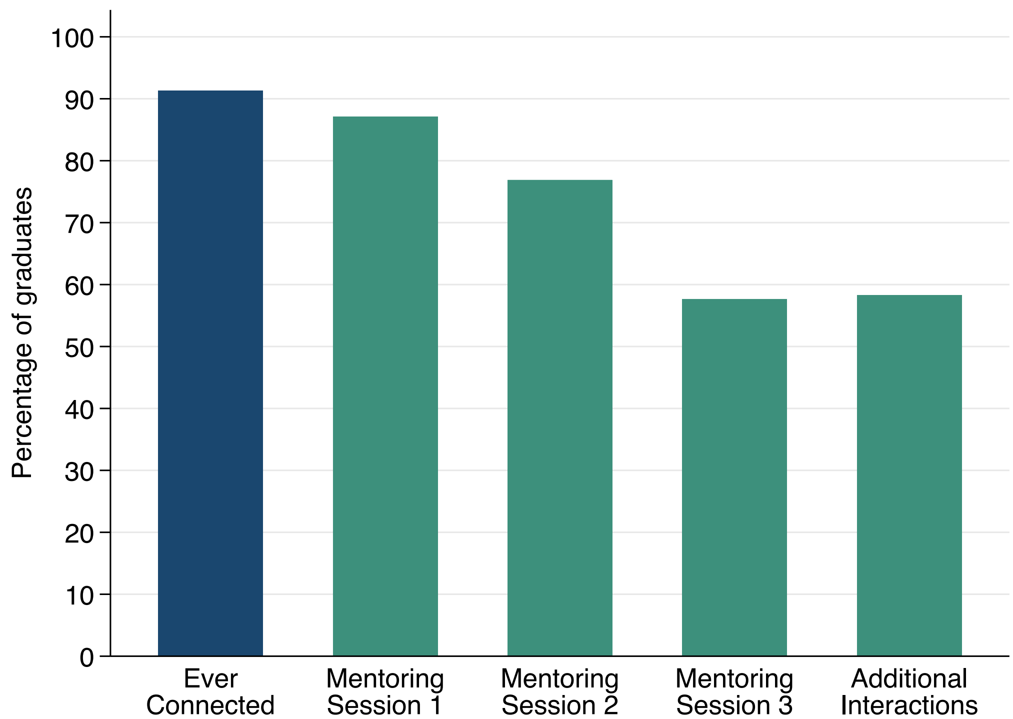


Figure A.2.2: Overoptimism Using (Pre-Covid-19) Mentors Data

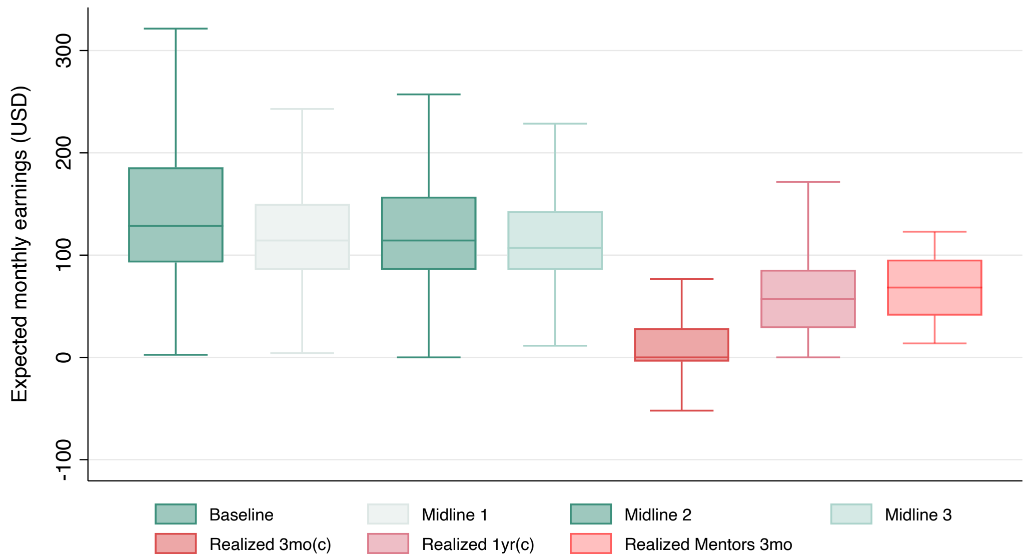


Figure A.2.3: Understanding the Treatment: Observers Data

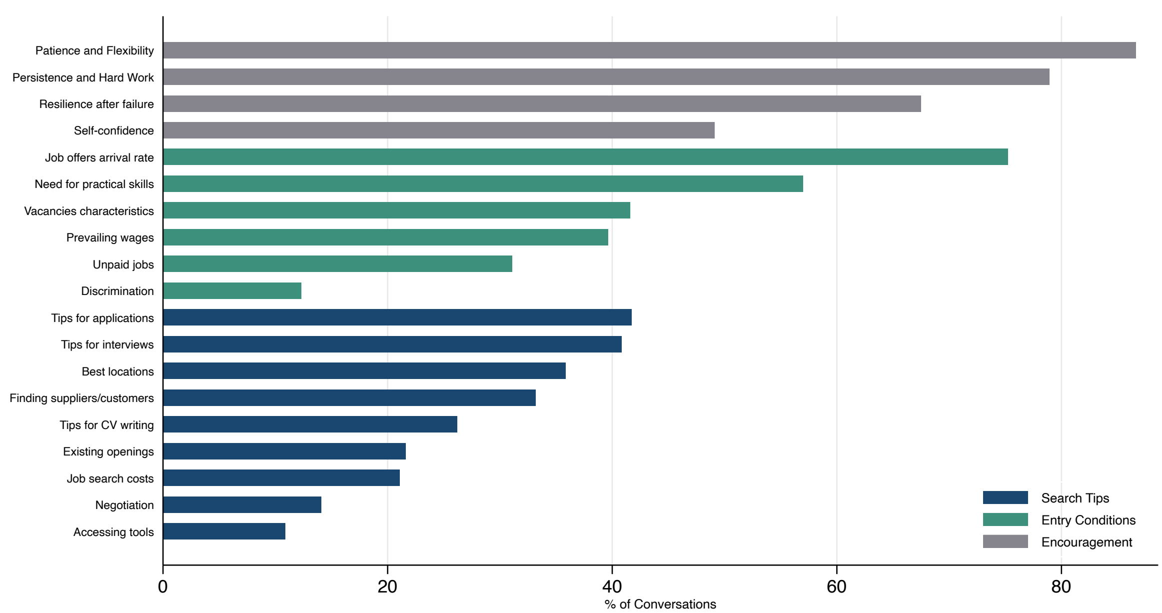
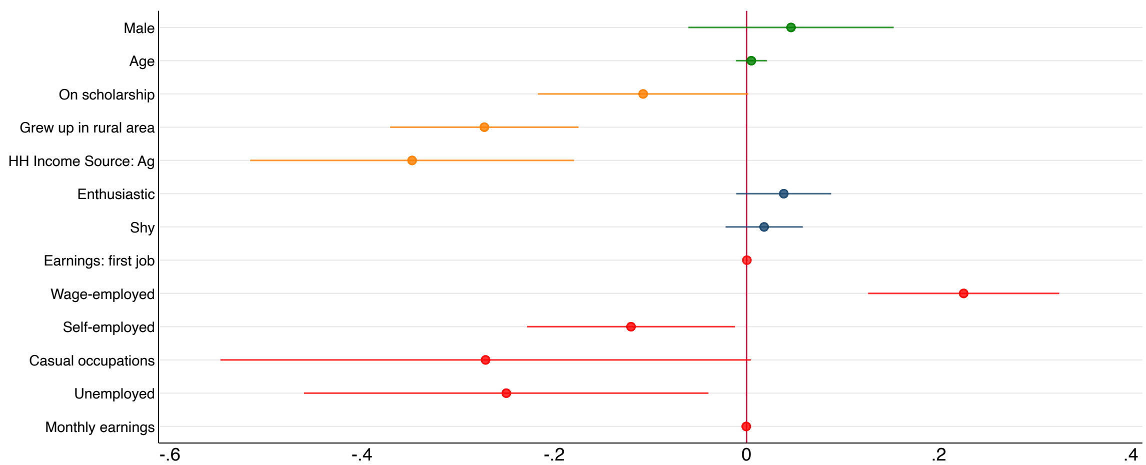
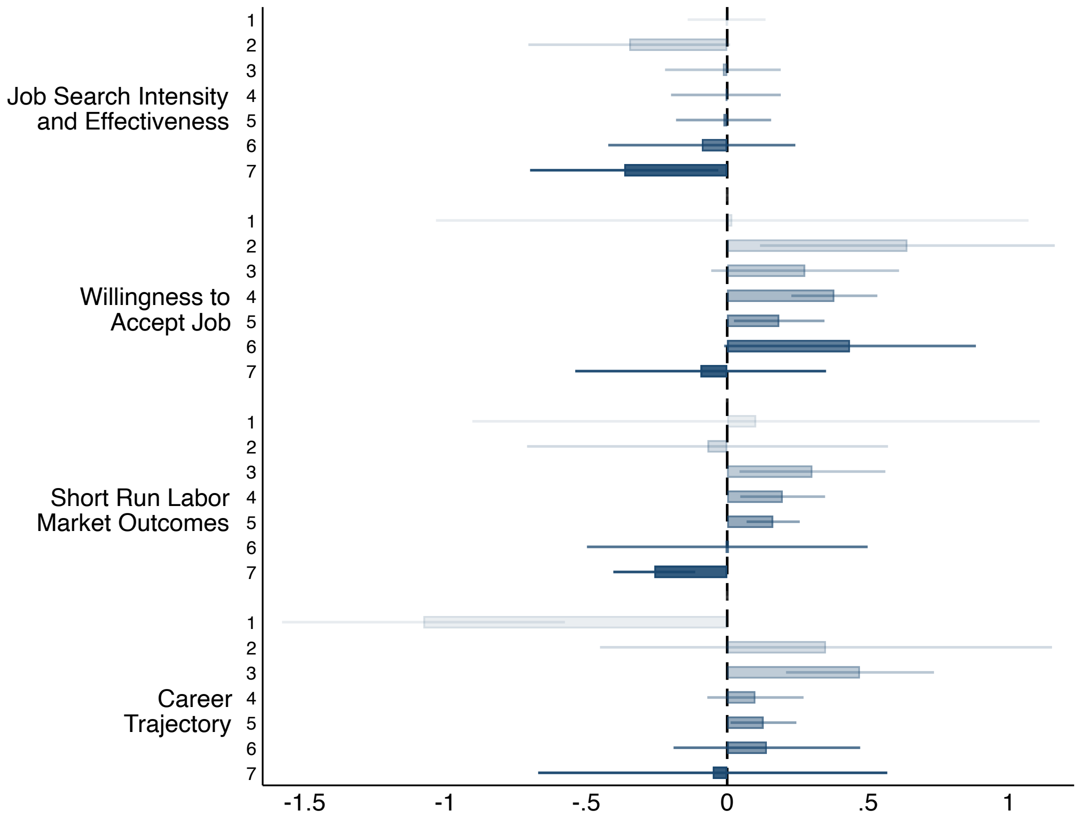


Figure A.2.4: Treatment Effects on Career Trajectory Index by Mentors Demographics



Notes: In this figure we report mentor effectiveness, as measured by the Career Trajectory Index, in relation to mentors demographic characteristics.

Figure A.2.5: Mentors Heterogeneity by Number of Assigned Mentees

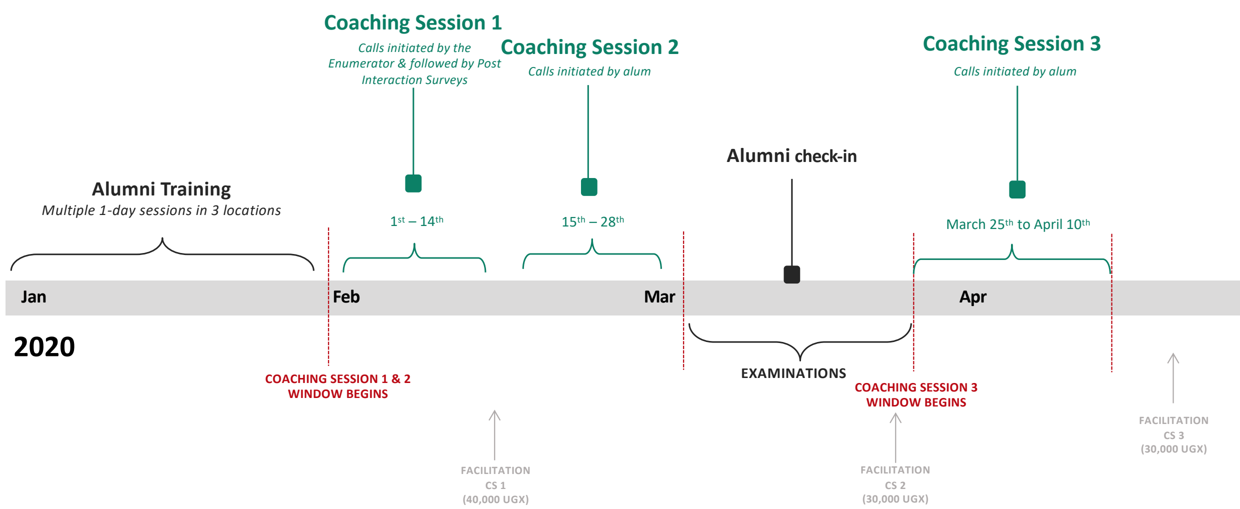


Notes: In this figure we report mentor effectiveness in relation to the number of assigned mentees. We conduct four distinct regressions, one for each index that corresponds to the four families of outcomes in the primary analysis. The coefficients in blue represent those of seven indicator variables built based on the number of mentees exogenously assigned to each mentor. The exclude category is the control group.

A.3 Material for Online Appendix

A.3.1 Program Details and Survey Rounds

Figure A.3.1: The MYF Program in Detail



The mentors trainings were one day in-person events carried out by the research team. During the training mentors were explained the structure and admin of the program as described in Figure A.3.1. They were also given logbooks and instructed on how to fill them (Figure A.3.2). During the Mentors-Check in we collected data on the content and duration of each mentorship sessions 2 as well as information on additional interactions (whether they took place, who initiated them, duration, mode and content). Further, the mentor was asked about: his/her identification with each student and a ranking between the students, each student's employability after the program and the students' interest in the program.

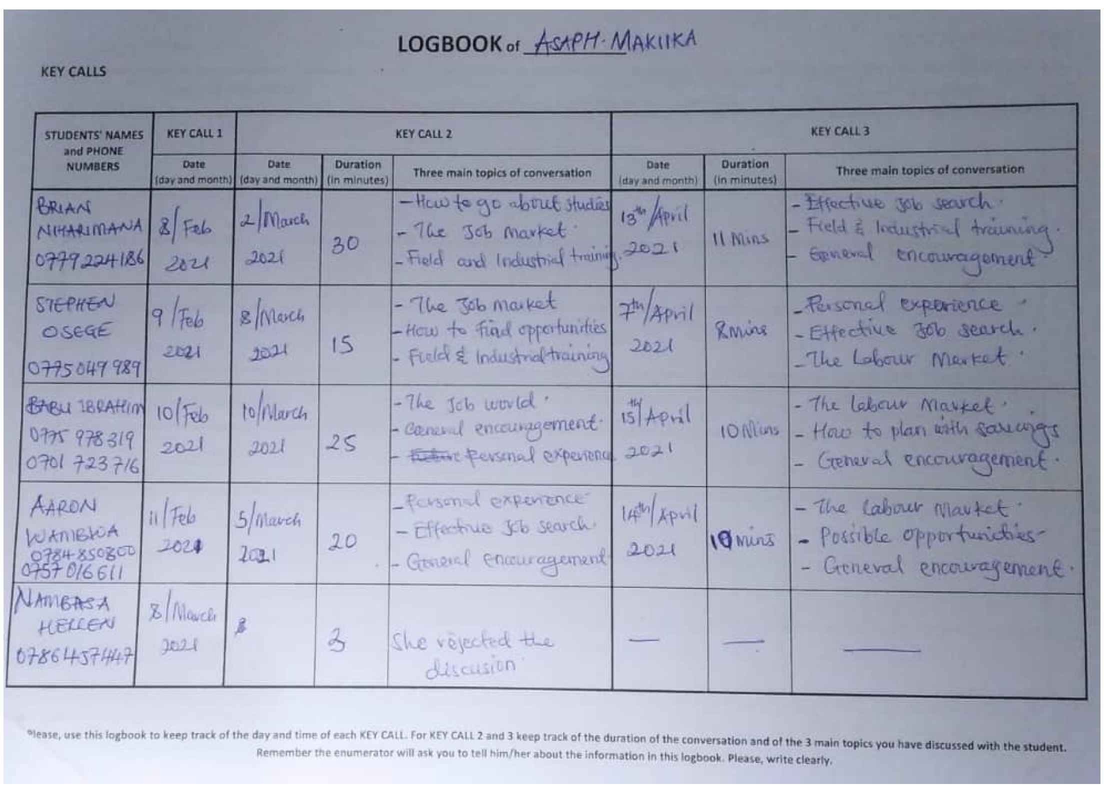
Figure A.3.2: Mentors Loogbooks

LOGBOOK of __________

KEY CALLS

STUDENTS' NAMES and PHONE NUMBERS	KEY CALL 1	KEY CALL 2			KEY CALL 3		
	Date (day and month)	Date (day and month)	Duration (in minutes)	Three main topics of conversation	Date (day and month)	Duration (in minutes)	Three main topics of conversation

Please, use this logbook to keep track of the day and time of each KEY CALL. For KEY CALL 2 and 3 keep track of the duration of the conversation and of the 3 main topics you have discussed with the student. Remember the enumerator will ask you to tell him/her about the information in this logbook. Please, write clearly.



LOGBOOK of ASAPH·MAKIIKA

KEY CALLS

STUDENTS' NAMES and PHONE NUMBERS	KEY CALL 1	KEY CALL 2			KEY CALL 3		
	Date (day and month)	Date (day and month)	Duration (in minutes)	Three main topics of conversation	Date (day and month)	Duration (in minutes)	Three main topics of conversation
BRIAN NIHARIMANA 0779224186	8/Feb 2021	2/March 2021	30	- How to go about studies - The Job market. - Field and Industrial training.	13th/April 2021	11 Mins	- Effective Job search. - Field & Industrial training. - General encouragement.
STEPHEN OSEGE 0775049989	9/Feb 2021	8/March 2021	15	- The Job market - How to find opportunities - Field & Industrial training	7th/April 2021	8mins	- Personal experience. - Effective Job search. - The Labour Market.
BABU IBRAHIM 0775 978319 0701 723716	10/Feb 2021	10/March 2021	25	- The Job world. - General encouragement. - ~~Future~~ Personal experience	~~15~~ 14/April 2021	10 Mins	- The labour Market. - How to plan with savings - General encouragement.
AARON WAMBWA 0784850800 0757016611	11/Feb 2021	5/March 2021	20	- Personal experience. - Effective Job search. - General encouragement	14th/April 2021	10 mins	- The Labour Market. - Possible opportunities- - General encouragement.
NAMBASA HELLEN 0786457447	8/March 2021	8	3	She rejected the discusion	—	—	—

Please, use this logbook to keep track of the day and time of each KEY CALL. For KEY CALL 2 and 3 keep track of the duration of the conversation and of the 3 main topics you have discussed with the student. Remember the enumerator will ask you to tell him/her about the information in this logbook. Please, write clearly.

Table A.3.1: Survey Waves

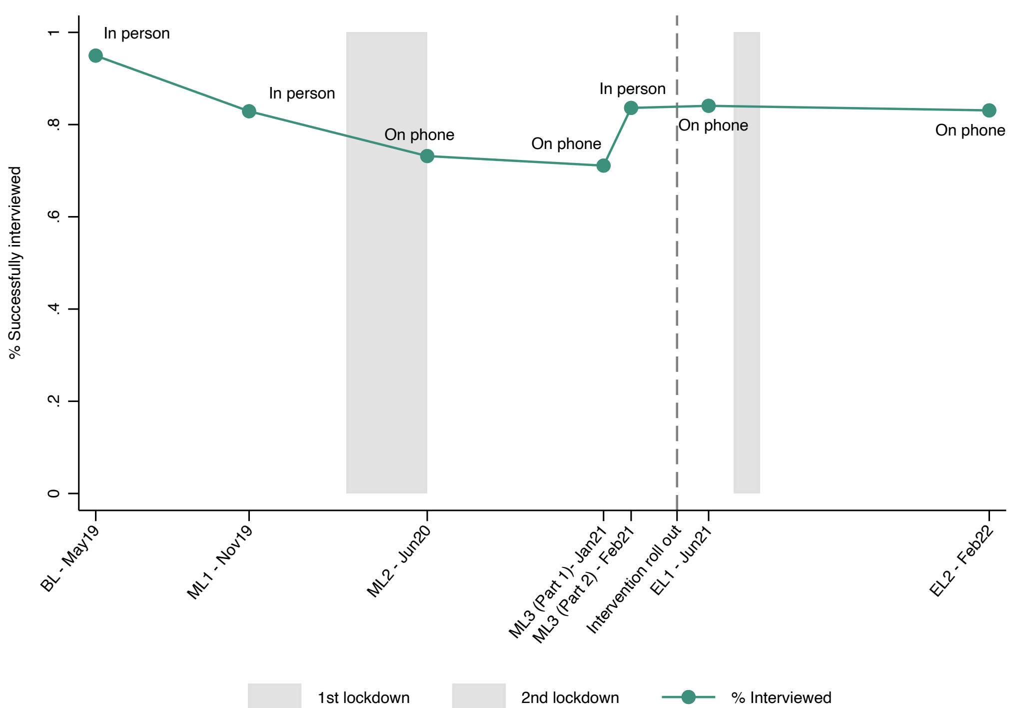
Survey	Targeted Sample	Date	Key information collected	Mode
Students Surveys				
Baseline	1112 students	May 2019	Demographics; Time preferences; Risk aversion; Raven's test; Savings; Detailed information about 4 employment network members; Life worries and self-esteem; Expectations about own performance in the labor market.	In person
Midline 1	1112 students	Nov 2019	Savings; Updated detailed information about network members; Life worries and self-esteem; Expectations about own and class-level performance in the labor market.	In person
Midline 2	1112 students	Jun 2020	Time use during school closure (due to Covid-19); Labor market outcomes; Life worries and self-esteem, Expectations about own and class-level performance in the labor market; Savings; Migration.	Phone
Midline 3	1112 students	Jan 2021	Expectations about own and class-level performance in the labor market. Midline 3 was divided in two waves, one on phone and one in person for intensive tracking.	Phone/ In person
Intervention-related Surveys				
Post-Interaction Survey	645 students assigned to treatment	Feb 2021	Survey to record student's main take-aways and reactions immediately following the first interaction with the alum (Coaching Session 1). Contains questions on: engagement in the conversation, topics of discussion, identification and connection with the alum, main take-always, plans for future interactions.	Phone
Artificial Survey and Recordings	645 students assigned to treatment	Feb-Mar 2021	During Key Calls 1 the enumerators listen and keep track of mutual and relative engagement levels, conversation pace, topics of discussions. Recordings are be used to validate the information collected in the Artificial Survey and get more detail on the content.	Phone
Alumni Check-In	158 alumni	Mar 2021	Content and duration of each Coaching Session 2; additional interactions. Alumni are asked about his/her identification with each student and a ranking on their employability one and three months after the program.	Phone
Post MYF Surveys				
Endline 1	1112 students	Jun 2021	Job search and Labor market outcomes. Content and frequency of additional interactions with alum. Expectations about own and class-level performance in the labor market	Phone
Endline 2	1112 students	Feb 2022	Job search and Labor market outcomes. Content and frequency of additional interactions with alum. As part of endline 2 we targeted 357 supervisors (i.e. the employers of those in wage employment or internships who shared their contacts) and reached 86% of them: we collected info on their satisfaction with the employee.	Phone
Alumni Surveys				
Baseline	1368 alumni	Jan-Feb 2020	Demographics, characteristics of first job and current job. Availability to participate to the MYF Program.	Phone
Follow-up 1	714 alumni	Jun 2020	Labor market outcomes, district of current residence, economic outlook and expected own labor market prospects. Availability to participate to the MYF Program. Updated contacts information.	Phone
Follow-up 2	714 alumni	Dec 2020	Labor market outcomes, district of current residence, economic outlook and expected own labor market prospects. Availability to participate to the MYF Program. Updated contacts information.	Phone

A.3.2 Attrition and Compliance

Figure A.3.3 reports attrition rates by survey round. The baseline and first midline survey were conducted in person, with the enumerators interviewing students at schools. The decrease in the share of successfully completed interviews reported between baseline and midline surveys is unrelated to students dropping out of school. Rather, it can be attributed to the timing of the interviews – enumerators went to schools only after the exam period by when many students had already left the schools to go home.

Starting from the second midline survey we conducted all project activities on the phone due to the onset of Covid-19. A rise in the attrition rate followed: as students' mobile phone numbers had not been extensively collected, contacting them became more difficult. Therefore, a third midline survey was conducted both in-person and on the phone before the roll out of the MYF Program to collect students' alternative mobile phone numbers and details of contact person(s). The in-person tracking allowed the share of successful interviews at midline 3 to equal pre-pandemic values.

Figure A.3.3: Attrition



The overall attrition rate after the intervention was stable at approximately 9% with respect to the latest pre-intervention data collection and 18% with respect to baseline. In the first case, 9% is particularly low. In absolute numbers, this means that of the 1046 students surveyed in the third midline, 966 students were successfully found after the intervention. In the latter, the figure, 18%, is in line with the literature: 15% on average in a review of 91 RCTs published in top economics journals (Ghanem et al., 2020) and 18% in studies surveying youth (Bandiera et al., 2020). For the few studies that reported lower rates of attrition, substantial differences could be noted – for example, most studies among those mentioned in Bandiera et al. (2020) tracked students for one or two years only, whereas in this study, three years passed between baseline and the roll out of the intervention. Last, the studies that tracked students for four or more years typically focused on a random subsample with intensive tracking, while we aimed to track all students present from baseline. Given the constraints of the pandemic and the need to conduct interviews on the phone, we consider these attrition rates satisfactory.

Table A.3.2: Attrition

	Found in EL1			***Found in EL2***			***Ever found***		
MYF Treatment	-.006	-.009	.221	.014	.015	.063	.002	.000	.263
	(.021)	(.022)	(.211)	(.018)	(.018)	(.258)	(.014)	(.014)	(.173)
Gender (1=M)			-.053			.487*			-.039
			(.060)			(.248)			(.051)
Age			.011			-.006			.004
			(.007)			(.010)			(.005)
HH main income source: agriculture			.047			.077			.056*
			(.033)			(.053)			(.032)
Student has a scholaship			-.010			-.056			.003
			(.036)			(.047)			(.027)
HH assets index above mean			-.019			.024			.012
			(.037)			(.042)			(.030)
Ever worked pre MYF			.019			.054			.051
			(.037)			(.043)			(.034)
Treatment X Gender			.029			-.001			.011
			(.047)			(.046)			(.034)
Treatment X Age			-.013			-.000			-.011
			(.010)			(.013)			(.008)
Treatment X HH main income source: agriculture			-.003			-.035			-.041
			(.051)			(.047)			(.045)
Treatment X Student has a scholaship			.026			.038			-.028
			(.057)			(.048)			(.046)
Treatment X HH asset index above mean			.012			-.073			-.055
			(.053)			(.063)			(.048)
Treatment X Ever worked pre MYF			.004			-.016			-.014
			(.054)			(.050)			(.038)
Constant	.843***	.504***	.769***	.822***	.493***	.514	.910***	.500***	.883***
	(.029)	(.011)	(.172)	(.023)	(.009)	(.323)	(.021)	(.007)	(.118)
Control Mean	.84	.84	.84	.82	.82	.82	.91	.91	.91
R-squared	.00	.11	.12	.00	.09	.10	.00	.10	.11
N	1112	1112	1101	1112	1112	1101	1112	1112	1101
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Strata	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
F-statistc			.72			.45			.20

Considering attrition at endline 1, Column 1 of Table A.3.2 shows that being assigned to the MYF treatment does not predict attrition, and Column 2 suggests that the result is robust within strata. Column 3 shows that the result holds also when controlling for baseline characteristics and allowing for there to be differential attrition between treatment and control based on these characteristics (age, gender, agricultural household, scholarship, household assets index above mean and previous work experience). None of these characteristics predicts attrition (except for the indicator for agricultural household significant at 10% level) and there is no evidence of differential attrition across treatment and control groups by these characteristics. At the bottom of column 3, we also report the P-Value for the joint F-statistic on the characteristics and on the interactions which are jointly insignificant (P-Value of 0.72). The same holds for attrition between baseline and endline 2 and between baseline and the indicator dummy Ever found which takes value 1 if the student was found at endline 1 or endline 2. In brief, treatment does not predict attrition, nor do the strata dummies nor the baseline characteristics (except for gender in endline 2 and the indicator for agricultural household for the ever found dummy, both at 10% level).

Table A.3.3 presents a complete set of balanced checks for the baseline sample (1112 students) and the estimation sample: 1013 students who have been successfully found after the treament roll out and have been the focus of the core analysis. The table shows that on all dimensions, there are no significant differences among treatment and control groups, both looking at the baseline and at the estimation sample.[1] In light of the evidence presened, we treat post-treatment nonresponse as random and therefore do not adjust our estimates.

Table A.3.3: Attrition Analysis: Baseline Characteristics for Students Ever Found after Intervention

		Baseline sample (N=1112)					***Estimation sample***
	All	***Control***		***Treated***			***(N=1013)***
	Mean	Obs	Mean	Obs	Mean	P-value	P-value
Panel A: Socio-economic characteristics							
Age	19.85	466	19.87	646	19.83	0.74	0.56
Gender (1=M)	0.59	466	0.59	645	0.60	0.86	0.97
Christian	0.83	466	0.83	646	0.84	0.64	0.83
Amenities in the HH: mobile phone	0.46	464	0.47	645	0.46	0.76	0.59
Student has a scholaship	0.20	464	0.19	644	0.21	0.48	0.63
HH assets index above mean	0.39	458	0.42	643	0.37	0.14	0.09
HH main income source agriculture	0.47	464	0.47	645	0.47	0.77	0.52
Hard to find	0.32	466	0.33	646	0.31	0.57	0.80
Panel B: Labor market history							
Ever worked pre MYF	0.53	464	0.53	645	0.53	0.88	1.00
Panel C: Vocational Training Institutes							
VTI 1	0.14	466	0.14	646	0.15	0.48	0.38
VTI 2	0.20	466	0.20	646	0.20	0.72	0.60
VTI 3	0.05	466	0.05	646	0.05	0.81	0.63
VTI 4	0.42	466	0.43	646	0.41	0.56	0.82
VTI 5	0.19	466	0.18	646	0.19	0.74	0.75

[1]We do not report the table but the same results hold when comparing treatment 1 and treatment 2.

Last, Table A.3.4 presents a complete set of balanced checks based on students who complied or not with the treatment assignment. Conditional on being assigned to treatment, we find no significant differences on baseline characteristics between compliers and non compliers. There are only few exceptions: non compliers were more likely to be female and to have an household asset index above mean. They are also less likely to graduate from ECD. Neverthless, results suggest that conditioning on students assigned to treatment, students who complied with the treatmet are not significantly different (except for a few cases) in terms of baseline characteristics from students who did not complied with the treatment assignment.

Table A.3.4: Attrition Analysis: Baseline Characteristics between Compliers and Non-Compliers

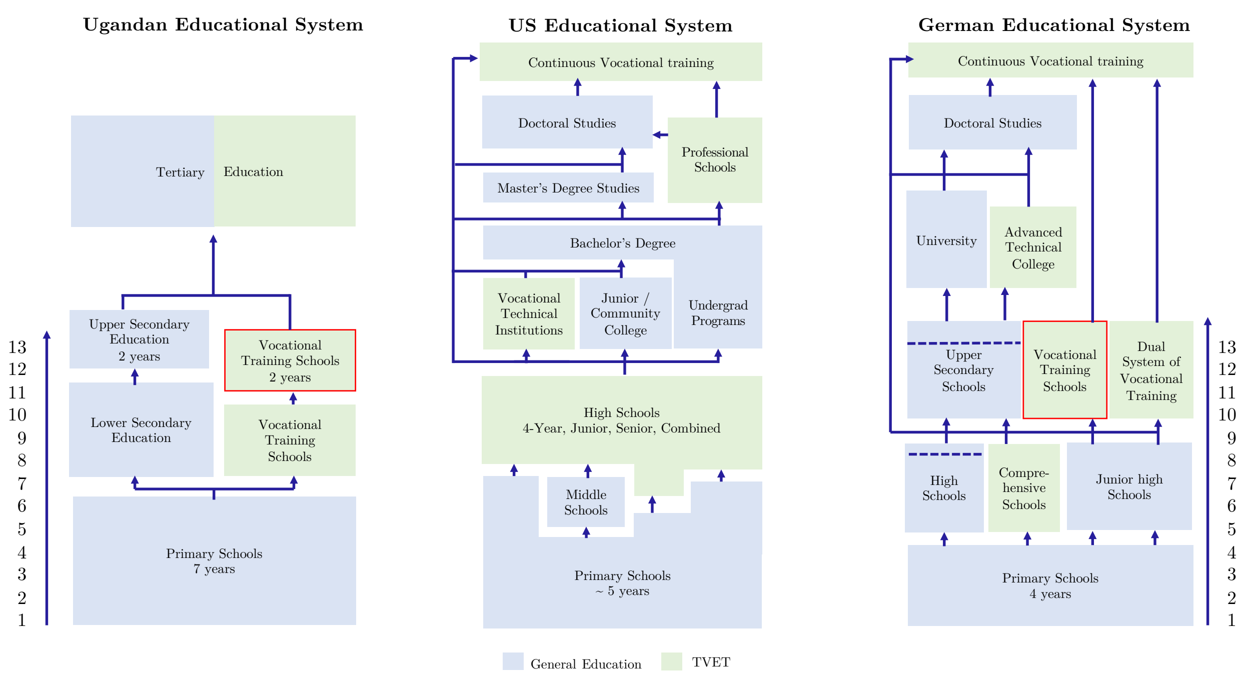
	Non Compliers		***Compliers***		
	Obs	Mean	Obs	Mean	P-value
Panel A: Socio-economic characteristics					
Age	57	19.51	589	19.86	0.21
Gender (1=M)	57	0.46	589	0.61	0.03
Christian	57	0.91	589	0.83	0.12
Single	56	0.80	586	0.90	0.04
Amenities in the HH: mobile phone with internet	57	0.53	589	0.45	0.29
Student has a scholaship	57	0.19	589	0.21	0.73
HH assets index above mean	56	0.50	587	0.36	0.04
HH main income source: agriculture	56	0.39	589	0.47	0.26
Hard to find	57	0.58	589	0.29	0.00
Panel B: Labor market history					
Ever worked pre MYF	57	0.51	589	0.54	0.69
Panel C: Vocational Training Institutes					
VTI 1	57	0.18	589	0.15	0.58
VTI 2	57	0.37	589	0.18	0.00
VTI 3	57	0.04	589	0.05	0.54
VTI 4	57	0.39	589	0.41	0.68
VTI 5	57	0.04	589	0.21	0.00
Panel D: Training areas					
Food service	57	0.11	589	0.09	0.77
Tailoring	57	0.16	589	0.13	0.54
Electrical work	57	0.16	589	0.20	0.48
Motor mechanics	57	0.25	589	0.19	0.35
Construction	57	0.04	589	0.08	0.24
Plumbing	57	0.04	589	0.10	0.12
Secretary/Accounting	57	0.04	589	0.05	0.60
Teacher/ECD	57	0.18	589	0.07	0.00
Hairdressing	57	0.04	589	0.03	0.73
Agriculture	57	0.02	589	0.01	0.61
Machining and fitting	57	0.00	589	0.02	0.30
Carpentry	57	0.00	589	0.04	0.15

A.3.3 External Validity

Table A.3.5: Sector Relevance and Balance Across Training Areas

	Young Adults UNHS	***VTI Graduates UNHS***	***All***	***Control***		***Treated***		
	Mean UNHS	Mean VTI UNHS	Mean	Obs	Mean	Obs	Mean	P-value
Food service	0.045	0.045	0.10	466	0.11	645	0.09	0.43
Tailoring	0.006	0.007	0.13	466	0.13	645	0.13	0.97
Electrical work	0.001	0.007	0.20	466	0.22	645	0.19	0.31
Motor mechanics	0.008	0.012	0.18	466	0.15	645	0.20	0.04
Construction	0.028	0.035	0.07	466	0.07	645	0.07	0.82
Plumbing	0.001	0.001	0.12	466	0.15	645	0.09	0.00
Secretary/Accounting	0.007	0.026	0.04	466	0.03	645	0.05	0.22
Teacher/ECD	0.021	0.180	0.08	466	0.08	645	0.08	0.81
Hairdressing	0.011	0.014	0.03	466	0.02	645	0.03	0.50
Agriculture	0.573	0.122	0.01	466	0.00	645	0.01	0.23
Machining and Fitting	0.004	0.010	0.01	466	0.01	645	0.02	0.23
Carpentry	0.007	0.003	0.03	466	0.02	645	0.03	0.38
Retail	0.138	0.148	0.00	.	.	.	.	.

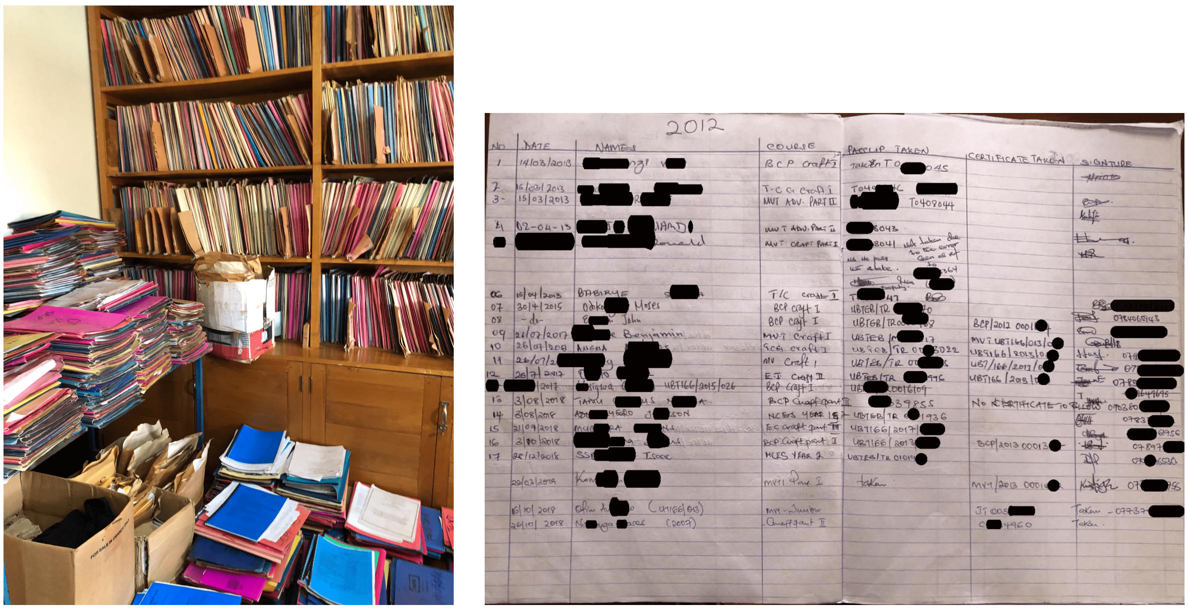
Figure A.3.4: US, Ugandan and German Educational Systems



A.3.4 Mentors Selection

Mentors were selected among alumni of the Vocational Institutets we partnered with. Like most similar institutes, such VTIs do not systematically store former students' contacts. For that reason, we collected and digitized alumni' contacts available in the VTIs' registries.

Figure A.3.5: Mentors Sample Construction - Records Digitization



Of the 1,368 alumni for whom we found a registry entry, we successfully contacted 714. We consider the tracking rate of 51% a success: the quality of the contact information collected by the VTIs is generally poor and outdated. Additionally, due to the written nature and manual entry of the records, the digitization process was prone to error. To select the mentors we defined a set of rules to ensure the overall quality of the mentorship as well as to ensure replicability. From a sample of 714 alumni, we exclude 90 alumni that did not provide their availability for the MYF program or never worked in the occupation of training. To select the most successful alumni among the remaining ones, we assign a score to a set of relevant characteristics and rank them based on their total score. The characteristics considered are:

- Accessibility: indicator for whether the alum has smartphone
- Quality of first and current job: indicator for ever having found a first job; indicator for above median earnings at first job; indicator for first job in sector of training; indicator for being currently employed; indicator for above median earnings at current job; indicator for current job in sector of training.

- General labor market indicators: indicator for having graduated between 2014 and 2018; indicator for below median longest unemployment spell.
- Education: dummy for having graduated with honors.
- Soft Skills: indicator for whether the alum describes him/herself as someone able to generate enthusiasm.

We rank alumni based on their total score (each indicator has a score of one). Our goal was to match students with alumni who attended their same VTI and course of study. For this reason, we select the N highest ranked alumni for each VTI-training area combination, where N is a function of the number of treated students in each VTI-training area. There are 12 out of 57 combinations of VTI-training areas for which we have slightly less alumni than we need. In these cases, we select the highest ranked alumni graduated from the training areas in question that have not been yet assigned, regardless of the VTI . After the selection, we end up with a sample of 171 alumni. Each alum is assigned one to five treated students at random. Each alum is assigned students belonging to the same treatment arm. The specific number of students in each combination of VTI-training area-treatment arm is determined based on the exact number of students assigned to each alum. When forming groups, we maximize the number of groups with three, four or five students per alum. Eventually we end up with: 30 groups with 5 students per alum; 29 with 4 students per alum, 19 with 3 students per alum, 5 with 2 students per alum and 5 “groups” with just one student per alum.

Table A.3.6: Mentors Characteristics

	Mean	SD
Panel A: Socio-economic characteristics		
Female	0.35	0.48
Age	25.01	3.17
Married	0.42	0.50
Has children	0.49	0.50
Number of school-age children in household	0.77	1.15
Traditional religious denomination	0.72	0.45
Ethnic minority	0.43	0.50
House of origin: rural	0.44	0.50
Region of origin: central	0.32	0.47
Region of origin: eastern	0.52	0.50
Region of origin: northern	0.06	0.25
Region of origin: western	0.09	0.29
Caretaker's years of education	10.68	5.17
Agricultural household of origin	0.09	0.29
HH of origin assets index	0.50	5.56
Panel B: Labor market characteristics		
Years in labor market	2.69	1.95
Wage employed	0.68	0.47
Self employed	0.17	0.38
Has permanent job	0.75	0.44
Works in / owns registered firm	0.41	0.49
Enrolled in further education	0.05	0.21
Involved in casual occupations	0.03	0.18
Other not wage- and self-employed	0.07	0.26

A.3.5 Strata and Balance Variables

Choice of the strata variables

First, we decide to stratify by VTI, as the implementation of the treatment could vary at the school level.[2] Second, we decide to stratify by a measure of "risk of attrition" to reduce the possibility of selective attrition. The variable we use is *hard to find*, an indicator for whether the student has not been successfully interviewed in three out of the first three pre-intervention survey rounds. Third, we choose to stratify along dimensions that are likely to be correlated with our outcomes of interest based on economic theory and existing data. To identify these variables, we perform two sets of analyses.

- Within the sample of students, we checked how a pre-determined set of students' characteristics correlate with employment indicators before the beginning of their course in the VTI and during the lockdown. We believed that the ability to find a job in the past, as well as having some work experience, could be positively correlated with the ability of finding a job after school completion.

- Within the sample of alumni, we checked how a set of alumni's characteristics correlate with the following set of labor market outcomes a) earnings at their first job, b) their most recent employment status and earnings. The variables we correlated with the outcomes above are: indicator for male student/alum; indicator for ownership of a smartphone; indicator for agriculture as household's main source of income (rather than wage- or self-employment in non-agricultural activities); asset index; scholarship status; caretakers' educational attainment; indicators for each of the VTIs; indicators for each of the training areas. These correlation analyses (whose results are available upon request) revealed the following:

  - The indicator for male is highly and positively correlated with labor market outcomes in both samples of students and alumni;
  - The indicator for smartphone ownership is highly and positively correlated with labor market outcomes in both samples.
  - The remaining variables display weaker or inconsistent correlation patterns.

To sum up, we stratify along the following four dimensions and obtain a total of 5x2x2x2=40 strata :

[2] We use indicators for schools in a similar way as Bruhn and McKenzie (2009) suggest using indicators for different geographic areas which are possibly subject to different shocks affecting the way in which interventions are administered

Strata variable name	Description	Motivation
Vti	Categorical variable with 5 levels corresponding to the 5 VTIs in our sample	Potentially correlated with treatment implementation
Male	Indicator for whether student's gender is male	Positively correlated with labor market outcomes
Hard_to_find	Indicator for not reaching the student in all pre-intervention survey rounds	To reduce the risk of having differential attrition by treatment status
Wa_sp	Indicator for smartphone ownership	Negatively correlated with labor market outcomes; to reduce the risk of having differential attrition by treatment status

Choice of the balance variables

We decide to replicate the randomization procedure until we achieve balance on a pre-determined characteristic that we believe could be highly correlated with the outcomes of interest: a dummy for whether the student has ever worked (either before beginning the course or during the lockdown). We ex-ante define the procedure that determines whether the randomization should be replicated. For any given treatment assignment:

- We regressed *ever worked* on indicators for control group, treatment 1 and treatment 2 groups, and we record the P-Value from the Wald (F)-test that the coefficients of all indicators are jointly equal.
- We useed t-tests to test whether the difference in means among 1) students assigned to the first and second treatment groups and 2) students assigned to the first and third treatment groups are statistically different from zero and we record the two-corresponding P-Values.
- We rejected the treatment assignment if any P-Value from Wald or t-test is below 0.3 and 0.1, respectively.

In practice, we achieved balance after one randomization, therefore, we did not replicate the randomization.

A.3.6 Model Derivations

In this section we provide proofs of three statements, which lie behind Propositions 1, 2 and 3 outlined in section 1.5.1 as well as we derive the expression for c^*.

Equilibrium condition for c^*
If $s = 0 \to U(\hat{\mu}) = b + \beta U(\hat{\hat{\mu}}, \hat{\omega})$
If $s = 1 \to U(\hat{\mu}) = -c + b + \beta\lambda \int max\{W(w), U(\hat{\hat{\mu}}, \hat{\omega})\} dF(w; \hat{\mu}, \sigma, \hat{\omega}) + \beta(1 - \lambda s)U(\hat{\mu}, \hat{\omega})$
In equilibrium, the student is indifferent between searching and not searching, $s = 0 == s = 1$.

$$b + \beta U(\hat{\mu}, \hat{\omega}) = -c + b + \beta\lambda \int max\{W(w), U(\hat{\mu}, \hat{\omega})\} dF(w; \hat{\mu}, \sigma, \hat{\omega}) + \beta(1 - \lambda s)U(\hat{\mu}, \hat{\omega})$$

$$c = \beta\lambda(\int max\{W(w), U(\hat{\mu}, \hat{\omega})\} dF(w; \hat{\mu}, \sigma, \hat{\omega}) - U(\hat{\mu}, \hat{\omega}))$$

$$c = \beta\lambda(\int max\{W(w) - U(\hat{\mu}, \hat{\omega})\} dF(w; \hat{\mu}, \sigma, \hat{\omega}))$$

Proof of Proposition 1
To prove Proposition 1 we need to prove that, ceteris paribus, reservation wages are increasing in λ, that is $\frac{\partial w_R(\hat{\mu}, \hat{\omega})}{\partial \lambda} > 0$. We additionally need to prove that the cutoff search draw (below which the student decides to search) is also increasing in λ, that is: $\frac{\partial c^*(\hat{\mu}, \hat{\omega})}{\partial \lambda} > 0$.

Proof of Proposition 2
To prove Proposition 2 we follow the steps Cortés et al. (2021) took to prove that, ceteris paribus, reservation wages are increasing in beliefs over the mean wage distribution at entry, that is $\frac{\partial w_R(\hat{\mu}, \hat{\omega})}{\partial \hat{\mu}} > 0$. We additionally need to prove that the cutoff search draw (below which the student decides to search) is increasing in beliefs over the mean wage distribution at entry, that is: $\frac{\partial c^*(\hat{\mu}, \hat{\omega})}{\partial \hat{\mu}} > 0$.

The value of unemployment for someone with beliefs $\hat{\mu}$ and $\hat{\omega}$ can be rewritten using the reservation wage rule and the optimal cutoff for search as:

$$U(\hat{\mu}, \hat{\omega}) = b + \beta U(\hat{\mu}, \hat{\omega}) + \int_0^{c^*(\hat{\mu}, \hat{\omega})} H(c)$$

$$U(\hat{\mu}, \hat{\omega}) = b + \beta U(\hat{\mu}, \hat{\omega}) + H\left(c^*(\hat{\mu}, \hat{\omega})\right) c^*(\hat{\mu}) - \int^{c^*(\hat{\mu})} c dH(c)$$

where $c^*(\hat{\mu}, \hat{\omega})$ and $w_R(\hat{\mu}, \hat{\omega})$ are as described in the text.
Differentiating this value with respect to $\hat{\mu}$ gives:[3]

$$\begin{aligned} \frac{\partial U(\hat{\mu})}{\partial \hat{\mu}}(1 - \beta) &= \left[h\left(c^*(\hat{\mu})\right) \frac{\partial c^*(\hat{\mu})}{\partial \hat{\mu}} c^*(\hat{\mu}) + H\left(c^*(\hat{\mu})\right) \frac{\partial c^*(\hat{\mu})}{\partial \hat{\mu}} \right] - \left[c^*(\hat{\mu}) h\left(c^*(\hat{\mu})\right) \frac{\partial c^*(\hat{\mu})}{\partial \hat{\mu}} \right] \\ &= H\left(c^*(\hat{\mu})\right) \frac{\partial c^*(\hat{\mu})}{\partial \hat{\mu}} \end{aligned}$$

[3]For the rest of this proof we omit $\hat{\omega}$ for easing notation.

Differentiating $c^*(\hat{\mu})$ gives:

$$\begin{aligned}\frac{\partial c^*(\hat{\mu})}{\partial \hat{\mu}} &= \frac{\partial}{\partial \hat{\mu}}\beta\lambda \int_{\hat{w}(\hat{\mu})}[W(w,\hat{\mu})-U(\hat{\mu})]dF(w;\hat{\mu},\sigma)\\ &= \beta\lambda \int_{\hat{w}(\hat{\mu})} \frac{\partial U(\hat{\mu})}{\partial \hat{\mu}} f(w;\hat{\mu},\sigma)dw + \beta\lambda \int_{\hat{w}(\hat{\mu})}[W(w,\hat{\mu})-U(\hat{\mu})]\frac{\partial f(w;\hat{\mu},\sigma)}{\partial \hat{\mu}}dw\\ &= \beta\lambda \frac{\partial U(\hat{\mu})}{\partial \hat{\mu}}[1-F(\hat{w}(\hat{\mu}))] + \beta\lambda \int_{\hat{w}(\hat{\mu})}[W(w,\hat{\mu})-U(\hat{\mu})]\frac{\partial f(w;\hat{\mu},\sigma)}{\partial \hat{\mu}}dw\end{aligned}$$

Plugging the expression for $\frac{\partial c^*(\hat{\mu})}{\partial \hat{\mu}}$ into the expression for $\frac{\partial U(\hat{\mu})}{\partial \hat{\mu}}$ gives:

$$\begin{aligned}\frac{\partial U(\hat{\mu})}{\partial \hat{\mu}} &= \left.\frac{\beta\lambda H\left(c^*(\hat{\mu})\right)\left\{\int_{\hat{w}(\hat{\mu})}[W(w,\hat{\mu})-U(\hat{\mu})]\frac{\partial f(w;\hat{\mu},\sigma)}{\partial \hat{\mu}}dw\right\}}{\left(1-\beta\left(1-\lambda H\left(c^*(\hat{\mu})\right)\left[1-F(\hat{w}(\hat{\mu}))\right]\right)\right)}\right]\\ &= \frac{\beta\lambda H\left(c^*(\hat{\mu})\right)\left\{\int_{\hat{w}(\hat{\mu})}\left\{[W(w,\hat{\mu})-U(\hat{\mu})]\frac{1}{\sigma}\left[\frac{w-\hat{\mu}}{\sigma}\right]f(w;\hat{\mu})\right\}dw\right\}}{\left(1-\beta\left(1-\lambda H\left(c^*(\hat{\mu})\right)\left[1-F(\hat{w}(\hat{\mu}))\right]\right)\right)} > 0.\end{aligned}$$

Proof of Proposition 3

To prove Proposition 3 we need to prove that, ceteris paribus, reservation wages are decreasing in beliefs over the seepness of the job ladder, i.e., $\frac{\partial w_R(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} < 0$. We additionally need to prove that the cutoff search draw is increasing in beliefs over the steepness of the job ladder, that is $\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} > 0$.

A.3.7 An Uneffective Cash Transfer

Table A.3.7: Students Characteristics and Balance Table: T1 vs. T2

	Treatment 1		***Treatment 2***		
	Obs	Mean	Obs	Mean	P-value
Panel A: Socio-economic characteristics					
Age	320	19.86	326	19.81	0.74
Gender (1=M)	320	0.60	325	0.59	0.81
Christian	320	0.85	326	0.83	0.45
Amenities in the HH: mobile phone with internet	320	0.49	325	0.43	0.13
Student has a scholaship	319	0.22	325	0.20	0.61
HH assets index above mean	319	0.40	324	0.35	0.17
HH main income source: agriculture	320	0.46	325	0.47	0.90
Hard to find	320	0.31	326	0.32	0.79
Panel B: Labor market history					
Ever worked pre MYF	320	0.54	325	0.53	0.65
Panel C: Vocational Training Institutes					
VTI 1	320	0.14	326	0.16	0.65
VTI 2	320	0.20	326	0.19	0.75
VTI 3	320	0.05	326	0.06	0.77
VTI 4	320	0.42	326	0.40	0.61
VTI 5	320	0.18	326	0.20	0.70
Panel D: Training areas					
Food service	320	0.09	326	0.10	0.95
Tailoring	320	0.14	326	0.12	0.37
Electrical work	320	0.18	326	0.21	0.33
Motor mechanics	320	0.21	326	0.18	0.37
Construction	320	0.08	326	0.06	0.33
Plumbing	320	0.07	326	0.12	0.02
Secretary/Accounting	320	0.06	326	0.04	0.13
Teacher/ECD	320	0.07	326	0.08	0.60
Hairdressing	320	0.02	326	0.03	0.36
Agriculture	320	0.01	326	0.01	0.69
Machining and fitting	320	0.02	326	0.02	0.79
Carpentry	320	0.04	326	0.03	0.48

Figure A.3.6: T2 Spending Categories

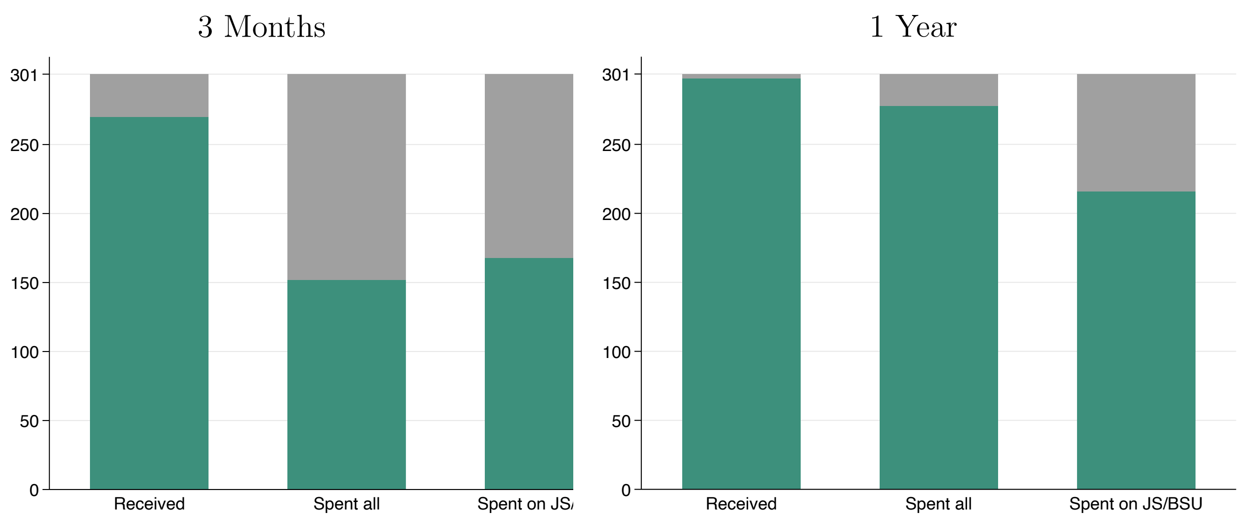


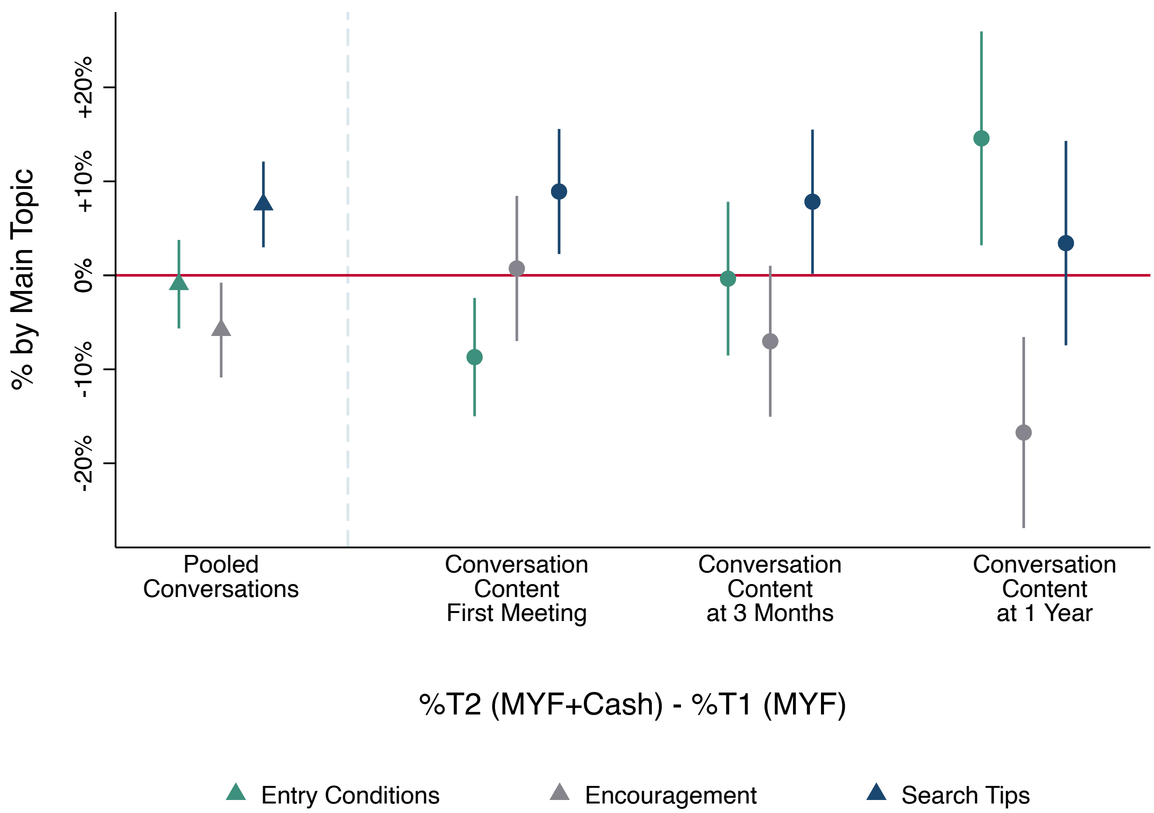
Table A.3.8: ITT Estimates: Savings and Job Search Expenditures

	Job Search Daily Expenditure (1)	Saving BL (2)	Saving ML1 (3)	Saving ML2 (4)	Saving ML3 (5)	Saving EL1 (6)	Savings Above EL1 (7)	Savings Amount EL1 (8)	Saving EL2 (9)
T1 (MYF)	-.241	-.009	.042	.031	.008	-.028	.007	.545	-.009
	(.730)	(.032)	(.035)	(.028)	(.042)	(.047)	(.057)	(5.297)	(.046)
T2 (MYF+Cash)	-.257	.031	.008	.026	.037	.071**	.103***	7.566	-.038
	(.499)	(.042)	(.047)	(.028)	(.043)	(.034)	(.035)	(8.910)	(.043)
Control Mean	2.56	.33	.25	.26	.29	.41	.47	29.44	.50
Control SD	5.72	.47	.43	.44	.46	.49	.50	57.31	.50
T1 Effect (%)	-9.41	-2.75	16.86	11.77	2.63	-6.73	1.55	1.85	-1.71
T2 Effect (%)	-10.06	9.33	3.36	9.91	12.45	17.21	22.13	25.70	-7.57
N	697	1099	963	795	780	922	907	912	910
T1=T2	0.97	0.49	0.32	0.83	0.43	0.03	0.05	0.49	0.43

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF and the MYF + Cash interventions separately. Below each coefficient estimate, we report the strata-level clustered standard errors. For each outcome, we report the mean outcome for the control group and each treatment effect. At the foot of each column, we also report the P-Value from an F-test of the null hypothesis that the impact of MYF only is equal to the impact of MYF + Cash. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). In Column 1 the dependent variable is the average daily expenditure in job saerch (during the search spell). This outcome is missing for those who did not search for a job. In Columns 2 to 6 and in Column 9 the dependent variable is an indicator variable that takes value one if the respondent was saving at the time of the survey. In Columns 7 the dependent variable is an indicator variable that takes value one if the respondents' savings at endline 1 were above median. In Columns 8 the dependent variable is the total amount of savings in USD

.

Figure A.3.7: Conversation Content by Macro Topic and Treatment Arm Over Time



Notes: In this figure we report the difference and confidence intervals in shares of conversations by main students' takeaways in MYF only (T1) and students in MYF + Cash (T2) both pooled and by conversation: the first conversation (MS1), the last conversation prior to endline 1 and, the last conversation prior to endline 2

Table A.3.9: ITT Estimates: Willingness to Accept a Job and Job Search Behavior by Treatment Arm

	Job Search			**Willingness to Accept a Job**			**Search Duration**
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Efficacy Index (4)	Search Intensity Index (5)	Started Job Search (6)	Search Duration | Searched (7)
T1 (MYF)	-13.42***	.08**	-.02	-.10	.01	.03*	-11.60**
	(3.89)	(.04)	(.03)	(.07)	(.08)	(.02)	(4.49)
T2 (MYF+Cash)	-9.74***	.07*	-.09**	-.02	.03	.03	-5.61
	(3.59)	(.04)	(.03)	(.08)	(.07)	(.02)	(4.68)
Control Mean	36.76	.54	.21	.04	-.01	.93	28.28
T1 Effect (%)	-36.50	14.15	-10.42	-279.37	-75.11	3.37	-41.02
T2 Effect (%)	-26.50	12.03	-43.30	-42.91	-242.67	2.86	-19.84
N	737	739	745	934	934	934	885
T1=T2	0.27	0.79	0.04	0.31	0.69	0.74	0.17

Notes: In this table, we report the intent-to-treat estimates of the direct effects of MYF and MYF + Cash on willingness to accept a job and job search outcomes. These are obtained by ordinary least squares (OLS) estimation of Equation 1.7. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses and q-values in brackets, obtained using the sharpened procedure of Benjamini et al. (2006). For this table, we use data from baseline, the post-intervention survey and endline 1. For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). In Column 1 the dependent variable is based on a question about the lowest wage the respondent would be willing to accept, In Column 2 the dependent variable measures the willingness to accept an unpaid job as reported by the respondents. In Column 3 the dependent variable is an indicator variable equal to 1 if the respondent has ever rejected a job offer during their first job search spell after graduation. The variable is missing for those who have never searched for a job. The results are unchanged if we condition on having received a job offer. In Column 4 the dependent variable is an indicator variable equal to 1 if individuals have engaged in any job search following their graduation (and therefore, following the treatment roll-out). The Index of Search Efficacy in Column 5 is a standardized index of three components: (i) the ratio between the number of interviews and the number of applications; (ii) the ratio between the number of offers received and the number of applications submitted and (iii) the number of CVs dropped during search. This index is only available for students who looked for a job, not for those who tried to start a business as first activity. The Index of Search Intensity in Column 6 is a standardized index of four components: (i) hours per day spent searching/starting up a business; (ii) days per week spent searching/starting up a business (iii) total number of applications submitted and (iv) total savings devoted to job-search/starting up a business. For both indexes we follow Anderson (2008) and account for the covariance structure in the components. We normalize by the standard deviation of the index in the control group to ease interpretation. In Column 7 the dependent variable measured the length of the first job search spell after graduation, conditional on having started a search. The beginning of the spell is reported by the respondents. The end of the spell is either, the start of the first employment spell, the reported date on which the respondent stopped the search, or the first day of rollout of endline 1.

Table A.3.10: ITT Estimates: Short Run Labor Market Outcomes by Treatment Arm

	Out of the Labor Force (1)	Days Worked Last Month (2)	Hours Practicing Technical Skills (3)	Total Earnings Last Month (4)	First Job Duration (5)
T1 (MYF)	-.05**	1.54**	22.71***	3.19	17.96**
	(.02)	(.65)	(7.16)	(2.55)	(7.40)
T2 (MYF+Cash)	-.06**	1.00	12.39**	.67	18.92**
	(.02)	(.63)	(5.59)	(2.41)	(7.01)
Control Mean	.21	16.15	52.15	11.35	81.18
T1 Effect (%)	-22.90	9.56	43.55	28.11	22.13
T2 Effect (%)	-30.04	6.22	23.75	5.91	23.30
N	934	934	838	933	833
T1=T2	0.59	0.43	0.19	0.35	0.92

Notes: In this table, we report the intent-to-treat estimates of the effects of MYF and MYF + Cash on primary employment outcomes. These are obtained by ordinary least squares (OLS) estimation of Equation 1.7. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses and q-values in brackets, obtained using the sharpened procedure of Benjamini et al. (2006). For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). In Column 1 the dependent variable is an indicator variable equal to 1 if individuals have not engaged in any work activity in the previous month and have not looked for a job in the previous month. These individuals are predominantly engaged in subsistence farming, casual occupation of sitting at home. In Column 2 the dependent variable is the total number of days worked in either wage- or self-employment in the last month, unconditional of employment status. In Column 3 the outcome variable is the number of hours spent applying newly acquired skills in the occupation of training in the 30 days preceding endline 1. The tasks may have been performed as part of the respondent's work activity, but also informally for a friend, family member, or themselves. To construct this variable, we designed an innovative survey module to track how much time the respondent spent performing each of a set of detailed typical trade-specific tasks a list we compiled by combining information from focus group discussions with the alumni and resources from the O*NET Program. In Column 4 the dependent variable is a measure of total monthly earnings in the main work activity (either a wage- or self-employment spell) in the month prior to the 3 month endline. Individuals reporting no wage employment earnings and no self-employment earnings are assigned a value of zero. The top 1% of earnings value are top-coded at the 99th percentile. All monetary variables are converted into February 2022 USD. In Column 5 the dependent variable is the duration in days of the first work spell after graduation.

A.3.8 Text Data

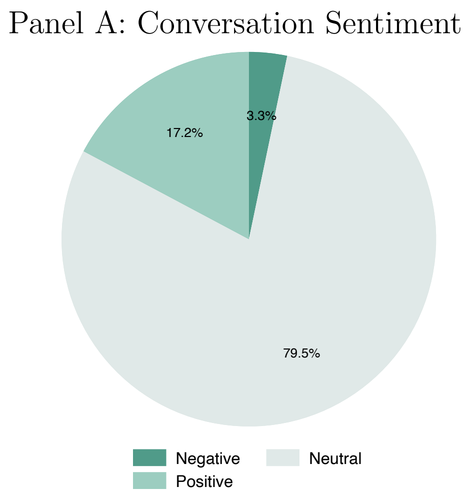
Figure A.3.8: Example of a Conversation

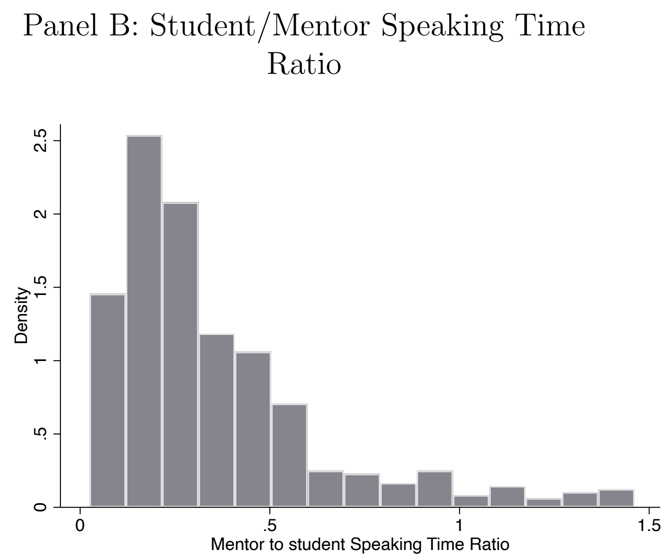
'Hello Samuel the reason why we have called you, is that you were selected to participate in a career coaching program organize
d by BRAC in partnership with JINJA vocation and you are to be matched with an alum called AYUGI LINDA she did construction/bui
lding, she will share her personal experience: the tips she used to get her job, to start a business and also contacts she has
that can help you. You will interact three times and this is the first time the next two times she will call you in person. Sa
muel good evening. good evening. my name is AYUGI LINDA. I finished my certificate in 2017/2018 and results came back in 2019 t
hen I did building as madam has told you already and I will share with you some few things that you need to know, get somewhere
to write my number. I don't have a piece of paper but instead let me read my contact and you write it down 0777739132/ 07042451
63 AIRTEL number. okay. yes, now you will call me and I get your contacts. Today let me share with your knowledge about interns
hip: for example, if you get an internship offer but the pay at work is less than the transport costs the job may not be favora
ble for you because you will require extra money to survive, you can only plan to take on the job if you have the option of goi
ng closer to the place of work. Also while for internship and you work hard, your chances of being retained as you work for the
company are high, so you need to be disciplined and work hard. "now like for me I was retained and I was being paid 5000shs per
day I remained and worked for two more months and after we shifted to another place my pay was increased to 15000shs per day" s
o in case you are retained in the place and you being paid its better to stay and work so as to gain more skills and also be ab
le to learn how to use some machines that are not available at school. Also while in field don't show them you know more so tha
t they get to teach you a lot. Also builders tend to use vulgar language so try as much as possible not to get involved in such
as the supervisor can bump into you and gives him/her a bad impression about you which can also spoil your recommendation. oka
y. now do you have any questions? yes. okay ask. Last time building and the work was so tedious for me this time I want to do i
nterior designing, painting, fixing tiles, talaza. Is there company you can recommend me to? getting such company is hard given
that companies contract the work from foundation to finishing unless if the company employs you to do that alone. Also most com
panies don't want to use students they want people who have finished studies and have the certificate. okay. so which companies
do you recommend? I can recommend you to TAI companies headed by an Indian but the other engineers are Africans its located in
JINJA along IGANGA road its where I did my internship from and the very building where the offices are located is what we const
ructed and also I went back in second year for internship. I will try and get for you the number of the field engineer and give
it to you the next time we talk. okay. But all you should know is that these companies want hard working people and people who
are disciplined. okay am hard working. that's good then. Do you still have any other questions? not really they are done. okay
thanks for listening to me, have a lovely day. have a lovely day too.'

Sentiment Analysis

For sentiment analysis we rely on VADER, a widely used model for text sentiment analysis sensitive to polarity (positive/negative) (Hutto and Gilbert 2014).

Figure A.3.9: mentorship Sessions Text Data: Form





Topic Modelling

Topic models analyze the semantic content of text corpora and reveal the hidden thematic structure in the data. They are a dimension-reduction strategy that condense the complex informative content of unprocessed text into a few relevant dimensions.

We rely on a BART Model trained on the Multi-Natural Language Inference (Multi-NLI) dataset to accomplish this. Specifically, we leverage a zero-shot sequence classifiers developed by Yin et al. (2019).[4] In the zero-shot classification scenario, a classifier is required to work with labels that it was not specifically trained with. The method operates by positing the sequence to be classified as the NLI premise and deriving a hypothesis from each potential label. Probabilistic topic models, such as the one we employ, are superior to a simpler document-term matrix or bag of words approach because they do not simply assign terms to topics, but instead assign each term a relative weight within the topic.

This technique is remarkably effective in many instances, especially when used with large pre-trained transformer architectures such as BART (Lewis et al. 2019). For instance, if we wanted to determine if a sequence belonged to the category "search tips" we could formulate a hypothesis of "This content pertains to search tips". The probabilities for entailment and contradiction are transformed into labels probabilities, which can be thought of as similarity

[4]Note that the BART-NLI model we use is based on a recent seq2seq architecture with a bidirectional encoder (e.g. BERT) and a left-to-right decoder (e.g. GPT), which outperformed BERT in NLI tasks. All pre-trained models used in our study can be downloaded in the following library://huggingface.co/transformers/.

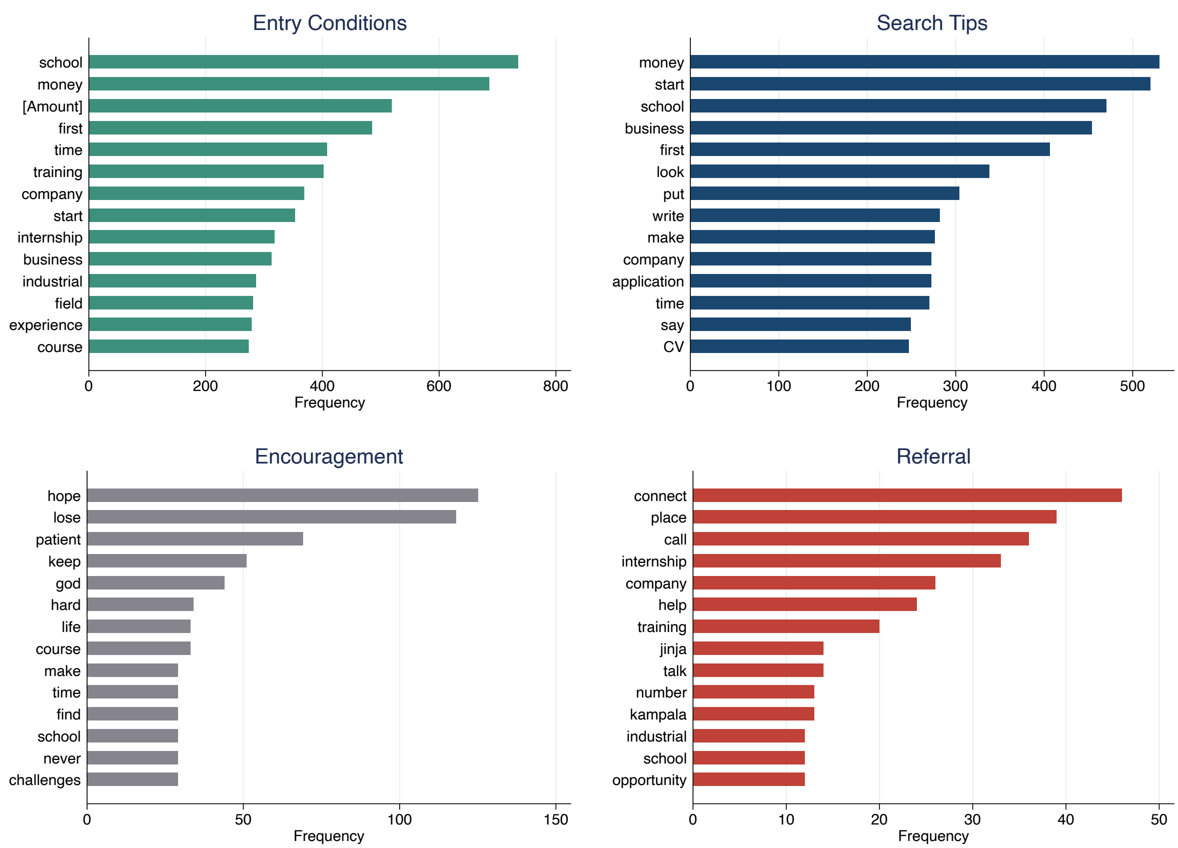
scores. For our use case, we recognize the wide space of sentences that could fall under each category and hence break down each topic into smaller micro-topics or labels, as show below:

- Encouragement: overcome failure, self-confidence, persistence, resilience, patience
- Entry Level Conditions: earnings, salary, wage, discrimination, contract, practical skills, unpaid jobs, time to find job, employment opportunity
- Search Tips: job search, job search timings, accessing tools, finding suppliers, finding customers, negotiations, tips for applications, tips for CV writing, applications, tips on application material, best locations

Given the similarity scores for each of the 25 labels in total, we use the highest scoring similarity score in each category to represent the similarity to that category at large. Comparing these obtained similarity scores for each category, provided they lie above the threshold of 0.90, we classify the sentence to the category with the highest similarity score. If all 3 scores lied below 0.90, the sentence was deemed neutral. To produce Figure 1.4 we weighted the number of sentences that fall into each topic category for the conversation, by the number of words each sentence is made of. Ultimately, we obtained the weighted shares of each topic discussed, where the weights are the number of words in each sentence.

Example of how it works: Say the sentence was “Old Town, in Kampala is a great place to start your plumbing business”. The similarity scores for all the 25 labels across all 3 categories would be computed. Say within the General Info category, the highest score was 0.84 corresponding to “employment opportunity”, within Encouragement it was “persistence” with a score of 0.67 and within Search Tips it was “best locations” with a score of 0.91. Then, the sentence would be classified to fall under “Search Tips”.

Figure A.3.10: Most Common Words by Topic



Examples of Sentence Classification

Information About Entry Conditions

- ⋆ For the start they tell you since you don't have any experience we give you 10,000 UGX.
- ⋆ At first the permit was costing 450,000 shillings but now they increased it is at 500,000.
- ⋆ Where I started from I was working and they would pay me just 7,000 shillings a day. I worked for 7,000 shillings for 8 months.
- ⋆ In December 2014 in the garage we were assigned some work. We had five vehicles but they were not paying us we would only get allowance and that was after the first month. The first month we worked for free.

- ⋆ So what I can tell you that will be your first job though some companies or enterprises they may not pay you but it is your first job you must know.
- ⋆ They can give you training for some period of time like some three months and after seeing how you are performing, they can either can confirm you or give you more three months and after giving you some lunch.
- ⋆ Sometimes you are employed in a company but with little experience and they just help you by giving you a job and you work for like three month or five without being paid and after gaining experience they give you some amount of money like 15,000 shillings per day.
- ⋆ After all those allowances they are going to be paying you let me say 100,000 shillings.

Encouragement

- ⋆ When you come out you will meet those small challenges but still you can solve them by being persistent and patient to see yourself having a way forward.
- ⋆ I don't want you to lose morale when you find that they are paying you little money in the start first look at experience because sometimes patience is needed.
- ⋆ At national water they told me they did not have other jobs other than digging trenches so despite having studied I agreed because it was still in my field. I was flexible, patient, and disciplined, the manger had kept on observing me.
- ⋆ After like 5 months you leave because now you have what they call experience which can push you where you want.
- ⋆ So for the start they might pay you less than your expectations but you need to be patient for the beginning then they keep on up grading.
- ⋆ So, some companies might feel like they are over working you and there isn't any payment and later with time them might start paying you and that's what most people do now days. I hope you are getting me.
- ⋆ Because sometimes you are employed in the company but with little experience and they just help you by giving you a job and they tell you to work for like three month or five without being paid and after gaining experience we shall give you some amount of money.
- ⋆ What I can encourage you is to be patient, don't lose hope, work hard, you need to work hard, everything you have to work for it.
- ⋆ You can start poorly but if you are patient, flexible and disciplined you will be lifted and promoted easily.

* After working for 5months I kept doing interviews getting positive feedback so in 2019 I decided to start hawking clothes and I raised money and in November I opened up my own boutique from which am now getting money to help me and my family.

* Don't lose hope in everything cause your determination, it is what, it will determine you.

* You need to welcome all types of jobs so when they see you are patient they start sending you for the jobs you studied for which opens up your opportunities.

Search Tips

* If you are writing an application, either to a company or a workshop, we look at the headlines, you get a paper, on the right write your address, then you jump one line and write the company address where you are applying.

* Now lets go to interview, how do you dress wen going for an interview?

* Getting a job sometimes depends on the way you express yourself, dress code and even the way as you enter someone's office.

* You can look for a job through Newvision, Bukedde, those newspapers. The first thing to do when you see a job is to write an application and you take it there.

* With like 5000 shillings you can print a light cv and seal it in the envelope.

* Some people may pretend they are askaris yet they are interviewers.

* You need to keep your CV good at that work place because one of your major intentions is for you to gain that experience and also to learn much more new things due to the fact that your CV has to keep on changing every now and then.

* When you are going in an office, or going for interview you have to put on good clothes so that you can look smart.

* Let me tell about writing a CV, you have your certificates, you make 2 copies of each, then you go to the cafe to photocopy them, you have to write your heading like curriculum vitae, then you put things like married status, date of birth, your full names, then the second heading should be education background. Below that, you draw a table then you write there institution, year and award, then in the first line you write UCE, the year you started from senior 1 to 4, the school under institution, then under award you put UCE.

* I had 2 types of letters, okay 3, a cover letter, an application letter and CV.

Job Referrals

- ⋆ I have someone who told me I should get her a worker who can sew uniforms and if you say you know how to sew them, I will connect you with her.
- ⋆ I will try to get the number of the field engineer and give it to you the next time we talk.
- ⋆ So when you are done I will recommend you to some places like SHAKA ZULU, JAVA HOUSE and you can drop your applications.
- ⋆ You can call 0786107334 and ask them but they don't hire trainees but if you ready to work they can take you on, I have worked there before it has a logo of a rhino.
- ⋆ Me currently am in soroti I can give you ideas of how to apply and I tell you what hotels always want.
- ⋆ There is some place where I did my internship from I will have to give them a call and ask if they can take you up then later on I can get back to you.
- ⋆ Yes even if you want to do it from a driving school I can help you because I have some driving schools I know in Jinja where you can go.

A.3.9 Spillovers

This section explores the potential indirect effects on the outcomes of untreated students who regularly interact with program participants. To achieve this, we take advantage of the fact that, as part of our intensive data collection effort, we have mapped the VTIs' friendship networks of each treated and untreated student. Specifically, we gathered information on each student's two closest friends in the cohort, regardless of classroom or field of study. We are able to determine the treatment status of each student's two closest friends as a result of the fact that, for the primary experiment, we constructed a panel data comprising the entire cohort of interest.

Several recent studies on the labor markets of developing countries have observed these types of social contact, which are consistent with qualitative and descriptive data from our environment (Angelucci and De Giorgi, 2009; Caria et al., 2018; Magruder, 2010). The spillover design is relatively simple. By treating students at random, we automatically altered the proportion of treated friends control students will have. To examine the presence of spillovers we run the following regression:

$$Y_{i,s,t} = \alpha + \beta_1 S_1 C_i + \gamma_0 S_0 T_i + \gamma_1 S_1 T_i + X_i'\delta + \lambda_s + \epsilon_{i,s,t} \tag{A.1}$$

where T_i identifies students who have been assigned to the MYF treatment, while C_i identifies students who have not been assigned to the MYF treatment.[5] S_1 is an indicator variable for students with at least one friends assigned to MYF. β_1 captures the difference in outcomes between control students with at least a treated friend and control student with no treated friends. Further, γ_1 measures the difference in outcomes between treated students with treated friendw and control students with no treated friends.

During this analysis, we lose nearly half of the data points. Firstly, the network of friends was mapped at midline 3, which corresponds to the survey round with the highest attrition rate (see Figure A.3.3). In addition, because each student could choose friends from the entire cohort (over 300 students) while coding the survey tool, we decided against creating pre-fixed lists of names from which to choose (such long lists would frequently froze the tablets). Names were entered as strings instead. As a result, we had to match based on first name, last name, and field of study, resulting in a partially incomplete network of friends due to spelling errors and frequent incomplete names (e.g., only first name, too common to match with certainty).

The results are shown in the Table A.3.11. As we lose nearly half of the sample, we start by checking whether our main results replicate in the sample for which we have friend information in Panel A. Even though we lose a substantial portion of the sample, the main findings remain unchanged. In this sample, the medium run results are, if anything, stronger.

By examining Panel B of A.3.11, we conclude that there may have been some spillovers, which, if at all, have caused our overall estimates to be conservative. With the exception

[5] The sample for this analysis is restricted to students for whom we collected friendships data. Becuase the friendship module was rolled out in Midline 3, the data collection with highest attrition rate, and because of the string match not always been precice, we were able to match 669 out of the 976 names collected.

of Column 6, which indicates some discouragement (consistent with the hypothesis that while information is more easily transferred to control friends, encouragement is much less so), Columns 1 through 16 demonstrate that information spread from their treated friends, resulting in better career trajectories for control groups with treated friends.

Table A.3.11: Spillovers: Leveraging the Network of Friends

	Willingness to Accept a Job			Job Search				Short Run Labor Market Outcomes					Career Trajectory			
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Efficacy Index (4)	Search Intensity Index (5)	Started Job Search (6)	Search Duration | Searched (7)	Out of the Labor Force (8)	Days Worked Last Month (9)	Hours Practicing Technical Skills (10)	Total Earnings Last Month (11)	First Job Duration (12)	Retained post Internship (13)	Internship to Job Transition (14)	Out of the Labor Force (15)	Total Earnings Last Month (16)
Panel A																
MYF Treatment	-12.97***	.09**	-.09***	-.06	-.05	.02	-2.81	-.08**	1.55**	4.13	3.22	24.41***	.08***	.09	-.06**	11.48**
	(4.07)	(.04)	(.03)	(.08)	(.08)	(.02)	(6.26)	(.03)	(.76)	(5.08)	(3.44)	(8.21)	(.02)	(.06)	(.03)	(4.26)
Panel B																
Control + Treated Friends	-17.89	.16	-.07	.10	.17	-.07**	-27.26	-.00	1.30	25.66	-11.12	16.97	-.06	.05	.09	6.49
	(12.96)	(.13)	(.08)	(.14)	(.11)	(.03)	(18.02)	(.08)	(1.61)	(27.84)	(8.47)	(18.82)	(.09)	(.08)	(.08)	(6.03)
MYF + Treated Friends	-26.08**	.22**	-.15*	.06	.08	-.02	-24.04	-.09	2.80*	22.57	-5.16	40.67***	.03	.12	.02	16.88***
	(10.76)	(.10)	(.08)	(.13)	(.10)	(.02)	(17.21)	(.08)	(1.44)	(25.06)	(8.23)	(12.90)	(.05)	(.09)	(.07)	(4.99)
MYF + 0%	-30.88**	.17	-.15*	-.15	.14	-.06	-27.51*	-.03	1.62	36.50	-8.88	25.91	.02	.14	-.01	16.44
	(13.02)	(.16)	(.08)	(.17)	(.19)	(.04)	(13.97)	(.11)	(1.77)	(31.83)	(8.71)	(26.80)	(.09)	(.12)	(.08)	(10.64)
Control Mean	32.43	.52	.20	-.03	-.03	.98	28.04	.19	17.00	61.83	16.89	80.21	.23	.43	.19	39.25
Control SD	46.16	.50	.40	.86	.82	.15	74.04	.39	8.59	151.93	42.48	95.69	.42	.50	.39	47.49
N	382	382	382	471	471	471	454	471	471	471	470	470	471	471	456	454

A.3.10 Robustness Analysis

Lasso Link Creation

Table A.3.12: Strength of the Mentor-Mentee Connection - Lasso

	Ever Connected	Connected More Than Once	Strong Link
	(1)	(2)	(3)
Same VTI		0.108*	0.0821
		(2.48)	(1.44)
Age difference >5y		-0.0400	-0.0554
		(-1.44)	(-1.37)
Same Tribe			-0.0619
			(-1.45)
Same Primary Language			-0.0753
			(-1.53)
Same Region			0.0963*
			(2.22)
Same Gender			-0.00321
			(-0.06)
Constant	0.913***	0.764***	0.540***
	(82.30)	(18.50)	(6.91)
Observations	645	651	603

t statistics in parentheses

Notes: T-statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ATE Results with Take-up Defined as Having Completed at least 1 Mentorship Session

Table A.3.13: ATE Estimates: Short Run Labor Market Outcomes

	Short Run				
	Out of the Labor Force (1)	Days Worked Last Month (2)	Hours Practicing Technical Skills (3)	Total Earnings Last Month (4)	First Job Duration (5)
MYF Treatment	-.06***	1.29**	17.51***	1.93	18.76***
	(.02)	(.53)	(4.92)	(2.04)	(5.05)
Control Mean	.21	16.15	52.15	11.35	81.18
Control SD	.41	9.20	102.84	39.07	102.12
T Effect (%)	-27.04	7.98	33.57	17.03	23.11
N	934	934	838	933	833

Notes: In this table, we report the average treatment effects of the MYF program on primary employment outcomes. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.2 for the description of the variables.

Table A.3.14: ATE Estimates: Labor Market Trajectory in the Medium Run

	Transitions		**Medium Run**	
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Total Earnings Last Month (4)
MYF Treatment	.04**	.08**	-.03	6.49*
	(.02)	(.03)	(.02)	(3.66)
Control Mean	.18	.37	.26	34.84
Control SD	.39	.48	.44	47.62
T Effect (%)	23.28	21.07	-9.99	18.63
N	934	934	916	916

Notes: In this table, we report the average treatment effects of the MYF program on match quality and labor market dynamics. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.3 for the description of the variables.

Table A.3.15: ATE Estimates: Willingness to Accept a Job and Job Search Behavior

	Willingness to Accept a Job			**Job Search**			**Search Duration**
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Efficacy Index (4)	Search Intensity Index (5)	Started Job Search (6)	Search Duration | Searched (7)
MYF Treatment	-11.90***	.07**	-.06**	-.06	.02	.03**	-8.65**
	(3.29)	(.03)	(.03)	(.06)	(.07)	(.01)	(3.97)
Control Mean	36.76	.54	.21	.04	-.01	.93	28.28
Control SD	48.14	.50	.41	.96	.81	.25	68.22
T Effect (%)	-32.36	13.44	-27.71	.	.	3.16	-30.59
N	737	739	745	934	934	934	885

Notes: In this table, we report the averagetreatment effects of the MYF program on willingness to accept a job and job search outcomes. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.4 for the description of the variables.

ATE Results with Take-up Defined as Having Completed 3 (or more) Mentorship Sessions

Table A.3.16: ATE Estimates: Short Run Labor Market Outcomes

	Short Run				
	Out of the Labor Force (1)	Days Worked Last Month (2)	Hours Practicing Technical Skills (3)	Total Earnings Last Month (4)	First Job Duration (5)
MYF Treatment 3 convo	-.07***	1.55**	20.66***	2.32	22.18***
	(.02)	(.63)	(5.89)	(2.46)	(5.94)
Control Mean	.21	16.15	52.15	11.35	81.18
Control SD	.41	9.20	102.84	39.07	102.12
T Effect (%)	-32.47	9.59	39.62	20.46	27.32
N	934	934	838	933	833

Notes: In this table, we report the average treatment effects of the MYF program on primary employment outcomes. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.2 for the description of the variables.

Table A.3.17: ATE Estimates: Labor Market Trajectory in the Medium Run

	Transitions		**Medium Run**	
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Total Earnings Last Month (4)
MYF Treatment 3 convo	.05**	.09**	-.03	8.05*
	(.02)	(.04)	(.03)	(4.57)
Control Mean	.18	.37	.26	34.84
Control SD	.39	.48	.44	47.62
T Effect (%)	27.96	25.30	-12.39	23.11
N	934	934	916	916

Notes: In this table, we report the average treatment effects of the MYF program on match quality and labor market dynamics. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.3 for the description of the variables.

Table A.3.18: ATE Estimates: Willingness to Accept a Job and Job Search Behavior

	Willingness to Accept a Job			**Job Search**			**Search Duration**
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Efficacy Index (4)	Search Intensity Index (5)	Started Job Search (6)	Search Duration | Searched (7)
MYF Treatment	-13.92***	.08**	-.05	-.04	-.02	.03**	-9.54**
	(3.79)	(.04)	(.03)	(.06)	(.06)	(.01)	(4.19)
Control Mean	38.66	.54	.20	.02	.01	.93	28.90
Control SD	50.01	.50	.40	.96	.82	.25	68.38
T Effect (%)	-36.00	14.27	-22.76	.	.	3.23	-33.00
N	614	616	668	844	844	844	798

Notes: In this table, we report the averagetreatment effects of the MYF program on willingness to accept a job and job search outcomes. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.4 for the description of the variables.

ATE Results on the Balanced Panel

Table A.3.19: ATE Estimates: Short Run Labor Market Outcomes

	Short Run				
	Out of the Labor Force (1)	Days Worked Last Month (2)	Hours Practicing Technical Skills (3)	Total Earnings Last Month (4)	First Job Duration (5)
MYF Treatment	-.05***	1.14**	17.51***	.16	18.76***
	(.02)	(.54)	(4.92)	(1.85)	(5.05)
Control Mean	.22	16.32	52.15	12.38	81.18
Control SD	.41	9.10	102.84	39.16	102.12
T Effect (%)	-25.24	7.01	33.57	1.29	23.11
N	844	844	838	843	833

Notes: In this table, we report the average treatment effects of the MYF program on primary employment outcomes. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.2 for the description of the variables.

Table A.3.20: ATE Estimates: Labor Market Trajectory in the Medium Run

	Transitions		**Medium Run**	
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Total Earnings Last Month (4)
MYF Treatment	.03	.06*	-.03	7.72**
	(.02)	(.03)	(.02)	(3.58)
Control Mean	.20	.41	.27	34.18
Control SD	.40	.49	.44	46.85
T Effect (%)	14.35	14.71	-11.60	22.60
N	844	844	838	838

Notes: In this table, we report the average treatment effects of the MYF program on match quality and labor market dynamics. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.3 for the description of the variables.

Table A.3.21: ATE Estimates: Willingness to Accept a Job and Job Search Behavior

	Willingness to Accept a Job			Job Search			Search Duration
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Efficacy Index (4)	Search Intensity Index (5)	Started Job Search (6)	Search Duration | Searched (7)
MYF Treatment 3 convo	-15.09***	.09**	-.07**	-.07	.02	.04**	-10.39**
	(4.20)	(.04)	(.03)	(.07)	(.08)	(.02)	(4.74)
Control Mean	36.76	.54	.21	.04	-.01	.93	28.28
Control SD	48.14	.50	.41	.96	.81	.25	68.22
T Effect (%)	-41.04	17.05	-32.94	.	.	3.79	-36.74
N	737	739	745	934	934	934	885

Notes: In this table, we report the averagetreatment effects of the MYF program on willingness to accept a job and job search outcomes. These are obtained by 2SLS estimation. We use treatment assignment as an IV for treatment take-up and report 2SLS regression estimates, which measure the effect of treatment on the compliers. Below each coefficient estimate, we report the strata-level clustered standard errors. See Table 1.4 for the description of the variables.

ITT Results Excluding Referred Students

Table A.3.22: ITT Estimates: Short Run Labor Market Outcomes

	Short Run				
	Out of the Labor Force (1)	Days Worked Last Month (2)	Hours Practicing Technical Skills (3)	Total Earnings Last Month (4)	First Job Duration (5)
MYF Treatment	-.057***	1.244**	17.237***	1.505	19.355***
	(.019)	(.538)	(5.112)	(2.164)	(5.326)
Control Mean	.21	16.15	52.15	11.35	81.18
Control SD	.41	9.20	102.84	39.07	102.12
T Effect (%)	-26.47	7.70	33.05	13.25	23.84
N	919	919	824	918	819

Table A.3.23: ITT Estimates: Labor Market Trajectory in the Medium Run

	Transitions		Medium Run	
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Total Earnings Last Month (4)
MYF Treatment	.043**	.077**	-.021	5.791
	(.021)	(.034)	(.023)	(3.742)
Control Mean	.18	.37	.26	34.84
Control SD	.39	.48	.44	47.62
T Effect (%)	23.79	20.84	-8.12	16.62
N	919	919	902	902

Table A.3.24: ITT Estimates: Willingness to Accept a Job and Job Search Behavior

	Willingness to Accept a Job			Job Search			Search Duration
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Search Efficacy Index (4)	Search Intensity Index (5)	Started Job Search (6)	Search Duration | Searched (7)
MYF Treatment	-11.127***	.066**	-.057**	-.061	.017	.028*	-8.397**
	(3.357)	(.030)	(.025)	(.060)	(.070)	(.015)	(4.111)
Control Mean	36.76	.54	.21	.04	-.01	.93	28.28
Control SD	48.14	.50	.41	.96	.81	.25	68.22
T Effect (%)	-30.27	12.21	-27.12	.	.	3.03	-29.69
N	722	724	734	919	919	919	870

Appendix B

Whom Would You Rather Work With? An Experiment on Gender Discrimination in the Referral System

B.1 Hypothetical profiles

The hypothetical profiles were built using information from a younger cohort of vocational training students and several templates of CVs compiled by a team of enumerators. Specifically, we used the templates to build the structure of the hypothetical profiles, so they would look similar to other profiles in Ugandan labor markets. Based on the templates, the hypothetical profiles had four sections: personal information (name, gender, date of birth, nationality, home district), academic background (vocational training institute, high school), work experience (with the listing of work experience, with duration, business name, role title, and a brief description of main activities), and languages. Figure B.3.2 provides an example of a profile sent to respondents through phone.

The profiles matched the respondent's sector of specialization, which is a good predictor of respondents' current sector of employment[1], but the nationality and the languages were kept constant across all profiles (respectively, Ugandan and English, Luganda, and Lusoga) and the name and the gender were conditional on the randomization of the gender of the high-quality candidate. Vocational training institute was conditional on the respondents' training institute, so the profile's institute would always match the respondent's institute. Home district was conditional on respondent's training institute and could be either Kampala or Jinja. Since in our sample all vocational training institutes that offer a certain training are located in the same district, home district is invariant given a sector of specialization.

Given the respondent's sector of specialization, work experience was conditional on the

[1]Of 454 subjects that were employed when interviewed, 64% (290) were employed in the same sector of training when interviewed. Half of those that were not working in the sector of training were employed in retail (58) or agriculture.

quality of the profile. Both high-quality and low-quality profiles have 3 months of internship in their sector of specialization and 3 months of pre-training general experience in retail, but the high-quality profiles had extra 5 months of temporary work in the sector of training. High school was conditional on quality of the profile and home district. We selected similar firms and high schools for the high- and low-quality profiles, which enumerators corroborated to be similar, but, given a certain sector of specialization, the names are systematically different. Therefore, the difference between the high- and low- quality profiles amounts mainly to the 5 extra months of work experience, but differences in the name of the firm and high school could also be embedded in it. We maneuver this issue by including sector of specialization fixed effects in our estimations as controls. In any case, this difference exists both in the HEM group and in the HEF group and pose any threat to the identification of the gender bias.

In total, we created 156 unique profiles, which were variations of 26 underlying profiles with unique high school and work experience for the 13 sectors of specialization. The profiles were displayed on the survey software and enumerators sent them through SMS or, if available, a software of instant messaging. To make sure respondents received the correct profile, enumerators made checks about the identity of the CV.

We highlight that, by matching the vocational training institutes of the profile and the respondent, we are able to make the experiment with the hypothetical profiles more realistic. This ensures that respondents are not referring completely unknown workers, but rather comparing young workers that studied in the same institutions as theirs and that have pursued a similar path. In this way, the audit experiment approximates a real referring setting to the extent that subjects are able to support information that goes beyond what is written in the profile and that is particularly linked to their shared background in the VTI.

B.2 Alternative identification strategy

In this second strategy, we compare the differences in the referral probability of the pairs of high- and low-experience profiles, $\Delta^H = P_H^{\text{refer}} - P_L^{\text{refer}}$. Had we shown two perfectly equivalent profiles to candidates, we would expect both profiles to be equivalently referred by respondents:

$$\begin{aligned} E(\Delta) &= E(P_1^{\text{refer}} - P_2^{\text{refer}}) \\ &= 0.5 - 0.5 = 0 \end{aligned}$$

As before, with a difference in quality, we expect the difference in referral probability between the high-experience and low-experience profiles to be equal to the experience effect:

$$\begin{aligned} E(\Delta^H) &= E(P_H^{\text{refer}} - P_L^{\text{refer}}) \\ &= \gamma_1 \cdot \text{ExperienceDiff} \lesseqgtr 0 \end{aligned}$$

where ExperienceDiff $= 1$ if there is a difference in experience.

Introducing gender difference, we have:

$$\begin{aligned} E(\Delta^H | G = g) &= E(P_H^{\text{refer}} - P_L^{\text{refer}} | G = g) \\ &= \gamma_1 \cdot \text{ExperienceDiff} + \gamma_2 \cdot \text{GendDiff}_g \end{aligned}$$

where $g = m, f$ denotes the gender of the high-quality candidate and, as before, GendDiff_g is a variable such that $\text{GendDiff}_f = 1$ and $\text{GendDiff}_m = -1$.

Denoting the experience difference as equal to 1, the potential outcomes for the groups in which the high-experience candidate is a woman and in which the high-experience candidate is a man are:

$$\begin{aligned} \text{Group 1 (HEM):} \quad & E(\Delta^H | G = m) = \gamma_1 - \gamma_2 \\ \text{Group 2 (HEF):} \quad & E(\Delta^H | G = f) = \gamma_1 + \gamma_2 \end{aligned}$$

Again, if $\gamma_2 > 0$, there is a positive bias for women. If $\gamma_2 < 0$, there is a negative bias against women. The coefficient γ_1 captures the quality effect (the effect of QualityDiff).

The parameter γ_2 can be causally estimated by regressing the difference $\Delta_{g,i}^{HQ}$ on GendDiff_g:

$$\Delta_{g,i}^{HQ} = a \; + \underbrace{b}_{\gamma_2} \text{GendDiff}_g \; + \underline{c}\, \underline{S} + \underline{d}\, \underline{X} + \varepsilon_i +$$

The coefficient of interest is b. Differently from the previous identification strategy, under this approach, we can estimate the average quality effect, which is captured by the coefficient a, and compare it to the gender bias effect. However, in the lack of randomization of quality differential, it does not have a causal interpretation. Strata dummies $\underline{S}$ and sector of specialization and VTI fixed effects $\underline{X}$ are also included, but, to ensure that a is interpreted as the average quality effect, the values of the strata variables are recoded as -1 and 1 rather than zero and 1 (otherwise, a would only capture the quality effect for the subjects which have strata variables equal to zero).

B.3 Additional Figures and Tables

Table B.3.1: External validity: Socio-economic and labor market characteristics

	(1) UNHS Sample		(2) UNHS VTI Sample		(3) Study Sample		T-test P-value	
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(3)-(1)	(3)-(2)
Female	20409	0.55 (0.00)	785	0.52 (0.03)	555	0.39 (0.02)	0.00***	0.00***
Age	20409	27.18 (0.06)	785	29.37 (0.26)	554	29.05 (0.14)	0.00***	0.28
Married	20409	0.63 (0.00)	785	0.66 (0.03)	481	0.34 (0.02)	0.00***	0.00***
Completed primary school	17216	0.59 (0.01)	785	1.00 (0.00)	555	1.00 (0.00)	0.00***	N/A
Completed secondary school	17216	0.16 (0.00)	785	1.00 (0.00)	555	1.00 (0.00)	0.00***	N/A
Completed vocational training	16499	0.05 (0.00)	785	1.00 (0.00)	555	1.00 (0.00)	0.00***	N/A
Any work in last 7 days - no agri.	20409	0.46 (0.00)	785	0.75 (0.02)	555	0.81 (0.02)	0.00***	0.03**
Any work in last 7 days - agri. included	20088	0.77 (0.00)	785	0.84 (0.02)	555	0.86 (0.01)	0.00***	0.54
Monthly earnings (USD) - wage employed	3850	79.46 (2.36)	434	106.66 (6.69)	217	114.05 (4.67)	0.00***	0.37
Female sample								
Age	11209	27.09 (0.08)	391	28.67 (0.39)	215	28.27 (0.21)	0.00***	0.37
Married	11209	0.67 (0.01)	391	0.68 (0.03)	188	0.34 (0.03)	0.00***	0.00***
Completed primary school	9462	0.56 (0.01)	391	1.00 (0.00)	216	1.00 (0.00)	0.00***	N/A
Completed secondary school	9462	0.14 (0.01)	391	1.00 (0.00)	216	1.00 (0.00)	0.00***	N/A
Completed vocational training	9117	0.05 (0.00)	391	1.00 (0.00)	216	1.00 (0.00)	0.00***	N/A
Any work in last 7 days - no agri.	11209	0.36 (0.01)	391	0.69 (0.03)	216	0.68 (0.03)	0.00***	0.70
Any work in last 7 days - agri. included	11040	0.72 (0.01)	391	0.76 (0.03)	216	0.75 (0.03)	0.26	0.80
Monthly earnings (USD) - wage employed	1398	63.06 (3.17)	191	83.06 (7.85)	43	72.50 (9.21)	0.33	0.38
Male sample								
Age	9200	27.29 (0.09)	394	30.12 (0.34)	339	29.54 (0.18)	0.00***	0.13
Married	9200	0.57 (0.01)	394	0.63 (0.04)	293	0.34 (0.03)	0.00***	0.00***
Completed primary school	7754	0.62 (0.01)	394	1.00 (0.00)	339	1.00 (0.00)	0.00***	N/A
Completed secondary school	7754	0.19 (0.01)	394	1.00 (0.00)	339	1.00 (0.00)	0.00***	N/A
Completed vocational training	7382	0.05 (0.00)	394	1.00 (0.00)	339	1.00 (0.00)	0.00***	N/A
Any work in last 7 days - no agri.	9200	0.58 (0.01)	394	0.82 (0.03)	339	0.90 (0.02)	0.00***	0.01**
Any work in last 7 days - agri. included	9048	0.84 (0.01)	394	0.93 (0.01)	339	0.92 (0.01)	0.00***	0.80
Monthly earnings (USD) - wage employed	2452	88.59 (3.18)	243	125.91 (10.12)	174	124.31 (5.08)	0.00***	0.89

Notes: The value displayed for t-tests are p-values. Observations are weighted using variable wgt as pweight weights.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table B.3.2: External validity: Sector Relevance

	UNHS Sample			UNHS VTI Sample			Study Sample		
	% All	% Female	% Male	% All	% Female	% Male	% All	% Female	% Male
Food and hospitality	4.82	65.88	34.12	4.35	32.28	67.72	5.84	73.08	26.92
Tailoring	0.64	78.83	21.17	0.47	55.42	44.58	6.74	93.33	6.67
Electrical work	0.11	15.56	84.44	1.31	2.85	97.15	20.45	2.20	97.80
Motor mechanics	0.81	9.51	90.49	0.85	33.29	66.71	14.16	3.17	96.83
Construction	3.17	0.54	99.46	5.01	2.34	97.66	4.49	5.00	95.00
Plumbing	0.07	0.00	100.00	0.01	0.00	100.00	8.76	2.56	97.44
Retail	15.57	58.98	41.02	19.67	67.57	32.43	13.03	63.79	36.21
Secretary/accounting	0.77	51.98	48.02	2.79	77.19	22.81	5.17	43.48	56.52
Teaching (primary and pre-primary education)	2.32	58.20	41.80	16.64	56.23	43.77	3.37	80.00	20.00
Hairdressing	1.21	65.82	34.18	1.89	67.46	32.54	3.15	71.43	28.57
Agriculture	54.06	57.91	42.09	11.72	40.71	59.29	8.99	32.50	67.50
Machining and Fitting	0.48	4.35	95.65	1.32	0.00	100.00	0.45	0.00	100.00
Other unskilled	8.05	25.28	74.72	11.67	29.42	70.58	2.02	66.67	33.33
Other skilled	7.91	38.16	61.84	22.32	49.78	50.22	3.37	20.00	80.00

Table B.3.3: Attrition

Variable	(1) Non-Attrited N	Mean/SE	(2) Attrited N	Mean/SE	T-test P-value (1)-(2)
Age	554	27.05	152	26.79	0.38
		(0.14)		(0.25)	
Gender (male=1)	555	0.61	156	0.49	0.01***
		(0.02)		(0.04)	
Married	481	0.34	58	0.36	0.75
		(0.02)		(0.06)	
Traditional Religious Denomination	555	0.74	145	0.77	0.57
		(0.02)		(0.04)	
Ethnic minority	555	0.46	145	0.35	0.02**
		(0.02)		(0.04)	
Household asset index	555	0.10	147	-0.41	0.32
		(0.25)		(0.36)	
Alum had a scholarship while at VTI	555	0.26	154	0.21	0.29
		(0.02)		(0.03)	
Rural	481	0.52	78	0.46	0.34
		(0.02)		(0.06)	
Years active in the labor market	476	2.78	70	2.36	0.14
		(0.10)		(0.25)	
Years active in current job	341	2.28	79	2.47	0.40
		(0.09)		(0.22)	
No. of employees in the current firm	441	42.71	3	5.67	0.71
		(8.33)		(2.60)	

Notes: The value displayed for t-tests are p-values. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Figure B.3.1: Main method to find job of wage employed in baseline

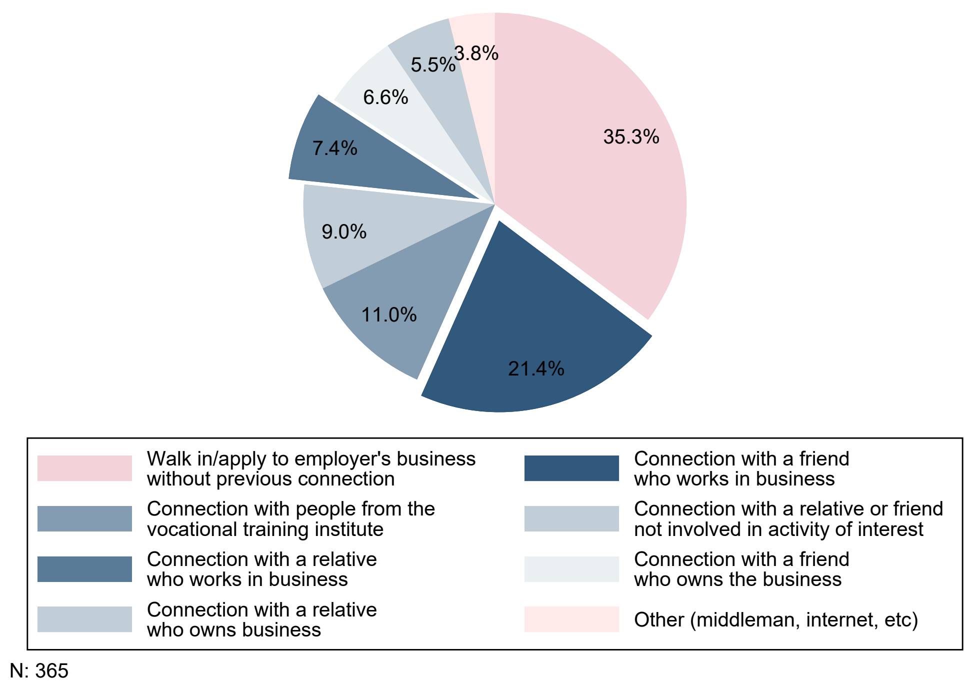


Figure B.3.2: Example of profile shown to subjects

CANDIDATE ID 8284

PERSONAL PROFILE

Name: Rachel

Date of birth: 18 May 1999

Sex: Female

Nationality: Ugandan

Home district: Kampala

ACADEMIC BACKGROUND

2019-2021 Pioneer: National Certificate in Tailoring and Fashion Design

2014-2019 Mengo Senior School: Uganda Certificate of Education (UCE)

EXPERIENCE

April 2021 -June 2021 Tina Fashions: Traineeship. Responsibilities: fixing defective clothes and sewing garments.

July 2017 - September 2017 A&K ACCESSORIES: Part-time job. Responsibilities: receiving payments from clients, selling phone accessories, stock management.

LANGUAGES

Excellent written and spoken English, Luganda and Lusoga

3:50 PM

Table B.3.4: Main results: first part (benchmarked with alternative grattini-specification)

	(1) Probability of selecting Hi Exp	(2) Probability of selecting Hi Exp	(3) Difference in probabilities (Hi Exp minus Lo Exp)	(4) Difference in probabilities (Hi Exp minus Lo Exp)
Hi Exp Candidate female	-0.119**	-0.111**		
	(0.041)	(0.042)		
GenDiff			-0.119**	-0.111**
			(0.041)	(0.042)
Constant	0.628***	0.673***	0.299***	0.385**
	(0.041)	(0.078)	(0.053)	(0.144)
Control Mean	0.68	0.68	0.37	0.37
Control SD	0.47	0.47	0.93	0.93
Strata FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Treatment Effect (%)	-17.33	-16.26	-32.09	-30.10
Treatment Effect (sd)	-0.26	-0.24	-0.13	-0.12
N	555	555	555	555

Standard errors in parentheses
GenDiff = 1 if female is high-quality, and -1 otherwise.
Controls include training area and vocational training institute fixed effects.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3.5: Main results: first part (benchmarked with alternative specification, sector of specialization FE not omitted)

	(1) Probability of selecting Hi Exp	(2) Probability of selecting Hi Exp	(3) Difference in probabilities (Hi Exp minus Lo Exp)	(4) Difference in probabilities (Hi Exp minus Lo Exp)
Hi Exp Candidate female	-0.119**	-0.111**		
	(0.041)	(0.042)		
Plumbing		-0.024		-0.049
		(0.079)		(0.157)
Food and hospitality		-0.056		-0.111
		(0.098)		(0.196)
Tailoring		0.054		0.108
		(0.107)		(0.215)
Hairdressing		0.079		0.157
		(0.130)		(0.259)
Construction		-0.105		-0.209
		(0.100)		(0.199)
Electrical Work		-0.055		-0.109
		(0.064)		(0.128)
Welding		-0.683***		-1.366***
		(0.096)		(0.192)
Carpentry		0.411***		0.822***
		(0.083)		(0.165)
Teaching (primary and pre-primary edu.)		-0.074		-0.148
		(0.121)		(0.241)
Agriculture		0.037		0.074
		(0.175)		(0.349)
Machining and fitting		-0.409		-0.818
		(0.290)		(0.580)
GenDiff			-0.119**	-0.111**
			(0.041)	(0.042)
Constant	0.628***	0.673***	0.299***	0.385**
	(0.041)	(0.078)	(0.053)	(0.144)
Strata FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	555	555	555	555

Standard errors in parentheses
GenDiff = 1 if female is high-quality, and -1 otherwise.
Controls include training area and vocational training institute fixed effects.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3.6: Main results: second part (continuation)

	(1) Referred female netw.	(2) Referred female netw.	(3) Referred female netw. (cond. on mention)	(4) Referred female netw. (cond. on mention)	(5) Referred female netw. (cond. on refer netw.)	(6) Referred female netw. (cond. on refer netw.)	(7) Referred network (cond. on mention)	(8) Referred network (cond. on mention)
Hi Exp Candidate female	-0.005	-0.011	-0.010	-0.014	-0.040	-0.036	0.019	0.014
	(0.031)	(0.031)	(0.039)	(0.039)	(0.042)	(0.041)	(0.043)	(0.043)
Control Mean	0.20	0.20	0.26	0.26	0.36	0.36	0.75	0.75
Control SD	0.40	0.40	0.44	0.44	0.48	0.48	0.44	0.44
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Treatment Effect (%)	-2.32	-5.44	-3.62	-5.36	-11.39	-10.07	2.58	1.86
Treatment Effect (sd)	-0.01	-0.03	-0.02	-0.03	-0.08	-0.07	0.04	0.03
N	555	555	401	401	302	302	401	401

Standard errors in parentheses

Strata added as controls but omitted in the table.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3.7: Heterogeneity by gender: second part (with network)

	Panel A: Full sample			
	(1) Referred female 1st part*	(2) Referred female 2nd part**	(3) Mentioned female netw.	(4) Referred network
Hi Exp Candidate female	0.252***	0.143***	0.003	-0.027
	(0.040)	(0.037)	(0.039)	(0.043)
Control Mean	0.32	0.33	0.45	0.56
Control SD	0.47	0.47	0.50	0.50
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Treatment Effect (%)	80.11	42.96	0.65	-4.77
Treatment Effect (sd)	0.54	0.30	0.01	-0.05
N	555	555	555	555
	Panel B: Sample - Female respondent			
	(1) Referred female 1st part*	(2) Referred female 2nd part**	(3) Mentioned female netw.	(4) Referred network
Hi Exp Candidate female	0.310***	0.190**	0.014	0.066
	(0.066)	(0.064)	(0.066)	(0.073)
Control Mean	0.39	0.55	0.66	0.44
Control SD	0.49	0.50	0.48	0.50
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Treatment Effect (%)	79.67	34.81	2.13	14.84
Treatment Effect (sd)	0.63	0.38	0.03	0.13
N	216	216	216	216
	Panel C: Sample - Male respondent			
	(1) Referred female 1st part*	(2) Referred female 2nd part**	(3) Mentioned female netw.	(4) Referred network
Hi Exp Candidate female	0.215***	0.110*	-0.019	-0.088
	(0.052)	(0.047)	(0.049)	(0.055)
Control Mean	0.27	0.19	0.32	0.63
Control SD	0.44	0.40	0.47	0.48
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Treatment Effect (%)	80.56	56.92	-5.91	-14.03
Treatment Effect (sd)	0.48	0.28	-0.04	-0.18
N	339	339	339	339
	Difference in effects across subsamples (test Panel B = Panel C)			
Difference	0.095	0.080	0.033	0.154
P-Value	0.177	0.129	0.657	0.084

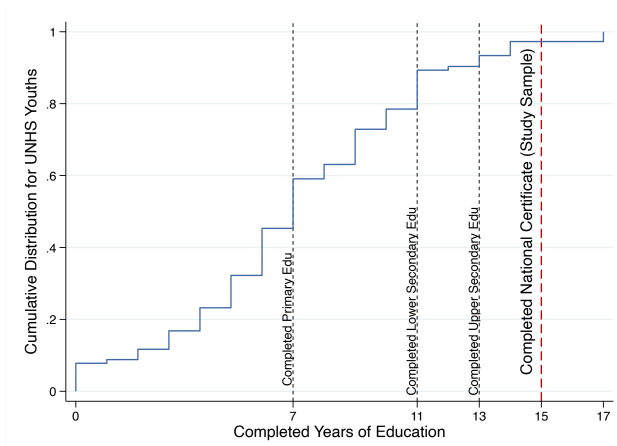
Table B.3.8: Heterogeneity by gender attitude: main results

	Panel A: Full sample		
	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	-0.111**	-0.307+	-0.045**
	(0.042)	(0.170)	(0.016)
Control Mean	0.68	7.62	0.74
Control SD	0.47	1.93	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	-16.26	-4.03	-6.08
Treatment Effect (sd)	-0.24	-0.16	-0.25
N	555	555	555
	Panel B: Sample - Gender attitude above median		
	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	-0.017	-0.142	-0.028
	(0.060)	(0.232)	(0.023)
Control Mean	0.62	7.59	0.74
Control SD	0.49	1.94	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	-2.68	-1.86	-3.74
Treatment Effect (sd)	-0.03	-0.07	-0.15
N	276	276	276
	Panel C: Sample - Gender attitude below median		
	(1) Probability of selecting Hi Exp	(2) Likability of Hi Exp	(3) Perceived prob. of retention of Hi Exp
Hi Exp Candidate female	-0.193**	-0.489+	-0.063**
	(0.059)	(0.258)	(0.024)
Control Mean	0.75	7.64	0.74
Control SD	0.44	1.93	0.18
Strata FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Treatment Effect (%)	-25.81	-6.39	-8.54
Treatment Effect (sd)	-0.44	-0.25	-0.34
N	278	278	278
	Difference in effects across subsamples (test Panel B = Panel C)		
Difference	0.176	0.347	0.035
P-Value	0.026	0.282	0.333

Appendix C

Gender Gaps: Back and Here to Stay? Evidence from Skilled Ugandan Workers during COVID-19

Figure C.1.1: Educational Attainment of Ugandan Youths from UNHS and Study Sample



Notes: This figure shows the cumulative distribution function of years of education for the population of Ugandan adults aged 18–39 from the Uganda National Household Survey 2016/2017 (UNHS). The UNHS sample of young adults is reweighted so that its age and gender distribution matches that of the study sample. The four dashed lines indicate the number of years of education corresponding to completing primary education (7), completing lower secondary education (11), completing upper secondary education (13) and completing the National Certificate program at a Vocational Training Institute (15). The latter corresponds to the minimum education level attained by the respondents in our sample.

Table C.1.1: Ever and Never Attritors' Baseline Characteristics: Summary Statistics and Balance Tests

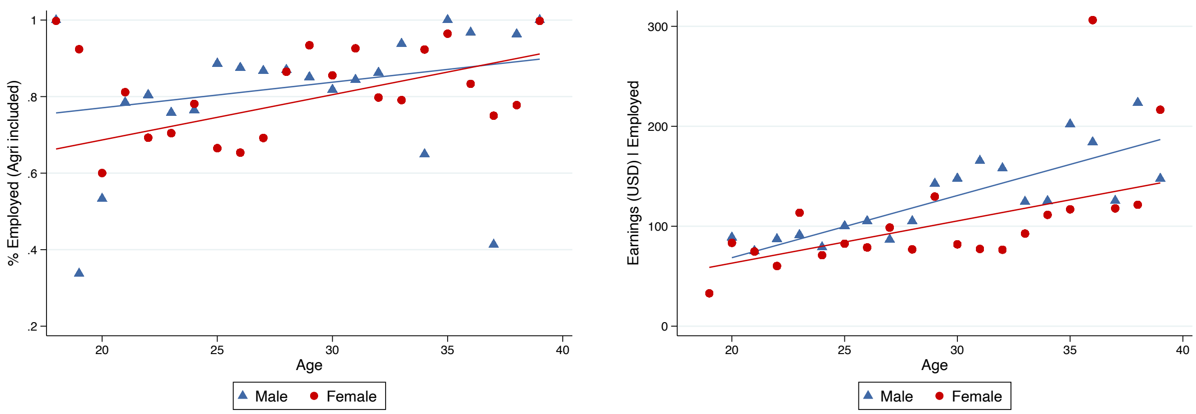
	Ever Attritors		*Never Attritors*			
	Obs	Mean	Obs	Mean	Diff	p-value
Panel A: Socio-economic characteristics						
Female	258	.473	456	.379	.093**	.016
Age	253	24.984	456	25.022	-.038	.882
Married	87	.368	316	.361	.007	.904
Has children	102	.510	454	.460	.049	.368
Num. school-age children in household	100	.840	453	.872	-.032	.811
Traditional religious denomination	247	.725	456	.761	-.036	.297
Ethnic minority	247	.389	456	.465	-.076*	.050
House of origin: rural	106	.481	456	.518	-.036	.500
Region of origin: central	253	.423	452	.341	.082**	.032
Region of origin: eastern	253	.415	452	.440	-.025	.516
Region of origin: northern	253	.075	452	.142	-.066***	.005
Region of origin: western	253	.087	452	.077	.010	.662
Caretaker's years of education	161	9.739	301	10.402	-.663	.197
Agricultural household of origin	243	.206	454	.176	.030	.349
Household of origin assets index	249	.293	456	-.160	.452	.190
Panel B: Labor market characteristics						
Years since graduation	248	3.052	456	3.145	-.092	.585
Years employed since graduation	96	2.652	453	2.756	-.105	.661
Years in current job	140	2.507	282	2.241	.266	.179
Wage employed	243	.539	448	.565	-.026	.519
Self employed	243	.243	448	.194	.049	.145
Permanent job	126	.865	245	.747	.118***	.004
Formal firm	180	.500	325	.440	.060	.197
Employed in Skilled Sector	243	.626	446	.668	-.043	.266
Employed in Skilled Sector | Employed	190	.800	340	.876	-.076**	.025
Employed in Training Sector	243	.531	446	.587	-.057	.154
Employed in Training Sector | Employed	190	.679	340	.771	-.092**	.025
Earnings (USD)	157	69.524	274	63.918	5.606	.436
Earnings (USD) | Employed	104	104.955	168	104.248	.707	.930
Enrolled in further education	243	.033	448	.060	-.027*	.089
Engaged in casual occupations	243	.037	448	.060	-.023	.161
Other non-employed	243	.148	448	.121	.028	.316

Notes: The table reports summary statistics for a set of baseline socio-economic and labor market characteristics separately for "Ever Attritors", (i.e., respondents successfully interviewed in fewer than four survey rounds) and "Never Attritors", (i.e., respondents successfully interviewed in all the four survey rounds) and tests for differences between these two groups in the full sample of respondents. See the notes to Table 3.1 for details on how the variables are constructed.

Figure C.1.2: Vocational Graduates' Careers in the UNHS

(a) Employment Profile by Gender

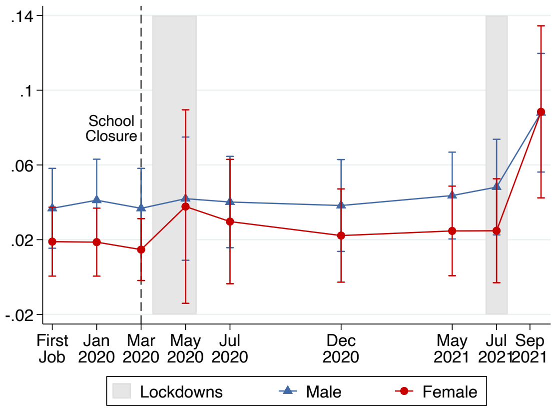
(b) Earnings Profile by Gender



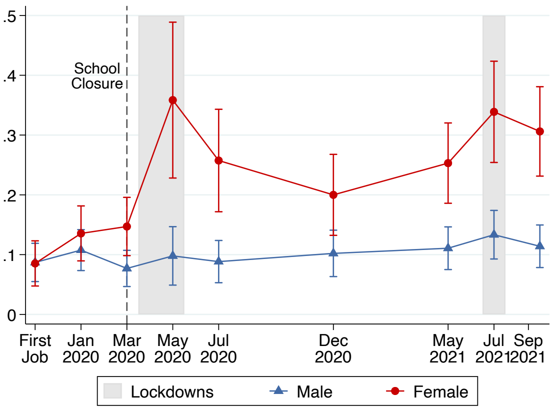
Notes: This figure shows average employment rate (panel [a]) and monthly earnings conditional on employment (panel [b]) by age and a fitted line separately for female and male respondents who completed post-secondary vocational education from the Uganda National Household Survey 2016/2017 (UNHS). The UNHS sample is restricted to respondents aged 18–39 and then reweighted so that its age and gender distribution matches that of the study sample. In panel (a), the slopes and standard errors of the fitted lines are 0.014 (0.01) for males and 0.012 (0.01) for females. In panel (b), they are 6.74 (1.28) for males and 2.72 (1.34) for females.

Figure C.1.3: The Emergence of Gender Disparities in Unskilled Employment After the Lockdowns

(a) % Employed in Agriculture | Employed



(b) % Employed in Non-Agricultural Unskilled Sectors | Employed



Notes: The figure illustrates the average share of respondents employed in agriculture (panel [a]) and in non-agricultural unskilled sectors (panel [b]) conditional on employment over time and by gender. Non-agricultural unskilled sectors include retail, and "Other Unskilled". For details on this residual category, see the notes to Table 3.1. The first data point refers to the respondents' first activity after completing vocational education. It may coincide with the activity in January 2020 and its start and end date may be different for each respondent. 95% robust confidence intervals are reported.

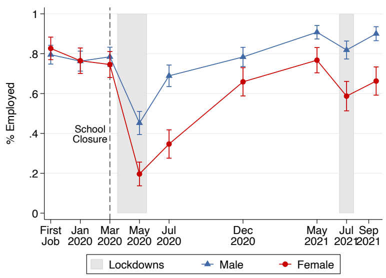
Table C.1.2: The Effects of the Lockdowns on Hours Worked, Borrowing, Selling Assets, Mental Health, Ability to Work

		Male			Female			T-test	
Time	Outcome	N	Mean	Std. dev.	N	Mean	Std. dev.	Diff F-M	p-value
Panel a. Hours worked									
May 2020	Reduced hours worked | Self-employed	74	.595	.494	42	.595	.497	.001	.995
May 2020	Reduced hours worked | Wage-employed	89	.438	.499	19	.684	.478	.246*	.052
Jul 2020	Business open but reduced hours of operation	264	.492	.501	104	.606	.491	.113*	.050
Dec 2020	Hours worked | Wage-employed	178	9.320	1.728	86	9.047	3.169	-.274	.365
May 2021	Hours worked	310	9.681	1.993	170	9.753	2.325	.072	.721
Jul 2021	Hours worked	283	8.830	2.481	131	8.435	2.434	-.395	.130
Sep 2021	Hours worked	307	9.684	2.073	147	9.320	2.537	-.364	.105
Panel b. Borrowing									
Jul 2020	Since lockdown borrowed money to cover living expenses	309	.184	.388	200	.145	.353	-.039	.247
Jul 2020	ln the next 2 weeks will borrow money to cover living expenses	309	.107	.309	200	.090	.287	-.017	.539
Dec 2020	In the last 4 months borrowed to cover living expenses	326	.261	.440	226	.230	.422	-.031	.413
Jul 2021	Borrowed money to cope with 2^{nd} lockdown | Self-employed	108	.102	.304	80	.200	.403	.098*	.058
Jul 2021	Borrowed money to cope with 2^{nd} lockdown | Wage-employed	189	.095	.294	80	.062	.244	-.033	.382
Panel c. Selling Assets									
Jul 2020	Sold assets as result to COVID-19	376	.152	.359	265	.132	.339	-.020	.488
Jul 2020	ln the next 2 weeks will sell assets to cover living expenses	309	.023	.149	200	.010	.100	-.013	.291
Dec 2020	In the last 4 months sold assets to cover living expenses	332	.123	.329	231	.121	.327	-.002	.935
Jul 2021	Sold assets to cope with 2^{nd} lockdown | Self-employed	108	.019	.135	80	.000	.000	-.019	.223
Jul 2021	Sold assets to cope with 2^{nd} lockdown | Wage-employed	189	.026	.161	80	.013	.112	-.014	.480
Panel d. Mental health									
Jul 2020	Anxious due to COVID-19 oubtreak	364	.764	.425	252	.849	.359	.085***	.009
Dec 2020	Anxious due to COVID-19 oubtreak	326	.653	.477	226	.743	.438	.090**	.025
Sep 2021	Anxious due to COVID-19 oubtreak	339	.732	.444	217	.797	.403	.066*	.078
Panel E. Childcare and ability to work									
Sep 2021	Schools closure affected ability to work via childcare (0-10)	338	.964	2.330	217	2.336	3.163	1.372***	.000

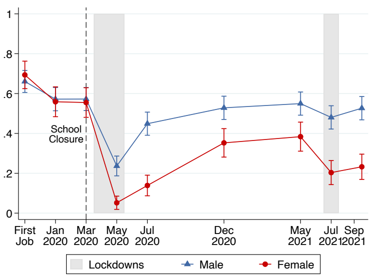
Notes: The table reports summary statistics by gender and tests for gender differences for a set of outcomes.

Figure C.1.4: Robustness of Gender Gaps in Employment and Occupation Type in the Balanced Panel

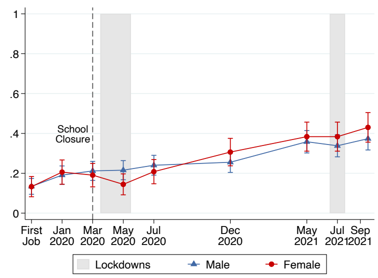
(a) % Employed



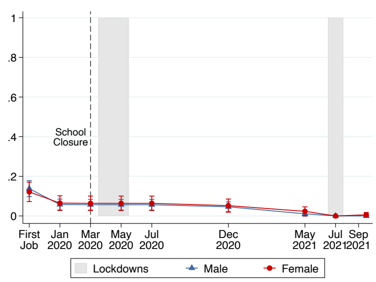
(b) % Wage employed



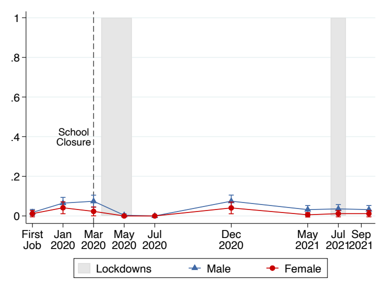
(c) % Self employed



(d) % Enrolled in further education



(e) % Engaged in casual occupations



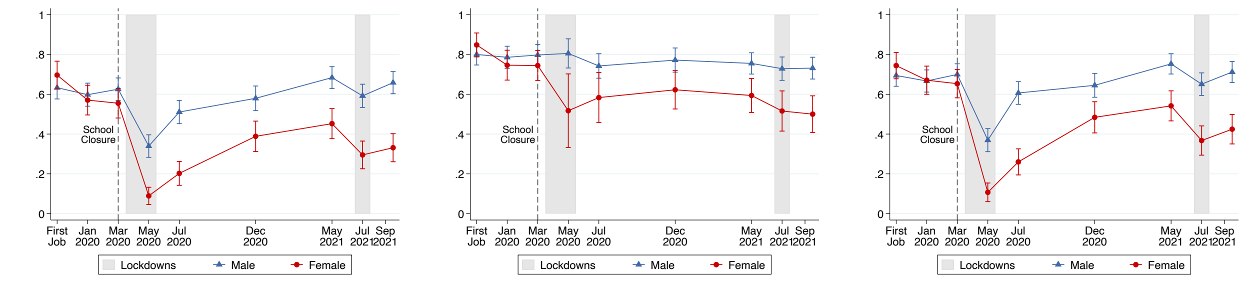
Notes: The figure illustrates the average share of respondents that are employed (panel [a]), wage-employed (panel [b]), self-employed (panel [c]), enrolled in educational programs (panel [d]), and engaged in casual occupations (panel [e]) over time and by gender in the balanced panel of respondents. This sample includes the 456 respondents we successfully interviewed in all the four survey rounds. See the notes to Table 3.1 for details on how the variables are constructed. 95% robust confidence intervals are reported.

Figure C.1.5: Robustness of Gender Gaps in Employment Quality in the Balanced Panel

(a) % Employed in Training Sector

(b) % Employed in Training Sector | Employed

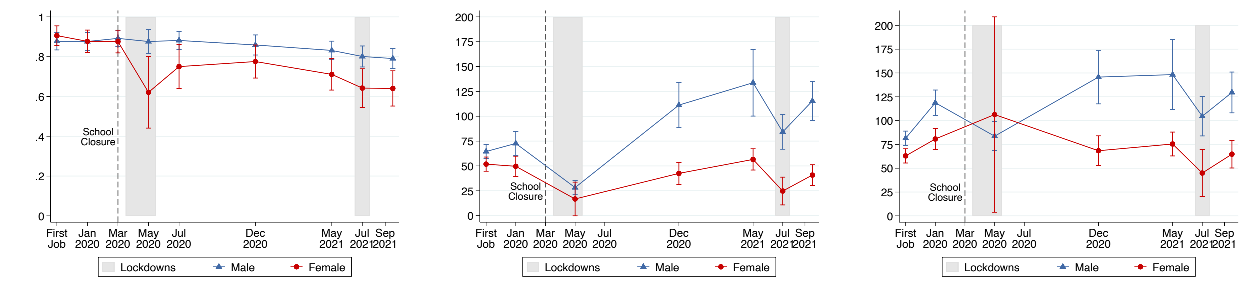
(c) % Employed in Skilled Sector



(d) % Employed in Skilled Sector | Employed

(e) Monthly Earnings (USD)

(f) Monthly Earnings (USD) | Employed



Notes: The figure illustrates the average employment rate in the training sector, unconditional (panel [a]) and conditional on employment (panel [b]), employment rate in skilled sectors, unconditional (panel [c]) and conditional on employment (panel [d]), and monthly earnings, unconditional (pannel [e]) and conditional on employment (panel [f]), over time and by gender in the balanced panel of respondents. This sample includes the 456 respondents we successfully interviewed in all the four survey rounds. See the notes to Table 3.1 for details on how the variables are constructed. 95% robust confidence intervals are reported.

Table C.1.3: Robustnes of Gender Gaps in Employment and Employment Quality in the Balanced Panel

Outcome:	(1) % Employed	(2) % Wage-employed	(3) % Self-employed	(4) Employed in Training Sector	(5) Employed in Training Sector | Employed	(6) Employed in Skilled Sector	(7) Employed in Skilled Sector | Employed	(8) Monthly Earnings (USD)	(9) Monthly Earnings (USD) | Employed
Female × First job	0.070	0.050	0.020	0.133***	0.081*	0.096**	0.052	9.822	5.758
	(0.045)	(0.054)	(0.040)	(0.051)	(0.047)	(0.048)	(0.038)	(9.373)	(16.137)
Female × Jan 2020	0.037	0.006	0.031	0.045	0.012	0.048*	0.024		
	(0.030)	(0.034)	(0.028)	(0.031)	(0.032)	(0.029)	(0.021)		
Female × May 2020 (Lckdn 1)	-0.217***	-0.167***	-0.050*	-0.169***	-0.091**	-0.210***	-0.105**	11.384	45.275
	(0.058)	(0.054)	(0.027)	(0.051)	(0.036)	(0.052)	(0.052)	(12.793)	(49.406)
Female × Jul 2020	-0.303***	-0.293***	-0.011	-0.236***	-0.038	-0.298***	-0.046*		
	(0.053)	(0.049)	(0.015)	(0.044)	(0.035)	(0.050)	(0.028)		
Female × Dec 2020	-0.086*	-0.159***	0.072**	-0.116**	-0.049	-0.109**	-0.028	-48.123***	-53.703**
	(0.048)	(0.048)	(0.037)	(0.045)	(0.043)	(0.048)	(0.041)	(13.930)	(23.827)
Female × May 2021	-0.102*	-0.150**	0.048	-0.158***	-0.107*	-0.168***	-0.090*	-55.557***	-44.208**
	(0.055)	(0.058)	(0.052)	(0.058)	(0.059)	(0.055)	(0.050)	(18.397)	(19.978)
Female × Jul 2021 (Lckdn 2)	-0.191***	-0.260***	0.069	-0.220***	-0.129**	-0.239***	-0.123**	-37.785***	-36.582*
	(0.061)	(0.059)	(0.054)	(0.059)	(0.064)	(0.060)	(0.058)	(13.424)	(20.995)
Female × Sep 2021	-0.197***	-0.277***	0.080	-0.259***	-0.155**	-0.241***	-0.111**	-51.030***	-37.452*
	(0.059)	(0.059)	(0.054)	(0.058)	(0.063)	(0.058)	(0.056)	(13.523)	(19.081)
Observations	4,087	4,087	4,087	3,975	2,787	3,975	2,787	2,795	1,913
R-squared	0.431	0.528	0.556	0.543	0.663	0.520	0.682	0.396	0.472
Mean Dep. Var. Pre-Shock	0.770	0.566	0.204	0.599	0.778	0.682	0.886	63.92	104.2
sd Dep. Var. Pre-Shock	0.421	0.496	0.403	0.491	0.416	0.466	0.318	71.35	63.98

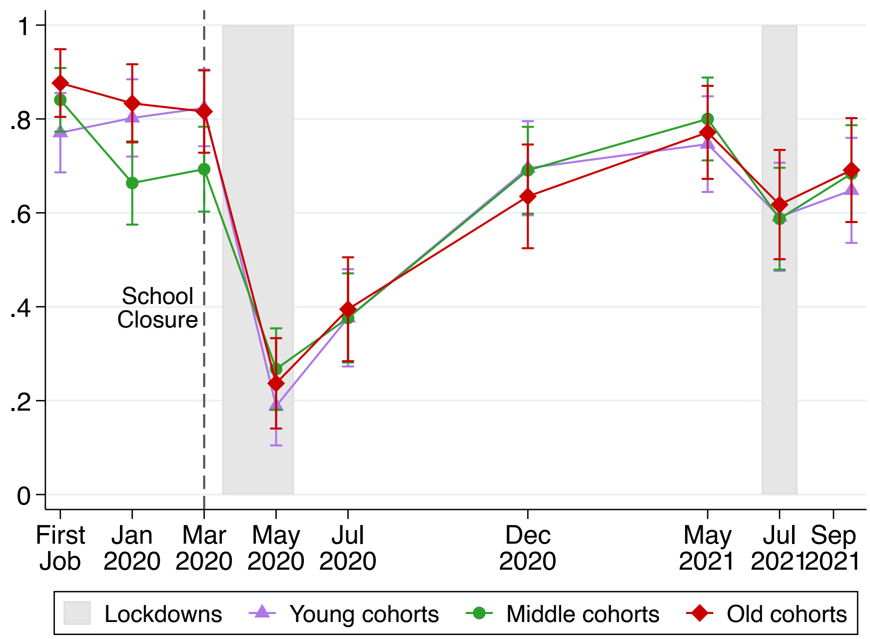
Notes: The table reports the β_y coefficients obtained estimating Equation 3.1 in the balanced panel of respondents. This sample includes the 456 respondents we successfully interviewed in all the four survey rounds. The dependent variables are an indicator for respondents that are employed (column [1]), wage-employed (column [2]), self-employed (column [3]), employed in their trainig sector, unconditional (column [4]) and conditional on employment (column [5]), employed in skilled sectors, unconditional (column [6]) and conditional on employment (column [7]), and monthly earnings in USD, unconditional (column [8]) and conditional on employment (column [9]). See the notes to Table 3.1 and Figure 3.5 for the details about how the variables were built. The coefficient on $Female \times Mar2020$ is normalized to zero for all outcomes except for monthly earnings, in which case the coefficient on $Female \times Jan2020$ is normalized to zero. The table reports the mean and the standard deviation of the dependent variable measured in March 2020 (columns [1]-[7]) and January 2020 (columns [8]-[9]). Standard errors are robust to heteroskedasticity and clustered at the individual level. * p<0.1, ** p<0.05, *** p<0.01.

Table C.1.4: Gender Gaps Under Different Assumptions on Attritors' Employment Status and Sector

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)
VARIABLES	**Employed**			**Employed in Training Sector | Employed**								**Employed in Skilled Sector | Employed**							
Female × First Job	.149***	-.030	.069*	.068*	.101**	.094**	.127***	.095**	.034	.067	.036	.067**	.100***	.086***	.119***	.086***	.026	.059	.026
	(.037)	(.038)	(.036)	(.040)	(.041)	(.040)	(.041)	(.040)	(.041)	(.041)	(.040)	(.032)	(.036)	(.032)	(.036)	(.032)	(.034)	(.038)	(.034)
Female × Jan 20	.067**	-.067**	.008	-.013	.044	.028	.085**	.038	-.059*	-.002	-.050	.028	.085***	.056***	.113***	.055***	-.032	.025	-.033
	(.030)	(.030)	(.026)	(.028)	(.032)	(.029)	(.033)	(.028)	(.031)	(.035)	(.030)	(.019)	(.028)	(.020)	(.028)	(.019)	(.025)	(.032)	(.024)
Female × May 20 (Lckdn 1)	-.147***	-.150***	-.135***	-.036	-.121***	-.059**	-.144***	-.093***	-.006	-.092***	-.040	-.042	-.127***	-.057**	-.142***	-.088***	-.004	-.089***	-.036
	(.045)	(.045)	(.044)	(.029)	(.029)	(.029)	(.029)	(.026)	(.029)	(.030)	(.027)	(.027)	(.034)	(.026)	(.033)	(.033)	(.028)	(.034)	(.034)
Female × Jul 20	-.227***	-.232***	-.209***	-.022	-.061**	-.028	-.067***	-.042	-.019	-.058**	-.033	-.037*	-.076***	-.038*	-.076***	-.047**	-.028	-.067***	-.037*
	(.041)	(.041)	(.040)	(.027)	(.026)	(.027)	(.025)	(.026)	(.027)	(.025)	(.026)	(.020)	(.023)	(.020)	(.023)	(.020)	(.021)	(.024)	(.022)
Female × Dec 20	-.124***	-.043	-.074**	-.020	-.125***	-.032	-.137***	-.062*	.026	-.079*	-.004	.006	-.099***	.001	-.104***	-.022	.059*	-.046	.036
	(.040)	(.039)	(.036)	(.033)	(.039)	(.034)	(.039)	(.035)	(.036)	(.041)	(.038)	(.029)	(.038)	(.029)	(.038)	(.032)	(.034)	(.042)	(.037)
Female × May 21	-.224***	.013	-.068*	.008	-.184***	-.015	-.207***	-.119**	.074	-.118**	-.030	.002	-.190***	-.015	-.207***	-.091**	.073*	-.119**	-.003
	(.045)	(.044)	(.039)	(.049)	(.050)	(.047)	(.048)	(.052)	(.049)	(.050)	(.053)	(.038)	(.046)	(.037)	(.044)	(.042)	(.042)	(.049)	(.047)
Female × Jul 21 (Lckdn 2)	-.298***	-.051	-.129***	.028	-.210***	-.002	-.240***	-.141**	.095*	-.143***	-.044	-.007	-.245***	-.029	-.266***	-.142***	.068	-.170***	-.046
	(.046)	(.048)	(.043)	(.051)	(.052)	(.049)	(.050)	(.056)	(.051)	(.051)	(.057)	(.043)	(.049)	(.041)	(.047)	(.050)	(.046)	(.052)	(.054)
Female × Sep 21	-.301***	-.051	-.123***	-.006	-.211***	-.028	-.233***	-.155***	.056	-.149***	-.071	-.024	-.230***	-.040	-.245***	-.137***	.044	-.161***	-.053
	(.046)	(.046)	(.041)	(.051)	(.050)	(.049)	(.049)	(.054)	(.050)	(.049)	(.055)	(.043)	(.048)	(.042)	(.046)	(.049)	(.046)	(.050)	(.053)
Observations	6,426	6,426	6,418	4,140	4,140	4,575	4,575	4,199	4,575	4,575	4,199	4,140	4,140	4,575	4,575	4,199	4,575	4,575	4,199
R-squared	.448	.409	.420	.654	.697	.603	.660	.647	.644	.665	.673	.667	.705	.626	.681	.665	.646	.660	.667
Male Attritors	Empl	Non-Empl	F empl + .1 sd	Non-Empl	Non-Empl	Right Sector	Right Sector	Right Sector	Wrong Sector	Wrong Sector	Wrong Sector	Non-Empl	Non-Empl	Right Sector	Right Sector	Right Sector	Wrong Sector	Wrong Sector	Wrong Sector
Female Attritors	Non-Empl	Empl	M empl - .1 sd	Right Sector	Wrong Sector	Right Sector	Wrong Sector	Non-Empl	Right Sector	Wrong Sector	Non-Empl	Right Sector	Wrong Sector	Right Sector	Wrong Sector	Non-Empl	Right Sector	Wrong Sector	Non-Empl

Notes: The table reports the β_y coefficients obtained estimating Equation 3.1 when making different assumptions about the employment status and sectors of attritors, following (Horowitz and Manski, 2006) and (Kling et al., 2007). The dependent variables are an indicator for employed respondents (columns [1]-[3]), an indicator for respondents employed in their training sector conditional on employment (columns [4]-[11]), and an indicator for respondents employed in skilled sectors conditional on employment (columns [12]-[19]). See the notes to Table 3.1 for the details about how the variables were built. In columns (1) and (2), we set the outcome variables of attritors to one if we assume they are employed (*Empl*) and to zero if we assume they are non-employed (*Non-empl*). In column (3), we assume that the employment rate of male attritors is 0.1 standard deviations above female mean employment rate, and that the employment rate of female attritors is 0.1 standard deviations below male mean employment rate. In columns (4)-(11), we set the outcome variable of attritors to one if we assume they are employed in their training sector (*Right Sector*), to zero if we assume they are employed outside of their training sector (*Wrong Sector*), and to missing if we assume they are non-employed. In columns (12)-(19), we set the outcome variable of attritors to one if we assume they are employed in a skilled sector (*Right Sector*), to zero if we assume they are employed in an unskilled sector (*Wrong Sector*), and to missing if we assume they are non-employed. We do not report the gender gaps in employment in the training sector and in a skilled sectors when both female and male attritors are non-employed because this seario is equivalent to the original scenario in which attritors' outcomes are missing.

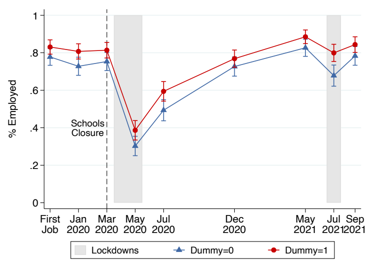
Figure C.1.6: The Evolution of Female Employment Rate for Different Cohorts of Vocational Graduates



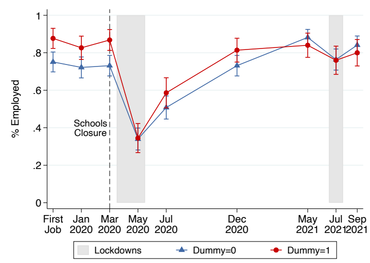
Notes: The figure illustrates average employment rates for female respondents of different cohorts (i.e., who completed vocational graduation in different years) over time. Young, middle, and old cohorts refer to female respondents who graduated in 2019+, 2017-2018, and 2016- respectively. The young cohort includes 98 respondents. The middle cohort includes 113 respondents; the old cohort includes 81 respondents. 95% robust confidence intervals are reported.

Figure C.1.7: Heterogeneities in Effect of Lockdowns on Employment by Socio-Demographics

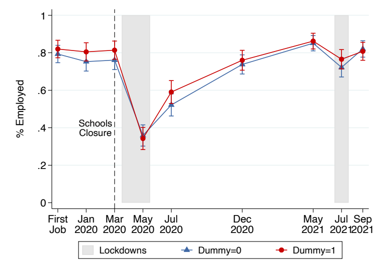
(a) Age above median



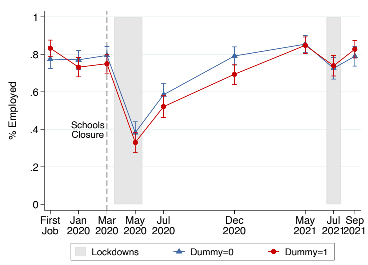
(b) Married



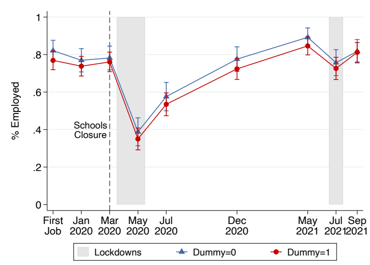
(c) Has children



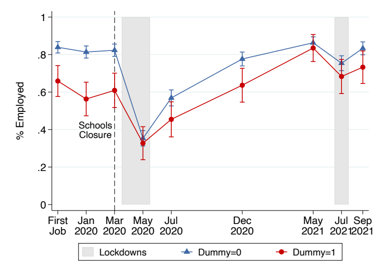
(d) Household of origin is rural



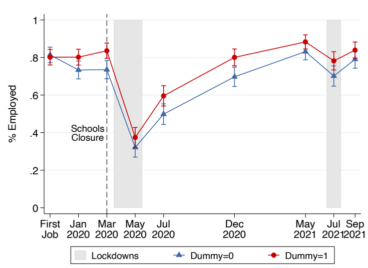
(e) Caretaker education above median



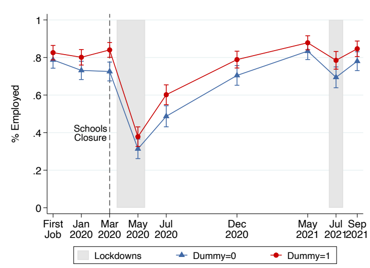
(f) Household of origin's main income source: agriculture



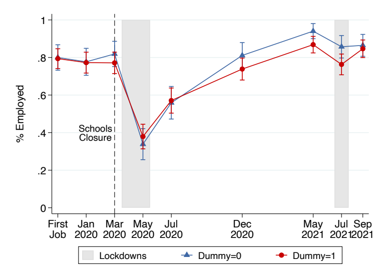
(g) Household of origin's assets index above median



(h) Own assets index above median



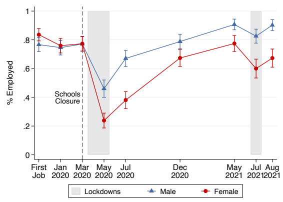
(i) Worried about COVID-19 infection above median



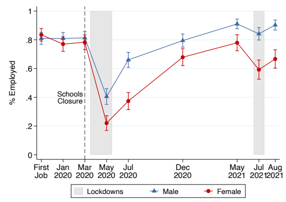
Notes: The figure illustrates average employment rates over time for respondents aged below and above the sample median; single and married; with and without children; from rural and urban households; with caretaker educated below and above the sample median; from agricultural and non-agricultural households; with own and household's asset indexes above and below the sample medians; anxious about covid above and below median. At each point in time, a respondent is coded as employed if her main activity is either wage-employment or self-employment. The first data point refers to the respondents' first job after completing vocational education. It may coincide with the job in January 2020 and its start and end date may be different for each respondent. The data point referring to the first job can be interpreted as an indicator for individuals who ever worked after completing vocational education. 95% robust confidence intervals are reported.

Figure C.1.8: Gendered Effect of Lockdowns on Employment, Leaving Out one Training Sector at a Time

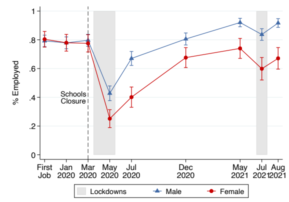
(a) Leaving out motor-mechanics



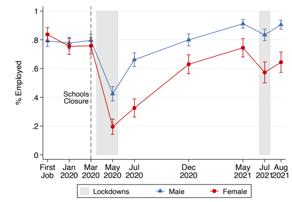
(b) Leaving out plumbing



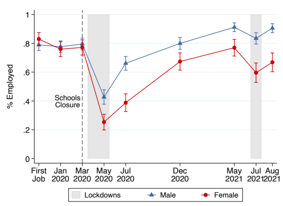
(c) Leaving out food and hospitality



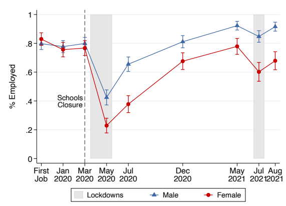
(d) Leaving out tailoring



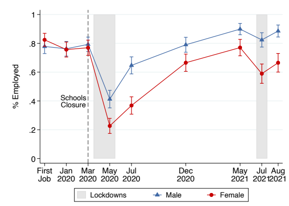
(e) Leaving out hairdressing



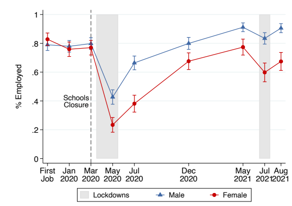
(f) Leaving out construction



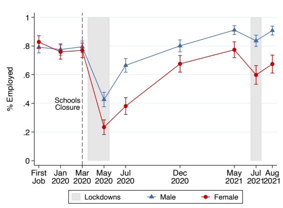
(g) Leaving out electrical work



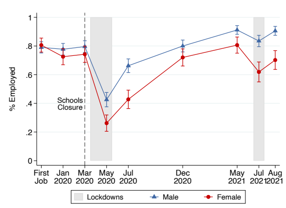
(h) Leaving out welding



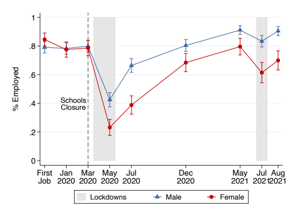
(i) Leaving out carpentry



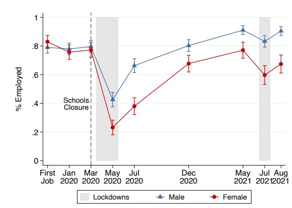
(j) Leaving out teaching



(k) Leaving out secretary and accounting



(l) Leaving out agriculture



(m) Leaving out machining and fitting

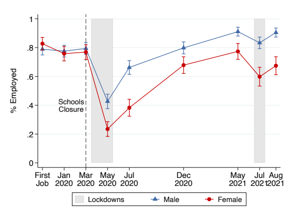
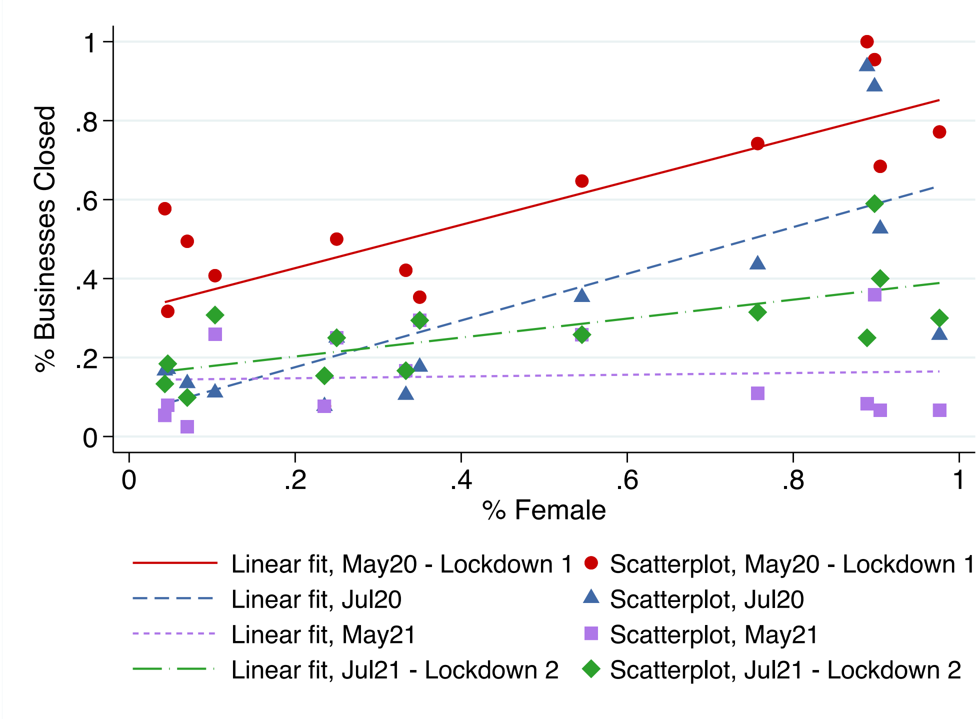
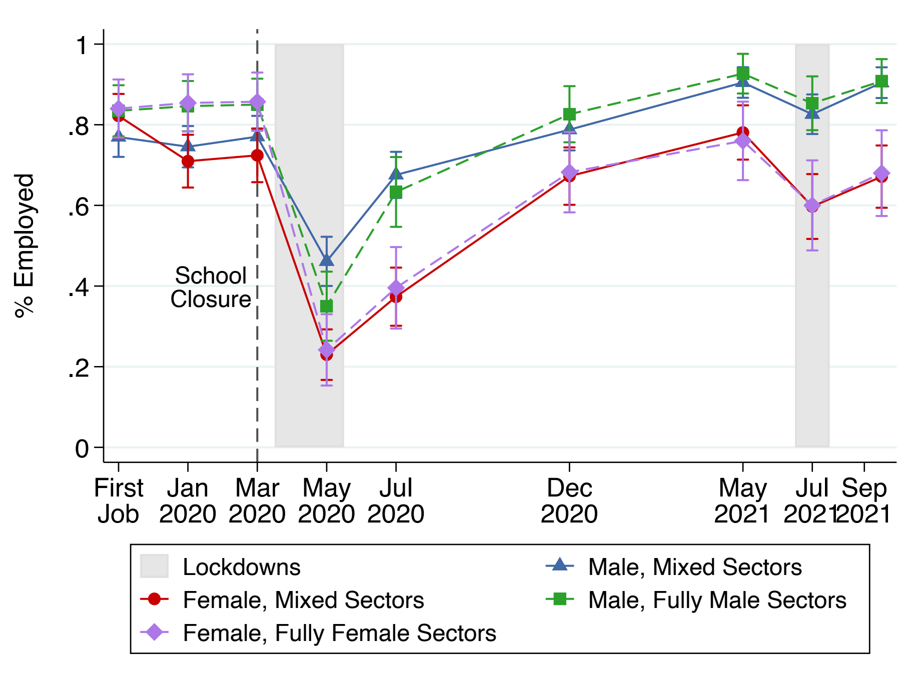


Figure C.1.9: Female Concentration in Severely Impacted Economic Sectors Over Time



Notes: The figure displays the economic sectors in which our workers were employed pre-pandemic by the share of female workers hosted before the pandemic and the share of businesses that were closed in May 2020, July 2020, May 2021, and July 2021. A linear fit was added for each period. In May 2021 and July 2021 the share of business closed is approximated by the share of non-employed respondents. This measure has been validated by comparing the share of business closed and the share of non-employed workers in previous periods, when both variables are available. The slope and standard errors (in parenthesis) of the fitted lines are: 0.55 (0.12) in May 2020; 0.59 (0.19) in July 2020; 0.02 (0.09) in May 2021; and 0.24 (0.09) in July 2021.

Figure C.1.10: The Emergence and Persistence of a Gender Gap in Employment for Respondents in Mixed and Single-Gender Sectors



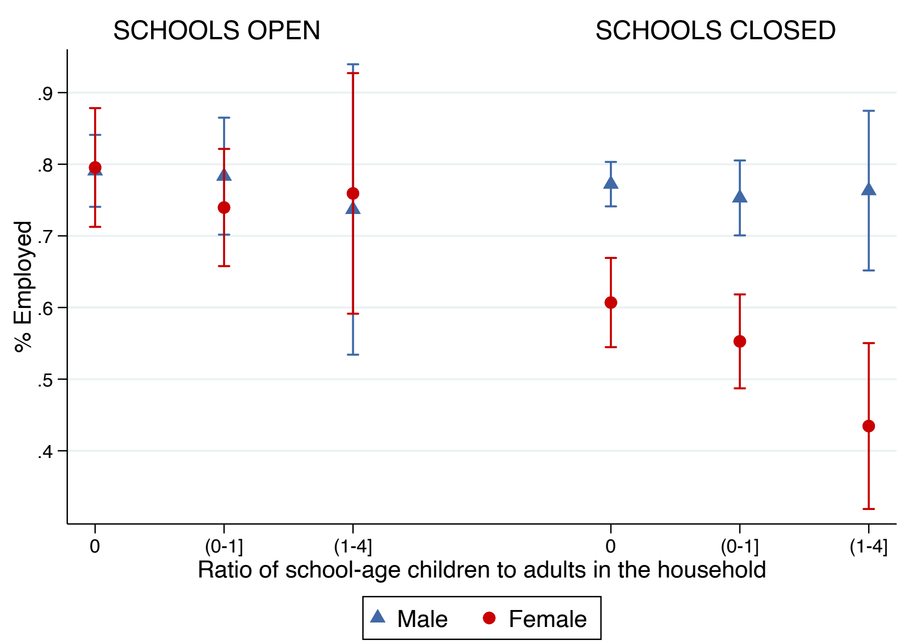
Notes: The figure illustrates average employment rates separately for male and female respondents who received training in mixed- or single-gender sectors over time. Single-gender sectors are sectors in which more than 95% of the trainees have the same gender, as measured in our sample. Using this definition, motor-mechanics, welding and carpentry are fully-male sectors; tailoring ad teaching are fully-female sectors. Mixed sectors include: plumbing, food service and hospitality, hairdressing, constfruction, electrical work, secretary and accounting, agriculture, and machining and fitting. There are 194 women and 285 men in mixed-gender sectors and 101 women and 134 men in single-gender sectors. At each point in time, a respondent is coded as employed if her main activity is either wage- or self-employment. The first data point refers to the respondents' first job after completing vocational education. It may coincide with the job in January 2020 and its start and end date may be different for each respondent. 95% robust confidence intervals are reported.

Table C.1.5: Gender Gap in Impact of School Closure on Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Male Sample						**Female Sample**					
Outcome: % Employed	Schools: Open	Schools: Closed	Schools: Open	Schools: Closed	Schools: Open	Schools: Closed	Schools: Open	Schools: Closed	Schools: Open	Schools: Closed	Schools: Open	Schools: Closed
Num children in hh = 1	0.113**	0.065**					-0.191**	-0.113*				
	(0.054)	(0.031)					(0.080)	(0.059)				
Num children in hh = 2+	0.026	-0.010					-0.109*	-0.141***				
	(0.049)	(0.032)					(0.056)	(0.047)				
Num children above 3yo in hh = 1			-0.001	-0.014					-0.056	-0.054		
			(0.061)	(0.034)					(0.071)	(0.056)		
Num children above 3yo in hh = 2+			-0.028	-0.021					-0.049	-0.096**		
			(0.058)	(0.038)					(0.063)	(0.048)		
Num children above 6yo in hh = 1					-0.046	0.003					0.007	-0.039
					(0.067)	(0.040)					(0.067)	(0.056)
Num children above 6yo in hh = 2+					-0.108	-0.091*					-0.045	-0.107**
					(0.076)	(0.050)					(0.072)	(0.052)
Observations	337	339	336	338	336	338	214	218	211	215	211	215
R-squared	0.010	0.011	0.001	0.001	0.008	0.014	0.030	0.039	0.004	0.019	0.002	0.019

Notes: The table illustrates the estimated coefficients of a regression of employmet on a constant and indicators for zero (omitted category), one, and two or more children in the household. The equation is estimated male and female respondents separately. Standard errors are robust to heteroskedasticity. * p<0.1, ** p<0.05, *** p<0.01.

Figure C.1.11: Gender Gap in Impact of School Closure on Employment and Household Childcare Support

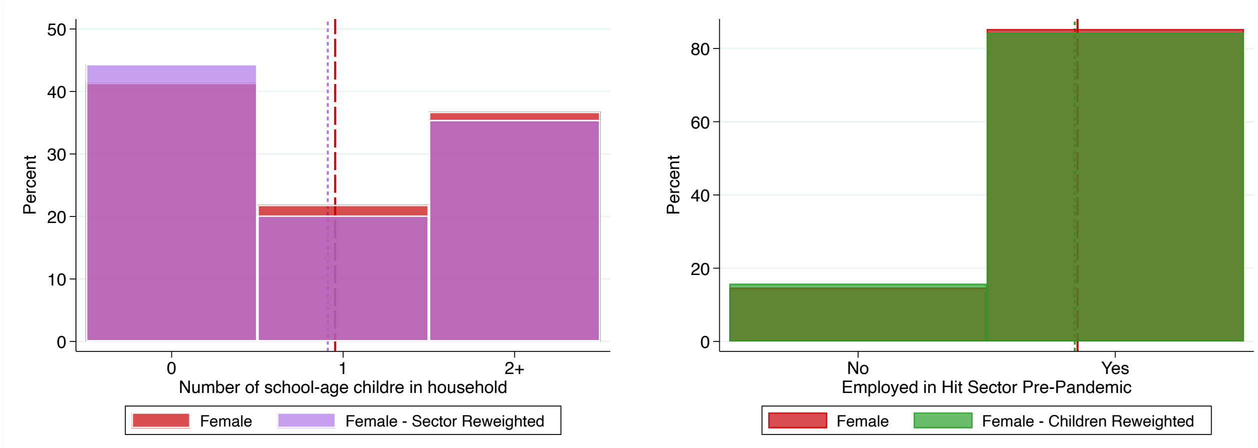


Notes: The figure displays the average employment rate for female and male respondents with different ratios of school-age children to adults in the households in periods in which schools were open (January and March 2020) and periods in which schools were closed (May, July and December 2020, May, July and September 2021). The higher the ratio, the heavier are childcare responsibilities. Respondents with a ratio equal to zero have no school-age children in the household. Respondents with a ratio between zero and one have more adults than school-age children in the household. Respondent with a ratio greater than one have multiple school-age children per adult in the household. There are 89 female and 229 male respondents with a ratio equal to zero; 98 female and 90 male respodnents with a ratio between zero and one; 28 female and 19 male respondents with a ratio greater than one. School-age children are children aged 3 or more. 95% robust confidence intervals are reported.

Figure C.1.12: Orthogonality of Sectors and Childcare Responsibilities for Women

(a) Distribution of Children in Real and Reweighted Samples

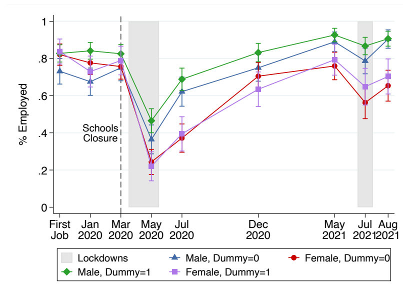
(b) Distribution of Hit Sector in Real and Reweighted Samples



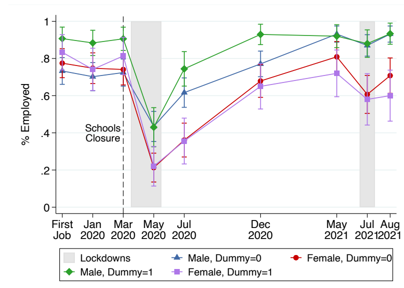
Panel (a) illustrates the distribution of the number of school-age children in the household in the original female sample and in the female sample when reweighted so that that the first moment of *Hit Sector*$_i$, an indicator for whether pre-pandemic the respondent was employed in a severely hit sector, matches that in the male sample. Weights are equal to one for male workers. School-age children are children aged three or more. Severely hit sectors are sectors in which more than 50% of the businesses in which our workers were employed pre-pandemic were closed during the first lockdown in May 2020: motor-mechanics, food and hotel, tailoring, hairdressing, teaching, secretary, and retail. The dashed and the dotted lines indicate the average number of school-age children in the original female sample and in the reweighted female sample respectively. Panel (b) illustrates the distribution of *Hit Sector*$_i$ in the original female sample and in the female sample when reweighted so that the proportions of respondent with zero, one, and two or more school-age children in the household in the female sample match those in the male sample. The dashed and the dotted lines indicate the average of *Hit Sector*$_i$ in the original female sample and in the reweighted female sample respectively.

Figure C.1.13: Heterogeneities in Gendered Effect of Lockdowns on Employment by Socio-Economic Characteristics

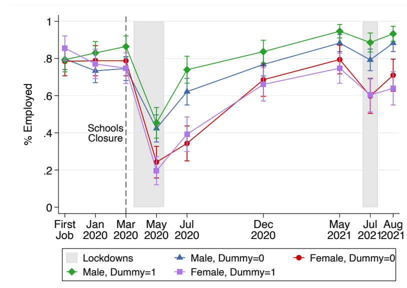
(a) Age above median



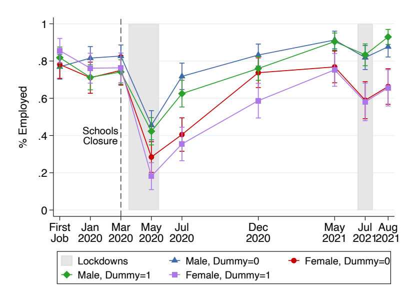
(b) Married



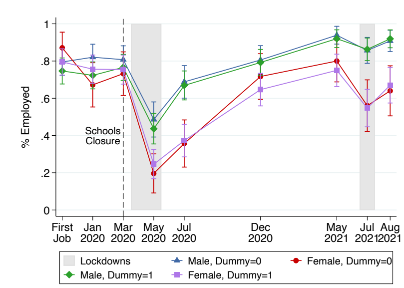
(c) Has children



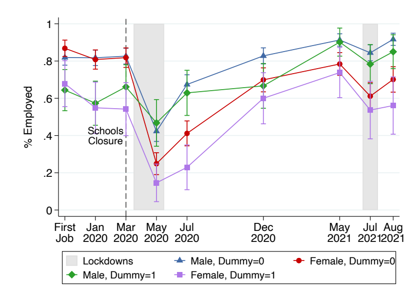
(d) Household of origin is rural



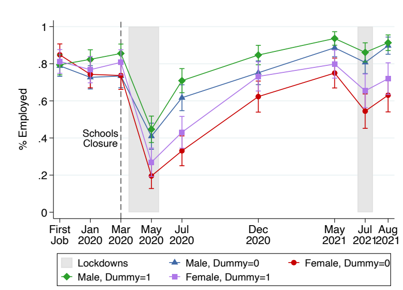
(e) Caretaker education above median



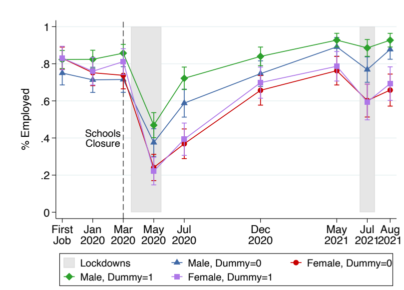
(f) Household of origin's main income source: agriculture



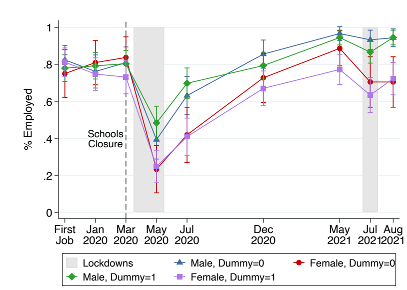
(g) Household of origin assets index above median



(h) Own assets index above median



(i) Worried about COVID-19 infection above median



Notes: The figure illustrates average employment rates over time for respondents with different gender and: aged below and above the sample median; single and married; with and without children; from rural and urban households; with caretaker educated below and above the sample median; from agricultural and non-agricultural households; with own and household's asset indexes above and below the sample medians; anxious about covid above and below median. At each point in time, a respondent is coded as employed if her main activity is either wage- or self-employment. 95% robust confidence intervals are reported.