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

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The most updated draft is available here.

Abstract

Can personalized mentorship by experienced workers improve young job seekers' labor market trajectories? To answer this question, we 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%.

JEL codes: D83, D84, J24, J64, O10

Keywords: Youth Unemployment, Job Search, Mentorship, Overoptimism, Experiment

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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, 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., 2022a; 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 $\sim$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

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

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 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

[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. (2022a); 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).

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 assignmento 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 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.

2 Context

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.

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.13, 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.13. 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 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.14 reports mentors' demographics and job history. They are 25 years old on average, and 41% are female. ne 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 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

[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.

[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.

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]

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 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 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 $\sim$65%, suggesting that

[7] One example of the digitized material is shown in Figure A.10.

[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 D.

[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.

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 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 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 proceeded to search less intensively over lower quality firms with persistent negative effects on employment outcomes

[11]Similar patterns occur if we compare students' expectations to mentors' realizations (Figure A.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.

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.

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.

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 E 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. 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 B we describe correlates of student attrition, confirm thet attrition is uncorrelated to treatment, and show that there is no

[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.

evidence of differential attrition across treatment and control based on observable characteristics (Table A.10). 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.9) 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.6). 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

[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.

and a short check-in survey, we provided mentors with ∼\$40 as well as reimbursements of airtime incurred to make the phone calls. 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 G 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.

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.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

[15] In Table A.12 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

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 their content when necessary.[18] Figure A.13 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 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.14, 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 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:

$$SL_{ij} = \beta_0 + \beta_1 |z_i - z_j| + \beta_2 (z_i + z_j) + \gamma |w_{ij}| + u_j \tag{1}$$

mentor seems prone to provide help".

[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.

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 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 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]

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

[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.20 reports the results. They are consistent with what we learn from the dyadic regression.

to potential employers (R); encouragement and confidence over a positive future outlook (E).[23] Panel A of Figure 4 presents the raw conversation content 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 H 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 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

[23]Figure A.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 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).

conversation content is useful to diagnose what was discussed, Panel B of Figure 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 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.

4 Results

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 J.[25] Our ITT estimates are based on the following ANCOVA specification for student i in strata s at endline $t = 1, 2$:

$$Y_{i,s,t} = \beta_0 + \beta_1 T_i + X_i' \delta + \lambda_s + \epsilon_{i,s,t} \tag{2}$$

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 E 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.21, A.22 and A.23). When we define take-up as having completed all three mentorship sessions, the results are stronger in magnitude (A.27, A.28 and A.29). 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.

[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)

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] 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 I 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.

4.2 Initial Labor Market Outcomes

Table 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,

[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 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).

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.

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 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 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).

[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.

5 Mechanisms

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 4: job referrals, search tips, information about entry level jobs, and encouragement. From the framework, we derive testable predictions. The proofs of the propositions listed below are in Online Appendix F.

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 that 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 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

[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

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 jobseekers 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).

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:

$$W(w, \hat{\omega}) = \frac{w + \beta\hat{\omega}}{1 - \beta} \tag{3}$$

The value of unemployment instead can be written as:

$$\begin{aligned} U(\hat{\mu}, \hat{\omega}) = \int_c \max_{s \in [0,1]} \bigg(-cs + b + \beta s \lambda \int max\{W(w, \hat{\omega}), U(\hat{\mu}, \hat{\omega})\} dF(w; \hat{\mu}, \sigma, \hat{\omega}) \\ + \beta(1 - \lambda s) U(\hat{\mu}, \hat{\omega}) \bigg) dH(c) \end{aligned} \tag{4}$$

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:

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

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.

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:

$$W(w_R(\hat{\mu}, \hat{\omega}), \sigma, \hat{\mu}, \hat{\omega}) - U(\hat{\mu}, \hat{\omega}) = 0 \tag{5}$$

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 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

[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.

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 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 believe 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.

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 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.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.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]

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 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

[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 effect on any index dimension or the overall index.

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.30, A.31 and A.32 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 of the experiment: first, the random assignment to each mentor. Second the randomization to T2, the additional cash transfer.

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:

$$Y_{i,j,d} = \sum_j M_{ij}\gamma_j + \lambda_d + \mu_i \tag{6}$$

where Y_i is the outcome of interest for student i as described in equation 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 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 6 reports the distribution of the fixed effects as well

[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

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 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:

$$Y_i = \beta_0 + \beta_1 Info_i + \beta_2 Enc_i + \beta_3 Search_i + X_i'\delta + \epsilon_i \tag{7}$$

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 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 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:

$$Info_{i,j,d} = \sum_j M_{ij}\gamma_{j1} + \lambda_{d1} + \mu_i \tag{8}$$

$$Search_{i,j,d} = \sum_j M_{ij}\gamma_{j2} + \lambda_{d2} + u_i \tag{9}$$

estimation of unbiased variance components under unrestricted heteroskedasticity.

[35] In Table A.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

$$Enc_{i,j,d} = \sum_j M_{ij}\gamma_{j3} + \lambda_{d3} + \tau_i \quad (10)$$

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:

$$Y_{i,d} = \beta_0 + \beta_1 \widehat{Info}_i + \beta_2 \widehat{Enc}_i + \beta_3 \widehat{Search}_i + \lambda_d + \epsilon_i \quad (11)$$

where Y_i are the same outcomes of interest in equation 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.

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 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.

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

Results Table 6 and the corresponding Figure 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.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.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 a more experienced mentor (one who has already led multiple mentorship sessions) differs from exposure to a first-time mentor.

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.18).[38] Instead, it attenuated the effects at 1 year (Table 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 8). These findings point towards a cash transfer stimulating discussion on more actionable

[37] Tables A.7 and A.8 report the results on the single components of each of the four indexes.

[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

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]

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. 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 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

[39] Figure A.12 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.

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 have demanded more time: the amount of engagement required for the program is thefore 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.

7 Conclusions

Today, Africa is home to one out of every five first-time job seekers (UN, 2019; Bandiera et al., 2022b). By 2050, it will be one out of three. The success of this job market shift will have a substantial impact on the rate of development across the entire continent. Currently, with estimates of unemployment and underemployment as high as 60 percent across the continent, less than half of them are projected to find a permanent work and launch a career (AfDB, 2018).

In the context of urban labor markets in Uganda, the second-youngest country in the world, we implement a novel, tractable, and generalizable mentorship intervention, Meet Your Future, and assess its ability to boost early career trajectories. 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: 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 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 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 the duration in days of the first work spell after graduation. In Columns 5 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.

Table 3: 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	.041**	.076**	-.025	6.149*
	(.019)	(.033)	(.022)	(3.601)
Control Mean	.18	.37	.26	34.84
Treatment Effect (%)	22.87	20.70	-9.53	17.65
N	934	934	923	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 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 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 Columns 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 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 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 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 2 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 3 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 4 the dependent variable is based on a question about the lowest wage the respondent would be willing to accept, In Column 5 the dependent variable measures the willingness to accept an unpaid job as reported by the respondents. In Column 6 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 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 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 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 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 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: Project Timeline

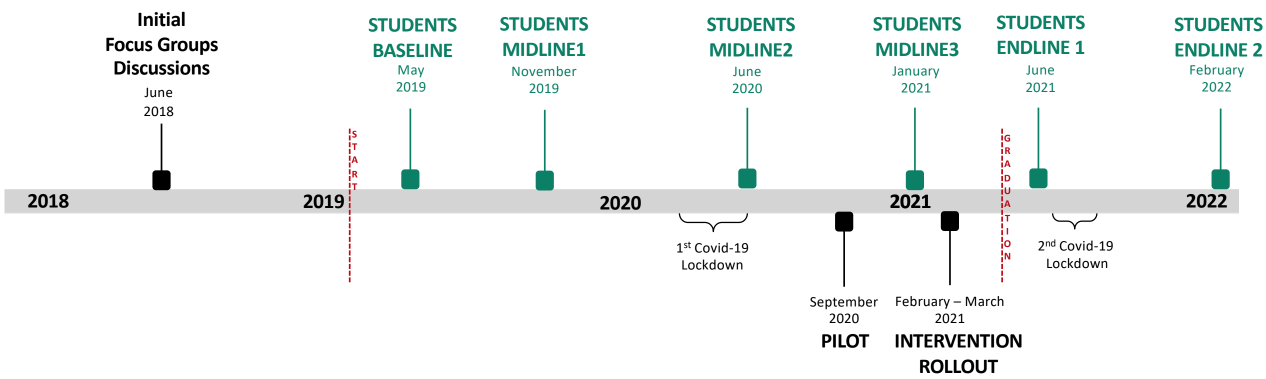


Figure 2: Experimental Design

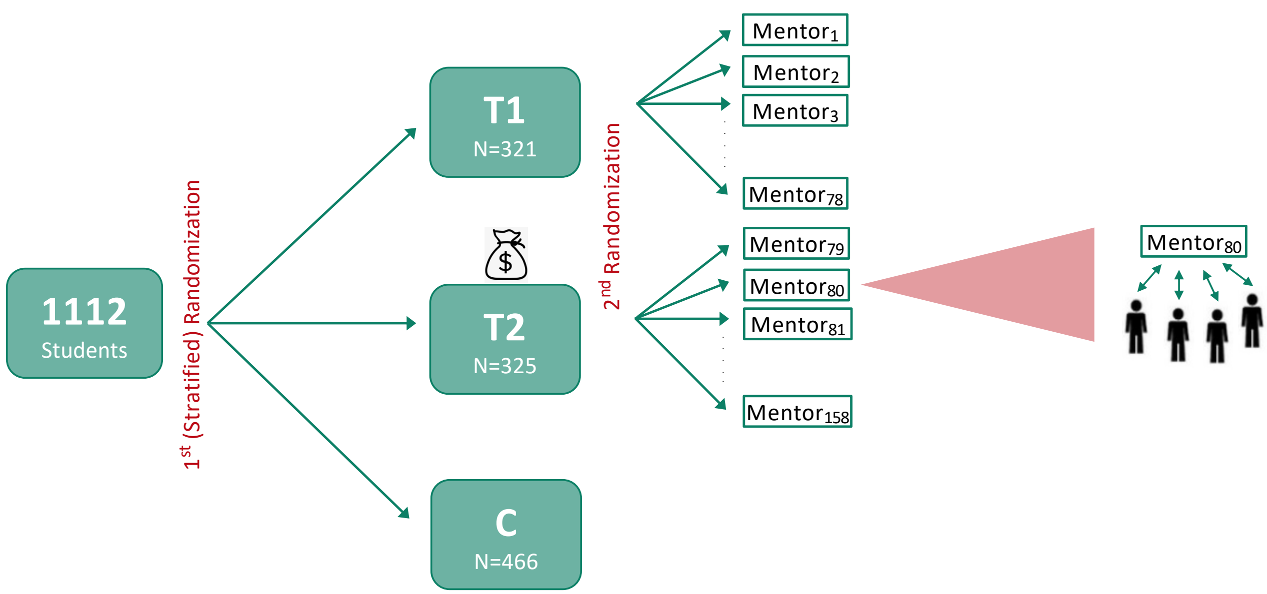
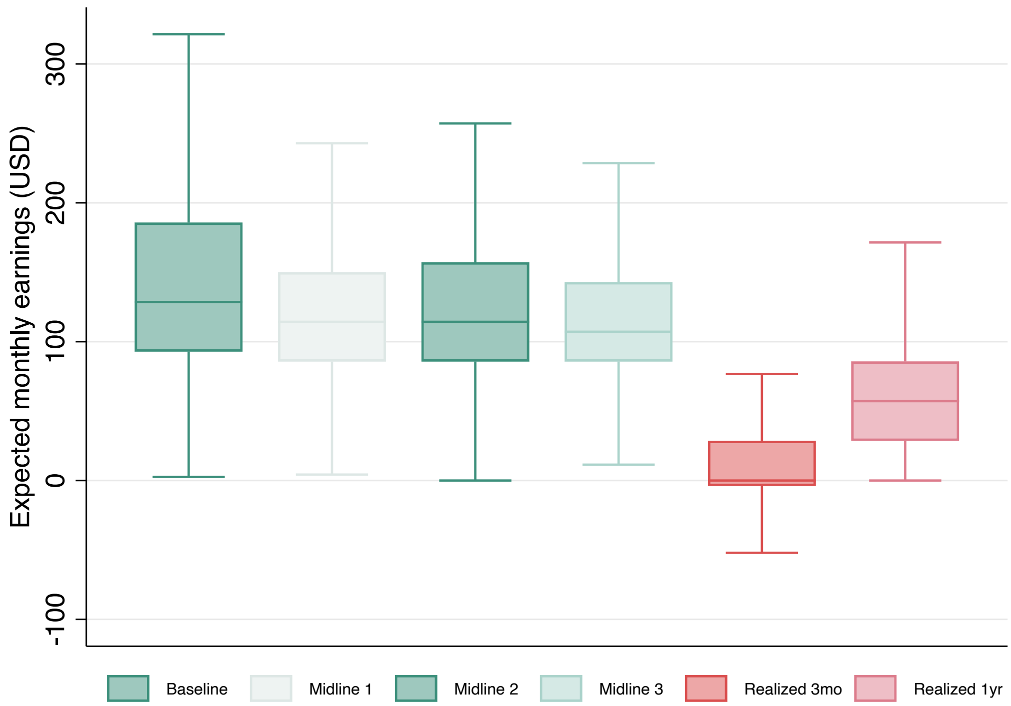


Figure 3: Overoptimism

Panel A: Expected and Actual Monthly Earnings | Employment



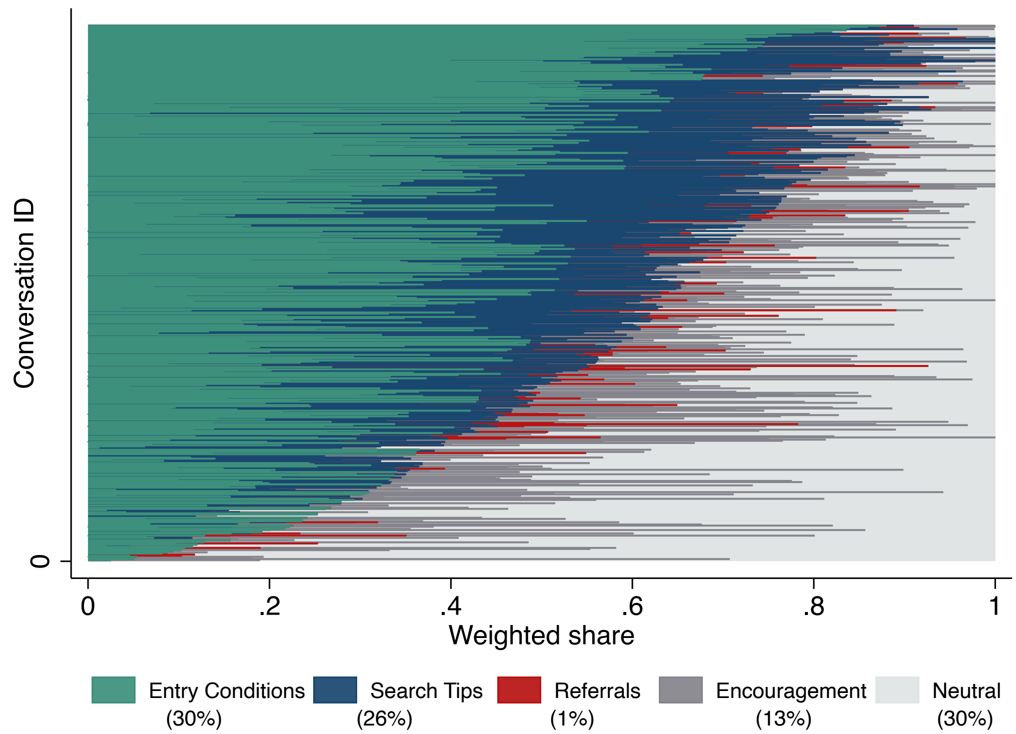
Panel B: Expected and Actual Job Ladders

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%

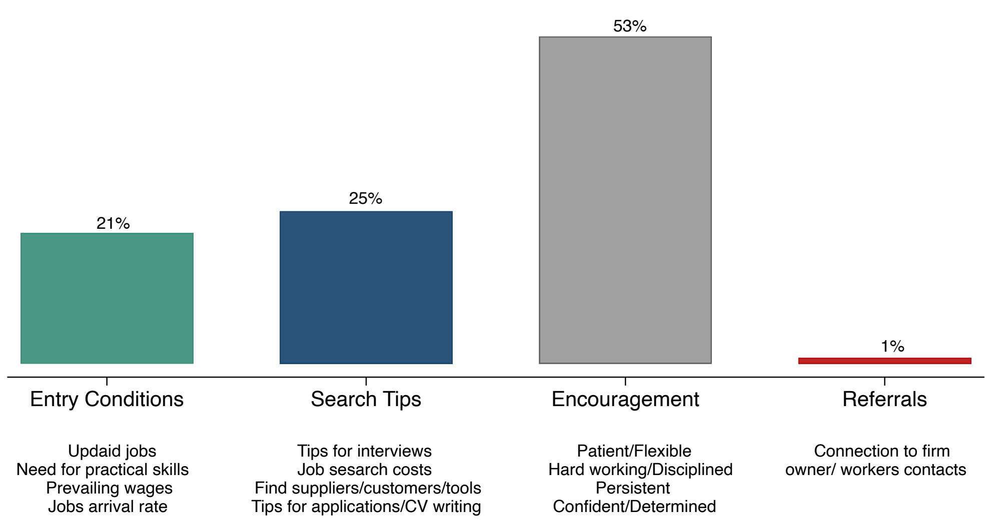
Notes: Panel A shows expected and realized conditional monthly earnings in the control group. In the first four box-and-whisker plots, we plot students' expected monthly earnings at their first job in all four pre-intervention data points. The fifth and sixth plots represent students' actual monthly 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 4: Conversations Content and Takeaways

Panel A: Coded Conversation Content From Audio Recordings



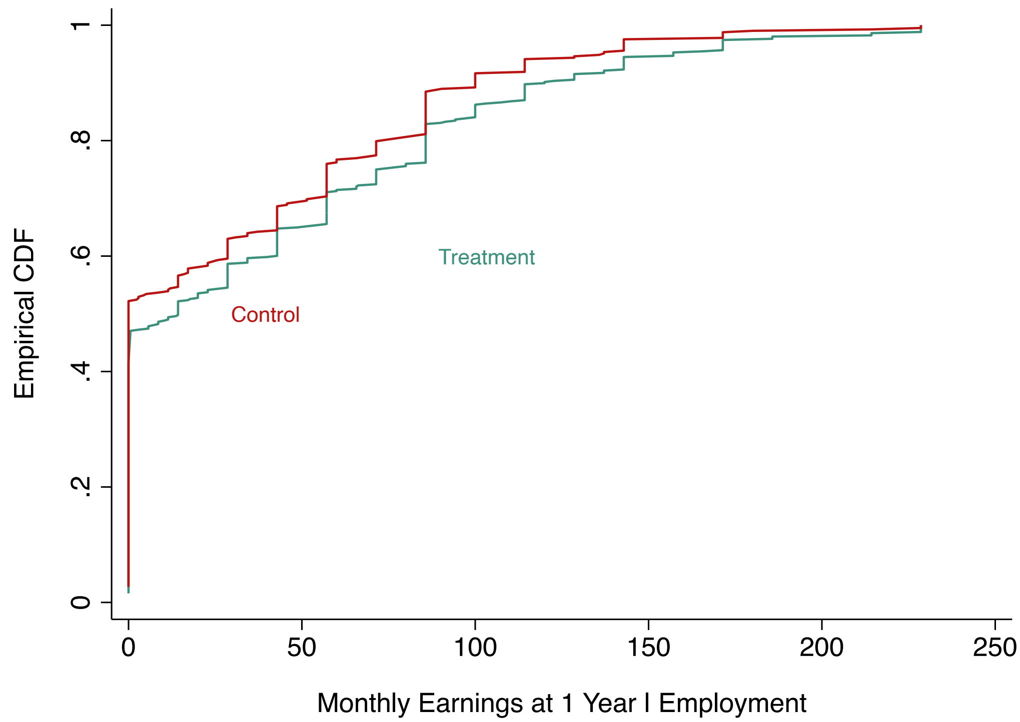
Panel B: Students Main Takeaway



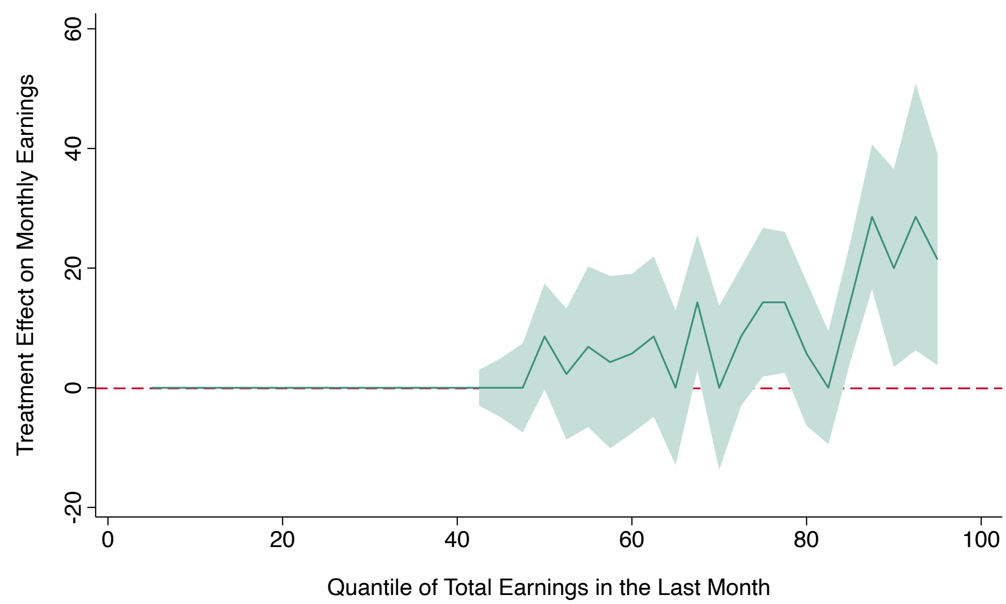
Notes: This figure shows the distribution of the main takeaways students withheld following their conversations with the mentor. We identified three macro-categories of support that mentors can provide to the students: information on entry conditions; search capital; and encouragement. Each bar represents 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 5: Quantile Treatment Effects on Monthly Earnings at 1 Year

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

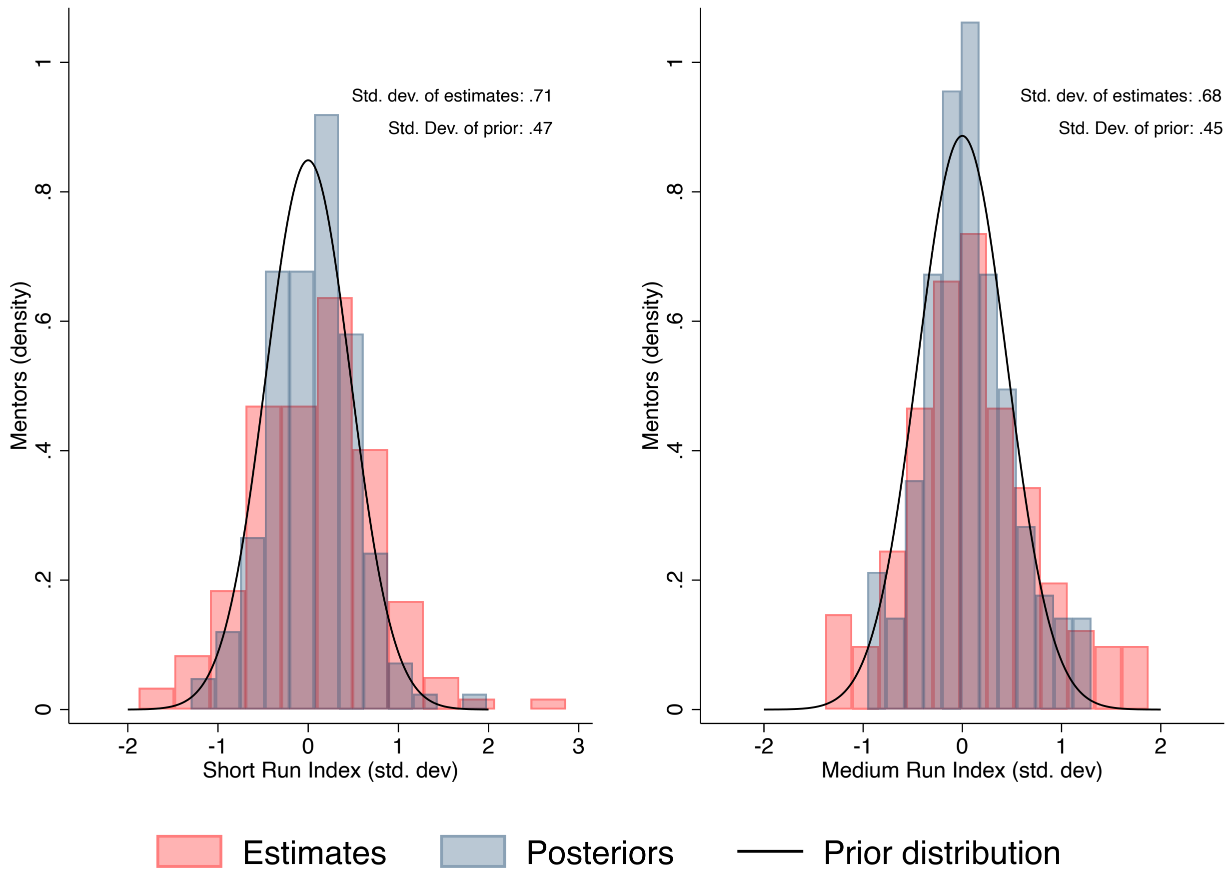


Panel B: Quantile Treatment Effects of MYF on Monthly Earnings



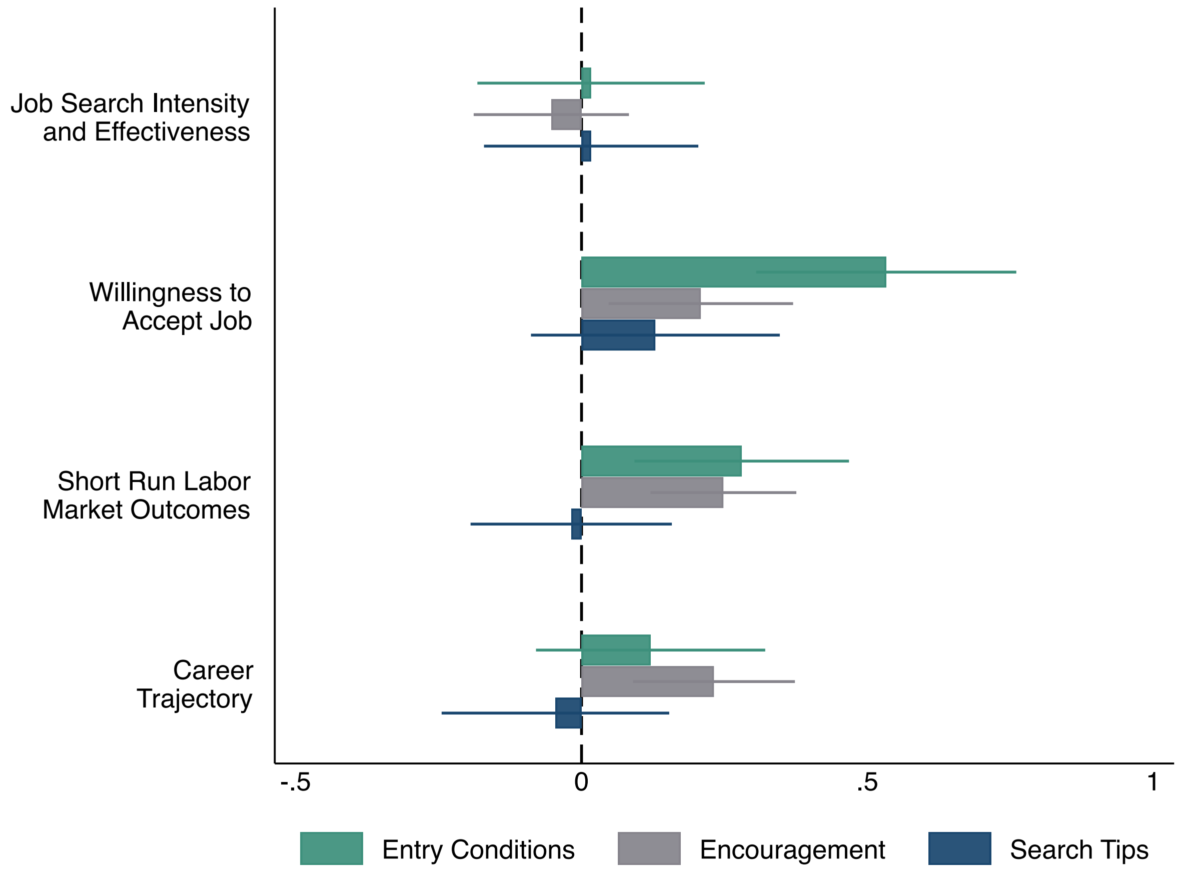
Notes: Panel A shows the empirical distributions of monthly earnings in the MYF treatment and control groups. Earnings are converted into February 2022 USD. Earnings are coded as zero for candidates who were not engaged in any work activity in the month prior to the survey. Earnings below the 42nd percentile 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 6: Reduced Form Estimates: Biased and Unbiased Mentors Fixed Effects



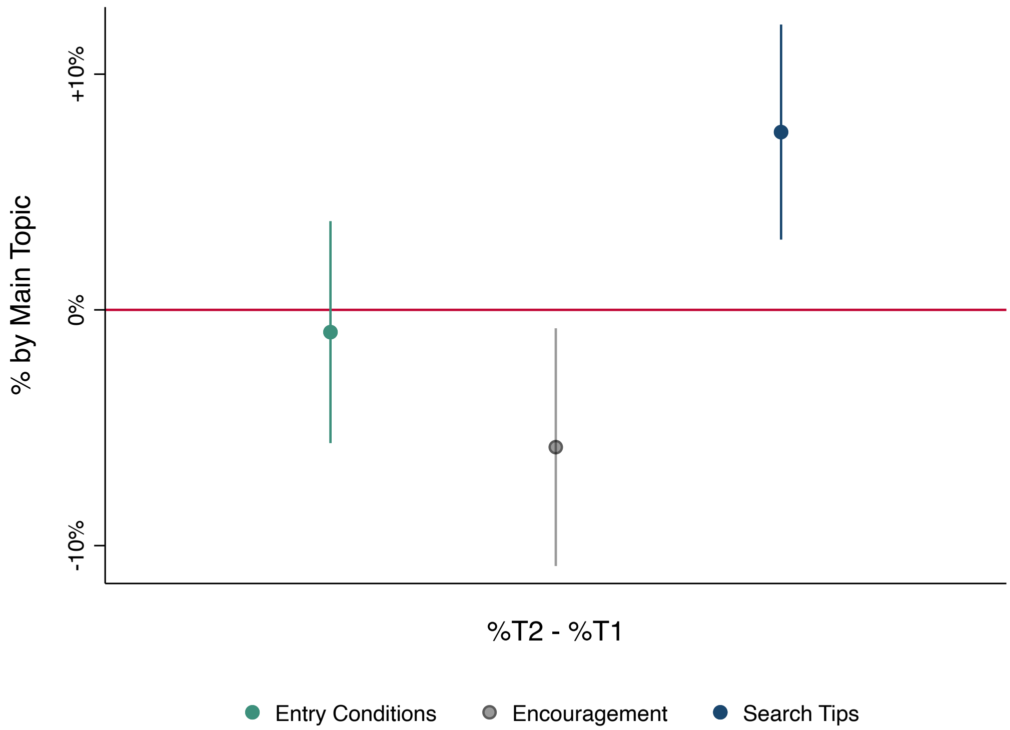
Notes: In this figure we report the biased (estimates) and unbiased (shrunked posteriors) distributions of the mentors fixed effects. We overlay the prior distribution, a normal centered on zero, with the bias-corrected standard deviation.

Figure 7: 2SLS: Type of Support Provided and Labor Market Outcomes



Notes: In this figure we report 2SLS regression estimates from equation 11. The 158 mentor dummies are used as instruments.

Figure 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.

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A.1 Appendix Tables

Table A.1: Strength of the Mentor-Mentee Connection

	Ever Connected	Connected More Than Once	Strong Link
	(1)	(2)	(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.

Table A.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.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.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 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.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 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.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.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.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.1: High Take-Up and Successful Creation of New Ties

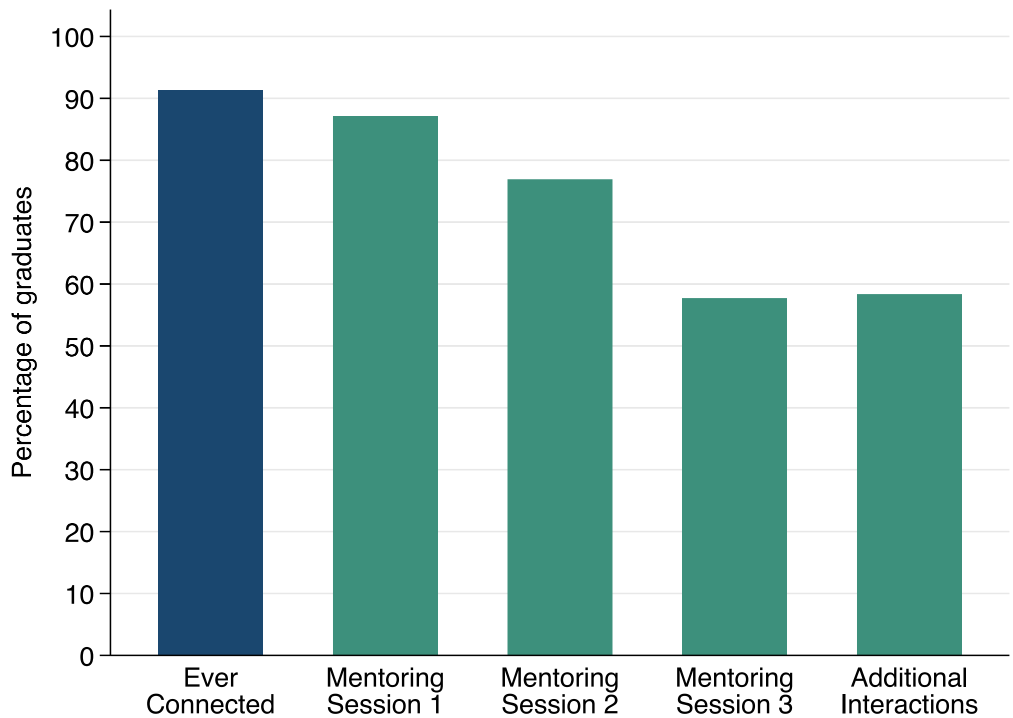


Figure A.2: Overoptimism Using (Pre-Covid-19) Mentors Data

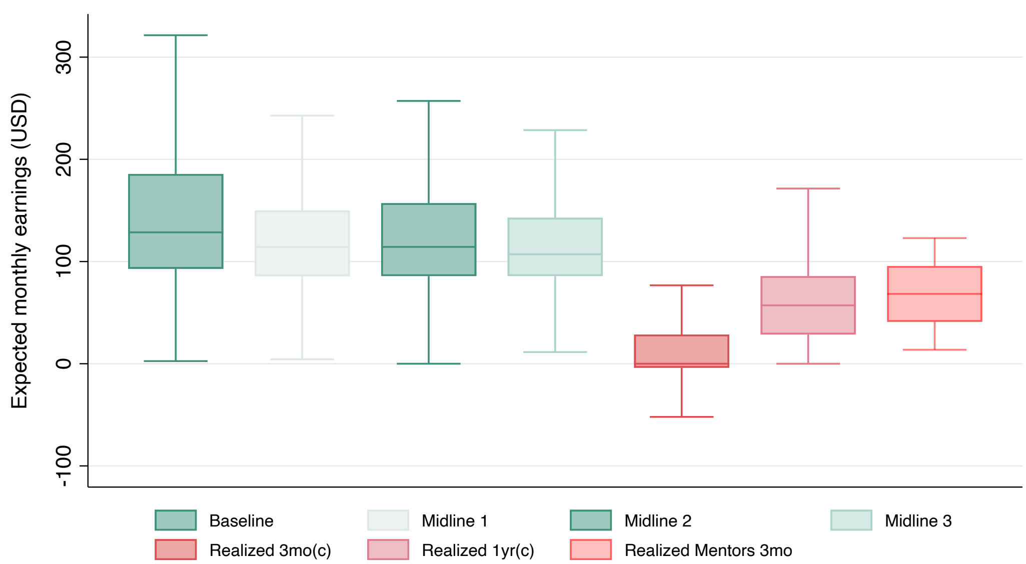


Figure A.3: Understanding the Treatment: Observers Data

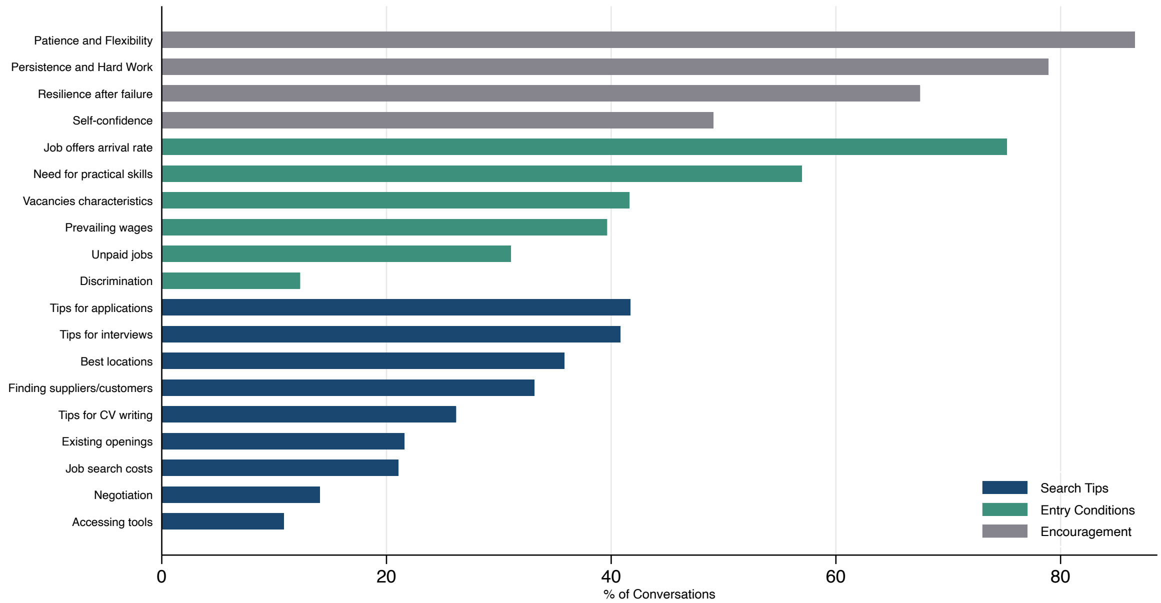
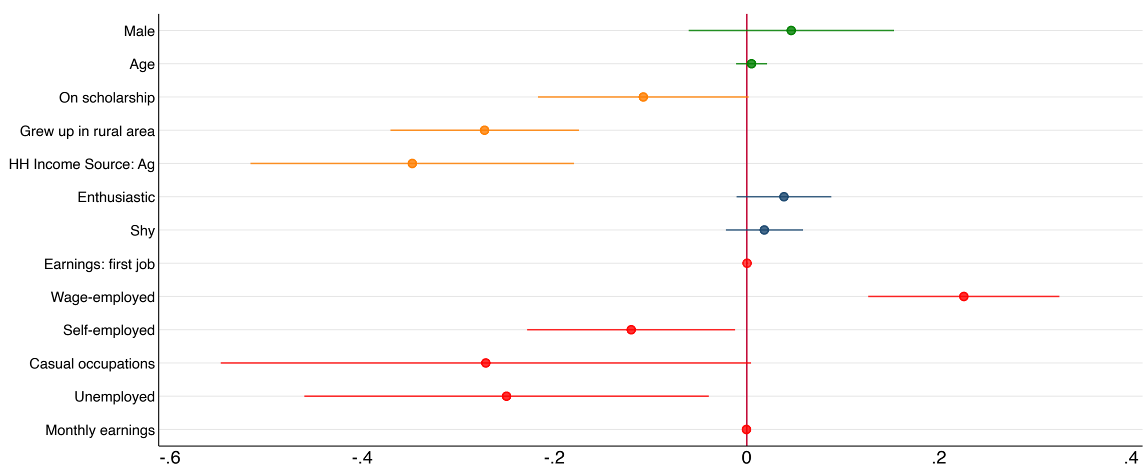
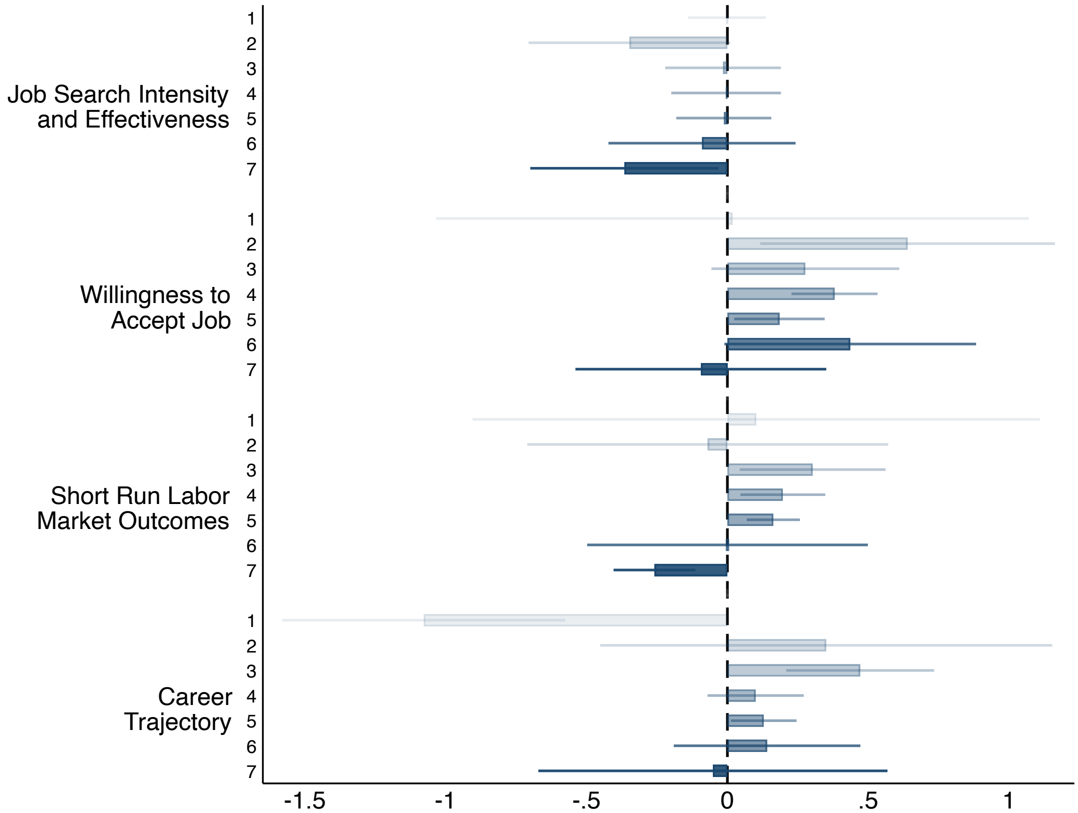


Figure A.4: Treatment Effects on Career Trajectory Index by Mentors Demographics



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

Figure A.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.

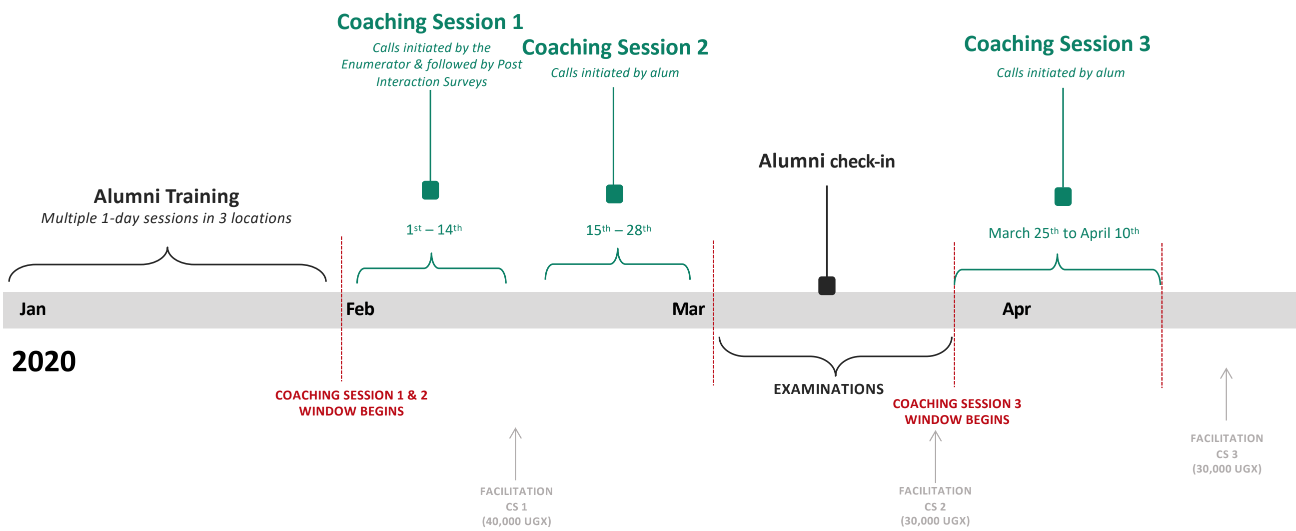
Meet Your Future: Experimental Evidence on the Labor Market Effects of Mentors

Material for Online Appendix

Appendices

Appendix A Program Details and Survey Rounds

Figure A.6: 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.6. They were also given logbooks and instructed on how to fill them (Figure A.7). 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.

Table A.9: 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

Figure A.7: 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	15th/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	—	—	—

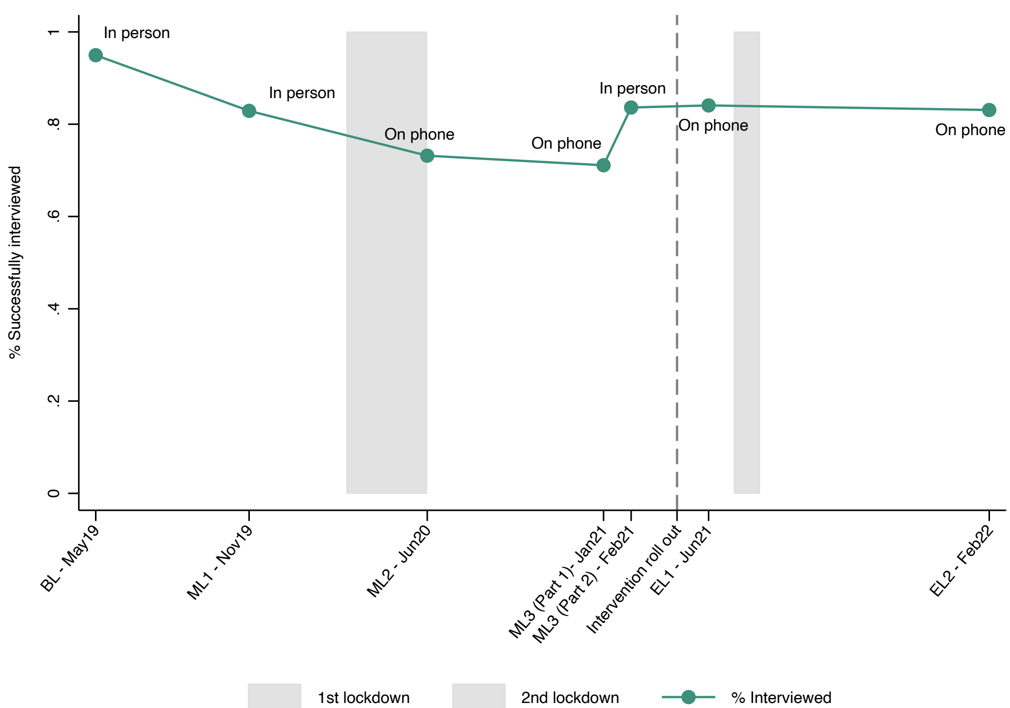
Please, use this logbook to keep track of the day and time of each KEY CALL. For KEY CALL 2 and 3 keep track of the duration of the conversation and of the 3 main topics you have discussed with the student. Remember the enumerator will ask you to tell him/her about the information in this logbook. Please,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

Appendix B Attrition and Compliance

Figure **??** 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.8: Attrition



The overall attrition rate after the intervention was stable at approximatey 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.10: 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.10 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.11 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.[40] In light of the evidence presened, we treat post-treatment nonresponse as random and therefore do not adjust our estimates.

Table A.11: Attrition Analysis: Baseline Characteristics for Students Ever Found after Intervention

		Baseline sample (N=1112)					***Estimation sample (N=1013)***
	All	***Control***		***Treated***			
	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

Last, Table A.12 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

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

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.12: Attrition Analysis: Baseline Characteristics between Compliers and Non-Compliers

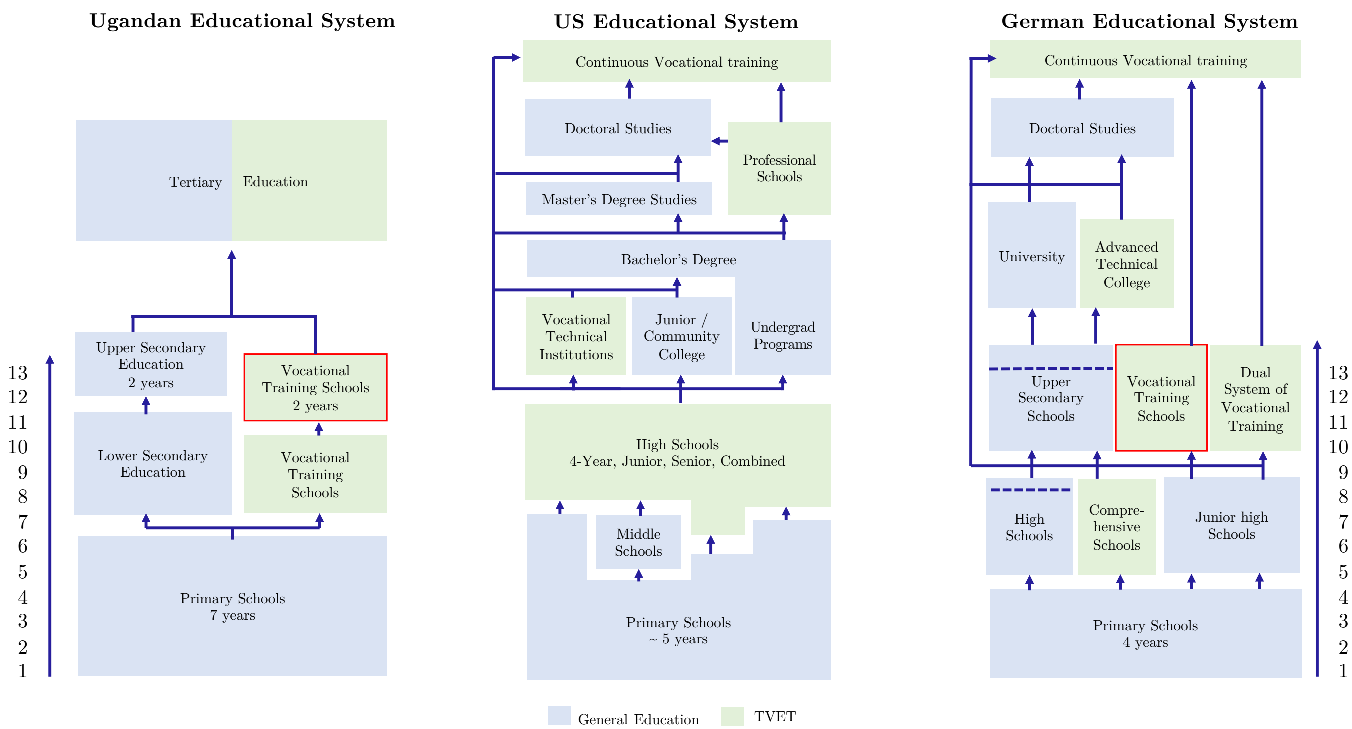
	Non Compliers		***Compliers***		
	Obs	Mean	Obs	Mean	P-value
Panel A: Socio-economic characteristics					
Age	57	19.51	589	19.86	0.21
Gender (1=M)	57	0.46	589	0.61	0.03
Christian	57	0.91	589	0.83	0.12
Single	56	0.80	586	0.90	0.04
Amenities in the HH: mobile phone with internet	57	0.53	589	0.45	0.29
Student 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

Appendix C External Validity

Table A.13: 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	.	.	.	.	.

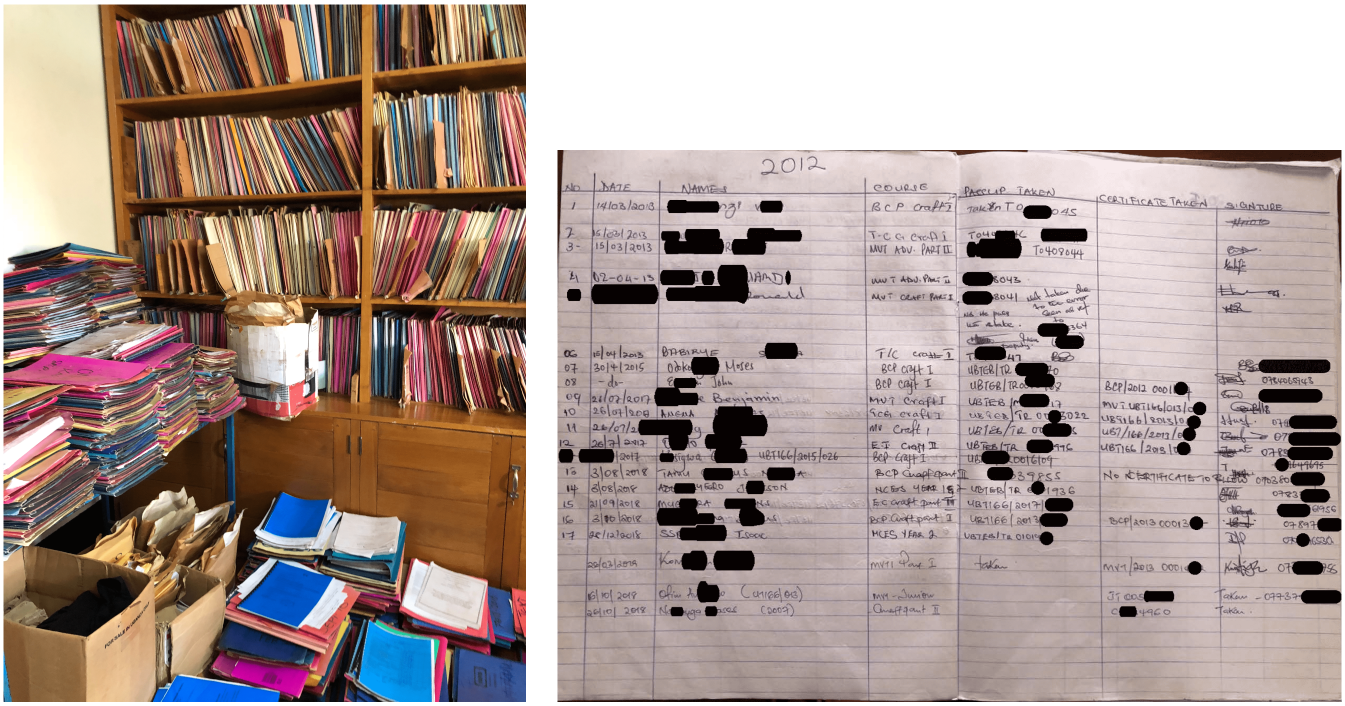
Figure A.9: US, Ugandan and German Educational Systems



Appendix D 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.10: 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.14: 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

Appendix E 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.[41] 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 :

[41] 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 lock-down). 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.

Appendix F Model Derivations

In this section we provide proofs of three statements, which lie behind Propositions 1, 2 and 3 outlined in section 5.1 as well as we derive the expression for c^*.

Equilibrium condition for c^*

If $s = 0 \rightarrow U(\hat{\mu}) = b + \beta U(\hat{\hat{\mu}}, \hat{\omega})$

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

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:[42]

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

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:

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

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 $\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} > 0$.

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

Appendix G An Uneffective Cash Transfer

Table A.15: 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.11: T2 Spending Categories

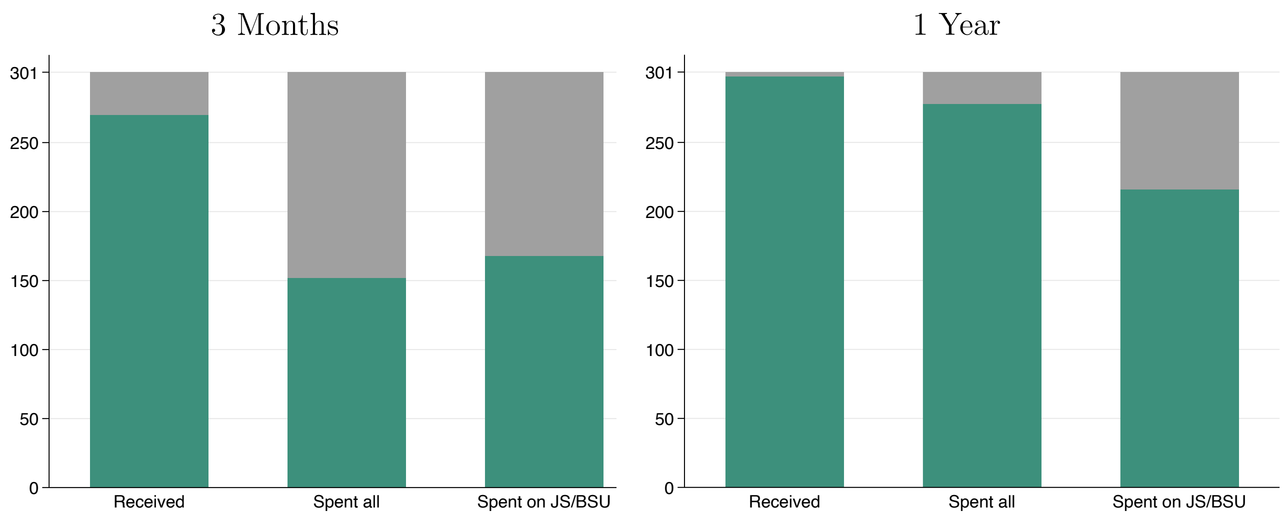


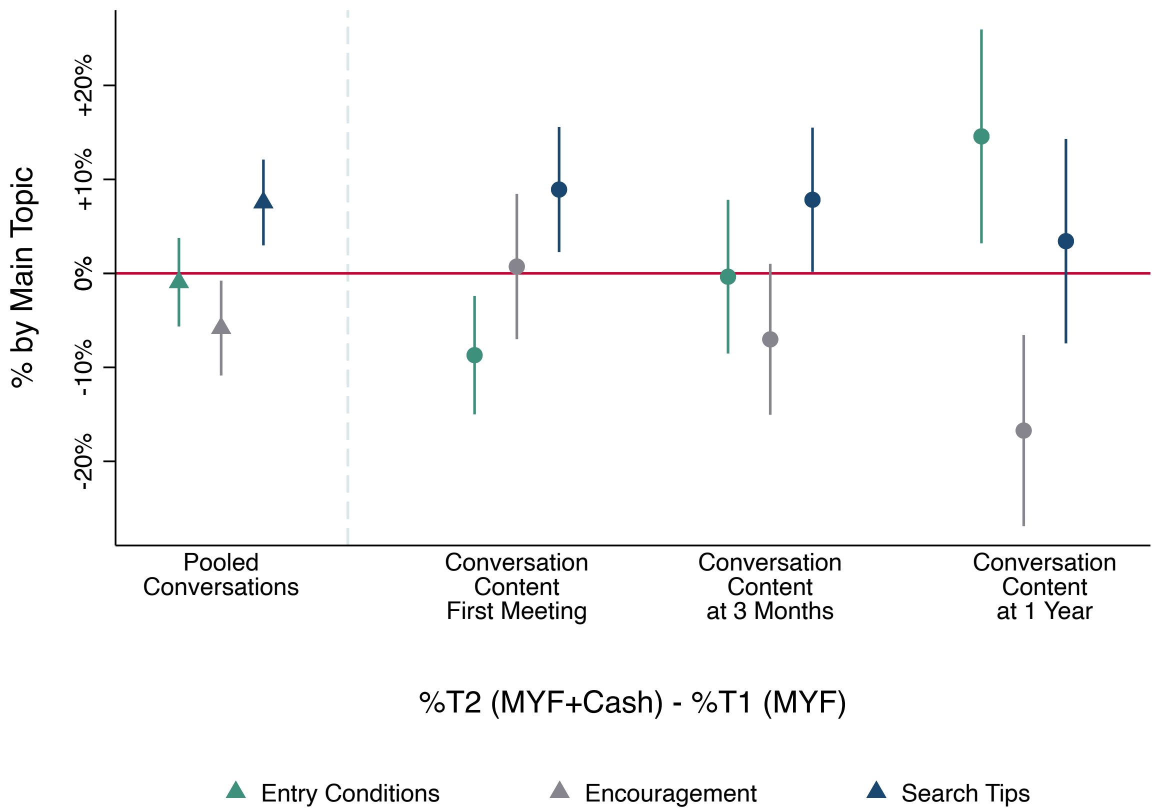
Table A.16: ITT Estimates: Savings and Job Search Expenditures

	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.12: 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.17: 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 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.18: 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 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 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 Columns 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.

Appendix H Text Data

Figure A.13: 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.'
```

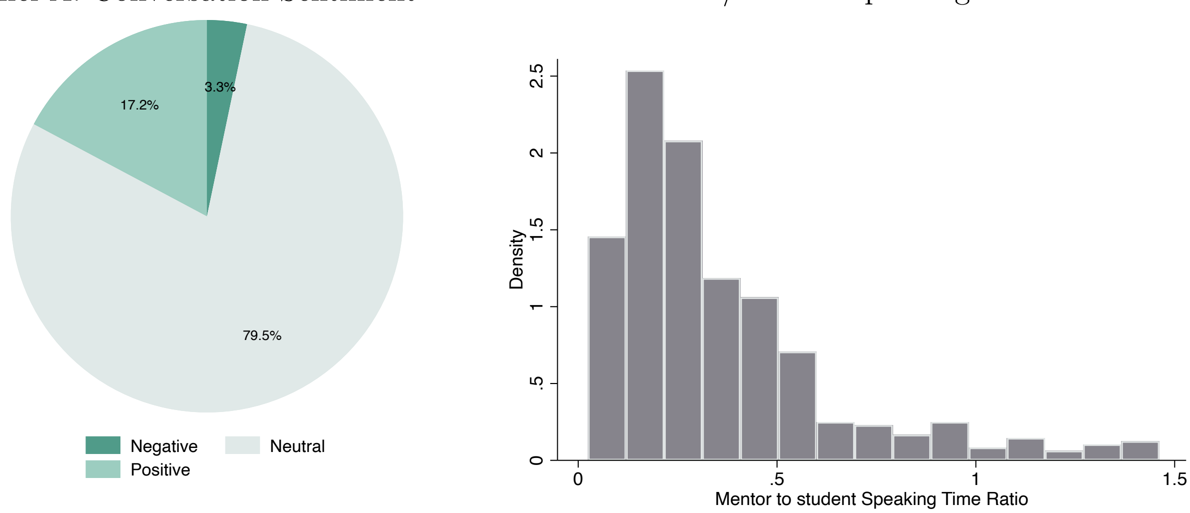
Sentiment Analysis

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.14: mentorship Sessions Text Data: Form

Panel A: Conversation Sentiment

Panel B: Student/Mentor Speaking Time Ratio



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).[43] 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 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:

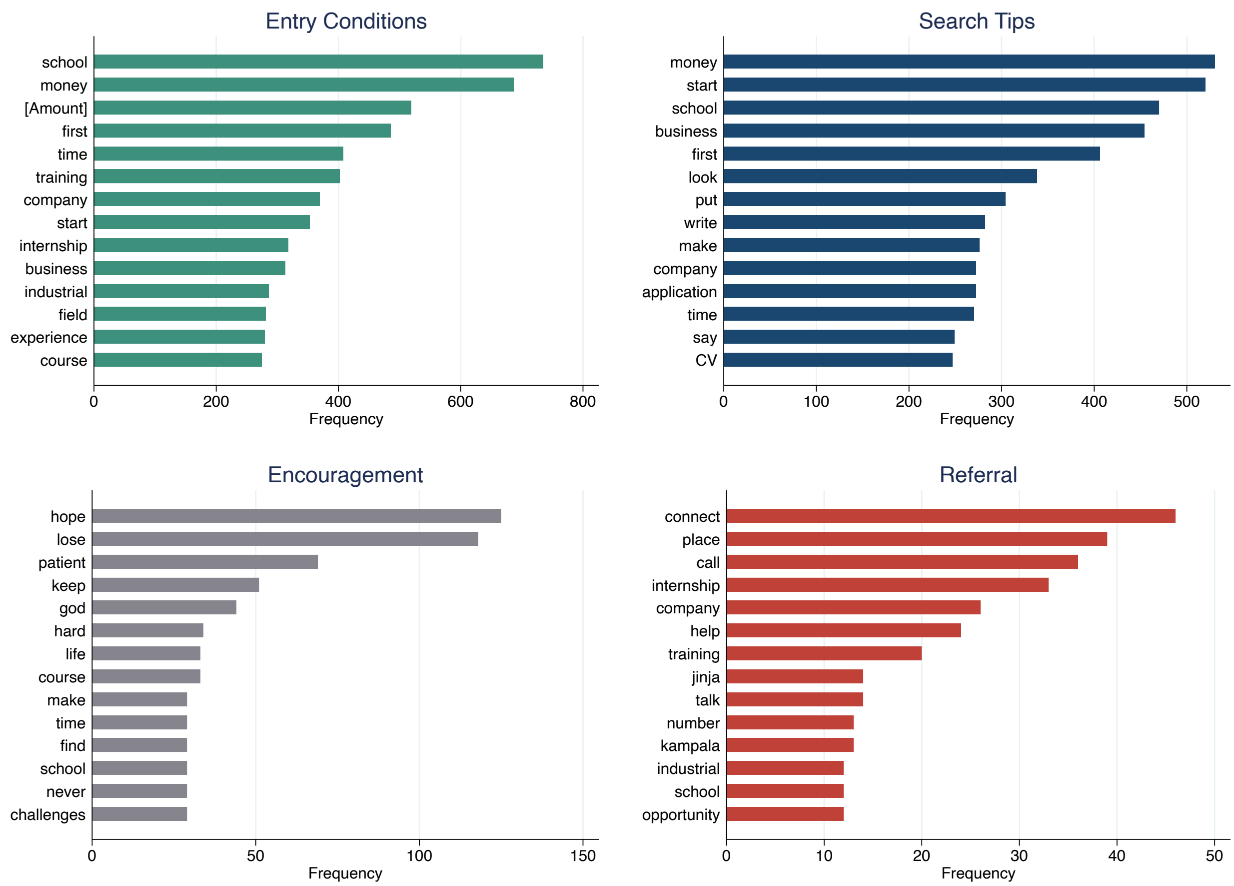
- Encouragement: overcome failure, self-confidence, persistence, resilience, patience
- Entry Level Conditions: earnings, salary, wage, discrimination, contract, practical skills, unpaid jobs, time to find job, employment opportunity
- Search Tips: job search, job search timings, accessing tools, finding 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 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.

[43] 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/.

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.15: 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.

Appendix I 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:

$$Y_{i,s,t} = \alpha + \beta_1 S_1 C_i + \gamma_0 S_0 T_i + \gamma_1 S_1 T_i + X_i'\delta + \lambda_s + \epsilon_{i,s,t} \tag{12}$$

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.[44] 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.8). 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.19. 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

[44] 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.

Table A.19: Spillovers: Leverging 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

unchanged. In this sample, the medium run results are, if anything, stronger.

By examining Panel B of A.19, we conclude that there may have been some spillovers, which, if at all, have caused our overall estimates to be conservative. With the exception 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.

Appendix J Robustness Analysis

Lasso Link Creation

Table A.20: Strength of the Mentor-Mentee Connection - Lasso

	Ever Connected (1)	Connected More Than Once (2)	Strong Link (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.21: 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 2 for the description of the variables.

Table A.22: 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 3 for the description of the variables.

Table A.23: 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 4 for the description of the variables.

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

Table A.24: 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 2 for the description of the variables.

Table A.25: 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 3 for the description of the variables.

Table A.26: 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 4 for the description of the variables.

ATE Results on the Balanced Panel

Table A.27: 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 2 for the description of the variables.

Table A.28: 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 3 for the description of the variables.

Table A.29: 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 4 for the description of the variables.

ITT Results Excluding Referred Students

Table A.30: 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.31: 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.32: 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