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

Livia Alfonsi Mary Namubiru Sara Spaziani

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Abstract

Can mentorship by experienced workers rectify labor market misconceptions among young job seekers, ultimately enhancing their employment trajectories? To answer this question, we designed and randomized *Meet Your Future*, a mentorship program implemented among 1,111 vocational students undergoing school-to-work transitions. The program improved participants' outcomes up to a year after graduation: mentored students were 27% less likely to exit the labor force, worked 8% more days, were 15% more likely to be employed in the sector of their vocational training, and earned 18% more after one year. Using call transcripts and survey data, we show that mentorship worked not by expanding job networks or search capital, but by correcting overoptimism about immediate payoffs and encouraging persistence. Mentored students adjusted their expectations downward for starting wages while maintaining a positive medium-run outlook—ultimately accepting lower-paying first jobs but experiencing steeper wage growth and overtaking controls in earnings within a year. This pattern reflects a shift in strategy: lower reservation wages, more active search, and greater willingness to accept stepping-stone jobs that accelerated career progression. The results highlight the power of combining realistic feedback with motivational support to sustain job search effort and improve employment outcomes.

JEL codes: D83, J24, J64, O10

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*Alfonsi: Harvard Business School. Corresponding author. lalfonsi@hbs.edu. Namubiru: BRAC Uganda. Spaziani: University of Warwick. We thank Hadar Avivi, David Card, Luisa Cefala, Gaia Dossi, Andrew Foster, Pat Kline, Eliana La Ferrara, Ethan Ligon, Jeremy Magruder, Aprajit Mahajan, Ted Miguel, Marco Gonzalez-Navarro, Dev Patel, Imran Rasul, Elisabeth Sadoulet, Jesse Shapiro, Bryce Millett Steinberg, Christopher Walters and seminar participants for insightful comments and suggestions. Pedro De Souza Ferreira, Ottavia Anna Veroux and Hao Wang provided outstanding research assistance. Funding from the following organizations is gratefully acknowledged: the International Development Research Centre; J-PAL PPE Initiative; IZA and the UK Foreign, Commonwealth & Development Office via the IZA/FCDO Gender, Growth and Labour Markets in LICs Programme. Alfonsi would like to thank the Center for African Studies at UC Berkeley for generous financial support. We received IRB approval from UC Berkeley and the Uganda National Council for Science & Technology via MUREC. All errors are our own.

1 Introduction

Today, one-fifth of the world's first-time job seekers live in Africa (United Nations, 2019), where youth unemployment and underemployment rates reach up to 60%. In response, governments across the continent have heavily invested in upskilling initiatives—particularly vocational training programs—to boost employability (McKenzie, 2017). While these programs have proven cost–effective in several contexts (Alfonsi et al., 2020; Maitra and Mani, 2017a), their overall job placement rates remain low, revealing that acquiring skills alone does not guarantee access to employment.

Recent evidence points to supply-side information frictions as major barriers to labor market entry. Young job seekers often have limited understanding of what employers value, lack effective job search strategies, and hold unduly optimistic beliefs, leading them to turn down accessible jobs in favor of hoped-for opportunities that ultimately do not materialize (Groh et al., 2016; Abebe et al., 2025; Banerjee and Chiplunkar, 2024; Bandiera et al., 2023; Donovan et al., 2023).

Providing effective job search advice to jobseekers is challenging (Belot et al., 2019). This challenge intensifies when dealing with overly optimistic individuals. On one hand, jobseekers tend to amplify positive news while overlook negative ones (Eil and Rao, 2011; Wiswall and Zafar, 2015; Jones and Santos, 2022; Mueller et al., 2021). On the other hand, there is a risk of causing discouragement when moderating optimism, which previous work showed can lead to voluntary unemployment, depressed job search, a decline in job quality, and a looming sense of despondency (Kelley et al., 2024; Banerjee and Sequeira, 2023; Bandiera et al., 2023). Information provision alone can backfire, exacerbating the very issues it aims to resolve.

In this paper, we test mentorship as a way of correcting overly optimistic beliefs of young jobseekers, without leading to discouragement. To do so, we design and administer Meet Your Future (MYF), a mentorship program that differs from traditional information-based approaches by pairing job seekers with near-peer mentors who both exemplify successful career trajectories and actively guide mentees to persevere. This approach draws on interdisciplinary research on delivering bad news, particularly from medical psychology, which underscores that conveying hope and the potential for positive outcomes is essential for promoting active coping (Ptacek and Eberhardt, 1996). By applying these insights, we pair soon-to-be graduates of vocational training institutes with relatable, successful workers—whom we call the "future you".

We evaluate the impacts of MYF using a randomized control trial with 1,111 vocational students poised to transition into urban labor markets across Uganda. We collect six rounds of data spanning two years prior to graduation and one year after, including a post-intervention survey of students and mentors. High-frequency data collection around the time of the intervention allows us to evaluate the nature of each mentorship and the lessons learned by all parties. As part of a unique data effort, we record over 350 hours of student-mentor conversations. Capturing not only the substantive content of these engagements but also more nuanced attributes like enthusiasm and conversational richness—difficult to assess through self-reported data—this unique dataset allows us to directly examine the mechanisms through which mentorship operates. By distinguishing between information about entry-level jobs, encouragement, job referrals and actionable search tips, we provide evidence on how mentorship influences job search behavior and employment outcomes.

We begin by documenting significant overoptimism about entry-level pay among job seekers. By tracking both respondents' expected and realized earnings at their first jobs, we can assess the individual-level accuracy of expectations. 94% of control students overestimate their first-job earnings. When comparing expectations to earnings one year later, this proportion drops to 50%, indicating that pay optimism is prevalent but particularly pronounced for first jobs, as students fail to account for the reality that many will be unpaid or low-paid.

Relatedly—and conceptually distinct from wage overoptimism—we uncover a novel dimension of misperception: students hold distorted beliefs not just about the starting point, but about the job ladder itself. They underestimate the returns to experience and fail to see how early, even unpaid, employment is often a stepping-stone to better jobs and higher earnings over time. This is evident in their expected job trajectories: when asked where different pathways would lead, students assign similar long-run outcomes to those starting with unpaid work and those starting with unemployment—despite stark differences in realized career paths. Our data reveal not just mistaken beliefs about what is available now, but flawed expectations about how careers unfold, and how early steps shape upward mobility on the job ladder.

Next, we examine the short- and medium-run impacts of the program on labor market outcomes. Leveraging rich, high-frequency data on job search behavior and employment outcomes collected at three months and one year after graduation, we move beyond a static snapshot of employment to examine both immediate employment and medium-term career advancement. This allows us to assess not just job placement, but also how mentorship shapes the type and progression of jobs individuals access over time, a crucial distinction when evaluating the sustained impact of labor market interventions. Three months after the school-to-work transition, we identify a significant improvement in labor market outcomes. Labor market participation is 27% higher for treated students, who work 8% more days per month. Not only are they working more, but they are 15% more likely to do so in their training sector, reinforcing and building on their vocational skills. These accelerated first employment spells also enabled treated students to climb the career ladder faster. Within the first year after the intervention, they are more likely to be promoted—both within the same firm where they had their first job and between firms. One year after the intervention, treated students earn 18% more than controls. Even conditional on employment, earnings are 14% higher, suggesting that gains reflect not only higher employment rates but also a faster progression to better-paying positions. We estimate the internal rate of return of this intervention to be approximately 300%.

To understand why the program led to these gains, we leverage our rich data and second randomization to analyze its mechanisms. We start by using the content of the audio recordings to confirm the main themes of student-mentor interactions. From this, along with the literature on supply-side frictions, we pinpoint four plausible channels through which mentors can affect labor market outcomes: (i) job referrals, (ii) actionable search tips, (iii) information about entry-level conditions, and (iv) encouragement.

To guide the interpretation of our results and clarify the role of encouragement in improving labor market outcomes, we introduce an extended version of the McCall (1970) search model, incorporating subjective beliefs about entry-level wages and experience premia along with two key decision metrics: the reservation wage and whether to search at all, as opposed to becoming discouraged and exiting the labor force. This framework generates several testable predictions about how mentorship might shape job search behavior and employment outcomes. Among others, that encouragement mitigates the discouragement effect by influencing both thresholds, leading to lower reservation wages but also lower discouragement.

We find empirical support for this prediction in our data, showing that encouragement positively impacts both job seekers' decisions to actively search (rather than disengage) and their willingness to accept job offers. Mentors were especially effective in correcting students' beliefs and warding off potential discouragement effects. Mentored students revised downward their unduly optimistic assumptions about their first jobs and improved their understanding of early employment's significance in shaping long-term career prospects. In response, their reservation wages dropped by 32%. We rule out direct job referrals or enhanced search abilities as viable pathways for the observed treatment effects.

Despite the sharp decline in reservation wages, the overall impact on labor market participation was large and positive. Treated students were 3% more likely to initiate job searches after graduation and 25% less likely to reject a job offer while seeking their first job, leading them to secure employment faster.

To further confirm that MYF influenced labor market outcomes through learning about entry-level market conditions and encouragement to persevere, we leverage the random assignment of students to mentors. We begin by using Empirical Bayes methods to estimate mentor-level heterogeneity. The large bias-corrected variance suggests that some mentors were significantly more effective than others. To unpack the sources of this variation, we employ an instrumental variables approach that draws on rich data about students' reported takeaways and the content of their conversations. We construct mentor types using a leave-one-out design: for each student, we define the mentor's type based on the takeaways of their other mentees—excluding the focal student's data. This approach ensures mentor types are not mechanically driven by individual-level characteristics. We then instrument mentor type with a full set of 158 mentor indicators. Results from this analysis show that mentors who primarily provided encouragement to persevere played a crucial role in prompting job search initiation and had the strongest influence on medium-run labor market outcomes, ensuring that students stayed motivated and did not prematurely disengage from the labor market.

We harness the depth of the data to explore heterogeneity in mentor effectiveness by analyzing variation across mentor-mentee pair characteristics and mentorship styles using conversation data. Specifically, we examine: (i) whether mentors tailor their guidance to individual students and how this relates to outcomes; (ii) how effectiveness varies across observable mentor-mentee pair characteristics; (iii) whether outcomes differ when mentors are more experienced within the program; and (iv) whether effectiveness declines when mentors are assigned more mentees.

Across the four main labor market outcome indexes and their components, effects were generally larger for mentees with more adaptive mentors, regardless of the measure used, though only some differences were statistically significant. Further exploratory analysis suggests that all main findings were largely consistent across subgroups, with no clear evidence that the effectiveness of mentorship varied meaningfully along most observable mentor-mentee characteristics. We find no differences in outcomes for a mentor's first versus later mentees, suggesting limited learning-by-doing. Mentor effectiveness was highest with one to three mentees, declining beyond that.

Last, we implemented a complementary cash intervention to probe whether liquidity constraints were limiting job search or job offer acceptance. A random subset of MYF participants received 40,000 UGX (≈$12), with the suggestion to use it for job search. Contrary to our expectations, the transfer had no direct effect on short-run employment outcomes—most students reported saving the money rather than spending it immediately, suggesting they were not as hand-to-mouth as anticipated. However, the cash did alter the nature of the mentorship relationship: while the frequency of meetings remained unchanged, students who received the transfer were more likely to discuss actionable search tips and to report search strategies as their main takeaway—primarily at the expense of encouragement. This rebalancing away from motivational support, which we identify as the most impactful channel for a sustained impact, likely contributed to the diminished effects at one year. Once again, this finding confirms that encouragement to persevere was a key driver of program's success.

Taken together, our results demonstrate that access to mentors can rectify young job seekers' overly optimistic beliefs while credibly preventing discouragement, thus spurring career development.

Our findings speak to two bodies of work: the behavioral economics of job search and research on mentorship. We contribute to the literature on behavioral job search, which studies how job seekers' biases, such as overoptimism[1], affect unemployment dynamics, by proposing encouragement as a critical mechanism to counteract the unintended consequences of belief correction. Prior studies have shown that while correcting overly optimistic expectations may seem desirable, it can have unintended effects. Jones and Santos (2022) and Chakravorty et al. (2024) find that targeted information interventions for overoptimistic university graduates in Mozambique and vocational students in India either leave expectations unchanged or trigger training dropout, ultimately reducing human capital accumulation. Similarly, Kelley et al. (2024) show that high expectations can lead to voluntary unemployment, while Banerjee and Sequeira (2023) and Bandiera et al. (2023) document that distorted beliefs can induce search strategies that end in discouragement and lower-quality employment.

One context in which belief correction improved employment is Abebe et al. (2025), who show that exposure to firms through a job fair raised employment rates—but only among less educated job seekers. Among their more educated participants, the intervention had no effects. We study an educated population—vocational graduates preparing to enter skilled sectors—where the risks of discouragement are greater and early disengagement can carry lasting costs. Here, the challenge is not only learning which jobs are available or what starting wages to expect, but recognizing that the returns to education are real but take time to materialize. In this context, mentorship provides a more personalized channel for belief revision, helping students grasp the structure of the job ladder, accept lower entry wages, and sustain effort in pursuit of long-run gains.

This paper offers a solution to this challenge adopting different approach: rather than relying on information alone, which can backfire, it demonstrates that pairing belief correction with

[1] Research from high-income countries reveals considerable overconfidence among job seekers (Spinnewijn, 2015; Mueller et al., 2021; Potter, 2021). Recent studies from low-income settings document similar patterns and warn that distorted beliefs can delay exit from unemployment (Abebe et al., 2025; Kelley et al., 2024; Chakravorty et al., 2024; Bandiera et al., 2023; Banerjee and Sequeira, 2023; Jones and Santos, 2022).

encouragement preserves motivation and sustains engagement in harder than expected labor market conditions. Indeed, while mentored students significantly revised their expectations about immediate earnings and became more willing to accept lower-paid first jobs, they did not adopt a pessimistic view of their medium-term prospects. Encouragement works by helping reframe labor market entry as the beginning of an upward trajectory, not a setback, making that trajectory feel both credible and worth striving for. More broadly, our findings highlight the importance of balancing realism with hope: when early rewards are delayed, maintaining motivation requires shining light on the long-term path and sustaining effort through the uncertainty that characterizes early career stages. [2]

We contribute to the literature on mentorship in two ways.

First, it provides, to the best of our knowledge, the first experimental evaluation of a mentorship program specifically designed to support the transition from education to the labor market, a critical phase with lasting consequences for young workers' careers. While mentorship has been widely studied, mostly through observational studies, in educational (DuBois et al., 2002; Eby et al., 2008; Jacobi, 1991; Crisp and Cruz, 2009) and organizational settings (Brooks et al., 2018; Levinson, 1978; Allen et al., 2004, 2017; Sandvik et al., 2020), little is known about its role in helping job seekers navigate labor markets. To the best of our knowledge, two studies come closest to considering labor market outcomes. Rodríguez-Planas (2012) evaluates a mentorship program aimed at boosting school enrollment. While it tracked labor outcomes as well, education gains did not translate into employment, highlighting the need for interventions built for the school-to-work transition. Resnjanskij et al. (2024) target this transition but examine intermediate outcomes, such as soft skills, not employment. Our study was not only designed to guide first-time jobseekers, but also to evaluates the impact of mentorship on job outcomes at two key points: three months and one year after labor market entry, as well as on job seekers' beliefs, search strategies, and job preferences.

Second, by combining mentorship audio recordings, high-frequency panel data, and random mentor assignment, this study is the first to open the black box of mentorship in labor markets. We disentangle direct mechanisms (job referrals and search tips) from belief-based ones (information correction and encouragement), identifying not just whether mentorship works, but how and why it does, offering insight into which components are most relevant for youth labor market entry in contexts of uncertainty and informality. More broadly, by exogenously creating weak ties between job seekers and employed workers, we provide

[2] Similar in spirit are insights emerged outside the labor market: in the CMTO housing program, personalized encouragement helped low-income families pursue complex relocation decisions (Bergman et al., 2024; DeLuca et al., 2023).

experimental evidence on how these can generate surplus: not only by improving access to opportunities, but also by shaping expectations, encouraging perseverance, and influencing how young workers navigate the early stages of their careers.

Last, this study contributes to the policy discourse on reducing youth unemployment in the Global South by identifying a cost-effective, scalable intervention that enhances the effectiveness of upskilling programs in which governments heavily invest to promote employability. While these programs have proven effective at building human capital and increasing employment in some contexts (Alfonsi et al., 2020; Maitra and Mani, 2017b), their overall job placement rates remain low, leaving substantial talent underutilized (Bandiera et al., 2023). Personalized job search assistance has shown promise in online and high-tech settings (Belot et al., 2019, 2022), but scaling such support in low-resource environments, is challenging. This study provides experimental evidence that mentorship constitutes a cost-effective intervention, yielding some of the highest internal rates of return observed in this domain, particularly in contexts where youth need it the most.[3]

The paper proceeds as follows: Section 2 provides context for the labor market under study. Section 3 describes the experimental design and the Meet Your Future program. Section 4 describes the MYF program's impact 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. In section 6 we carry out two validations exploiting additional randomization features of the design. Section 7 presents IRR estimates. Section 8 concludes.

2 Labor Market Context and Study Population

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., 2023). Most youths, including skilled ones, fail to climb a job ladder. Rather, their employment is characterized by transience and informality. The relative magnitudes of the supply- and demand-side imbalances are unclear. Firms often cannot recruit workers who satisfy their needs. Simultaneously, jobseekers failure to obtain their ideal employment leads to their withdrawal from the job market (Bandiera

[3]This statement is based on our review of active labor market programs targeting youth in low-income countries, particularly those focused on upskilling or job search assistance. With the caveat that many studies do not report program costs or calculate internal rates of return, we found no interventions with IRRs of a comparable magnitude. Most training programs report IRRs in the range of 23%–35% (Adoho et al., 2014; Alfonsi et al., 2020; Blattman et al., 2014; Chakravarty et al., 2016; Maitra and Mani, 2017b) and of 3.4%-9.8% for job search assistance-type of programs (Crépon and Premand, 2024; Abebe et al., 2021).

et al., 2023).

In these urban labor markets, worker-firm matching 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.[4] The high degree of labor market informality and the lack of digital platforms make information acquisition costly. This has consequences for match quality. These features suggest that the creation of a connection to a successful worker is a promising intervention.

Vocational Training Institutes To boost productivity, the Ugandan government initiated a strategic plan for vocational education in the early 2000s. This commitment was reinforced with the approval of the Skilling Uganda Strategic Plan, a 10-year initiative, in 2011, and complemented in 2017 by the Skilling the Boy Child and Girl Child program. Today, as in many other East African economies, the vocational sector is well established in Uganda; VTIs are effective at generating productive human capital (Alfonsi et al., 2020), and firm owners are familiar with recruiting their graduates. Although training and credentials raise the propensity for stable employment, the market for VTI graduates fails to clear.

Students/Job Seekers Our sample comprises vocational students about to enter the labor market (Figure 2). Specifically, we surveyed the 2019 cohort of students enrolled in the National Certificate Program at five VTIs across Eastern and Central Uganda.[5] This is a two-year program aimed at instructing students in a specific occupation. The 1,111 students in our sample are trained in 13 skills: motor-mechanics, plumbing, catering, tailoring, hairdressing, construction, electrical engineering, carpentry, machining and fitting, teaching/early childhood development, agriculture, accounting and secretarial studies. 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. Our sample is representative of the population of Ugandan youth enrolled in practical tertiary training. It arguably represents a labor market segment with the potential to become among the most productive workers in the country. Table 1 reports students' baseline characteristics: they are on average 20 years old, 41% are female, the majority are single and largely of Christian faith. The sample is

[4]Similar shares emerge if we examine the broader population of both skilled and unskilled job seekers in this country(Merotto, 2020), showing that network connections are crucial in multiple labor market segments.

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

relatively heterogeneous in terms of socioeconomic background, and about 53% ever worked before (half, in casual work).

Mentors Mentors were selected from alumni of our partner VTIs who had completed their studies three to five years before the students' transition into the job market. This selection resulted in mentors with an average age of 25 years and an average tenure of 3 years in the labor market (Online Appendix Table A.4), ensuring relatability and minimizing recall bias by balancing relevant experience with approachability for students.[6] Due to the absence of systematic graduate tracking by VTIs, the identification process involved manually collecting and digitizing thousands of alumni records from physical registries and phone contacts. Through this process, we surveyed 714 alumni and ultimately engaged 158 mentors, selected based on their labor market outcomes, with an emphasis on having stable employment within their sector of training. Further details of the mentor selection process can be found in Online Appendix A. Each mentor was randomly assigned between one and five students based on VTI of attendance and occupation strata. Mentors received a stipend of $40 along with airtime reimbursements to acknowledge their time and effort. This compensation was not tied to student outcomes but was conditional on the completion of all mentorship sessions with their assigned students.

3 School-to-Work Transition: Expectations vs. Reality

Misperceptions About Early-Career Opportunities Consistent with the growing body of evidence from both low-income (Banerjee and Sequeira, 2023; Bandiera et al., 2023) and high-income settings (Spinnewijn, 2015; Mueller et al., 2021), we document widespread distortions in labor market entrants' expectations about their immediate employment prospects. Specifically, we tracked students' expectations regarding the likelihood of receiving job offers and their anticipated earnings at their first job.[7]

[6] We avoided the cohort with one year of labor market experience as they overlapped with our student sample. In our sample, only 3% of mentors had previously interacted with the students assigned to them.

[7] We elicited expectations regarding the time to first employment and expected earnings at first employment. Their evolution was tracked at four points (five for the treatment group): baseline, midline 1, midline 2, midline 3, and, for the treatment group, at the Post-Interaction Survey. Monetary incentives were provided to reward prediction accuracy in midlines 2 and 3. The monetary incentives were designed primarily to encourage respondents to take the questions seriously while ensuring they did not influence students' job search behavior. To achieve this, the reward amount—conditional on the accuracy of the prediction—was intentionally kept small, consisting of a lottery for 15,000 UGX in airtime. Participants were informed that their chances of winning would increase the closer their predictions were to their realized outcomes, with payouts realized only three and nine months later. To estimate expected earnings, we followed the approach of Alfonsi et al. (2020), asking individuals to provide their minimum and maximum expected earnings if offered a job in their sector immediately after graduation. We then asked them to assess the likelihood that their earnings would exceed the midpoint of the two values, fitting a triangular distribution to measure their average expectations.

We observe a striking optimism bias regarding the expected wage distribution for entry-level jobs, i.e., the first job they expect to land after graduation. Panel A of Figure 1 presents the distributions of expected monthly earnings elicited pre-treatment (green tones) alongside realized conditional monthly earnings in the control group (red tones). The patterns remain as striking when examining unconditional earnings.

The panel structure of our data collection allowed us not only to provide monetary incentives that rewarded individual-level prediction accuracy but also to validate, ex-post, how students' expectations compared to their *own* realized earnings at their first job. Even when accounting for individual observable and unobservable characteristics—such as where they perceive themselves to be on the skills distribution—we find that realized earnings were only 17% of what they had initially predicted. When compared to their *own* realized earnings after one year, this share rises to 78%, suggesting that while students significantly overestimate their immediate post-training earnings, their expectations align more closely with a longer-term reality.

Why do students hold such overly optimistic beliefs about their initial job prospects? The answer begins even before they enter vocational training. Baseline data from Alfonsi et al. (2020) show that prospective students expected vocational training to increase their earnings by nearly 200%, far exceeding the actual estimated returns of 42%. These beliefs are consistent with a model of thin labor markets, in which young job seekers primarily interact with employed individuals, often mid-career workers, but have little access to information about entry wages and the realities of early-career employment. This leads them to extrapolate from observed career outcomes, reinforcing their optimism. At the same time, the persistence of these distorted beliefs throughout their training suggests that vocational training institutes may not have strong incentives to correct them.

Misconceptions About Job Transitions We also document a new fact: labor market entrants in our context are not only too optimistic about their starting wages, they also have a poor sense of labor market dynamics and wage-growth opportunities. Panel B of Figure 1 shows the expected and realized transition matrices of employment pathways from three months to one year after the school-to-work transition. In comparing the two, we gain key insights into how these job seekers perceive job ladders. First, they undervalue unpaid initial job spells, believing they offer the same likelihood of leading to paid employment as an initial spell of unemployment (expected: 46%—virtually the same as for unemployment—vs. realized: 54%). They also underestimate the risk of being unemployed three months after graduation (expected: 45% vs. realized: 15%)—essentially undervaluing both the benefits of accepting a stepping-stone job and the costs of rejecting it and staying unemployed. Finally, while they underestimate overall unemployment prevalence at one year, the gap between their expectations and realizations (55% vs. 62%), conditional on being in paid employment at three months, suggests that they are not simply holding out for paid jobs because they expect them to be a permanent fix. Instead, they seem aware of high separation rates and the relative instability of first jobs, but fail to appreciate the value of alternative pathways.

The Case for Mentorship This presents a key challenge: while, when effectively implemented, vocational training has high internal rates of return and remains a valuable upskilling and policy tool, it inadvertently contributes to unrealistic beliefs about immediate post-training job opportunities and wages, which fall short of students' inflated expectations. Failing to recalibrate beliefs early on may leave students misaligned with the realities of the job market, ultimately dampening the effectiveness of training investments. We test mentoring as a complementary intervention capable of adjusting expectations while preserving motivation through the school-to-work transition. By providing direct access to experienced workers, living proof that you can reach long-term success, mentorship offers a way to both realign expectations and sustain motivation, ensuring that students remain engaged while aligning their strategies with actual labor market conditions.

4 The Experiment

To study the impacts of mentorship on job seekers' performance and test its potential to rectify optimism while mitigating discouragement, 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 1,111 students into the program.

4.1 Randomization and Treatment Arms

The experimental design is summarized in Figure 3. Of the 1,111 students in our sample, 30% were randomly assigned to the Meet Your Future Program (T1) and 30% were assigned to the Meet Your Future Program with Cash (T2). The remaining 40% form our control group.[8] We stratified the randomization at the student level and included all strata and balance variables in every treatment regression. In all our choices, we followed the principles highlighted by Bruhn and McKenzie (2009) and Athey and Imbens (2017).[9] The identification strategy for

[8]To design our intervention and refine survey tools 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.

[9]We stratify along four dimensions—VTI (to account for potential variation in treatment implementation), gender, whether a student was hard to reach (to mitigate the risk of differential attrition), and smartphone

our RCT assumes that within each stratum, treatment and control students do not differ on average in observable and unobservable characteristics. To support this, we check for balance across treatment arms on observable characteristics likely correlated with the outcomes of interest. The experimental design is balanced across nearly all variables of interest, as shown in Table 1. Furthermore, we have low attrition: 9% overall, with 16% attrition at endline 1 and 17% at endline 2. We consider these rates satisfactory for highly mobile subjects over three years. Online Appendix B describes the correlates of student attrition; they confirm that attrition is uncorrelated with treatment and show no evidence of differential attrition based on observable characteristics (Online Appendix Table A.5). Therefore, 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.[10] 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 could ask questions as well as share their doubts, fears, and dreams. These interactions were unrestricted: each student-mentor pair could discuss what they found most useful for the student's transition to the labor market. This tailored the mentorship to each student's needs, resembling real-life interactions with a network member. The first mentorship session (MS1) occurred about a month before graduation. It was a conference call between the student, mentor, and enumerator, who initiated and recorded the conversation. Treated students learned about the MYF program from the enumerator during this session. After introductions, the enumerator remained silent. A post-intervention survey followed MS1 to capture students' main takeaways. The second (MS2) and third mentorship sessions (MS3) occurred two weeks before and after graduation, respectively (Online Appendix Figure A.4). These sessions, initiated by the mentor, were private conversations between mentor and student. Mentors had to send a text after the completion of each of these sessions to confirm they happened. We double checked this information with the students during endline 1. Students and mentors could interact beyond these three sessions. Mentors recorded the frequency, duration, content, and means of any additional interactions during the two-month program in a logbook (Online Appendix Figure A.5).

Mentors attended a one-day training led by the research team before the program began. They learned their responsibilities as program ambassadors and how to assist students with

ownership —resulting in 40 strata. In Online Appendix C, we provide more details from our pre-analysis plan on the selection of strata and balance variables —the set of variables for which we required no imbalance.

[10] 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 enough available graduates. In such instances, students were paired with successful graduates from the VTI nearest to their own.

workforce transition. To thank them for their participation, mentors received ∼$40 and airtime reimbursements after completing three mentorship sessions with all students assigned to them and a short survey. Their compensation was not tied to the students' success in the labor market.

To test whether relaxing liquidity constraints would compound the effects of the mentorship program, we provided a random subset of MYF program participants with 40,000 UGX (∼$12) upon graduation. This cash transfer was unconditional, though students were advised to use it for job search and required to report their spending to BRAC. The transfer proved largely ineffective - and possibly even backfired - as we describe in section 7.2. For most analyses, we pool T1 and T2 and refer to the effects as those of the MYF program.

4.2 Program Take-up and Participants' Engagement

Take-up was high on both the extensive and intensive margin: 91% of the students assigned to the MYF program corresponded with their assigned mentors at least once.[11] The intensive margin reflects the substance of these connections: over the three-month part of the program, there were an average of 2.6 interactions, each lasting on average 51 minutes. After one year, the average number of interactions increased to 7.8. 70% of student-mentor pairs interacted more than the three times dictated by the program, and, conditional of having ever connected, 45% of the pairs were still in touch a year after the MYF rollout.[12]

We collected self-reported measures of engagement, identification, transportation, and perceived usefulness from students. We observed high satisfaction rates across all indicators and student-mentor pairs.[13] Similarly, the identification and transportation indexes, adapted from Banerjee et al. (2019), were consistently high. Enumerators' observations of student-mentor conversations also assessed ease and engagement.

We validate these patterns using our exclusive data source: 512 audio recordings of the mentorship sessions, transcribed and translated when necessary.[14] Sentiment analysis with

[11]Noncompliance was mostly due to the inability to contact some students. In Online Appendix Table A.6 we show that non-compliers (56 students) have the same probability of having worked before enrolling in the VTI but they were more likely to be married women and to have a household asset index above the mean.

[12]The average total amount of interaction time between students and mentors is 3.2 hours. This is a relatively light touch mentorship program; a meta-analysis of mentorship programs found an average length of 6.8 hours across 55 mentorship interventions (DuBois et al., 2002).

[13]Between 85% and 95% of treated students agreed or strongly agreed with the following statements: "You felt at ease asking questions and discussing personal issues with your mentor"; "The mentor cared about your personal experience"; "Speaking with the mentor felt comfortable, like being with a friend"; "The mentor seems prone to provide help."

[14]Missing audio recordings were absent because the recording quality was insufficient for transcription or because the recording was lost.

VADER (Hutto and Gilbert, 2014) shows that all participants perceived the conversations as neutral or positive, particularly the students. The mentor-to-student speaking time ratio indicates that mentors mainly led the conversations, transferring content to students while ensuring every student was actively engaged. To conclude our engagement analysis, we examine when strong links form between mentors and mentees, defined as interactions beyond the three required mentorship sessions. We analyze data dyadically, considering both student and mentor characteristics, using a simplified version of the Fafchamps and Gubert (2007) regression model since strong links in our setting can only be unidirectional. Table A.1 shows that strong links are more likely to form when students and mentors come from the same VTI, have smaller age gaps, and share a district of origin.

5 Results

5.1 Estimation

In this section, we document how the mentorship program influenced students' labor market outcomes three months and one year after the school-to-work transition. We report ITT estimates, the most useful from a policymaker's perspective, as they reflect likely binding challenges to rolling out similar mentorship interventions.[15] Our estimates are based on the following ANCOVA specification for student i in strata s at endline $t = 1, 2$:

$$Y_{i,s,t} = \beta_0 + \beta_1 T_i + X_i'\delta + \lambda_s + \epsilon_{i,s,t} \tag{1}$$

Y_i is the outcome of interest for student i measured at endline 1 or endline 2 (i.e., at three 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 Online Appendix C and individual covariates measured at baseline selected on the basis of their ability to predict the primary outcomes to improve statistical power (McKenzie, 2012). λ_s are strata fixed effects. $\epsilon_{i,s,t}$ is the error term. We cluster errors at the strata level.[16] β_1 measures the causal effect of being selected to participate to the MYF program on Y_i under SUTVA. This will not hold if treatment displaces control students because treated students are relatively more attractive to employers. As we implemented the program in five out of

[15] The ATE specification, which we estimate for robustness, instruments treatment assignment with treatment take-up (with the same controls). In our preferred ATE specification, take-up is defined as a dummy equal to 1 if the student spoke with the assigned mentor at least once. When we define take-up as having completed all three mentorship sessions, treatment effects strengthen. Overall, because of the high compliance rate in the experiment, ATE and ITT estimates are extremely similar.

[16] In our main specification we do what we pre-specified. However, following discussions with others in the field, we also report our estimates with robust standard errors in Appendix H, with no change in results.

715 accredited VTIs in Central and Eastern Uganda (1,270 nationwide), which are located in the region's three largest urban labor markets, any advantage for treated students will likely not come at the expense of the control group.[17] Indeed, treated students are a small fraction of the job seekers entering the country's largest labor markets during our period of study. SUTVA could also be violated in the case of spillovers from mentored to control students. To limit their occurrence, our intervention happened after classes were concluded and students had returned home (most of these VTIs are boarding schools.) We are not overly concerned with spillovers, as, given our methodology, they are likely to make the estimates conservative. In any case, we mapped the VTIs' friendship networks of each treated and untreated student to rigorously measure them. We examine the spillover effects more in detail in Online Appendix G and confirm that, if at all, they caused our overall estimates to be conservative.

5.2 Short-Run Labor Market Outcomes

Table 2 presents ITT estimates of the impacts on labor market outcomes at three months. We begin by examining 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 in the labor force, which we define as either working or searching for a job. In other words, treated students are significantly less likely to have exited the labor market, meaning they are not working, not searching, and instead are staying at home, helping with household duties, or engaged in subsistence farming as their main activity (Column 1). They worked 8% more days in the month preceding the survey (Column 2) and were 15% more likely to work in the sector in which they received training (Column 3). Yet they are earning little and not more than the control group (Column 4).[18] Lastly, Column 6 shows that these first matches are more stable, as they last 25% longer.

5.3 Transitions and Medium-Run Labor Market Outcomes

Table 3 reports treatment effect on the transition across job spells as well as employment and earnings at one year. What emerges is that the more numerous and stable matches treated students landed early in their search allowed them to transition sooner to a worker-type position following an initial traineeship. In other words, they ascend the job ladder faster as they are both more likely to be retained within the same firm (Column 1) and promoted

[17] 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) or UBTEB (291) or both (69) was 715.

[18] The same conclusion is true if we look at earnings conditional on employment.

across firms (Column 2).[19] In sum, early on, treated students land more jobs, and more jobs in their training sector, while they do not make significantly more than their control counterparts, and much less than what they expected. However, they work more intensively and build on their technical skills in those jobs. Hence, they stay longer in them and leverage those jobs for better 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 coefficient on the participation dimension is positive and relatively large, at around one standard deviation, but the lack of precision limits our ability to make decisive statements.[20] However, treated students earn 18% more than control students (14% more, p-val .06, conditional on being employed). In Figure 5, we show the empirical CDF as well as the distribution of the quantile treatment effects at three months and one year. We confirm no statistically significant differences in earnings in the short run and higher earnings at one year with QTEs of \$8.26 (p-val .10) at the 50th percentile, \$13.77 (p-val .06) at the 75th percentile and \$18.15 (p-val .06) at the 90th percentile. This same narrative is confirmed by the pathways analysis reported in Table A.2, where we show reduced-form estimates of the effects of MYF on various pathways to employment in a year. Each pathway is described by the combination of one of three possible labor market statuses: unemployed; working for a zero or negative pay; and working for a positive pay, three months and one year after graduation. The sample is restricted to respondents found at both endlines. While these outcomes are contingent on respondents' employment status at three months and hence we lose causality, they provide compelling suggestive evidence in line with our thesis that the treatment makes students more likely to accept stepping-stone jobs, which in turn help them climb the job ladder. All main results are unaffected by the inclusion of an additional set of controls selected through a double LASSO procedure (Belloni et al., 2014).

[19] 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 only 7% hinting to the fact that these are entry level jobs.

[20] We do see that treated students 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. We also see that for students who only received the mentor and not the cash transfer, the treatment effect on the extensive margin remains strong and statistically significant at one year as well (we discuss this important point more in section 7.2.)

6 Mechanisms

6.1 Interaction Content and Students' Takeaways

The combination of audio recordings of the mentorship sessions and students' self-reported primary takeaway provides an invaluable window into the conversations. Panel A of Figure 4 presents the raw conversation content as computed using the text data. To perform topic analysis and discern the content of these conversations, we employ a generative pre-trained transformer model. Specifically, we use the state-of-the-art Claude 3.7 Sonnet (`claude-3-7-sonnet-20250219`) model developed by Anthropic to label the topic of each sentence within a conversation. This approach enables us to unpack the black box of interactions between mentors and mentees.

Informed by economic theory and the context of our experiment, we posit (and pre-specified) that mentors can affect students' career trajectories by providing different kinds of support, which we classify into four main groups: job referrals, search tips, information about entry-level conditions, and encouragement. Accordingly, we provide Claude 3.7 Sonnet model with natural language descriptions for each of the four categories plus a neutral, residual, category.[21]

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 entry-level jobs, search tips, job referrals, and encouragement to persevere. 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.[22] Second, the majority of conversations discussed all three remaining categories of support, with information about entry-level jobs and encouragement having the highest correlation in terms of frequency. Lastly, no other major topic was discussed.[23]

[21] In previous versions of this paper, we relied on the at the time state-of-the-art BART Model and GPT-4o model. BART Model trained on the Multi-Natural Language Inference (Multi-NLI) dataset. Specifically, we employed 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. Savelka and Ashley (2023) demonstrated that large language models outperforms traditional transformer models such as BERT (Devlin et al., 2018) in the context of unsupervised text classification (see Online Appendix F for details on the procedure as well as examples of classified sentences).

[22] The unexpectancy of the result is partly due to the comprehensive measure we employed: we adopt a broad definition of referrals, including any instance in which a mentor discussed a potential job opportunity, a personal contact who could assist the mentee, the name of a possible workplace, or an explicit commitment to providing a referral. This approach captures even indirect or preliminary indications of job connections rather than restricting the definition to direct referrals alone.

[23] Manual reading of the content categorized as neutral suggests that (1) the vast majority of the neutral sentences consist of greetings, personal introductions, phone numbers exchange, resolutions of issues related

While learning about the 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 and note that the elasticity of retention is significantly higher for the encouragement category than for the search tips and information on entry requirements. This pattern may be explained by the concept of emotionally arousing experiences, where positive emotional impact increases memory retention, making encouragement more memorable than technical advice (Cahill and McGaugh, 1998). Additionally, since much of the labor market information was sobering, students may have subconsciously focused on the encouraging aspects of mentorship to sustain their motivation and optimism.

6.2 An Illustrative Model

In this section, we present a stylized model to guide the interpretation of our results and derive testable predictions about the mechanisms through which mentors improved young job seekers' labor market outcomes.

Setup We consider a partial equilibrium environment with a utility maximizing job seeker whose behavior follows a reservation wage strategy. We model their dynamic responses to the MYF program through the lens of a discrete time 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. (2023).[24] Specifically, our representative job seeker has subjective beliefs about the entry wage distribution, $F(w)$, as well as the experience premium, ω, i.e., the transition matrix from wage w at time t to wage w' at time $t+q$. Time t is discrete and job seekers have preferences over consumption, given by $u(x) = x$. Job seekers are homogeneous in skill level and infinitely lived. When not working, they earn their value of leisure, b.

Absent the MYF program, in each period t, unemployed job seekers choose whether 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 $F(w) \sim N(\mu)$ with associated density $f(w)$. The 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 that you enjoy if, in the previous period, you accumulated experience. We simplify the model by requiring that

to the poor network quality in the call, or short sentences hard to classify, such as "yes, that completely makes sense"; (2) there are only two relatively recurring topics we are currently disregarding in our analysis: examinations and Covid-19 prevention and worry, when the conversation is not linked to the job market.

[24] More details on where we simplify and expand Cortés et al. (2023) are in Online Appendix D.

ω becomes zero for a tenure greater than one spell. If they decline the offer, they return to the search decision step. We do not allow for on-the-job search or job destruction.

Biased Beliefs To replicate what we establish experimentally in section 3, we assume that job seekers do not know μ, the mean wage offer they will receive, nor ω, the wage evolution given by the experience premium.[25] Instead, they form beliefs about μ and act based on a perceived probability distribution $F(\hat{\mu})$ of the entry-level wages. Likewise, they form beliefs about ω and act accordingly. We say that job seekers' beliefs are biased if $\hat{\mu} \neq \mu$ or if $\hat{\omega} \neq \omega$. Job seekers with $\hat{\mu} > \mu$ are optimistic. While we assume that beliefs change over time, we also assume that job seekers are myopic; i.e., when making their decisions, they do so under the assumption that the expected offer is the same forever (Cortés et al., 2023). Like 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, our 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.

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 allow for wage growth, the value of employment will depend on the beliefs over the job ladder:

$$W(w, \hat{\omega}) = \frac{w + \beta\hat{\omega}}{1 - \beta} \tag{2}$$

The value of unemployment instead can be written as:

$$\begin{aligned} U(\hat{\mu}, \hat{\omega}) = \int_c \max_{s \in 0,1} \Bigg(-cs + b + \beta s \lambda \int max\{W(w, \hat{\omega}), U(\hat{\mu}, \hat{\omega})\} dF(w; \hat{\mu}, \hat{\omega}) \\ + \beta(1 - \lambda s) U(\hat{\mu}, \hat{\omega}) \Bigg) dH(c) \end{aligned} \tag{3}$$

and it depends on the job seeker's beliefs because the expectation is taken over the subjective

[25] Our framework comprises of distorted beliefs and subsequent learning about the mean-wage offer distribution at entry. Alternatively, biases in job search prospects have been modeled as misperceptions about job offers arrival rate, λ (Spinnewijn, 2015; Bandiera et al., 2023). However, students in our study accurately estimate job search duration but misjudge the type of positions (traineeship vs. temporary vs. permanent) and associated earningsThey overwhelmingly seek permanent, paid jobs, despite such opportunities being rare for their profile. Similarly, Banerjee and Sequeira (2023) find that young job seekers in South Africa overestimate their earnings, largely due to inflated expectations of securing high-wage jobs.

offer distribution $F(w;\hat{\mu},\hat{\omega})$. 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 do search, they face a probability λ of receiving an offer. By comparing the returns to search, to the returns not to search we obtain 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:

$$c^*(\hat{\mu},\hat{\omega}) = \beta\lambda \int max\{W(w,\hat{\mu},\hat{\omega}) - U(\hat{\mu},\hat{\omega}), 0\} dF(w;\hat{\mu},\hat{\omega}) \tag{4}$$

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

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

6.3 Predictions on MYF

We predict that a MYF mentor can affect outcomes in three ways.

1. It can directly affect λ, the job offer arrival rate, by providing job referrals and therefore connecting the student to more jobs or by offering search tips, making the students better at searching, i.e., increasing λ.

2. It can rectify beliefs over the mean offer distribution of their first job. As we saw in section 3, students are overly optimistic about the mean wage offer. The mentor can correct overly optimistic beliefs by sharing information about entry-level jobs' characteristics, therefore lowering $\hat{\mu}$.[26]

3. It can shift beliefs over the future value of the first job by providing encouragement and hope and enhance their confidence in wage growth opportunities, raising $\hat{\omega}$.

We derive predictions on the reservation wage behavior and discouragement effects, depending on which of these mechanisms prevail. The proofs for the propositions are provided in Online Appendix D:

> **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

[26]Mentors can correct pessimism as well and raise $\hat{\mu}$. However, less than 4% of control students realized a higher than expected wage at their first job. We therefore only consider more or less optimistic job seekers.

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.[27]

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 ($c^*(\hat{\mu}, \hat{\omega}) \downarrow$).

By shrinking the *expected* early stream of high-wage job offers, the mentor can induce individuals to revise their beliefs downwards. Once self-confidence is sufficiently low (either immediately, leading to no search at all, or as the search progresses), job seekers become discouraged and give up on searching. This proposition simply requires the reservation wage to be monotonic in belief ($\hat{\mu}$), i.e., 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 declining over an unemployment spell because of natural learning (Barnes, 1975; Feldstein and Poterba, 1984). However, more recent evidence points toward 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, three months after graduation, remain overly overoptimistic about their prospects. In the next section we will see that these sticky reservation wages are shifted abruptly by the treatment.

Proposition 3: Encouragement and confidence over a positive 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 leaving the labor force. Control students' reservation wages and search behavior are consistent with the belief that wages evolve according to a Markov process: under these 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. With this assumption, the starting salary is a sufficient statistic for the present value of career earnings. When mentors inform students of heterogeneity in wage dynamics, including the fact that unpaid

[27] 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 update following the interactions with the mentors.

or low-paid jobs are more prevalent than expected 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.

In sum, following participation in the MYF program, job seekers' employment outcomes may shift for two reasons. The first is an *actual* change in prospects, modeled as an increase in their arrival rates of offers. The first proposition 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). The second is a *perceived* change in prospects. 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 the channels exert opposing forces.[28] Using our survey data, we now test empirically which dominates.

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

Can mentors' success be linked to their ability to correct distorted beliefs without causing discouragement? We start by examining the direct impacts the mentorship program had on job seekers' willingness to accept a job and search behavior. Columns 1 and 2 of Table 4 report treatment effects on student' reservation wages and self-reported willingness to accept an unpaid position as their first job. The results are clear: the treatment substantially lowered the reservation wage (-32%) and increased the willingness to accept an unpaid job (+13%). These changes translated into changes in search behavior, most notably with respect to job offers acceptance: conditional on having searched for a job, treated students are 25% less likely to turn down a job offer while looking for their first job. The unconditional outcome shows a treatment effect of 22% with a p-value of .09. Table A.3 shows that, in line with Corollary 1, results on willingness to accept a job and search behavior are driven by the more 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 individuals began their

[28] An alternative framework that can generate increased search effort alongside a drop in reservation wages is the model proposed by Abebe et al. (2025), which introduces a minimum admissible match quality and a direct cost of unemployment, leading to a discontinuity in the value function. While this is a compelling approach, we ultimately chose not to adopt it for our setting as the assumption that job seekers impose a strict lower bound on match quality is not directly supported by our data (even sector alignment after two years of sector-specific training was not a binding constraint). Additionally, while unemployment is undoubtedly costly, our job seekers systematically underestimate the risks of prolonged unemployment. This suggests that they do not internalize the dynamic costs of joblessness in a way that would generate the type of reservation wage adjustment emphasized in Abebe et al. (2025).

job searches after receiving training. Column 4 of Table 4 shows that treated students are more likely to initiate a job search. Despite the sharp decline in reservation wages, the overall impact on labor market participation is positive. This finding points to the importance of mentors' encouragement. Accordingly, we might explain the positive effect on the willingness to accept an unpaid job as follows: treated students received bad news and internalized it, as indicated by the decline in reservation wage. However, via encouragement to persevere, mentors raised the perceived future value of a low paying job today, helping the students adjust to the bad news without letting discouragement set in. According to our model, these findings suggest that benefits of encouragement on the cutoff search strategy (Proposition 3) outweigh harms described in Proposition 2.

We then test whether treated students improved their search skills following the mentorship sessions, which included substantial discussion of actionable search tips. To achieve this, we construct an index of search efficacy that measures the students' conversion rates during their searches (Column 5). 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 Columns 6 and 7, we rule out variations in two more aspects of searching: intensity, as measured by hours per day, days per week, number of applications submitted, and money spent on searching, and broadness, as measured by number of search and fundraise methods, geographical scope of search and number of sectors searched in.[29] In short, treated students do not seem to have searched any differently.

Finally, in Column 8, 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. Longstanding research has demonstrated that the unemployment exit rate falls as the duration of unemployment grows, due to behavioral changes among the unemployed — for example, because discouragement leads to less searching and thus a lower exit rate (Kaitz, 1970).

To conclude, following a shock to beliefs about the wage distribution and job ladder, treated students were no more likely to give up. Instead, they accept available jobs more quickly, accumulate practical experience, leverage human capital complementarities, build persistence, and eventually get retained (promoted) or transferred to a better job.

[29]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. Index components were pre-specified. Again, we observe no treatment effect on any index dimension or the overall index.

These figures highlight the relative importance of the information and encouragement, compared to the search tips and job referrals, and they suggest that search tips may be relatively ineffective. However, key questions remain: Did mentors provide valuable job referrals? Was the belief correction so influential in shaping the reservation wage response that it overshadowed other channels? Or were these channels not effectively activated? To address these questions, we first return to our comprehensive survey data. To gauge the relevance of job referrals, we asked treated students for each work activity whether they found it through a connection made by the mentor. While 7.4% reported receiving or being offered a referral by the alum, only 2.9% actually secured their first job through one of these referrals (half of which were direct hires by the alum). The results remain consistent if we exclude them from the analysis. Then, in section 7, we carry out two validations exploiting additional randomization features of the design and data we gathered on the students, their network, and the mentors.

7 Validations and Extensions

7.1 Mentor Heterogeneity

As a first validation, we investigate how students' assignment to different mentors, each of whom is capable of conveying a certain type of support better than others, affected their labor market outcomes. We begin by leveraging Empirical Bayes (EB) approaches to demonstrate the existence of mentor-level heterogeneity of interest. Then we employ an instrumental variable strategy (IV).

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 1. 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 .02 and .04 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 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).[30] Figure A.1 reports the distribution of the fixed effects as well as the shrunken posterior means for the coefficients, assuming a normal/normal model. While the original estimates are noisy, the posterior distribution shrinks toward the prior mean on the basis of the signal-to-noise ratio. The bias-corrected variance estimates we obtain are large. Specifically, .667 for the short run index and .672 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 .667 and the medium run index by .672 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 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 a set of three mediators to explain the observed heterogeneity in mentor effectiveness. These channels correspond directly to the three main types of support that emerged during student-mentor conversations, which in turn map onto the mechanisms proposed in our 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 represents the outcome of interest for student i, and we focus on the four standardized outcome indexes described earlier. $Info_i$, Enc_i and $Search_i$ are three indicator variables denoting whether the mentor primarily provided information on entry conditions, encouragement, or job search tips. We construct mentor types based on students' post-session rankings of conversation content, using a leave-one-out aggregation strategy informed by the peer effects literature (Caeyers and Fafchamps, 2024). After each session, students rank the main themes discussed. However, mentees shape discussions through their questions and engagement. As a result, observed associations between conversation content and student outcomes may reflect both mentor tendencies and student selection into conversation themes, rather than the causal effects of content itself. To address this concern, for each mentor, we aggregate the rankings provided by all other mentees to compute average salience across topics, excluding the focal student. This approach minimizes such reflection concerns and ensures that the mentor type used in second-stage regressions is not mechanically

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

correlated with any individual student's assessment.[31] Since mentor-student pairings were randomized within treatment arms, and mentors differ in their natural inclination to focus on information-sharing, encouragement, or job search advice, we use the 158 mentor indicators as instruments for conversation content. This allows us to test whether students assigned to mentors who emphasize particular types of support, independent of the students' own preferences or needs, experience different outcomes.

The first stage regressions are given by:

$$Info_i = \sum_j M_{ij}\gamma_{j1} + \lambda_{d(i),1} + \mu_i \tag{8}$$

$$Search_i = \sum_j M_{ij}\gamma_{j2} + \lambda_{d(i),2} + u_i \tag{9}$$

$$Enc_i = \sum_j M_{ij}\gamma_{j3} + \lambda_{d(i),3} + \tau_i \tag{10}$$

where M_{ij} is an indicator for whether student i was assigned to mentor j, and $\lambda_{d(i),k}$ denotes fixed effects for student i's dual VTI-course assignment $d(i)$ in each equation. The residuals μ_i, u_i, and τ_i capture individual-level unobserved variation. The second stage regression is:

$$Y_i = \beta_0 + \beta_1\widehat{Info}_i + \beta_2\widehat{Enc}_i + \beta_3\widehat{Search}_i + \lambda_{d(i)} + \epsilon_i \tag{11}$$

where $\widehat{Info}_i$, $\widehat{Enc}_i$, and $\widehat{Search}_i$ are the fitted values from the first-stage regressions, and $\lambda_{d(i)}$ again captures VTI-course fixed effects. The validity of this strategy relies on two assumptions. First, relevance of the instruments: the mentor indicators must be correlated with the three endogenous variables (conversation content). We formally test for weak identification using the Sanderson-Windmeijer first-stage F-statistic. As reported in Table 5, we reject the null hypothesis of weak instruments for all three endogenous regressors, (p=0.00 for all three), confirming sufficient variation even after partialling out the other two endogenous variables.

Second, the exclusion restriction: mentor assignment should only influence outcomes through the three identified channels. This assumption could be violated if unobserved conversation content, beyond these categories, affects outcomes. To test this, we leverage the overidentifi-

[31] We rule out the possibility that one mentee could substantially influence a mentor's conversation with another. Anecdotally, mentors reported preparing in advance and tailoring conversations to each student's background and needs. Two additional pieces of evidence support this: first, we find that more effective mentors tend to adapt their guidance across students; second, neither the content nor the effectiveness of sessions varies with the order in which mentees were matched or contacted (Figure A.3).

cation conditions (using 158 instruments to identify three endogenous variables) and conduct a Sargan-Hansen test. We fail to reject the null hypothesis, suggesting that our instruments capture the primary dimensions of heterogeneity in mentor effects. Given the large number of instrument at our disposal, it is sufficient for these other channels, were they to exist, to be uncorrelated with the fitted values of the second stage for our approach to still yield valid estimates (Kolesár et al. 2015).

We conduct a series of robustness checks, the results of which are reported in Online Appendix Table A.10. Panel A reports Two-Stage Least Squares estimates, our primary specification. In Panel B we implement Limited Information Maximum Likelihood, which is less sensitive to weak instruments and provides approximately median-unbiased estimates. Panel C reports Jackknife IV estimates (Blomquist and Dahlberg, 1999), a robustness check that mitigates overfitting by removing one observation at a time when constructing the fitted values in the first stage. JIVE ensures that a student's assigned mentor type is not influenced by their own outcomes, strengthening the validity of our estimates. Last, in panel D, we use Jackknife IV Estimation introduced by (Angrist et al., 1999) (denoted as UJIVE in the table), an alternative to the Jackknife IV that refines the first stage by adjusting for finite-sample bias, offering further protection against many weak instrument concerns. Across all specifications, our key findings remain robust: mentors who primarily provided information about entry-level jobs and encouragement were the most effective in the short run. In the medium run, encouragement plays an even greater role, reinforcing the idea that persistence and patience yield long-term career benefits.

7.2 The Cash Transfer

To understand whether simultaneously relaxing liquidity constraints can amplify the effects of the mentorship program, we unconditionally provided 40,000 UGX ($\sim$ \$12) to a random subset of MYF program participants. We recommended that they use the money for their job searches or to contact the mentors.[32] The additional cash transfer led to no differential impacts on short run labor market outcomes, search behavior, or willingness to accept a job (Tables A.8 and A.9).

Instead, it attenuated the effects at one year. Table 6 shows that students eligible for *only* MYF, and not the cash transfer, are reaping all the benefits of the mentorship. For this group, the treatment effects are strong and persist on both the earnings and participation margins at one year, when these students are 23% more likely to be in the labor force and

[32]While the take-up of the transfer was close to universal in T2, only half of the treated students reported having spent the funds after three months. Most of them saved it and spent it within endline 1 and endline 2, as confirmed by our direct observation of increased savings at endline 1, significantly higher in T2.

they are earning 31% more (p-val .01). Instead, the effects dissipate for students in T2 (MYF+Cash).

To investigate what caused these patterns, we examined differences in engagement as well as conversation content and students' takeaways. We ruled out any significant differences in the frequency, timing, engagement level, and duration of interactions between students assigned to MYF only (T1) and those assigned to MYF+Cash (T2). Instead, something we did not predict but found distinctly in both text data and, most importantly, in data on students' main takeaways was the difference in content. In particular, the cash transfer stimulated more discussions on actionable search tips, which were talked about more by mentor-student pairs in T2 and retained more by those students. But this ultimately crowded out exactly the kind of support that proved useful in the medium run: encouragement (Figure A.2).

7.3 Extensions

To inform the optimal design of mentorship programs, we explore how characteristics of the program design affected its success. We first examine the extent to which mentors tailor conversations to individual students and whether this impacts student outcomes. We construct two measures of mentor adaptivity, one based on text embeddings of conversation transcripts and one based on lexical variety. The first is an embedding-based metric that measures the average distance between a mentor's conversations. A larger average distance indicates greater diversity in content across different mentees. The second is a lexicon-based metric that quantifies linguistic variation using a generalized resemblance score inspired by Broder (1997). A lower resemblance score suggests greater diversity in word choice across conversations. More details on these measures are provided in Online Appendix F. Figure A.3 shows that students with more adaptive mentors tended to have slightly better labor market outcomes in the medium run (the strongest effects emerged for job retention and total earnings). Although these results are not always statistically significant, they are consistent with a broad literature showing that personalized advice giving is more effective in promoting behavioral change (De Vries et al., 2008; Celis-Morales et al., 2017).

We also examine heterogeneity in mentor effectiveness by analyzing variation across mentor-mentee pair characteristics. Across all main results, the observed patterns are largely consistent across subgroups, with no clear evidence that the effectiveness of mentorship varied meaningfully along most observable mentor-mentee characteristics (Figure A.3).

Next, we assess whether exposure to a mentor who has conducted multiple sessions differs from exposure to a mentor leading their first session (Figure A.3). We find no significant differences in student outcomes, likely because all mentors share the same training and labor market, making their guidance relevant even without prior mentoring experience. Moreover, our suggestive evidence on personalization indicates that mentors tailor their advice rather than recycling information between mentees. Last, we assess the impact of the number of mentees per mentor and find that a mentor's effectiveness decreases with each additional mentee beyond four.

8 Replicability and Cost Effectiveness

Our key goals in designing this intervention were replicability and cost-effectiveness, driven by interest from VTIs and the BRAC Youth Empowerment Program. Consequently, the intervention is straightforward and inexpensive to replicate. The main challenge is obtaining alumni contacts since VTIs typically do not track them. However, once a system is established, tracking costs are minimal. The mentor selection algorithm is easy to replicate, relying on accessible survey and administrative data. Table 7 presents IRR calculations for all students, assuming a 5% social discount rate and a 10-year duration for the treatment's income impact (a \$5.89 increase in monthly income). Panel A shows the cost breakdown per intended beneficiary. The total cost comprises: (1) program costs (i.e., the per capita cost for training, airtime, and mentor compensation), (2) students' opportunity cost, and (3) mentors' opportunity cost for extra interaction. The program cost per mentor is $\sim$ \$5 for a half-day training (including a snack, a face mask, hand sanitizer, stationery, and a venue); $\sim$ \$15 for airtime (equivalent to 70 hours of talking time); and $\sim$ \$40 to cover travel costs for mentors training and to thank them for conducting the mentoring sessions (conditional on having completed them all with their assigned mentees). Given that a mentor is connected to an average of 4.1 students, the cost per student is \$15. To calculate the opportunity costs for mentors and students, we use their baseline income levels. On average, participants dedicated 3.6 hours to the program. To be conservative in our estimates, we increased the time dedicated to the program to two days. For mentors, to avoid double counting, we account for the opportunity cost of the time spent on interactions beyond the three mandatory sessions included in the program's costs. The costs discussed exclude administrative expenses. However, institutionalizing the intervention at the school level will eliminate the need for enumerators, further reducing costs. While airtime and training costs are likely to endure, facilitation will be needed only for, at most, the first few years of the program. Because most mentors interacted with students well beyond the three required sessions, for which they were compensated, we predict that, once the mentorship program is institutionalized and early beneficiaries become ambassadors, monetary facilitations will not be necessary. Panel B shows the NPV of 10 years of earnings, highlighting the high benefits-cost ratio and IRR. Panel C reports the outcomes of various sensitivity analyses.

Even with shorter medium-run effects (five or two years), the IRR remains above 250%. The returns stay positive under extreme assumptions, reaching a positive minimum of 4% only with maximum opportunity costs. While this intervention is delivered to workers who have undergone two years of vocational training and we cannot ensure the same effects and therefore IRR would hold for unskilled workers, our results show that similar programs can enhance the effectiveness of vocational training.

9 Conclusions

Today, Africa is home to one out of every five first-time job seekers (United Nations, 2019; Bandiera et al., 2022). By 2050, that figure will be one out of three. The success of this job market shift will substantially affect the rate of development across the continent. Currently, with estimates of unemployment and underemployment as high as 60 percent in Africa, less than half of first-time job seekers are projected to find a permanent job and launch a career (African Development Bank, 2016).

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. Mentored students are 27% less likely to have left the labor force three months after graduating; they obtain their first jobs faster and are 15% more likely to work in their sector of training. These accelerated first jobs last longer, permitting the accumulation of human capital, and accelerate students' career progression. 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 labor market conditions at entry along with encouragement to persevere. Contrary to our expectations, neither direct job referrals nor the improvement of job seekers' search technology played a role. Instead, 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 resulted in lower reservations wages and a greater willingness to accept unpaid work. As a consequence, they rejected fewer job offers and started working 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 employment prospects and providing hope for improved future outcomes. Distorted beliefs are an important channel by which information frictions decrease earnings and career advancement. Our findings highlight the importance of balancing bad news with hope for an upward trajectory when addressing overly optimistic beliefs. Striking this balance is crucial to preventing *discouragement*, labor force withdrawal, and, particularly among skilled workers, the underutilization of human capital. Abstracting from the specific context of our experiment, our findings suggest a broader principle: when providing a reality check—whether in labor markets or in life—it is essential to balance immediate challenges with evidence of a possible upward trajectory. Even if not immediately observable, seeking and presenting data on long-term progress can help recalibrate expectations without leading to unintended consequences.

Main Tables

Table 1: Baseline Balance on Students' Characteristics and Labor Market Outcomes

Variable	N	(1) Control Mean/(SE)	N	(2) Treatment Mean/(SE)	(1)-(2) Pairwise t-test P-value
Panel A: Socio-economic characteristics					
Age in 2020	466	20.309	645	20.282	.825
		(.093)		(.080)	
Gender (1=M)	466	.590	645	.595	.861
		(.023)		(.019)	
Christian	466	.828	645	.839	.646
		(.017)		(.014)	
Single	462	.905	642	.886	.320
		(.014)		(.013)	
Has Children	466	.024	645	.023	.970
		(.007)		(.006)	
Region of Origin: Central	464	.297	643	.322	.384
		(.021)		(.018)	
Region of Origin: Eastern	464	.539	643	.513	.401
		(.023)		(.020)	
HH Assets Index Above Mean	458	.421	643	.373	.108
		(.023)		(.019)	
HH Main Income Source Agriculture	464	.474	645	.465	.767
		(.023)		(.020)	
Panel B: Labor market history pre MYF					
Ever Worked Pre MYF	466	.528	645	.535	.818
		(.023)		(.020)	
Ever Worked in Training Sector	441	.070	614	.085	.386
		(.012)		(.011)	
Has Done Any Casual Work	464	.256	645	.248	.751
		(.020)		(.017)	
Has Done Any Wage Employment	464	.293	645	.302	.740
		(.021)		(.018)	
Has Done Any Self Employment	464	.078	645	.085	.644
		(.012)		(.011)	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports means with standard errors in parentheses for the Control and Treatment groups. The last column shows the robust p-value for the t-test of equality of means between the two groups. Data is from the baseline and midline 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 held any contractual obligations toward the other, and the principal requested the agent on a need-based basis.

Table 2: ITT Estimates: Short Run Labor Market Outcomes

	Short Run					Index
	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment Assignment	-.057***	1.267**	.081***	1.700	19.227***	.150***
	(.019)	(.540)	(.026)	(2.176)	(4.872)	(.050)
	[.004]	[.008]	[.004]	[.078]	[.001]	[.004]
Control Mean	.21	16.15	.54	12.02	78.07	-.00
Treatment Effect (%)	-26.57	7.85	15.11	14.15	24.63	-
N	934	934	934	931	929	934

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 estimating equation 1. 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 and the balance variable *ever_worked*. Dependent variables: Column 1: indicator variable equal to 1 if individuals have neither engaged in any work activity nor looked for a job in the previous month. Column 2: number of days worked in previous month. Column 3: indicator variable equal to 1 if individuals are employed in their sector of training. Column 4: measure of total monthly earnings in the main work activity in the previous month (unconditional on employment). The top and bottom 1% of earnings value are top-coded at the 99th percentile. All monetary variables are converted into February 2022 USD. Column 5: duration in days of the first work spell after graduation. The Short Run Index is a standardized index of the five outcomes in Columns 1-5. We follow Anderson (2008) and account for the covariance structure in the components. Statistical significance throughout the paper is indicated by *, **, and *** for p-values of 0.10, 0.05, and 0.01, respectively.

Table 3: Labor Market Trajectory in the Medium Run

	Transitions		Medium Run			Index
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment Assignment	.041**	.076**	-.025	.265	5.889*	.135**
	(.019)	(.033)	(.022)	(.925)	(3.456)	(.057)
	[.072]	[.072]	[.154]	[.348]	[.074]	[.072]
Control Mean	.18	.37	.26	12.50	33.48	.00
Treatment Effect (%)	22.87	20.70	-9.53	2.12	17.59	-
N	934	934	923	923	916	844

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on labor market dynamics. These are obtained by estimating equation 1 as we described in the notes to Table 2. Dependent variable: Column 1: indicator equal to 1 if the respondent was retained after a trial period (students were usually hired as trainee in their first job after graduation). Column 2: 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. Column 3: indicator 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. Column 4: number of days worked in previous month. Column 5: measure of total monthly earnings in the main work activity in previous month (unconditional on employment). The top and bottom 1% of earnings value are top-coded at the 99th percentile. All monetary variables are converted into February 2022 USD. The Career Trajectory Index in Column 6 is a standardized index of the five outcomes in Columns 1-5. Again, we follow Anderson (2008).

Table 4: Mechanisms: Willingness to Accept a Job and Job Search Behavior

	Willingness to Accept a Job			**Job Search**				**Search Duration**	**Indexes**	
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Efficacy Index (5)	Search Broadness Index (6)	Search Intensity Index (7)	Search Duration | Searched (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment Assignment	-11.410***	.071**	-.045**	.029**	-.009	.002	-.086	-8.289**	-.029	.277***
	(3.307)	(.031)	(.022)	(.014)	(.068)	(.065)	(.078)	(3.930)	(.076)	(.080)
	[.003]	[.058]	[.058]	[.058]	[.625]	[.625]	[.182]	[.058]	[.542]	[.003]
Control Mean	36.22	.54	.18	.93	-.00	.00	.00	27.73	.00	-.00
Treatment Effect (%)	-31.50	13.09	-24.85	3.10	-	-	-	-29.89	-	-
N	737	739	890	934	934	934	934	885	934	668

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 estimating equation 1 as we described in the notes to Table 2. Dependent variables: Column 1: lowest wage the respondent is willing to accept. Column 2: self-reported willingness to accept an unpaid job. Column 3: indicator equal to 1 if the respondent has ever rejected a job offer during their first job search spell after graduation. Results are unchanged if we condition on having received an offer. Column 4: indicator equal to 1 if individuals have engaged in job search following their graduation. The Index of Search Efficacy in Column 5 is a standardized index of three components: (i) the ratio between number of interviews and applications; (ii) the ratio between offers received and applications submitted and (iii) number of CVs submitted during search. This index is only available for students who looked for a job. The Search Broadness Index in Column 6 is a standardized index of three components:(i) number of search methods; (ii) number of fundraise methods; (iii) geographical scope of search in measured travel time; (iv) number of sectors in which the student conducted job search. The Index of Search Intensity in Column 7 is a standardized index of four components: (i) hours per day and (ii) days per week spent searching (iii) number of applications submitted and (iv) savings devoted to job-search. Column 8: length of the first job search spell after graduation, conditional on having started a search. The Willingness to Accept Job Index in Column 10 is a standardized index of the outcomes in Columns 1-3. The Search Behavior Index in Column 9 is a standardized index of the outcomes in Columns 4-7. Again, we follow Anderson (2008).

Table 5: Treatment Effects by Mentor Types

	Mechanisms		Labor Market Outcomes	
	Search Behavior Index (1)	Willingness to Accept Job Index (2)	Short Run Index (3)	Career Trajectory Index (4)
Panel A — 2SLS				
Entry Conditions	-.09	.59***	.23***	.08
	(.10)	(.09)	(.08)	(.10)
Encouragement	-.04	.27***	.20***	.21***
	(.07)	(.09)	(.06)	(.07)
Search Tips	.05	.02	-.02	-.00
	(.13)	(.14)	(.08)	(.10)
Control Mean	.00	-.00	-.00	.00
N Mentors	158	158	158	157
N	934	668	934	844
F-Test of joint significance (pval)	.67	.00	.00	.01
AP Partial F (pval)- Entry Conditions	.00	.00	.00	.00
AP Partial F (pval)- Encouragement	.00	.00	.00	.00
AP Partial F (pval)- Search Tips	.00	.00	.00	.00
Overidentification test (pval)	.51	.64	.42	.40
Panel B — OLS				
Entry Conditions	-.07	.42***	.19**	.09
	(.10)	(.08)	(.08)	(.09)
Encouragement	-.03	.28***	.20***	.17***
	(.07)	(.09)	(.06)	(.06)
Search Tips	.03	.15	-.02	.01
	(.12)	(.13)	(.08)	(.09)
N	934	668	934	844
F-Test of joint significance (pval)	.83	.00	.02	.04

Notes: Panel A of the table presents the 2SLS estimates of the effect of mentor types based on the predominant form of support the students received from them: Entry Conditions, Encouragement, or Search Tips. The estimates are reported for our four key outcome indexes: (1) Search Behavior Index, (2) Willingness to Accept Job Index, (3) Short-Run Labor Market Outcomes Index, and (4) Career Trajectory Index. In the top panel, the coefficients represent the estimated effects, with standard errors reported in parentheses. These are obtained by estimating equation 11 on our four main indexes. The panel at the bottom reports control means and sample sizes, including the number of mentors (which is also the number of instruments) and total observations for each index, which vary as data are collected across waves. For certain variables, observations are required to be present in both waves. Joint significance tests and first-stage F-tests are included to verify instrument strength, while Hansen's test assesses instrument validity. Panel B of the table presents the OLS estimates of the effect of mentor types.

Table 6: Labor Market Trajectory in the Medium Run by Treatment Arm

	Transitions		**Medium Run**			**Index**
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
T1 (MYF)	.059**	.114**	-.059*	1.146	10.379**	.256***
	(.024)	(.042)	(.030)	(1.132)	(4.010)	(.074)
T2 (MYF+Cash)	.025	.041	.006	-.523	1.871	.028
	(.025)	(.042)	(.031)	(1.034)	(3.647)	(.071)
Control Mean	.18	.37	.26	12.50	33.48	.00
T1 Effect (%)	32.69	30.93	-22.79	9.17	31.00	-
T2 Effect (%)	13.57	11.01	2.32	-4.18	5.59	-
N	934	934	923	923	916	844
T1=T2	.28	.15	.13	.15	.02	.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 five outcomes for which there are significantly (or almost 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 and the balance variable *ever_worked*. For a detailed description of the outcomes, please refer to the notes to Tables 2, 3 and 4.

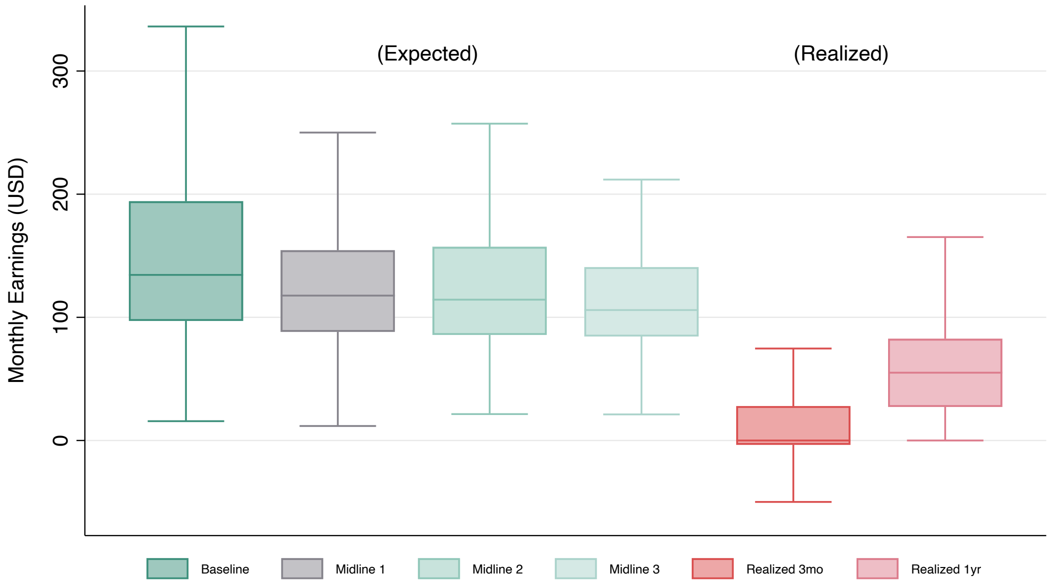
Table 7: IRR

	All students
Social discount rate	.05
Remaining expected productive life	10 years
Panel A. External parameters	
Total cost per individual	23.42
· Student opportunity cost (2 days of work)	4.99
· Alum opportunity cost (1 day of work, ext. interaction only)	3.43
· Program costs	15.00
Panel B. Estimated earning benefits	
Extra monthly earnings	5.89
NPV change in steady state earnings (from model estimates)	522.25
Benefits/cost ratio	23.30
IRR	2.90
Panel C. Sensitivity	
Sensitivity to different expected remaining productive life of beneficiaries	
Remaining expected productive life = 5 years	2.90
Remaining expected productive life = 2 years	2.63
Sensitivity to different earnings	
Opportunity costs = 90th percentile	2.10
Opportunity costs = max	0.22
Opportunity costs = double max	0.04
Sensitivity to different engagements	
5 days of work foregone	1.50
7 days of work foregone	1.14

Main Figures

Figure 1: Overoptimism

Panel A: Expected and Realized Monthly Earnings Conditional on Employment



Panel B: Expected and Realized Job Ladders

1 YEAR \ 3 MONTHS	Expected: Paid	Expected: Unpaid	Expected: Unemp	Realized: Paid	Realized: Unpaid	Realized: Unemp
Paid	55%	46%	45%	62%	54%	15%
Unpaid	20%	25%	35%	3%	6%	3%
Unemp	25%	29%	20%	36%	39%	82%

Notes: Panel A shows expected and realized monthly earnings conditional on employment, 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' realized monthly earnings at their first job as well as at one year. Data comes from the control group. Each plot shows the 10th, 25th, 50th, 75th, and 90th percentiles of realized/expected earnings distributions. The expected monthly earnings are calculated by taking the reported likelihood that earnings are above the midpoint of the minimum and maximum, and then fitting a triangular distribution. Panel B shows the expected and realized transition matrix from the three-month to the 1-year 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. These were collected from a comparable sample of first and second-year students from a later cohort, surveyed specifically to elicit these expectations. The one on the right contains the *realized* shares as computed in our sample (control group only).

Figure 2: Project Timeline

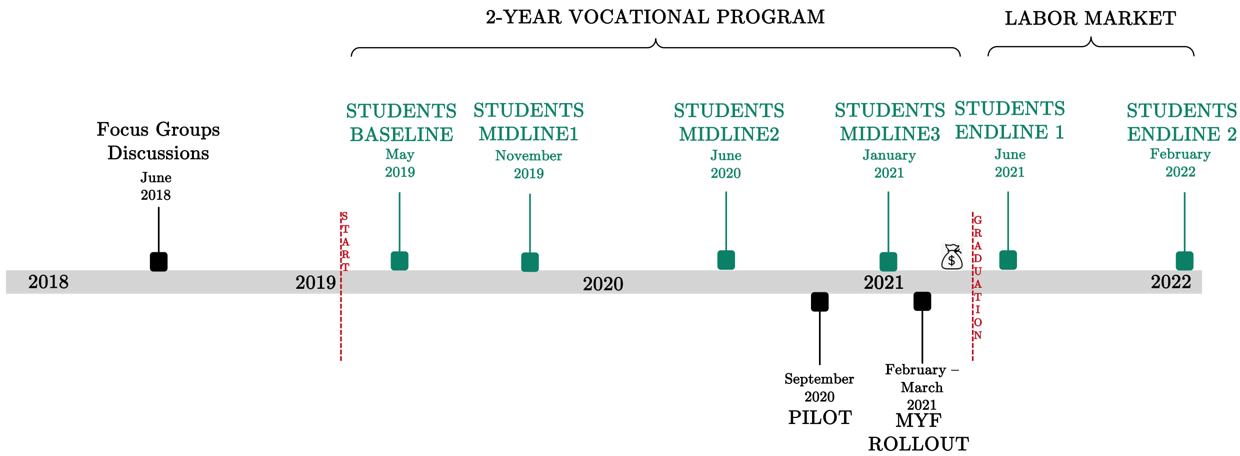


Figure 3: Experimental Design

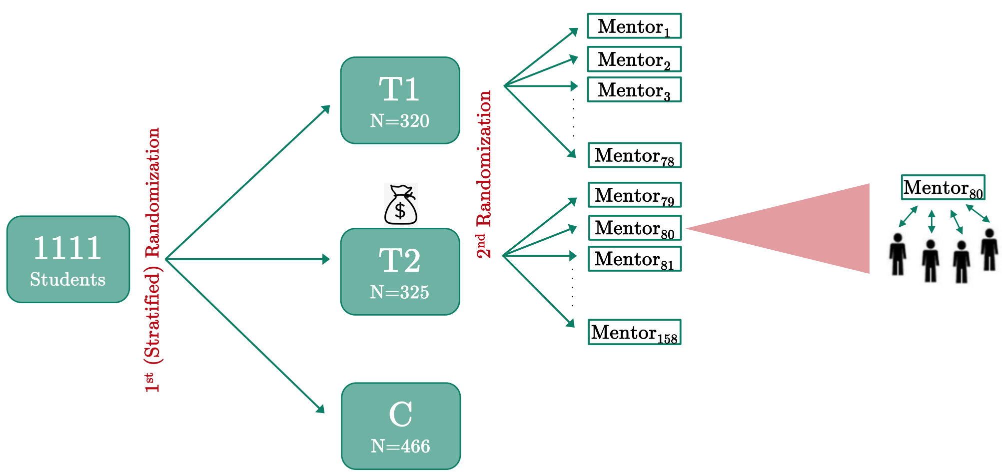
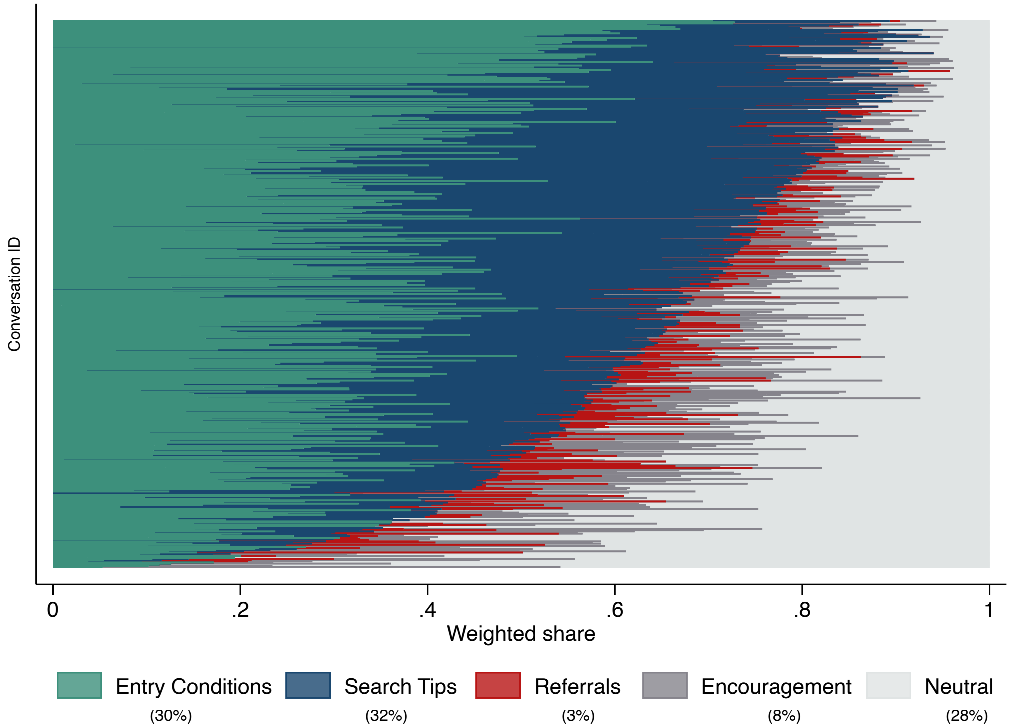
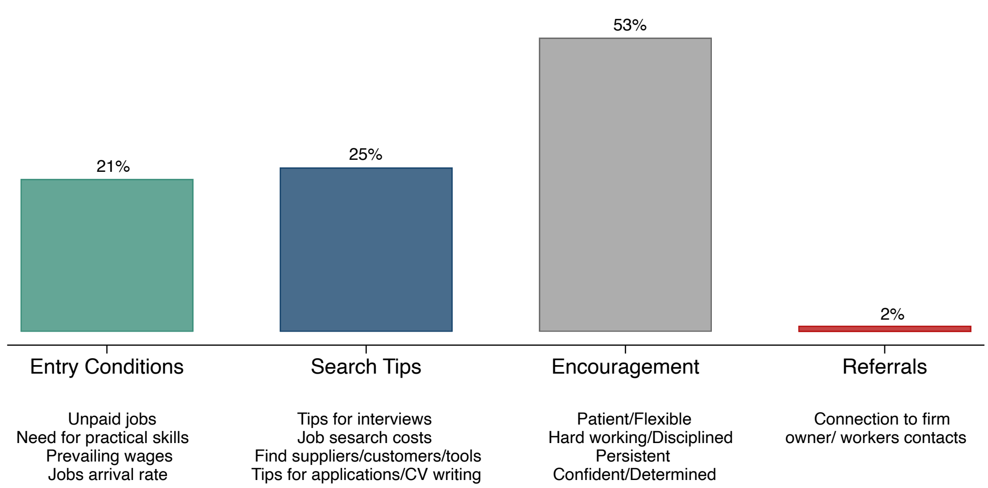


Figure 4: Conversations' Content and Takeaways

Panel A: Coded Conversation Content From Audio Recordings

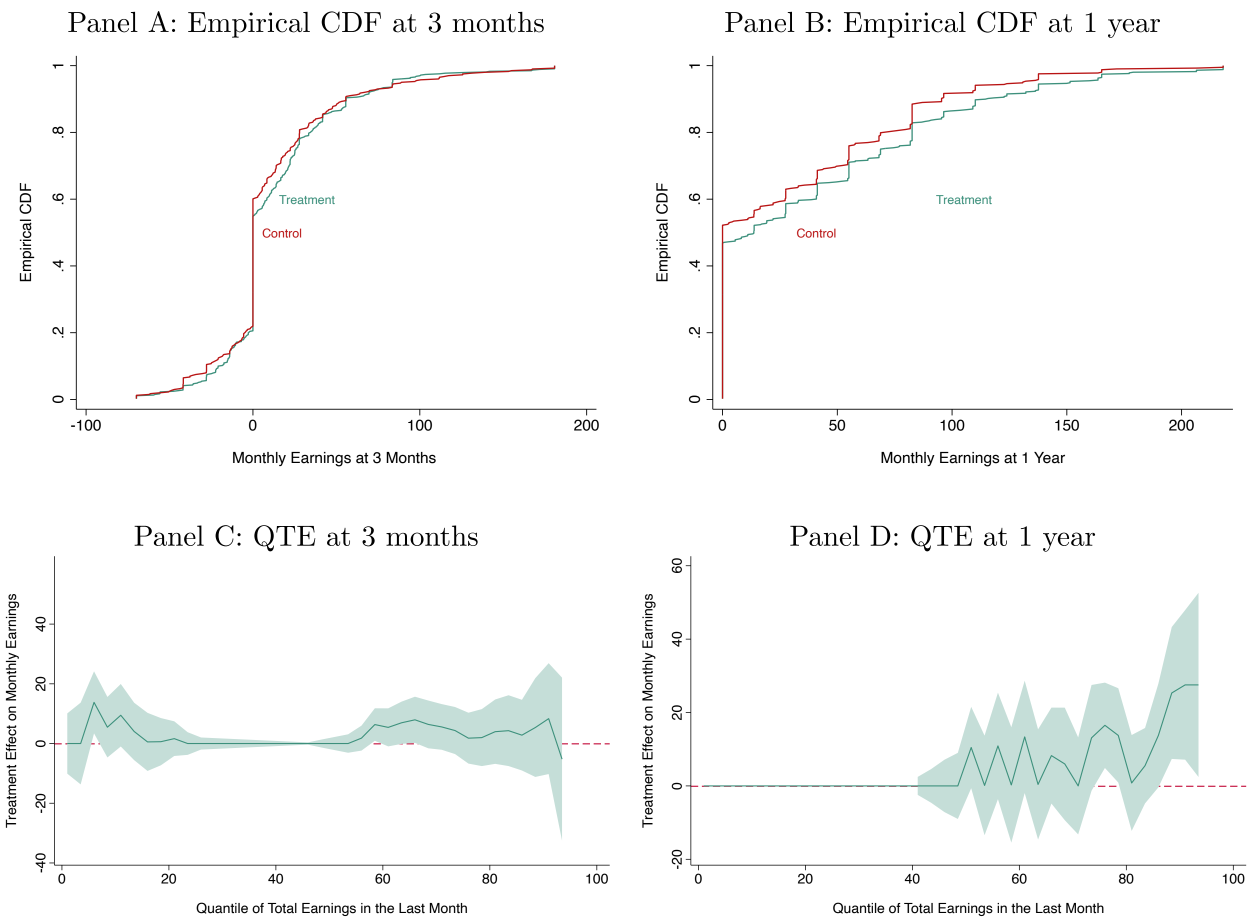


Panel B: Students' Main Takeaway



Notes: Panel A shows the raw conversation data from MS1: each observation represents a conversation, with sentences weighted by word count. Panel B displays the distribution of students' main takeaways from their mentor conversations. Each bar indicates the percentage of students whose takeaway falls into each macro-category, with the most common micro-topic listed below.

Figure 5: Treatment Effects on Monthly Earnings



Notes: Panel A and B show the empirical distributions of monthly earnings (unconditional on employment) in the MYF treatment and control groups at three months and at one year. 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. Panels C and D show the quantile treatment effects (QTEs) of the MYF treatment on monthly earnings F. 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 1 and endline 2. In Panel D, earnings below the 42nd percentile are zero (almost entirely unemployed).

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Appendix Tables and Figures

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

	Ever Connected (1)		Connected More Than Once (2)		Strong Link (3)	
Panel A - Dyad has same:						
Tribe	-0.14	(-0.43)	-0.12	(-0.38)	-0.24	(-1.36)
Language	-0.53	(-1.62)	-0.04	(-0.11)	-0.31	(-1.38)
District of origin	0.04	(0.14)	0.20	(0.68)	0.37**	(2.01)
VTI	0.74*	(1.86)	0.66*	(1.85)	0.35	(1.46)
Gender	-0.73	(-1.53)	-0.48	(-0.95)	-0.09	(-0.36)
Panel B - Sum of:						
Age	0.04	(0.90)	0.06*	(1.65)	0.03	(1.09)
Household Asset Index	-0.19**	(-2.03)	-0.09	(-0.98)	-0.05	(-0.77)
Panel C - Difference in:						
Age	-0.06	(-1.22)	-0.06	(-1.22)	-0.05*	(-1.68)
Household Asset Index	-0.15	(-0.91)	0.14	(0.86)	-0.08	(-0.77)
Observations	578		577		578	

Notes: This table reports estimates from Equation $SL_{ij} = \beta_0 + \beta_1|z_i - z_j| + \beta_2(z_i + z_j) + \gamma|w_{ij}| + u_j$ 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}. Standard errors are clustered at the mentor level.

Table A.2: 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 Takeup	-.023	-.024	.059*	.005	.015
	(.016)	(.030)	(.032)	(.024)	(.029)
Control Mean	.07	.25	.25	.13	.22
T Effect (%)	-31.15	-9.69	23.19	3.84	7.02
N	844	844	844	844	844

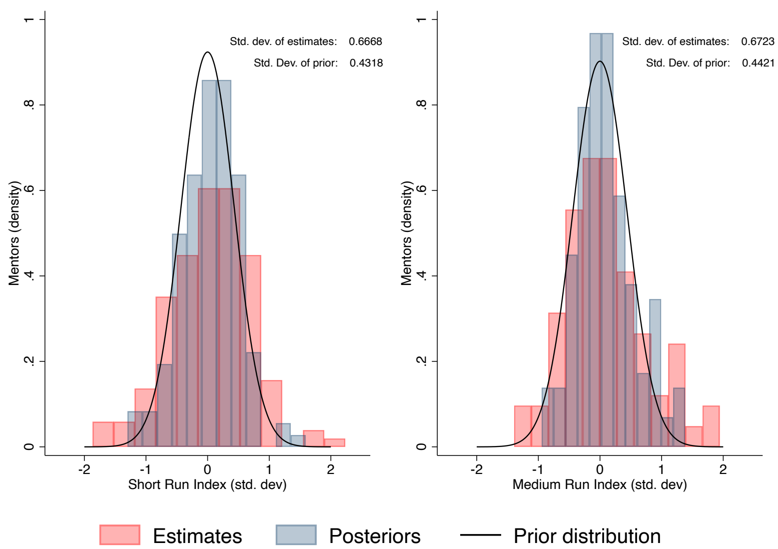
Notes: This table shows reduced-form estimates of the effects of MYF on various pathways to employment in year 1. There are nine possible pathways, although we only report those with a minimum of 5% of the total number of students (the treatment effects on the pathways we do not report are not statistically 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.

Table A.3: 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)	Started Job Search (4)	Search Duration | Searched (5)
MYF Treatment					
× Pre-MYF Expected Earnings Above Median	-23.177***	.139**	-.114	.045	-4.521
	(5.897)	(.060)	(.069)	(.028)	(6.398)
× Pre-MYF Expected Earnings Below Median	1.405	.023	-.057	.023	-6.052
	(3.084)	(.046)	(.048)	(.028)	(5.711)
Difference	-24.582	.116	-.057	.022	1.530
P-Value	.000	.131	.384	.538	.863
N	485	487	492	512	490

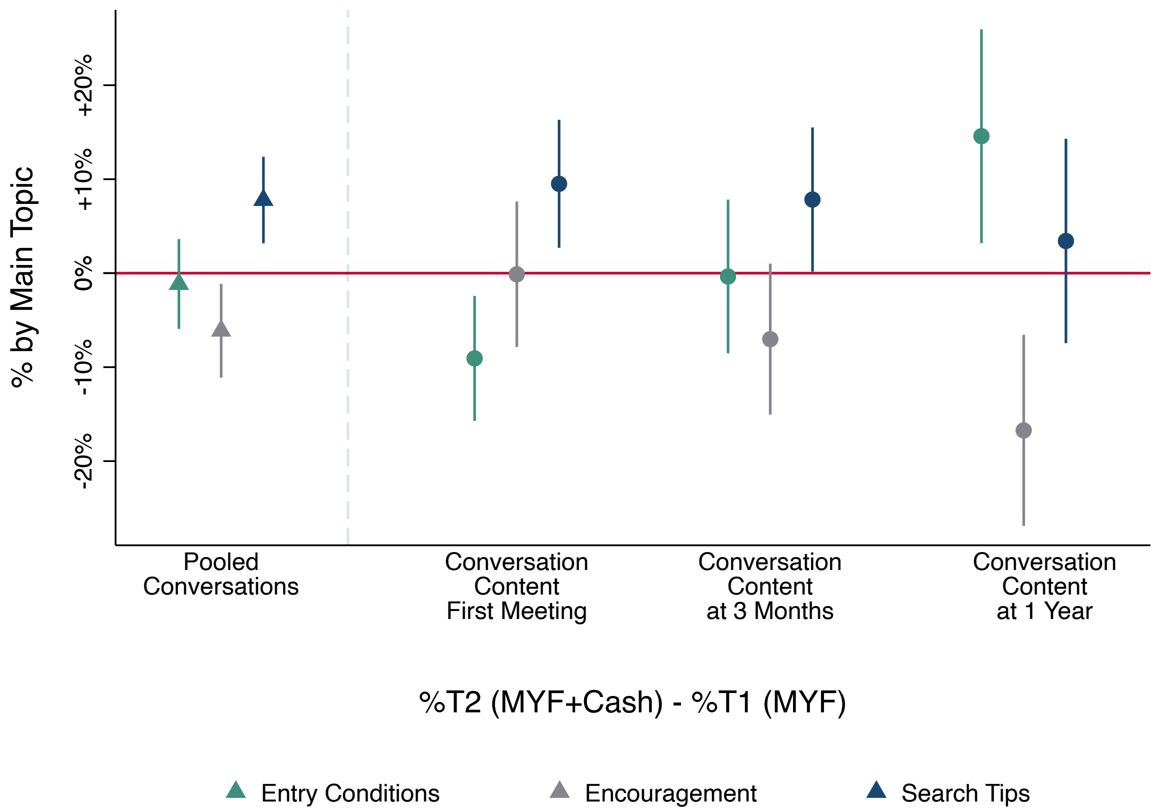
Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on willingness to accept a job and job search outcomes. We do so for the overall sample (in the top panel) and in two different samples: those with pre-MYF above median and those with below median expectations over their earnings prospect.

Figure A.1: Reduced Form Estimates: Biased and Unbiased Mentors Fixed Effects



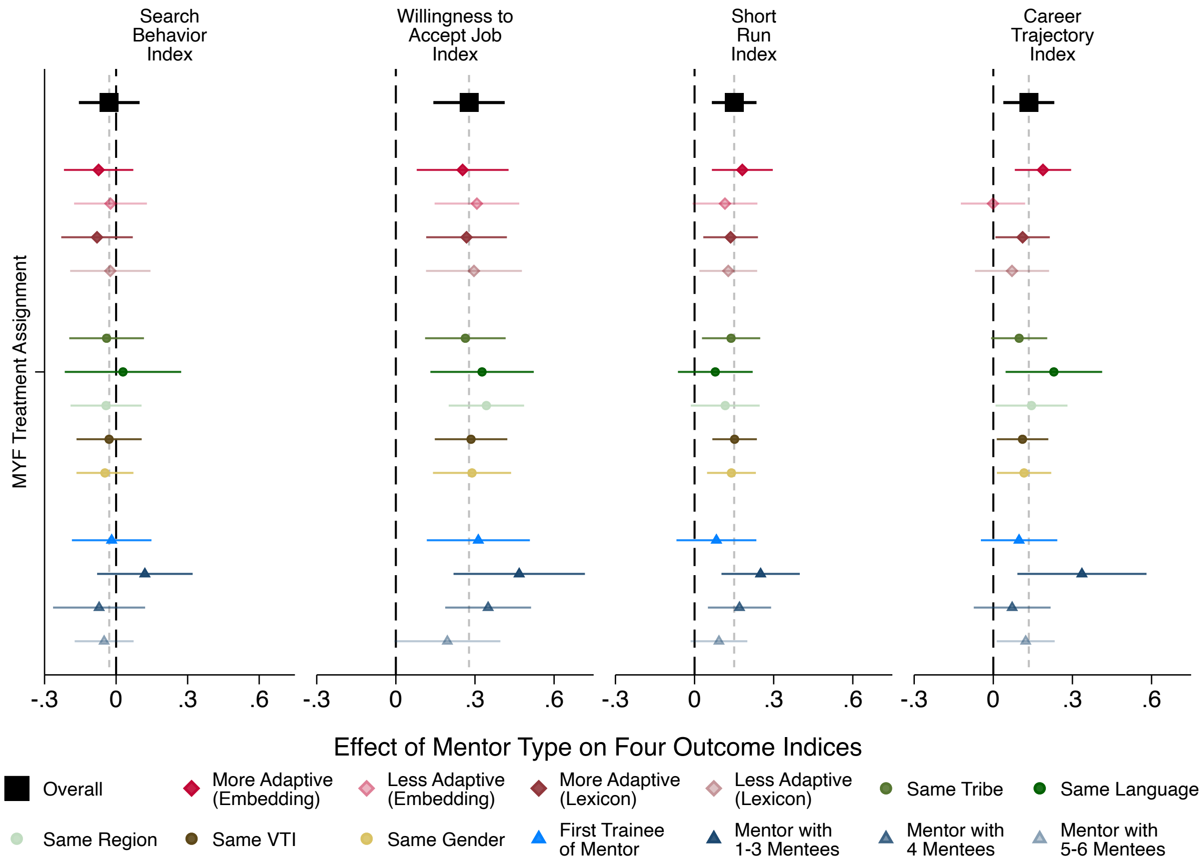
Notes: In this figure we report the biased (estimates) and unbiased (shrunk posteriors) distributions of the mentors fixed effects. We overlay the prior distribution, a normal centered on zero, with the bias-corrected standard deviation.

Figure A.2: Conversation Content by 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 mentoring session.

Figure A.3: Heterogeneity in Treatment Effect by Dyad Characteristics



Notes: In this figure we report heterogeneous ITT treatment effects on four main outcome indexes (Search Behavior Index, Willingness to Accept Job Index, Short-Run Index, and Career Trajectory Index) by mentor–trainee dyad characteristics. Dots depict point estimates, and bars represent 90 percent confidence intervals with standard errors clustered at the strata level. The first row reports the overall treatment effects on outcomes. The second and third rows report treatment effects for trainees paired with more adaptive mentors (see Section F.3 for details on adaptivity). Rows four to nine report treatment effects for trainees in dyads sharing characteristics such as tribe of origin, language, region of origin, etc. The last three rows report treatment effects for trainees paired with mentors who have 1–3 mentees, exactly 4 mentees, and 5 or more mentees, respectively. See Tables 2–4 for details on the construction of the outcome indexes.

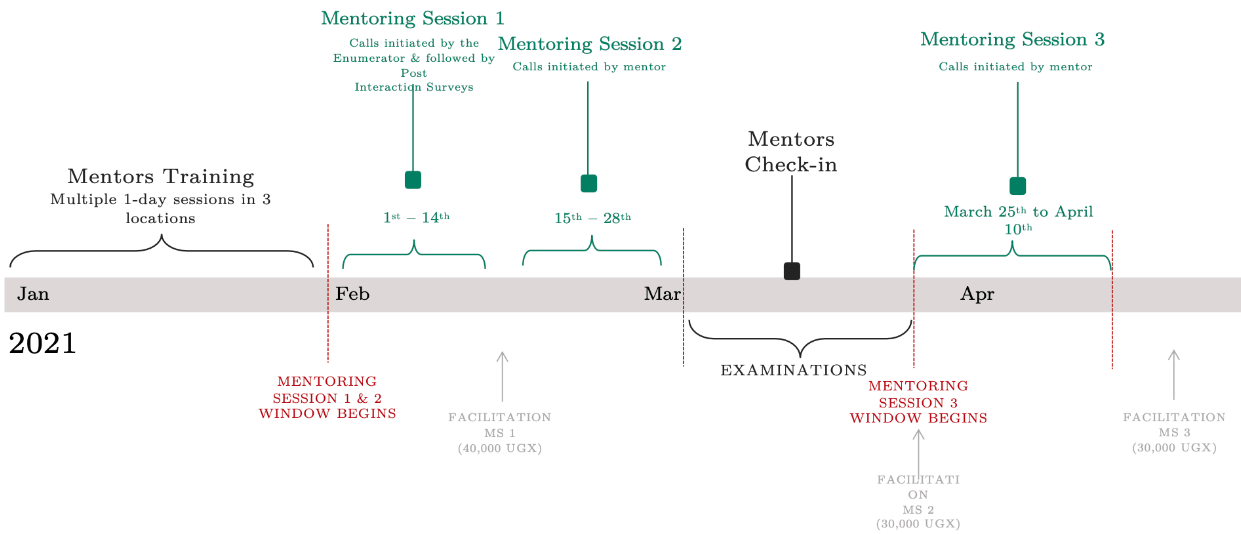
Meet Your Future: Experimental Evidence on the Labor Market Effects of Mentors

Material for Online Appendix

Contents

A Program Details

Figure A.4: The MYF Program in Detail



The mentor training sessions 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 Online Appendix Figure A.4. They were also given logbooks and instructed on how to fill them. During the Mentors' Check-in, we collected data on the content and duration of each mentorship session as well as information on additional interactions (whether they took place, who initiated them, duration, mode and content)

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. Of the 1,368 alumni for whom we found a registry entry, we successfully contacted and surveyed 714. We consider the tracking rate of 52% 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 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. To select the mentors we defined a set of rules to ensure the overall quality of the mentorship as well as to ensure replicability. After excluding 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:

- 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: indicator 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. 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 combinations of VTI-training areas for which we have fewer 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 mentors. Each mentor is assigned one to five treated students at random. Each mentor is assigned students belonging to the same treatment arm. When forming groups, we maximize the number of groups with three, four or five students per mentor.

Figure A.5: 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

LOGBOOK of AS[redacted]KA

KEY CALLS

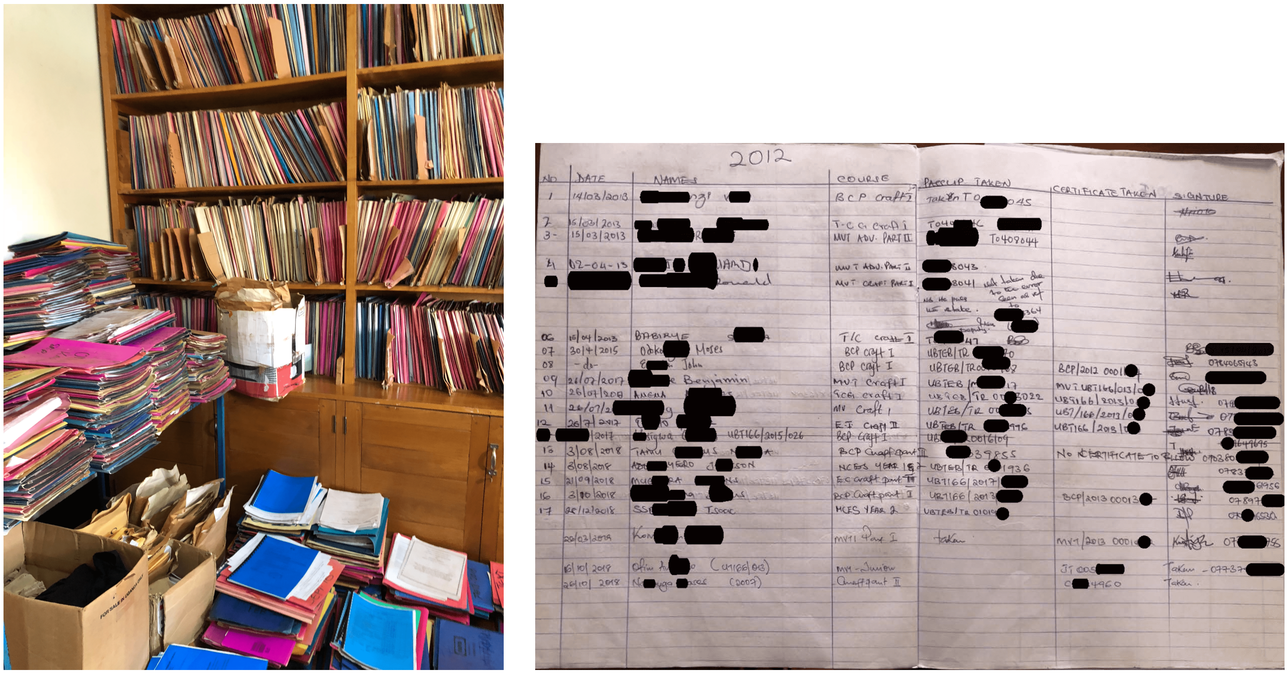
STUDENTS' NAMES and PHONE NUMBERS	KEY CALL 1	KEY CALL 2			KEY CALL 3		
	Date (day and month)	Date (day and month)	Duration (in minutes)	Three main topics of conversation	Date (day and month)	Duration (in minutes)	Three main topics of conversation
BRIAN [redacted]	8/ Feb 2021	2/March 2021	30	- How to go about studies - The Job market. - Field and Industrial training.	13th/April 2021	11 Mins	- Effective Job search. - Field & Industrial training. - General encouragement.
STEPHEN	9/Feb	8/March		- The Job market - How to find opportunities	7th/April	8 mins	- Personal experience. - Effective Job search.

Table A.4: Mentors' Characteristics

	Mean	SD
Panel A: Socio-Economic Characteristics		
Female	.23	.42
Age	25.05	3.17
Married	.42	.49
Has children	.49	.50
Traditional religious denomination	.71	.45
House of origin: rural	.43	.50
Region of origin: Central or Eastern	.84	.36
Caretaker's years of education	10.61	5.27
Panel B: Labor Market Characteristics		
Years in labor market	2.72	1.93
Wage employed	.69	.46
Self employed	.17	.38
Has permanent job	.75	.43
Works in / owns registered firm	.43	.50
Enrolled in further education	.04	.20
Involved in casual occupations	.02	.15
Works in area of training	.93	.26
Works in area of training (uncond. on working)	.93	.26
Ever worked in area of training	.08	.27
Panel C: Dyad Characteristics		
Same gender	.87	.33
Same training area	1.00	.00
Same VTI	.84	.37
Age Difference	5.03	3.13
Age gap within 6 years	.66	.47
Same district of origin	.49	.50

Notes: In this table, we report demographic and labor market characteristics of MYF mentors.

Figure A.6: Mentors Sample Construction - Records Digitization

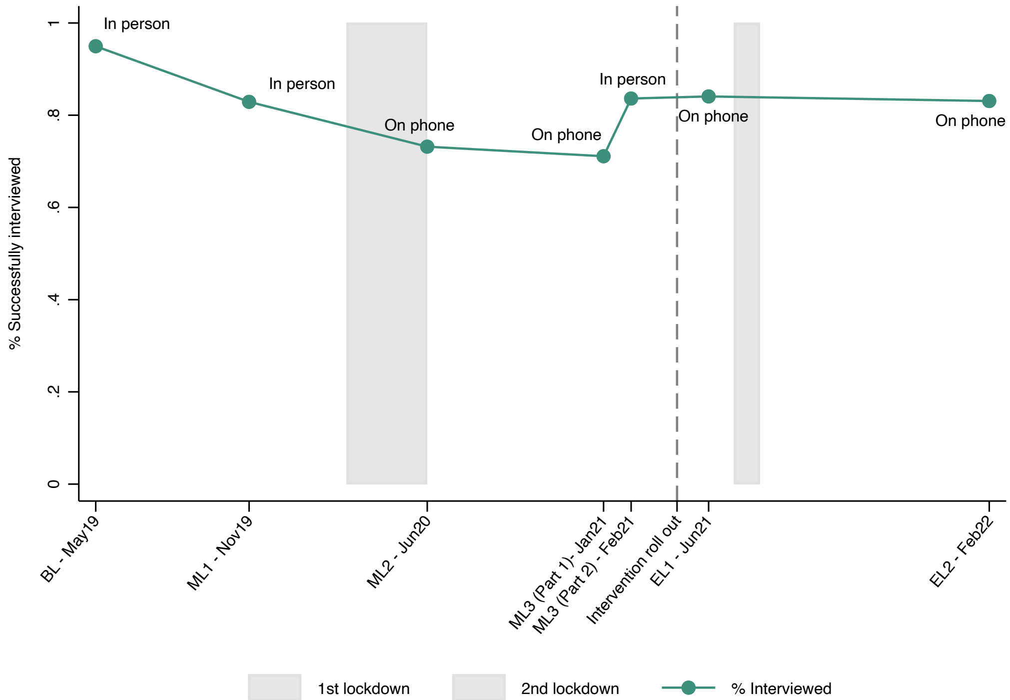


B Attrition and Compliance

Online Appendix Figure A.7 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 which time 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, making it more difficult to contact them. 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.7: Attrition



The overall attrition rate after the intervention was stable at approximately 7.6% with respect to the latest pre-intervention data collection and 8.7% with respect to baseline. In the first case, a 7.6% attrition rate is particularly low. In absolute numbers, this means that of the 1046 students surveyed in the third midline, 1013 students were successfully found after the intervention. In the latter case, the figure of 8.7% is low compared with the literature, where a review of 91 RCTs published in top economics journals (Ghanem et al., 2023) shows an average of 15%, and studies surveying youth (Bandiera et al., 2020) report 18%. 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, two years elapsed between baseline and the roll out of the intervention and three years between the baseline and the second endline. 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. Considering the constraints imposed by the pandemic and the necessity to conduct interviews over the phone, we find these attrition rates satisfactory.

Table A.5: Attrition

	Found in EL1			***Found in EL2***			***Ever found***		
MYF Treatment Assignment	-.005	-.008	.227	.015	.016	.066	.003	.001	.261
	(.021)	(.022)	(.221)	(.018)	(.018)	(.260)	(.014)	(.014)	(.178)
Gender (1=M)			.001			.248			.028
			(.100)			(.174)			(.074)
Age			.011			-.006			.003
			(.008)			(.010)			(.005)
HH Main Income Source Agriculture			.047			.078			.057*
			(.033)			(.053)			(.032)
Student Has a Scholarship			-.010			-.057			.003
			(.036)			(.046)			(.027)
HH Assets Index Above Mean			-.016			.028			.015
			(.037)			(.041)			(.030)
Ever Worked Pre MYF			.019			.054			.051
			(.037)			(.043)			(.034)
Treatment × Gender			.030			-.001			.012
			(.046)			(.047)			(.033)
Treatment × Age			-.013			-.000			-.011
			(.011)			(.013)			(.008)
Treatment × HH Main Income Source Agriculture			-.003			-.038			-.041
			(.051)			(.047)			(.045)
Treatment × Student Has a Scholarship			.025			.038			-.028
			(.057)			(.048)			(.046)
Treatment × HH Asset Index Above Mean			.008			-.080			-.058
			(.052)			(.064)			(.048)
Treatment × Ever Worked Pre MYF			.003			-.013			-.016
			(.055)			(.051)			(.038)
Constant	.843***	1.004***	.783***	.822***	.992***	1.077***	.910***	.999***	.879***
	(.029)	(.011)	(.163)	(.023)	(.009)	(.193)	(.021)	(.007)	(.110)
Control Mean	.84	.84	.84	.82	.82	.82	.91	.91	.91
R-squared	.00	.11	.12	.00	.09	.10	.00	.10	.11
N	1111	1111	1101	1111	1111	1101	1111	1111	1101
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Strata	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
F-statistc									
Characteristics			.71			.13			.09
Characteristics + Interactions			.41			.20			.86

Considering attrition at endline 1, Column 1 of Online Appendix Table A.5 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 report the p-value for the joint F-statistic on the characteristics and on the interactions, which are jointly insignificant (p-value of .71). The same holds, at 5% significance level, 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 agricultural household for the ever found dummy, at 10% significance level).

Lastly, Online Appendix Table A.6 presents a comprehensive set of balance checks based on students who complied with or were assigned to the treatment. We find no significant differences in baseline characteristics between compliers and non-compliers, except for a few cases: non-compliers were more likely to be female and to have a household asset index above the mean.

Table A.6: Attrition Analysis: Baseline Characteristics for Compliers and Non-Compliers

Variable	(1) Non-Compliers N	(1) Non-Compliers Mean/(SE)	(2) Compliers N	(2) Compliers Mean/(SE)	(1)-(2) Pairwise t-test P-value
Panel A: Socio-economic characteristics					
Age in 2020	56	19.963	589	20.312	.227
		(.279)		(.083)	
Gender (1=M)	56	.464	589	.608	.040**
		(.067)		(.020)	
Christian	56	.911	589	.832	.056*
		(.038)		(.015)	
Single	56	.804	586	.894	.098*
		(.054)		(.013)	
Has Children	56	.054	589	.020	.280
		(.030)		(.006)	
Region of Origin: Central	56	.518	587	.303	.002***
		(.067)		(.019)	
Region of Origin: Eastern	56	.411	587	.523	.104
		(.066)		(.021)	
HH Assets Index Above Mean	56	.500	587	.361	.047**
		(.067)		(.020)	
HH Main Income Source Agriculture	56	.393	589	.472	.249
		(.066)		(.021)	
Panel B: Labor market history pre MYF					
Ever Worked Pre MYF	56	.518	589	.537	.790
		(.067)		(.021)	
Ever Worked in Training Sector	51	.078	563	.085	.863
		(.038)		(.012)	
Has Done Any Casual Work	56	.321	589	.241	.217
		(.063)		(.018)	
Has Done Any Wage Employment	56	.179	589	.314	.014**
		(.052)		(.019)	
Has Done Any Self Employment	56	.089	589	.085	.912
		(.038)		(.011)	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C 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.[33] 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 look at correlates of pre-VTI employment in our student sample while for the alumni we look at correlates of their labor market outcomes. We stratify along the following four dimensions and obtain a total of 5×2×2×2=40 strata:

Table A.7: Strata Variables

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
Smartphone	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

The randomization procedure was replicated until balance was achieved on a predetermined characteristic we expected to be highly correlated with the outcomes of interest: a dummy variable indicating whether the student had ever worked (either before beginning the course or during the lockdown). The procedure determining whether the randomization should be replicated was defined ex-ante and detailed in the pre-analysis plan. In practice, balance was achieved after one randomization, thus no replication of the randomization was necessary.

[33] 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.

D Model Derivations

Our model builds on the framework developed by Cortés et al. (2023) but differs in key ways to better suit our setting. We simplify Cortés et al. (2023) in three key ways: (1) we do not fully model belief evolution: their model, designed to interpret gender differences in job search behavior over time, includes a parameter governing the speed of convergence to the true mean. In principle, we could have incorporated a similar dynamic and framed the mentor intervention as a shock that accelerates belief convergence, but doing so would not have changed the model's core implications, so we opted for a simplification; (2) we do not model job search before graduation, since our experimental design ensures identical pre-treatment behavior between groups; and (3) we exclude individual risk aversion parameters, as we observe no systematic differences in risk preferences before or after treatment. We expand their framework by introducing the expected experience premiu,ω, which enables us to model how job seekers value career progression in our experimental setting.

In this section we provide proofs of three statements, which lie behind Propositions 1, 2 and 3 outlined in section 6.2 as well as we derive the expression for c^*.

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

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

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

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

$$c^*(\hat{\mu}, \hat{\omega}) = \beta\lambda \int_{w_R(\hat{\mu}, \hat{\omega})}^{\infty} [W(w, \hat{\omega}) - U(\hat{\mu}, \hat{\omega})] \, dF(w; \hat{\mu}, \hat{\omega})$$

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

To prove Proposition 1, we need to show that an increase in λ (the job offer arrival rate) leads to an increase in the reservation wage (w_R) and the cutoff search strategy ($c^*(\hat{\mu}, \hat{\omega})$).

The value of unemployment $U(\hat{\mu}, \hat{\omega})$ for someone with beliefs $\hat{\mu}$ and $\hat{\omega}$ is given by:

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

To find the effect of λ on $U(\hat{\mu},\hat{\omega})$, we differentiate $U(\hat{\mu},\hat{\omega})$ with respect to λ:

$$\frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \lambda} = \frac{\partial}{\partial \lambda}\left[b + \beta U(\hat{\mu},\hat{\omega}) + \int_0^{c^*(\hat{\mu},\hat{\omega})} H(c)\,dc + H(c^*(\hat{\mu},\hat{\omega}))c^*(\hat{\mu},\hat{\omega}) - \int_0^{c^*(\hat{\mu},\hat{\omega})} c\,dH(c)\right]$$

Simplifying and isolating the terms involving λ:

$$\frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \lambda}(1-\beta) = \left[h(c^*(\hat{\mu},\hat{\omega}))\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \lambda}c^*(\hat{\mu},\hat{\omega}) + H(c^*(\hat{\mu},\hat{\omega}))\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \lambda}\right] - \left[c^*(\hat{\mu},\hat{\omega})h(c^*(\hat{\mu},\hat{\omega}))\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \lambda}\right]$$

Since the last term cancels out:

$$\frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \lambda} = \frac{H(c^*(\hat{\mu},\hat{\omega}))\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \lambda}}{1-\beta}$$

The cutoff search strategy $c^*(\hat{\mu},\hat{\omega})$ is given by:

$$c^*(\hat{\mu},\hat{\omega}) = \beta\lambda \int_{w_R(\hat{\mu},\hat{\omega})}^{\infty} [W(w,\hat{\omega}) - U(\hat{\mu},\hat{\omega})]\,dF(w;\hat{\mu},\hat{\omega})$$

Differentiating this with respect to λ, since β, λ, and the integrand are positive it follows that:

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

Therefore, an increase in λ leads to an increase in the cutoff search strategy $c^*(\hat{\mu},\hat{\omega})$.

The reservation wage $w_R(\hat{\mu},\hat{\omega})$ is defined by the condition:

$$W(w_R(\hat{\mu},\hat{\omega}),\hat{\omega}) = U(\hat{\mu},\hat{\omega})$$

Given that $U(\hat{\mu},\hat{\omega})$ increases with λ as shown above, the reservation wage must adjust to maintain the equality. Specifically:

$$\frac{\partial W(w_R(\hat{\mu},\hat{\omega}),\hat{\omega})}{\partial \lambda} = \frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \lambda}$$

Since $W(w_R(\hat{\mu},\hat{\omega}),\hat{\omega})$ is increasing in w_R, and $\frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \lambda} > 0$, it follows that:

$$\frac{\partial w_R(\hat{\mu},\hat{\omega})}{\partial \lambda} > 0$$

Proof of Proposition 2

To prove Proposition 2 we follow the steps Cortés et al. (2023) 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:[34]

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

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

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

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:

$$\begin{aligned}\frac{\partial U(\hat{\mu})}{\partial \hat{\mu}} &= \frac{\beta\lambda H\left(c^*(\hat{\mu})\right)\left\{\int_{\hat{w}(\hat{\mu})}[W(w,\hat{\mu}) - U(\hat{\mu})]\frac{\partial f(w;\hat{\mu})}{\partial \hat{\mu}}dw\right\}}{\left(1 - \beta\left(1 - \lambda H\left(c^*(\hat{\mu})\right)[1 - F(\hat{w}(\hat{\mu}))]\right)\right)}\Bigg] \\ &= \frac{\beta\lambda H\left(c^*(\hat{\mu})\right)\left\{\int_{\hat{w}(\hat{\mu})}\left\{[W(w,\hat{\mu}) - U(\hat{\mu})](w-\hat{\mu})f(w;\hat{\mu})\right\}dw\right\}}{\left(1 - \beta\left(1 - \lambda H\left(c^*(\hat{\mu})\right)[1 - F(\hat{w}(\hat{\mu}))]\right)\right)} > 0.\end{aligned} \tag{12}$$

[34]For the remainder of this proof, we omit $\hat{\omega}$ for easing notation. Throughout, we use lowercase letters to denote the derivative of an uppercase function.

Differentiating the function implicitly defining the reservation wage gives:

$$\frac{\partial W\left(\hat{\omega}(\hat{\mu})\right)}{\partial w}\frac{\partial \hat{\omega}(\hat{\mu})}{\partial \hat{\mu}} = \frac{\partial U(\hat{\mu})}{\partial \hat{\mu}}.$$

Since the right-hand side is positive and

$$\frac{\partial W\left(\hat{\omega}(\hat{\mu})\right)}{\partial w} > 0, \quad \frac{\partial \hat{\omega}(\hat{\mu})}{\partial \hat{\mu}} > 0,$$

From 4, it follows that

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

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

To find the effect of $\hat{\omega}$ on $U(\hat{\mu},\hat{\omega})$, we differentiate $U(\hat{\mu},\hat{\omega})$ with respect to $\hat{\omega}$:

$$\frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} = \frac{\partial}{\partial \hat{\omega}}\left[b + \beta U(\hat{\mu},\hat{\omega}) + \int_0^{c^*(\hat{\mu},\hat{\omega})} H(c)\,dc + H(c^*(\hat{\mu},\hat{\omega}))c^*(\hat{\mu},\hat{\omega}) - \int_0^{c^*(\hat{\mu},\hat{\omega})} c\,dH(c)\right]$$

Simplifying and isolating the terms involving $\hat{\omega}$:

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

Since the last term cancels out:

$$\frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} = \frac{H(c^*(\hat{\mu},\hat{\omega}))\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}}}{1-\beta}$$

Differentiating now the cutoff search strategy with respect to $\hat{\omega}$:

$$\frac{\partial c^*(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} = \beta\lambda \int_{w_R(\hat{\mu},\hat{\omega})}^{\infty} \frac{\partial}{\partial \hat{\omega}}\left[W(w,\hat{\omega}) - U(\hat{\mu},\hat{\omega})\right] dF(w;\hat{\mu},\hat{\omega})$$

The value of employment $W(w,\hat{\omega})$ is given by:

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

Differentiating this with respect to $\hat{\omega}$:

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

Which is positive. Therefore, an increase in $\hat{\omega}$ leads to an increase in the cutoff search strategy $c^*(\hat{\mu},\hat{\omega})$.

The reservation wage $w_R(\hat{\mu},\hat{\omega})$ is defined by the condition:

$$W(w_R(\hat{\mu},\hat{\omega}),\hat{\omega}) = U(\hat{\mu},\hat{\omega})$$

Given that $U(\hat{\mu},\hat{\omega})$ increases with $\hat{\omega}$ as shown above, the reservation wage must adjust to maintain the equality. Specifically:

$$\frac{\partial W(w_R(\hat{\mu},\hat{\omega}),\hat{\omega})}{\partial \hat{\omega}} = \frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}}$$

Since $W(w_R(\hat{\mu},\hat{\omega}),\hat{\omega})$ is increasing in w_R, and $\frac{\partial U(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} > 0$, it follows that:

$$\frac{\partial w_R(\hat{\mu},\hat{\omega})}{\partial \hat{\omega}} > 0$$

E Additional Results and Extensions

Table A.8: ITT Estimates: Short Run Labor Market Outcomes by Treatment Arm

	Short Run					Index
	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
T1 (MYF)	-.049**	1.544**	.081**	2.617	18.374***	.162**
	(.022)	(.647)	(.037)	(2.637)	(6.686)	(.066)
T2 (MYF+Cash)	-.064**	1.004	.082***	.824	20.042***	.139**
	(.025)	(.628)	(.025)	(2.517)	(7.187)	(.055)
Control Mean	.21	16.15	.54	12.02	78.07	-.00
T1 Effect (%)	-22.90	9.56	15.00	21.78	23.53	-
T2 Effect (%)	-30.04	6.22	15.22	6.86	25.67	-
N	934	934	934	931	929	934
T1=T2	.59	.43	.97	.52	.87	.75

Notes: In this table, we report the intent-to-treat estimates of the effects of MYF and MYF + Cash on short run labor market outcomes. At the foot of each column, we 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, and the balance variable *ever_worked*. Outcomes are described in the notes to Table 2.

Table A.9: ITT Estimates: Willingness to Accept a Job and Search Behavior by Treatment Arm

	Willingness to Accept a Job			Job Search				Search Duration	Indexes	
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Efficacy Index (5)	Search Broadness Index (6)	Search Intensity Index (7)	Search Duration | Searched (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
T1 (MYF)	-13.218***	.077**	-.014	.031*	.008	-.075	-.062	-10.778**	-.041	.252**
	(3.828)	(.037)	(.024)	(.016)	(.080)	(.069)	(.090)	(4.550)	(.090)	(.097)
T2 (MYF+Cash)	-9.597***	.065*	-.075**	.027	-.024	.075	-.109	-5.928	-.017	.302***
	(3.535)	(.038)	(.029)	(.016)	(.077)	(.081)	(.075)	(4.137)	(.072)	(.077)
Control Mean	36.22	.54	.18	.93	-.00	.00	.00	27.73	.00	-.00
T1 Effect (%)	-36.50	14.15	-7.93	3.37	-	-	-	-38.87	-	-
T2 Effect (%)	-26.50	12.03	-41.02	2.86	-	-	-	-21.38	-	-
N	737	739	890	934	934	934	934	885	934	668
T1=T2	.27	.79	.06	.74	.69	.06	.40	.20	.68	.48

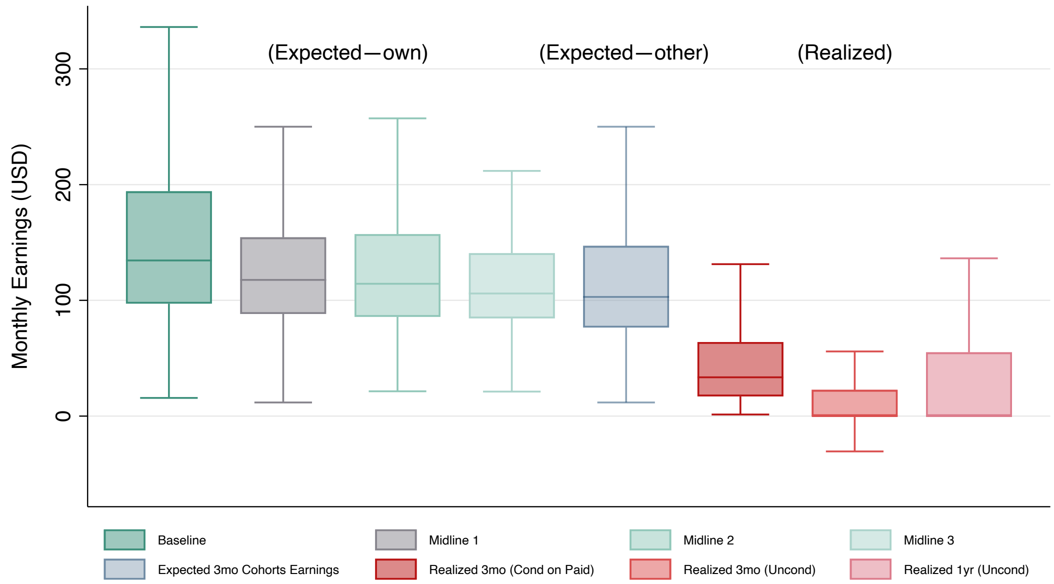
Notes: In this table, we report the intent-to-treat estimates of the effects of MYF and MYF + Cash on short run labor market outcomes. At the foot of each column, we report the p-value from an F-test of the null hypothesis that the impact of MYF alone is equal to the impact of MYF + Cash. All regressions control for strata dummies, and the balance variable *ever_worked*. Outcomes are described in the notes to Table 4.

Table A.10: Treatment Effects by Mentor Types

	Mechanisms		Labor Market Outcomes	
	Search Behavior Index	Willingness to Accept Job Index	Short Run Index	Career Trajectory Index
	(1)	(2)	(3)	(4)
Panel A — 2SLS				
Entry Conditions	-.09	.59***	.23***	.08
	(.10)	(.09)	(.08)	(.10)
Encouragement	-.04	.27***	.20***	.21***
	(.07)	(.09)	(.06)	(.07)
Search Tips	.05	.02	-.02	-.00
	(.13)	(.14)	(.08)	(.10)
Control Mean	.00	-.00	-.00	.00
N Mentors	158	158	158	157
N	934	668	934	844
F-Test of joint significance (pval)	.67	.00	.00	.01
AP Partial F (pval)- Entry Conditions	.00	.00	.00	.00
AP Partial F (pval)- Encouragement	.00	.00	.00	.00
AP Partial F (pval)- Search Tips	.00	.00	.00	.00
Overidentification test (pval)	.51	.64	.42	.40
Panel B — LIML				
Entry Conditions	-.10	.67***	.24***	.07
	(.11)	(.10)	(.09)	(.11)
Encouragement	-.05	.27***	.20***	.22***
	(.07)	(.10)	(.06)	(.07)
Search Tips	.06	-.06	-.02	-.01
	(.14)	(.16)	(.08)	(.10)
N	934	668	934	844
Panel C — JIVE				
Entry Conditions	-.06	.56***	.23**	.09
	(.09)	(.10)	(.09)	(.10)
Encouragement	-.05	.23***	.18**	.22***
	(.07)	(.08)	(.07)	(.09)
Search Tips	.01	.07	.05	.01
	(.09)	(.12)	(.10)	(.11)
N	934	668	934	844
Panel D — UJIVE				
Entry Conditions	-.08	.76***	.28**	.11
	(.12)	(.14)	(.12)	(.15)
Encouragement	-.06	.28**	.19**	.26***
	(.08)	(.11)	(.08)	(.10)
Search Tips	.03	-.09	.00	-.04
	(.12)	(.17)	(.13)	(.15)
N	934	668	934	844

Notes: For 2SLS and LIML we cluster errors at Strata level. For JIVE and UJIVE we report robust errors.

Figure A.8: Overoptimism, Earnings Conditional on Paid Work and Unconditional



Notes: 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 (exactly as in Figure 1). In the fifth plot, we report students' expected monthly earnings for their cohort. The sixth and seventh plots represent students' realized monthly earnings at their first job conditional on the first job being paid, and unconditional on having ever found a first job. The eight plot shows unconditional realized monthly earnings at one year. Data comes from the control group. Each plot shows the lower adjacent value, 25th, 50th, 75th percentiles, and upper adjacent value of the earnings distributions. Adjacent values are 25th percentile minus 1.5 times the interquartile range or 75th percentile plus 1.5 times the interquartile range.

F Text Data Processing and Classification

F.1 Topic Classification

To perform topic analysis and discern the content of these conversations, we utilize the state-of-the-art Claude 3.7 Sonnet (`claude-3-7-sonnet-20250219`) model developed by Anthropic to label the topic of each sentence within a conversation. Although some researchers argue that fine-tuned transformer models can outperform generative large language models like Claude, we refrain from supervised learning, due to our relatively small corpus and to avoid subjective input during training data labeling. To utilize the model, we define the categories— Information about job-market, Strategies, Encouragement, Referral, and Neutral— based on content from the mentors' training and questions from the Post Intervention survey:

We use the following prompt:

You are tasked with classifying each sentence in a conversation between a youth and a mentor. The youth is a vocational student during their school-to-work transitions in urban Uganda. The mentor is a recent graduate of the same vocational school and course of study, with substantive experience in the labor market and pertinent information on local labor market conditions. Each sentence is numbered for reference.

Conversation:

[1] Mentor: "Hello, how are you?"

[2] Student: "I am fine."

[3] Mentor: "May I have your number, please?"

[4] Student: "It's 070-865-6885."

[copy the rest of the conversation here]

Categories:

1. Information about the job market

2. Strategies to navigate the job market

3. Encouragement and psychological support

4. Referral of job opportunities

5. Anything that does not fall under the aforementioned 4 categories

1. Job Market and Workplace Information and Experience. Examples include but not limited to: Time to find a job; Prevailing wages; Possibility of finding a job/working; Commonality of unpaid jobs or paying for training; Practical skills for job finding; Types of positions/contractual arrangements; Women's conditions, discrimination, and rights in the workplace; Time to start a business from scratch; Profits for a new business; Mentor's current and past jobs/occupations; Mentor wages/profits; Time for mentor to find jobs; Mentor's wage/profits.

2. Tips for Job Search, Workplace Behavior, and Career Development. Examples include but not limited to: Job search strategies; Setting up a business; Recommended job/business locations; Interviewing skills; Number of job to apply; CV writing; Job application materials; Negotiation of job offers; Job application costs; Transportation costs during job search; Resources for starting a business; Networking with providers and clients; Finding basic tools/equipment; Business accounting; Obtaining loans for business/tools; Time management during job search; Mentor's own experience navigating the job market and work place; Strategies the mentor eSmployed during their job search / early career.

3. Encouragement and Motivation. Examples include but not limited to: Encouragement to persist and focus on goals; Confidence building; Support for optimism about the future; Uplifting students' spirits.

4. Referrals. Examples include but not limited to: Referrals or promises of job opportunities, internships, potential employers, and clients.

5. Other Topics. Examples include but not limited to: Anything that does not fall under the aforementioned 4 categories; General greetings, introductions, and contact exchanges.

Instructions:

- **Your task is to classify each sentence into one of the 5 categories above.**

- **Consider the context of each sentence within the conversation.**

- **For backchannels (e.g., "uh-huh", "yeah", "okay"), assign the category based on context, such as the category of the preceding sentence.**

- **For utterances that fall into multiple categories, assign the most dominant or primary category.**

- **For utterances that don't directly contain content from categories 1-4 but serve to initiate or redirect conversation towards a specific category, assign them the category they are leading towards.**

- **Provide the output in JSON format, with sentence numbers as keys and category numbers as values.**

- **Your response should contain the JSON only.**

Example Output Format:

“‘json

“1”: 3,

“2”: 1,

“3”: 3,

“4”: 2,

“5”: 2,

“6”: 1,

“7”: 1,

“8”: 2,

“9”: 3,

“10”: 3

”’

To maximize reproducibility, we set model temperature to 0.

F.1.1 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.

- ⋆ After all those allowances they are going to be paying you let me say 100,000 shillings.

Encouragement

- ⋆ You can start poorly but if you are patient, flexible, disciplined you will be promoted easily.
- ⋆ 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.
- ⋆ 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.
- ⋆ 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.

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.
- ⋆ 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.
- ⋆ When you are going for interview you have to put on good clothes and look smart.
- ⋆ 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.

F.2 Text Pre-Process for Embedding- and Lexicon-Based Metrics

We pre-process the conversation transcripts using GPT-4o-mini to correct spelling and grammatical errors in the transcripts, with the following prompt:
"Please correct any spelling and grammatical errors, and add missing punctuation to the following text, which is a transcription of an utterance in a conversation. Do not remove existing punctuation. Your response should only include the corrected text.
Text:
[text] "
We use the cleaned text for lexicon and embedding based analyses. For classification we use the original text to minimize text manipulation and because the generative model we use understands sentences with spelling and grammatical issues. We use the mini version of GPT-4o for text cleaning, a simple task that does not need advanced models.

F.3 Creation of Mentor Adaptivity

We construct two measures of mentor adaptivity, one based on text embedding of the conversation transcripts, and one based on the words (lexicon) in the conversation transcripts.

Mean Embedding Distance is a text embedding-based metric that measures the average distance between every conversation of a mentor. The larger the average distance, the more diverse the content is across different conversations. We create the measure for each mentor×trainee pair using following process:

- We pre-process the transcripts following steps described in Section F.2.
- We restrict each conversation to utterances by the mentor, and join all the utterances in the conversation into one sentence, with a line break (\n) between each utterance.
- We obtain the embeddings of the combined text using Nvidia's `NV-Embed-v2` model, which is the best performing embedding model in *Massive Text Embedding (MTEB) Benchmark* as of late 2024.
- After creating the embeddings, we calculate the mean Euclidean distance between each one of an mentor's conversations, leaving out the conversation with the current trainee.[35] There are $\binom{n}{2}$ distances for each mentor×trainee, where n is the number of conversation by the mentor excluding the one with the current trainee. For each mentor×trainee, we take the average of the $\binom{n}{2}$ distances.
- Lastly, for each mentor×trainee, we identify if their mean distance is above or below median and create a "Longer Distance" and "Shorter Distance" dummy.

[35] If the mentor×trainee pair does not have conversation transcription, we use all conversations of the mentor. Please note that mentors with 1 or 2 conversations do not have a mean Euclidean distance because their number of conversations is smaller than 2 after leaving out the current trainee.

Generalized Resemblance is a lexicon-based metric that measures lexical variety across conversations. Inspired by Broder (1997), we create a generalized resemblance score r for each mentor×trainee:

$$r = \frac{|S(Conversation_1) \cap S(Conversation_2)... \cap S(Conversation_n)|}{|S(Conversation_1) \cup S(Conversation_2)... \cup S(Conversation_n)|}$$

where $S(Conversation_k)$ is defined as the unique words of the mentor in conversation k, and n is the number of conversation of the mentor minus the one with the current trainee. A smaller value of r indicates that fewer words are shared across all texts, implying higher lexical variety across the conversations.

We create the measure for each mentor×trainee pair using following process:

- We pre-process the transcripts following steps described in Section F.2.
- We restrict each conversation to utterances by the mentor, and join all the utterances in the conversation into one sentence, with a line break (\n) between each utterance.
- We obtain r using the equation above.
- Lastly, for each mentor×trainee, we identify if their lexical variety is above or below median and create a "Higher Variety" and "Lower Variety" dummy.

G 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.

Recent work showed the importance of these types of social contact, consistent with qualitative and descriptive data from our environment (Caria et al., 2023). 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:

$$Y_{i,s,t} = \alpha + \beta_1 S_1 C_i + \gamma_0 S_0 T_i + \gamma_1 S_1 T_i + X_i'\delta + \lambda_s + \epsilon_{i,s,t} \tag{13}$$

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.[36] S_1 is an indicator variable for students with at least one friend 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 friends 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 (Online Appendix Figure A.7). 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, which were too common to match with certainty.)

The results are shown in the Online Appendix Table A.11. As we lose nearly half of the sample, we start by checking whether our main results replicate in the sample for which we

[36]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.11: 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)	Started Job Search (4)	Search Efficacy Index (5)	Search Broadness Index (6)	Search Intensity Index (7)	Search Duration | Searched (8)	Out of the Labor Force (9)	Days Worked Last Month (10)	Training Sector (11)	Total Earnings Last Month (12)	First Job Duration (13)	Retained post Internship (14)	Internship to Job Transition (15)	Out of the Labor Force (16)	Days Worked Last Month (17)	Total Earnings Last Month (18)
MYF Treatment Assignment	-12.77***	.09**	-.06**	.02	-.05	-.01	-.14	-3.63	-.08**	1.55**	.08**	3.05	24.41***	.08***	.09	-.06**	1.34	11.01**
	(4.01)	(.04)	(.03)	(.02)	(.06)	(.09)	(.08)	(5.79)	(.03)	(.76)	(.03)	(3.64)	(8.21)	(.02)	(.06)	(.03)	(1.30)	(4.07)
Control + Treated Friends	-17.62	.16	-.08	-.07**	-.16	-.22	-.14	-28.45	-.00	1.30	-.03	-10.96	16.97	-.06	.05	.09	-.01	6.25
	(12.77)	(.13)	(.08)	(.03)	(.14)	(.20)	(.20)	(17.95)	(.08)	(1.61)	(.13)	(8.74)	(18.82)	(.09)	(.08)	(.08)	(2.07)	(5.76)
MYF + Treated Friends	-30.43**	.17	-.13	-.06	-.37*	-.42*	-.31	-28.17**	-.03	1.62	.06	-8.47	25.91	.02	.14	-.01	1.82	15.83
	(12.83)	(.16)	(.08)	(.04)	(.20)	(.23)	(.20)	(13.68)	(.11)	(1.77)	(.16)	(8.92)	(26.80)	(.09)	(.12)	(.08)	(3.25)	(10.23)
MYF + 0%	-25.69**	.22**	-.13*	-.02	-.15	-.14	-.24	-26.01	-.09	2.80*	.06	-5.29	40.67***	.03	.12	.02	1.24	16.20***
	(10.60)	(.10)	(.07)	(.02)	(.12)	(.19)	(.19)	(16.27)	(.08)	(1.44)	(.10)	(8.51)	(12.90)	(.05)	(.09)	(.07)	(2.24)	(4.80)
Control Mean	31.95	.52	.18	.98	.03	.01	-.09	28.35	.19	17.00	.65	17.42	80.21	.23	.43	.19	13.97	37.73
Control SD	45.48	.50	.39	.15	.83	.89	.85	74.36	.39	8.59	.48	43.80	95.69	.42	.50	.39	12.28	45.66
N	382	382	456	471	471	471	471	453	471	471	471	469	470	471	471	456	456	454

have friendship data in Panel A. Even though we lose a substantial portion of the sample, the main findings remain unchanged. In this sample, the medium run results are, if anything, stronger.

By examining Panel B of Online Appendix Table A.11, we conclude that there may have been some spillovers, which, if at all, have caused our overall estimates to be conservative. With the exception of Column 4, which indicates some discouragement (consistent with the hypothesis that while information is easily transferred to control friends, encouragement is much less so), Columns 1 through 18 demonstrate that information spread from their treated friends, resulting in better career trajectories for control groups with treated friends.

H PAP Deviations and Additional Analyses

We submitted a detailed Pre-Analysis Plan to the *Journal of Development Economics* pre-results submission process. The PAP underwent a revise and resubmit process, during which several minor changes were made to the design and estimation strategy. However, the core hypotheses, outcome definitions, and experimental structure remained unchanged. The paper was accepted at *JDE* in early 2021, ahead of the collection of any post-intervention data. From such PAP, there have been close to no deviations, meaning that nearly everything we committed to doing was carried out as planned. The main difference lies in how the analyses of the mechanisms were conducted, which we describe in Appendix Section H.1, along with other minor deviations. Another key point to note is that not all planned analyses were included in the main paper. Appendix Section H.2 serves to document these additional analyses, ensuring full transparency and reporting key takeaways—all of which align with the paper's main findings.

H.1 Mechanism

Our approach to analyzing mechanisms evolved beyond what was initially planned. We adopted an Empirical Bayes approach, which was not in the original PAP, as we only learned about this methodology later and we explored why the cash transfer had such unintended consequences.

The original PAP stated (in blue):

- Process analysis and mechanisms.
  - Het effect in engagement (reported in Appendix H.2)
  - Het effect in interaction frequency (reported in Appendix H.2)
  - Het effect in interaction length (reported in Appendix H.2)
  - Het effect in type and amount of support provided
    - Deviation: Upon revisiting our original PAP a year later, it was unclear what exactly this last point was meant to translate into, possibly a suggestive heterogeneity analysis interacting treatment with the content of the first mentorship session or key takeaways for students. However, we believe our IV/EB approach represents a significant methodological improvement over the simpler interaction-based analysis originally proposed.
- Het effect in whether the mentee values the interactions.
  - Minor Deviation: Mentees' satisfaction levels were too high for any meaningful heterogeneity analysis.
- Predictors for the success of cash transfer and analyze how they correlate with the main effects on primary outcomes.

  - While the PAP did not specify exactly how we planned to analyze predictors nor which exact predictors, we certainly had not expected the finding reported in Figure A.2 of the main paper: the cash transfer influenced the mentoring discussions in ways we had not anticipated or intended. We explored all other possible predictors, particularly after finding the cash transfer results puzzling. However, none of the collected data added much explanatory power. In hindsight, we should have collected more detailed information on self-employment attempts, spending patterns, and mentor guidance on financial decisions.
- One of our additional exploratory analyses suggested in the PAP involved providing evidence on the condition of the economy, overall and by sector.
  - We ultimately did not pursue this, as we found no meaningful heterogeneity by sector or region.
- We pre-specified that all data was going to be probability-reweighted to reflect any intensive tracking.
  - There was no systematic intensive tracking.
- We pre-specified that standard errors would be robust to heteroskedasticity, given the individual-level randomization.
  - However, following discussions with others in the field, we clustered errors at the strata level for the main regressions. Based on Abadie et al. (2023), we remain undecided on the most appropriate SE specification, so we report regressions with robust SE in Online Appendix Table A.12 to Table A.15, with no change in results.
- We pre-specified a Matching Quality Index, intended to measure the quality of mentee-employer matches.
  - To construct this index, we had to incorporate data from two rounds of endlines as well as an employer survey. Due to the limited sample size (N = 306) of the employer survey and non-random selection, we did not build this Index.
- Cohort-level expectations for employment were not included in the analysis as planned, as we forgot to add them to the endline questionnaire.

H.2 Additional Analysis from the Pre-Analysis Plan

In this section, we report all the additional analyses we had committed to in the Pre-Analysis Plan. These analyses are divided into heterogeneities, additional outcomes, and alternative estimations of treatment effects. None of these results contradict the main findings, and they remain consistent with our primary analysis.

H.2.1 Heterogeneity by Mentee Characteristics

We examined heterogeneity in treatment effects based on mentee characteristics, including gender, previous work experience, cognitive ability (Raven's test score), locus of control, and socio-economic background.

The key takeaway is that none of these heterogeneity factors appear to be particularly relevant, meaning that the treatment was effective across all groups. There is suggestive, but not statistically significant, evidence indicating that individuals who had never worked prior to MYF experienced the largest increase in willingness to accept a job and showed the greatest improvements in short-run labor market outcomes. Similarly, those with higher cognitive ability (as measured by Raven's scores) exhibited stronger labor market outcomes (Tables A.16 – A.33).

H.2.2 Heterogeneity by Interaction Frequency, Length, and Engagement

Take-up of the program was exceptionally high, with strong engagement throughout: Treatment take-up was 91%, with an average of 6.8 interactions per mentee and a total average interaction time of 3.2 hours. Interaction patterns suggest that mentor-mentee pairs who were closer in age and from the same vocational training institutes engaged more frequently. Overall, satisfaction levels were high across all pairs, with strong identification and transportation indices. Sentiment analysis indicates that feedback was neutral to positive, and while mentors primarily led the conversations, students remained actively engaged throughout the program. We conducted sentiment analysis on interactions and examined engagement levels in terms of mentor-to-mentee ratios, frequency of contact, and overall time spent interacting. The heterogeneity analysis largely aligned with expectations:

- Closer mentor-mentee pairs were more effective.
- More engaged trainees benefited more in terms of skills development and labor market outcomes.
- Pairs with stronger connections had longer interactions, higher take-up, greater satisfaction, and stronger identification and transportation indices.

While some of these effects were statistically significant, many were only directionally aligned (i.e., pointing in the expected direction without achieving statistical significance). We prespecified heterogeneity analyses on engagement index, identification index, and usefulness index. While considerable effort was spent adapting and piloting these indices to fit the study context, they ultimately proved too high for meaningful heterogeneity analysis, as there was insufficient variability to be leveraged. This could, however, follow the effectiveness of the mentor selection process and the overall success of the program. This appendix does not include every exploratory correlation or robustness check conducted, as there were numerous

additional analyses performed. Further details can be provided upon request.
Finally, as part of this heterogeneity analysis, we also pre-specified an analysis on Network Link Formation, which was conducted and can be found in Appendix Table A.1.

H.2.3 Additional Labor market Outcomes Not in the Main Tables

Beyond the primary outcomes reported in the main tables, we examine several additional labor market indicators: Hours Practicing Technical Skills; Career Satisfaction; Employer-Reported Satisfaction with Employee; Ability to Keep the Job/Firm Running for at Least 3 Months. When assessing Intent-to-Treat estimates for these additional labor market outcomes, we find that—with the exception of career satisfaction—all treatment effect signs align with expectations: higher employer satisfaction, increased likelihood of maintaining a job for at least three months, and more time spent practicing technical skills. However, none of these effects reach statistical significance (Online Appendix Table A.34).

H.2.4 Alternative Estimation of Treatment Effects

We committed to reporting results from two additional estimation approaches for our primary outcomes measured in both Endline 1 and Endline 2.

1. Separate Analysis for T1 and T2 The first approach involved analyzing the treatment effects separately for T1 and T2. The findings replicate our main results, with T1 showing the strongest effects in the medium run, including statistically significant impacts on labor market participation (Tables A.35 – A.40).
We follow Goldsmith-Pinkham et al. (2024) and use "a linear regression which restricts estimation to the individuals who are either in the control group or the treatment group of interest" to avoid contamination bias. Specifically, We restrict the sample to T1 + Control, and run the regression using T1 as the treatment variable. Same for T2.
The ITT specification we use for the regression on Treatment overall is:

$$Y_{i,s,t} = \beta_0 + \beta_1 T_i + X_i'\delta + \lambda_s + \epsilon_{i,s,t} \tag{14}$$

where 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 and individual covariates measured at baseline selected on the basis of their ability to predict the primary outcomes. λ_s are strata fixed effects. $\epsilon_{i,s,t}$ is the error term. We cluster errors at the strata level. Estimation is performed over the entire sample of students. β_1 measures the causal effect of being selected for participating to the MYF Program on Y_i under SUTVA.

In this section, the new regression specifications we use to avoid contamination bias are:

$$Y_{i,s,t} = \beta_0 + \beta_1 T1_i + X_i'\delta + \lambda_s + \epsilon_{i,s,t} \text{ if } i \text{ in T1 or Control Group} \tag{15}$$

and

$$Y_{i,s,t} = \beta_0 + \beta_1 T2_i + X_i'\delta + \lambda_s + \epsilon_{i,s,t} \text{ if } i \text{ in T2 or Control Group} \tag{16}$$

2. Pooled Sample with Clustered Standard Errors For outcome variables measured in both rounds (EL1 and EL2), we had pre-specified that we would pool the samples and cluster standard errors at the respondent level. We implement this in Tables A.41 and A.42, and the results confirm that pooling across waves reveals overall gains in labor market participation and total earnings (both conditional and unconditional) as a result of the treatment.

3. ATEs vs. ITTs We also committed to reporting both Average Treatment Effects and Intention-to-Treat estimates in the main tables. The results indicate that ATEs and ITTs lead to nearly identical conclusions, with ATEs being slightly larger, if at all (Tables A.43 – A.45).

PAP Deviations and Additional Analyses Tables

Table A.12: ITT Estimates: Short Run Labor Market Outcomes

	Short Run					Index
	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment Assignment	-.057**	1.267**	.081**	1.700	19.227***	.150**
	(.026)	(.575)	(.032)	(2.532)	(7.192)	(.063)
	[.033]	[.033]	[.033]	[.091]	[.033]	[.033]
Control Mean	.21	16.15	.54	12.02	78.07	-.00
Treatment Effect (%)	-26.57	7.85	15.11	14.15	24.63	-
N	934	934	934	931	929	934

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on primary employment outcomes. We report robust standard errors. See Table 2 notes for description of outcomes and regression specification.

Table A.13: Labor Market Trajectory in the Medium Run

	Transitions		Medium Run			Index
	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment Assignment	.041	.076**	-.025	.265	5.889*	.135*
	(.026)	(.032)	(.029)	(.793)	(3.266)	(.070)
	[.168]	[.124]	[.214]	[.326]	[.136]	[.136]
Control Mean	.18	.37	.26	12.50	33.48	.00
Treatment Effect (%)	22.87	20.70	-9.53	2.12	17.59	-
N	934	934	923	923	916	844

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on labor market dynamics. We report robust standard errors. See Table 3 notes for description of outcomes and regression specification.

Table A.14: Willingness to Accept a Job and Job Search Behavior

	Willingness to Accept a Job			Job Search				Search Duration	Indexes	
	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Efficacy Index (5)	Search Broadness Index (6)	Search Intensity Index (7)	Search Duration | Searched (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment Assignment	-11.410***	.071*	-.045*	.029**	-.009	.002	-.086	-8.289**	-.029	.277***
	(3.911)	(.042)	(.025)	(.015)	(.064)	(.064)	(.063)	(3.863)	(.062)	(.086)
	[.017]	[.125]	[.120]	[.104]	[.415]	[.415]	[.160]	[.094]	[.319]	[.014]
Control Mean	36.22	.54	.18	.93	-.00	.00	.00	27.73	.00	-.00
Treatment Effect (%)	-31.50	13.09	-24.85	3.10	-	-	-	-29.89	-	-
N	737	739	890	934	934	934	934	885	934	668

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 report robust standard errors. See Table 4 notes for description of outcomes and regression specification.

Table A.15: Treatment Effects by Mentor Types (Robust SE)

	Mechanisms		Labor Market Outcomes	
	Search Behavior Index (1)	Willingness to Accept Job Index (2)	Short Run Index (3)	Career Trajectory Index (4)
Panel A — 2SLS				
Entry Conditions	-.09	.59***	.23**	.08
	(.10)	(.11)	(.10)	(.11)
Encouragement	-.04	.27***	.20***	.21**
	(.07)	(.09)	(.08)	(.09)
Search Tips	.05	.02	-.02	-.00
	(.10)	(.13)	(.10)	(.11)
Control Mean	.00	-.00	-.00	.00
N Mentors	158	158	158	157
N	934	668	934	844
F-Test of joint significance (pval)	.68	.00	.01	.11
AP Partial F (pval)- Entry Conditions	.00	.00	.00	.00
AP Partial F (pval)- Encouragement	.00	.00	.00	.00
AP Partial F (pval)- Search Tips	.00	.00	.00	.00
Overidentification test (pval)	.64	.58	.13	.25
Panel B — OLS				
Entry Conditions	-.07	.42***	.19**	.09
	(.09)	(.11)	(.09)	(.10)
Encouragement	-.03	.28***	.20**	.17*
	(.07)	(.09)	(.08)	(.09)
Search Tips	.03	.15	-.02	.01
	(.09)	(.12)	(.09)	(.10)
N	934	668	934	844
F-Test of joint significance (pval)	.79	.00	.02	.24

Notes: This table is the same as Table 5 except standard errors.

Table A.16: ITT Estimates: Short Run Labor Market Outcomes, by Gender

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment						
× Female Trainee	-.059*	1.372	.035	2.919	24.383***	.145
	(.033)	(.862)	(.045)	(4.697)	(8.305)	(.098)
× Male Trainee	-.058**	1.240*	.111***	.492	16.734**	.150**
	(.025)	(.691)	(.033)	(2.013)	(6.516)	(.056)
Difference	-.001	.132	-.077	2.427	7.648	-.005
P-Value	.978	.901	.153	.621	.451	.964
N	934	934	934	931	929	934

Table A.17: ITT Estimates: Labor Market Trajectory in the Medium Run, by Gender

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment						
× Female Trainee	.036	.092*	-.039	-.556	6.210*	.166*
	(.030)	(.051)	(.037)	(.927)	(3.284)	(.087)
× Male Trainee	.047*	.072	-.017	.880	5.756	.123
	(.027)	(.045)	(.029)	(1.336)	(5.425)	(.079)
Difference	-.011	.020	-.023	-1.435	.454	.043
P-Value	.768	.762	.620	.361	.941	.701
N	934	934	923	923	916	844

Table A.18: ITT Estimates: Willingness to Accept a Job and Job Search Behavior, by Gender

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Duration | Searched (5)	Search Efficacy Index (6)	Search Broadness Index (7)	Search Intensity Index (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment										
× Female Trainee	-6.230	.014	-.048	.017	-6.226	.031	.074	-.181	-.039	.153
	(5.735)	(.058)	(.040)	(.024)	(7.544)	(.137)	(.109)	(.154)	(.109)	(.147)
× Male Trainee	-14.983***	.110***	-.045*	.037**	-9.898**	-.033	-.037	-.024	-.018	.357***
	(4.136)	(.034)	(.025)	(.017)	(4.425)	(.066)	(.081)	(.093)	(.106)	(.091)
Difference	8.753	-.097	-.003	-.020	3.672	.064	.111	-.157	-.022	-.203
P-Value	.194	.133	.953	.472	.661	.659	.397	.365	.883	.215
N	737	739	890	934	885	934	934	934	934	668

Table A.19: ITT Estimates: Short Run Labor Market Outcomes, by Previous Work Experience

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment						
× Worked Pre MYF	-.045	1.001	.074	.127	23.744***	.109
	(.037)	(.983)	(.048)	(3.656)	(8.262)	(.098)
× Never Ever Worked Pre MYF	-.060	1.332	.073	5.264	18.285*	.202**
	(.037)	(.895)	(.046)	(3.141)	(10.576)	(.095)
Difference	.015	-.331	.001	-5.137	5.459	-.093
P-Value	.805	.825	.989	.310	.717	.561
N	934	934	934	931	929	934

Table A.20: ITT Estimates: Labor Market Trajectory in the Medium Run, by Previous Work Experience

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment						
× Worked Pre MYF	.040	.083	-.021	-.213	4.751	.148**
	(.031)	(.055)	(.026)	(1.280)	(4.255)	(.064)
× Never Ever Worked Pre MYF	.038	.065	-.018	.737	6.681	.122
	(.044)	(.046)	(.044)	(1.269)	(5.462)	(.120)
Difference	.002	.018	-.003	-.950	-1.930	.026
P-Value	.974	.807	.957	.571	.767	.856
N	934	934	923	923	916	844

Table A.21: ITT Estimates: Willingness to Accept a Job and Job Search Behavior, by Previous Work Experience

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Duration | Searched (5)	Search Efficacy Index (6)	Search Broadness Index (7)	Search Intensity Index (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment										
× Worked Pre MYF	-5.721	.069	-.018	.051**	-8.829**	.056	-.004	-.043	.035	.116
	(5.010)	(.058)	(.028)	(.021)	(3.950)	(.090)	(.099)	(.085)	(.118)	(.100)
× Never Ever Worked Pre MYF	-16.507***	.072	-.051	-.000	-7.093	-.003	-.034	-.176	-.111	.398***
	(5.177)	(.055)	(.039)	(.023)	(7.097)	(.099)	(.088)	(.142)	(.097)	(.112)
Difference	10.786	-.003	.033	.052	-1.736	.059	.030	.133	.146	-.282
P-Value	.123	.974	.485	.083	.823	.632	.814	.397	.308	.018
N	737	739	890	934	885	934	934	934	934	668

Table A.22: ITT Estimates: Short Run Labor Market Outcomes, by Raven's Test Score

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment						
× Raven Score above Median	-.112***	1.064	.082*	2.019	35.886***	.204***
	(.026)	(.879)	(.042)	(2.761)	(8.498)	(.068)
× Raven Score below Median	-.026	1.869**	.092**	4.851	9.549	.186*
	(.038)	(.921)	(.044)	(4.436)	(6.834)	(.098)
Difference	-.086	-.805	-.010	-2.832	26.338	.019
P-Value	.092	.482	.867	.633	.013	.884
N	889	889	889	886	884	889

Table A.23: ITT Estimates: Labor Market Trajectory in the Medium Run, by Raven's Test Score

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment						
× Raven Score above Median	.095**	.040	-.042	-.336	9.580	.226*
	(.040)	(.048)	(.044)	(1.188)	(5.680)	(.123)
× Raven Score below Median	-.008	.102**	-.008	1.191	3.287	.053
	(.033)	(.043)	(.046)	(1.458)	(5.523)	(.089)
Difference	.103	-.062	-.034	-1.527	6.292	.173
P-Value	.050	.271	.643	.393	.420	.292
N	889	889	874	874	868	803

Table A.24: ITT Estimates: Willingness to Accept a Job and Job Search Behavior, by Raven's Test Score

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Duration | Searched (5)	Search Efficacy Index (6)	Search Broadness Index (7)	Search Intensity Index (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment										
× Raven Score above Median	-5.823	.068	-.052	.069***	-11.345**	.100	.004	-.110	.034	.211**
	(4.324)	(.048)	(.037)	(.022)	(4.487)	(.082)	(.104)	(.099)	(.093)	(.090)
× Raven Score below Median	-15.042***	.071	-.037	.011	-7.453	-.112	-.016	-.058	-.079	.326**
	(5.191)	(.055)	(.043)	(.023)	(6.609)	(.112)	(.096)	(.102)	(.115)	(.136)
Difference	9.218	-.003	-.015	.058	-3.893	.212	.020	-.053	.114	-.114
P-Value	.168	.967	.815	.047	.582	.083	.876	.589	.316	.437
N	702	704	846	889	842	889	889	889	889	639

Table A.25: ITT Estimates: Short Run Labor Market Outcomes, by Locus of Control

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment						
× Locus of Control above Median	-.040	.654	.124***	4.621	17.138*	.191
	(.050)	(.971)	(.044)	(5.383)	(9.809)	(.114)
× Locus of Control below Median	-.064***	1.627**	.059	.121	18.138**	.127**
	(.021)	(.644)	(.037)	(2.819)	(7.140)	(.061)
Difference	.024	-.973	.065	4.500	-1.000	.064
P-Value	.655	.386	.260	.466	.934	.623
N	930	930	930	927	925	930

Table A.26: ITT Estimates: Labor Market Trajectory in the Medium Run, by Locus of Control

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment						
× Locus of Control above Median	.054	.076	-.025	-.925	3.589	.089
	(.047)	(.070)	(.045)	(2.024)	(7.250)	(.145)
× Locus of Control below Median	.034	.088**	-.018	.832	6.136*	.140*
	(.028)	(.037)	(.031)	(.836)	(3.422)	(.070)
Difference	.020	-.012	-.007	-1.758	-2.547	-.051
P-Value	.739	.868	.898	.347	.711	.748
N	930	930	920	920	913	842

Table A.27: ITT Estimates: Willingness to Accept a Job and Job Search Behavior, by Locus of Control

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Duration | Searched (5)	Search Efficacy Index (6)	Search Broadness Index (7)	Search Intensity Index (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment										
× Locus of Control above Median	-13.583**	.115	-.040	.036	-11.212*	.057	-.137	-.201	-.119	.402**
	(5.630)	(.082)	(.039)	(.035)	(6.081)	(.157)	(.145)	(.168)	(.168)	(.159)
× Locus of Control below Median	-11.663***	.064	-.061*	.028*	-6.473	-.024	.061	-.034	.018	.228*
	(3.998)	(.046)	(.035)	(.015)	(5.751)	(.073)	(.071)	(.081)	(.068)	(.122)
Difference	-1.920	.051	.021	.008	-4.739	.081	-.198	-.167	-.137	.174
P-Value	.759	.623	.716	.834	.584	.625	.204	.322	.377	.401
N	734	736	886	930	881	930	930	930	930	665

Table A.28: ITT Estimates: Short Run Labor Market Outcomes, by Socio-Economic Background

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment						
× HH Asset above Mean	-.017	1.150	.082	-1.639	21.665**	.065
	(.029)	(.884)	(.050)	(5.247)	(10.562)	(.095)
× HH Asset below Mean	-.078**	1.266*	.077**	4.133	14.681*	.201***
	(.030)	(.696)	(.033)	(2.989)	(7.426)	(.067)
Difference	.061	-.116	.005	-5.771	6.983	-.136
P-Value	.138	.907	.926	.384	.629	.252
N	928	928	928	925	923	928

Table A.29: ITT Estimates: Labor Market Trajectory in the Medium Run, by Socio-Economic Background

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment						
× HH Asset above Mean	.107**	.062	-.040	.494	6.395	.227*
	(.043)	(.077)	(.036)	(1.222)	(4.944)	(.112)
× HH Asset below Mean	-.006	.093**	-.012	.345	7.597*	.088
	(.032)	(.036)	(.039)	(1.093)	(4.216)	(.094)
Difference	.113	-.031	-.028	.149	-1.202	.138
P-Value	.051	.713	.647	.899	.815	.395
N	928	928	918	918	911	839

Table A.30: ITT Estimates: Willingness to Accept a Job and Job Search Behavior, by Socio-Economic Background

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Duration | Searched (5)	Search Efficacy Index (6)	Search Broadness Index (7)	Search Intensity Index (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment										
× HH Asset above Mean	-11.819*	.081	-.018	.024	-6.808	.024	.068	-.195	-.048	.184
	(6.160)	(.065)	(.041)	(.027)	(5.709)	(.117)	(.093)	(.153)	(.156)	(.152)
× HH Asset below Mean	-12.548***	.083*	-.044	.034*	-12.103**	-.003	-.061	-.059	-.037	.324***
	(3.483)	(.041)	(.034)	(.017)	(5.429)	(.086)	(.082)	(.076)	(.078)	(.109)
Difference	.729	-.002	.027	-.010	5.294	.027	.129	-.136	-.011	-.140
P-Value	.907	.979	.615	.754	.467	.843	.266	.352	.947	.416
N	735	737	884	928	879	928	928	928	928	666

Table A.31: ITT Estimates: Short Run Labor Market Outcomes, By Pre-MYF Expectation

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment						
× Pre-MYF Expected Earnings Above Median	-.064	.035	.054	3.020	35.790***	.136
	(.046)	(1.295)	(.048)	(4.426)	(11.737)	(.118)
× Pre-MYF Expected Earnings Below Median	-.072	1.116	.151**	-.878	16.623	.158
	(.066)	(1.238)	(.062)	(4.401)	(14.891)	(.108)
Difference	.008	-1.082	-.097	3.898	19.167	-.022
P-Value	.911	.515	.161	.437	.326	.874
N	512	512	512	510	509	512

Table A.32: ITT Estimates: Labor Market Trajectory in the Medium Run, By Pre-MYF Expectation

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment						
× Pre-MYF Expected Earnings Above Median	.135**	.023	.004	.210	10.855**	.209*
	(.058)	(.073)	(.054)	(1.435)	(4.719)	(.122)
× Pre-MYF Expected Earnings Below Median	-.009	.090	-.034	1.127	3.119	.071
	(.043)	(.056)	(.067)	(1.994)	(5.670)	(.102)
Difference	.144	-.067	.037	-.917	7.736	.138
P-Value	.032	.418	.690	.649	.272	.383
N	512	512	495	495	493	472

Table A.33: ITT Estimates: Willingness to Accept a Job and Job Search Behavior, By Pre-MYF Expectation

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Duration | Searched (5)	Search Efficacy Index (6)	Search Broadness Index (7)	Search Intensity Index (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment										
× Pre-MYF Expected Earnings Above Median	-23.177***	.139**	-.114	.045	-4.521	-.092	.058	-.050	-.010	.466***
	(5.897)	(.060)	(.069)	(.028)	(6.398)	(.144)	(.120)	(.117)	(.136)	(.149)
× Pre-MYF Expected Earnings Below Median	1.405	.023	-.057	.023	-6.052	-.133	-.207*	-.085	-.179	.044
	(3.084)	(.046)	(.048)	(.028)	(5.711)	(.122)	(.116)	(.115)	(.127)	(.121)
Difference	-24.582	.116	-.057	.022	1.530	.041	.265	.035	.169	.422
P-Value	.000	.131	.384	.538	.863	.783	.020	.814	.213	.013
N	485	487	492	512	490	512	512	512	512	446

Table A.34: ITT Estimates: Additional Labor Market Outcomes

	Hours Practicing Technical Skills (1)	Career Satisfaction (EL2) (2)	Employer Satisfaction (0-10, EL2) (3)	3 Mon+ Job Duration (4)
MYF Treatment Assignment	5.054	-.007	.259	.026
	(5.092)	(.124)	(.187)	(.023)
Control Mean	57.38	6.66	7.55	.45
Control SD	74.11	2.35	1.61	.50
T Effect (%)	8.81	-0.11	3.43	5.67
N	904	922	306	1013

Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on additional labor market outcomes. These are obtained by estimating equation 1. Below each coefficient estimate, we report the strata-level clustered standard errors in parentheses. For each outcome, we report the mean outcome for the control group and the treatment effect. All regressions control for strata dummies and the balance variable *ever_worked*. Dependent variables: Column 1: Career Satisfaction, on a scale of 0-10, measured at 1 Year. Column 2: Employer-reported satisfaction with the mentee, on a scale of 0-10, measured at 1 Year. Column 3: Indicator for being employed / have business in the same sector of training at 1 Year. Column 4: Ability to keep the job / keep firm running for at least 3 months. This variable takes the form of an indicator which takes the value of 1 if the duration of either first job, job at 3 Month, or job at 1 Year is 90 days or longer. Statistical significance throughout the paper is indicated by *, **, and *** for p-values of 0.10, 0.05, and 0.01, respectively.

Table A.35: ITT Estimates: Short Run Labor Market Outcomes (T1 Vs. Control)

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
T1 (MYF)	-.050**	1.547**	.081**	2.803	18.236**	.165**
	(.023)	(.661)	(.038)	(2.632)	(6.789)	(.067)
Control Mean	-23.28	9.58	15.09	23.32	23.36	-
Control SD	657	657	657	656	655	657

Table A.36: ITT Estimates: Short Run Labor Market Outcomes (T2 Vs. Control)

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
T2 (MYF+Cash)	-.063**	.973	.081***	.722	20.744***	.136**
	(.025)	(.631)	(.025)	(2.483)	(7.238)	(.055)
Control Mean	-29.70	6.03	15.00	6.01	26.57	-
Control SD	670	670	670	667	666	670

Table A.37: ITT Estimates: Labor Market Trajectory in the Medium Run (T1 Vs. Control)

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
T1 (MYF)	.062**	.122***	-.061*	1.180	10.573**	.273***
	(.025)	(.042)	(.031)	(1.155)	(4.065)	(.076)
Control Mean	34.34	33.00	-23.52	9.44	31.58	-
Control SD	657	657	638	638	633	583

Table A.38: ITT Estimates: Labor Market Trajectory in the Medium Run (T2 Vs. Control)

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
T2 (MYF+Cash)	.026	.038	.004	-.500	1.624	.024
	(.025)	(.042)	(.030)	(1.037)	(3.711)	(.072)
Control Mean	14.22	10.34	1.54	-4.00	4.85	-
Control SD	670	670	668	668	663	613

Table A.39: ITT Estimates: Willingness to Accept a Job and Job Search Behavior (T1 Vs. Control)

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Efficacy Index (5)	Search Broadness Index (6)	Search Intensity Index (7)	Search Duration | Searched (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
T1 (MYF)	-14.035***	.083**	-.015	.030*	.019	-.083	-.060	-10.928**	-.040	.268**
	(4.029)	(.040)	(.024)	(.016)	(.081)	(.071)	(.092)	(4.657)	(.092)	(.101)
Control Mean	-38.75	15.31	-8.13	3.19	-	-	-	-39.41	-	-
Control SD	464	465	624	657	657	657	657	619	657	422

Table A.40: ITT Estimates: Willingness to Accept a Job and Job Search Behavior (T2 Vs. Control)

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Efficacy Index (5)	Search Broadness Index (6)	Search Intensity Index (7)	Search Duration | Searched (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
T2 (MYF+Cash)	-9.794***	.069*	-.073**	.028*	-.030	.075	-.107	-6.000	-.018	.311***
	(3.535)	(.038)	(.029)	(.016)	(.077)	(.082)	(.076)	(4.146)	(.072)	(.079)
Control Mean	-27.04	12.67	-40.11	2.98	-	-	-	-21.64	-	-
Control SD	467	468	634	670	670	670	670	631	670	429

Table A.41: ITT: Pooled Variables (Estimation 1)

	Out of the Labor Force (1)	Days Worked Last Month (2)	Total Earnings Last Month (3)	Total Earnings Conidtional (4)	Training Sector (5)
MYF Treatment Assignment	-.043**	.795	3.761*	4.114*	.049*
	(.017)	(.492)	(2.094)	(2.206)	(.027)
Control Mean	.24	14.35	22.58	32.65	.47
Control SD	.43	10.74	44.27	50.05	.50
T Effect (%)	-18.15	5.54	16.66	12.60	10.52
N	1857	1857	1847	1298	1857

Notes: For outcome variables measured at both rounds (e.g. career satisfaction, time spent conducting occupation-related tasks, worked in last month, earnings, conditional earnings, worked in the same sector) we pool the samples. We use the following specification: $Y_{i,s} = \beta_0 + \beta_1 T_i + \beta_2 EL1_i + X_i'\delta + \lambda_s + \epsilon_{i,s}$ where $EL1_i$ is an indicator which takes a value of 1 if the observation is from Endline 1. The indicator takes a value of 0 if the observation is from Endline 2. We cluster standard errors at strata and trainee level.

Table A.42: ITT: Pooled Variables (Estimation 2)

	Out of the Labor Force (1)	Days Worked Last Month (2)	Total Earnings Last Month (3)	Total Earnings Conidtional (4)	Training Sector (5)
MYF Treatment Assignment	-.032	.497	6.884**	10.082**	.025
	(.021)	(.819)	(3.270)	(3.893)	(.036)
Control Mean	.25	12.72	30.71	36.95	.40
Control SD	.44	11.69	46.17	51.15	.49
T Effect (%)	-12.71	3.91	22.42	27.29	6.27
N	1013	1013	1012	859	1013

Notes: For outcome variables measured at both rounds, we only keep one observation per person. We use data from Endline 2 if possible, if data is not available from Endline 2, we use Endline 1. We cluster standard errors at strata level.

Table A.43: ATE Estimates: Short Run Labor Market Outcomes

	Out of the Labor Force (1)	Days Worked Last Month (2)	Training Sector (3)	Total Earnings Last Month (4)	First Job Duration (5)	Short Run Index (6)
MYF Treatment Takeup	-.058***	1.289**	.083***	1.730	19.571***	.153***
	(.019)	(.530)	(.025)	(2.134)	(4.793)	(.049)
Control Mean	.21	16.15	.54	12.02	78.07	-.00
Control SD	.41	9.20	.50	40.11	98.12	1.00
T Effect (%)	-27.04	7.98	15.38	14.40	25.07	-
N	934	934	934	931	929	934

Table A.44: ATE Estimates: Labor Market Trajectory in the Medium Run

	Retained post Internship (1)	Internship to Job Transition (2)	Out of the Labor Force (3)	Days Worked Last Month (4)	Total Earnings Last Month (5)	Career Trajectory Index (6)
MYF Treatment Takeup	.042**	.078**	-.026	.280	6.215*	.137**
	(.019)	(.033)	(.023)	(.944)	(3.514)	(.056)
Control Mean	.18	.37	.26	12.50	33.48	.00
Control SD	.39	.48	.44	11.85	45.74	1.00
T Effect (%)	23.28	21.07	-10.08	2.24	18.56	-
N	934	934	923	923	916	844

Table A.45: ATE Estimates: Willingness to Accept a Job and Job Search Behavior

	Reservation Wage (1)	Would Accept Unpaid Job (2)	Refused Job Offer | Searched (3)	Started Job Search (4)	Search Efficacy Index (5)	Search Broadness Index (6)	Search Intensity Index (7)	Search Duration | Searched (8)	Search Behavior Index (9)	Willingness to Accept Job Index (10)
MYF Treatment Takeup	-11.720***	.073**	-.046**	.030**	-.009	.002	-.088	-8.411**	-.029	.279***
	(3.244)	(.030)	(.021)	(.014)	(.067)	(.064)	(.076)	(3.843)	(.074)	(.077)
Control Mean	36.22	.54	.18	.93	-.00	.00	.00	27.73	.00	-.00
Control SD	47.43	.50	.39	.25	1.00	1.00	1.00	67.87	1.00	1.00
T Effect (%)	-32.36	13.44	-25.21	3.16	-	-	-	-30.33	-	-
N	737	739	890	934	934	934	934	885	934	668