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

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Abstract

We designed and randomized a mentorship program among students undergoing school-to-work transitions in Uganda. The program improved participants' career trajectories up to a year after graduation. Using call transcripts and survey data, we find that the mentorship acted by providing information about entry-level jobs and encouragement, rather than job referrals or search capital. Consistent with this finding, mentored students lowered their reservation wages, raised their expected returns to experience, and turned down fewer job offers. These results highlight the role of distorted beliefs in prolonging youth unemployment and point to a cost-effective and scalable solution.

JEL codes: D83, J24, J64, O10
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1 Introduction

Today, one-fifth of the world's first-time job seekers lives in Africa (United Nations, 2019). With youth unemployment and underemployment rates as high as 60%, moving young Africans into jobs is a top priority for most governments on the continent.

So far, the most common policy response to this challenge has been to invest in skills training to boost youth employability (McKenzie, 2017). While these programs have proven cost-effective in several contexts (Alfonsi et al., 2020; Maitra and Mani, 2017), their placement rates are often low, resulting in a mass of untapped talent. Recent evidence points at supply-side information frictions being a particularly significant barrier to entry for youth in low-income settings (Abebe et al., 2023; Donovan et al., 2023). Young job seekers often have limited information *and* unduly optimistic expectations, which leads them to turn down accessible jobs in favor of hoped-for opportunities that fail to materialize (Groh et al., 2016; Abebe et al., 2023; Banerjee and Chiplunkar, 2023; Bandiera 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, people tend to heighten their response to positive news and overlook negative news (Eil and Rao, 2011), as has been documented also in labor market settings (Jones and Santos, 2022; Mueller et al., 2021; Wiswall and Zafar, 2015). On the other hand, there is a risks of discouragement when providing unwanted bad news, which can lead to undesirable outcomes such as increased school dropouts, voluntary unemployment, a decline in job quality, and a looming sense of despondency (Kelley et al., 2024; Banerjee and Sequeira, 2023; Bandiera et al., 2023). This means that the provision of information alone can backfire, exacerbating the very issues it aims to resolve.

This paper proposes a low-cost and scalable way of providing relevant information to young job seekers in low-income settings, capable of rectifying their overly optimistic beliefs without leading to discouragement. We design and administer a mentorship program, which we call Meet Your Future (MYF). The program draws on insights from interdisciplinary research on messenger effects and from medical psychology on delivering bad news. Studies have shown that messengers who share characteristics with their audience can better communicate their messages (Durantini et al., 2006; Dolan et al., 2012). The medical psychology literature underscores that conveying hope and potential for positive outcomes, even when delivering bad news, promotes active coping (Ptacek and Eberhardt, 1996). By applying these insights, we pair soon-to-be graduates of vocational training institutes with relatable and successful workers, who we call the "future you'.'

We evaluate the impacts of MYF using a randomized controlled trial. Specifically, we conduct an experiment with 1,111 vocational students poised to make the school-to-work transition in urban labor markets across Uganda. We build a three-year panel of students consisting of six rounds of data collection beginning two years prior to and ending one year after the students' graduation, including a post-intervention survey from 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. In a novel dimensional measurement, we record voice conversations between students and mentors, totaling over 350 hours, allowing us to precisely assess the content of these engagements as well as attributes difficult to codify using self-reported data, such as enthusiasm and curiosity.

We start by documenting marked overoptimism regarding entry-level pay among our job seekers, in line with findings in recent literature. As we track both respondents' expected as well as realized earnings at their first jobs, we can assess the individual-level accuracy of expectations. Ninety-four percent of the students overestimate their first-job earnings. On average, first-job realized earnings were 14% of students' expectations. When their expectations are compared to their realized earnings one year later, the proportion rises to 65%, indicating that optimism about wages is prevalent but is especially pertinent to their first jobs; as students fail to account for the reality that many of them will be unpaid or low paid. Relatedly, we highlight a novel fact: not only are job seekers overly optimistic about starting pay, but they also have a limited grasp of job-to-job transition probabilities, returns to experience, and earnings growth potential. Most crucially, students undervalue initial unpaid employment spells, failing to see that they are frequently stepping stones to better employment and earnings down the line.

Next we use the content of the audio recordings and evidence from the literature on supply-side frictions to identify four plausible mechanisms through which our mentors can affect labor market outcomes: job referrals, actionable search tips, information about entry-level conditions, and encouragement. To guide the understanding of our results, we introduce an extended version of the McCall (1970) search model incorporating subjective beliefs. When bringing the predictions to the data, we rule out direct job referrals or stronger search abilities as viable routes for the observed treatment effects. Instead, we show that 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 determining career prospects. In response, their reservation wages drop by 32%.

Despite the sharp decline in reservation wage, the overall impact on labor market participation is large and positive. Treated students are 3% more likely to initiate job searches after graduation and 25% less likely to turn down a job offer while seeking their first job, which leads them to obtain those first jobs faster.

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. Not only they are working more, but they are 15% more likely to do so in the sector in which they trained, therefore leveraging and enhancing the human capital complementarities accumulated from their vocational education.

These accelerated first employment spells 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, the earnings of treated students are 18% higher than those of control students. We estimate the IRR of this intervention to be on the order of 300%.

To further confirm that MYF affected labor market outcomes through learning about entry-level market conditions and encouragement to persevere, we leverage a second randomization built into the research design, namely that of students to mentors. We accomplish this by analyzing the effect of each topic of conversation on labor market outcomes. To map the conversational material to our four mechanisms, we evaluate transcripts of the coaching sessions as well as supplementary data characterizing the students' key takeaways. At first, we use Empirical Bayes tools to estimate mentor-level heterogeneity. The large estimates of bias-corrected variance indicate that some mentors are better than others. To understand the determinants of this heterogeneity and to confirm our previous results, we employ an instrumental variables approach, capitalizing on the random assignment of students to mentors: we find that the best mentors are those providing mentees with information about entry-level conditions and encouragement.

Last, to rule out that candidates did not turn down low-paying jobs because of a liquidity constraint, we unconditionally provided 40,000 UGX (≈$12) to a random subset of MYF participants, with the recommendation that they use it to finance their job search. Contrary to our expectations, the cash transfer had no differential impact on short-run outcomes but attenuated the effects at one year. While the additional cash had no effect on the frequency of the student-mentor conversations, it shifted more of the conversations to actionable search tips and crowded out information about entry conditions and encouragement. Students provided with the cash transfer were consistently more likely to discuss actionable search tips with their mentors and to report search tips as their main takeaway. Once again, this finding confirms that students who learned about entry-level market conditions and wage-growth opportunities benefited most from the program.

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

This paper contributes to four strands of literature. The first of these is the extensive literature on the effects of active labor market strategies as a mean to decrease youth unemployment in low-income areas. Two sub-strands of this literature relate closely to our work: (i) a series of studies investigating ways of reducing information and search frictions to which we contribute by proposing a low-cost and scalable method of delivering trustworthy and individualized information to first-timejob seekers;[1] (ii) a series of studies evaluating the effectiveness of vocational education. Across low- and middle-income countries, subsidies for vocational education are one of the leading policy responses to promote upskilling and employability and reduce youth unemployment. These costly programs have proven effective at generating productive human capital and promoting employment in several contexts (Alfonsi et al., 2020; Maitra and Mani, 2017) but not everywhere.[2] Moreover, even when (certified) skills raise the likelihood of regular employment, overall job placement rates are low, resulting in underutilized talent (Bandiera et al., 2023). We examine the student population transitioning from the vocational education system to the labor market. This is a crucial transition with enduring effects on the future career paths of the students. By analyzing the content of the conversations between students and their mentors, we identify the labor market frictions that prevail among young, skilled job seekers in urban labor markets in Uganda. In addition, we provide an effective, scalable and low-cost policy solution, that enhances the efficacy of vocational training programs.

Second, this paper contributes to the literature on mentorships. Over the past decade, these programs have become widespread. They are often institutionalized by schools and universities in high-income settings to improve the academic achievements of at-risk adolescents. As a result, the mentorship literature focuses on programs that typically involve adolescents and attempt to improve high school graduation and college enrollment, and minimize risky behaviors (Rodriguez-Planas, 2012; Falk et al., 2020). This body of literature focuses less often on job seekers or workers, and, to our knowledge, none of these studies examines settings in low-income countries. Such studies have demonstrated that mentorship has a moderately beneficial impact overall. However, due to the cross-sectional, non-experimental nature common to most of these papers, it is unknown whether significant correlations be-

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[1] Abel et al. (2019); Abebe et al. (2021); Altmann et al. (2018); Banerjee and Sequeira (2020); Beam (2016); Beam et al. (2016); Behaghel et al. (2014); Belot et al. (2019); Bruhn et al. (2018); Carranza et al. (2022); Cottier et al. (2018); Dammert et al. (2015); Groh et al. (2016); Jensen (2012).

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

tween mentorship and outcomes demonstrate a causal effect. In addition, remarkably little is known about how exactly a mentor operates and what aspects of a mentor are beneficial in terms of labor market outcomes. Our contribution to this literature is twofold. First, we rigorously evaluate the effectiveness of one such program in a high-stakes setting in a low-income country, thereby filling a gap in the literature. We show that these programs have great potential in contexts characterized by a high degree of labor market informality and a high reliance on connections to navigate the labor market. Second, through close observation of the mentor-mentee interactions, high-frequency data collection, and the random assignment to mentors, we develop a framework to analyze and test what is useful, making ours one of the first studies to delve into how mentorship relationships actually work.

Third, this paper contributes to the literature on behavioral job search; this is a nascent and fast-growing literature that studies how job seekers' misperceptions about their own prospects delay their exit from unemployment and career progression. Recent survey data from high-income countries reveals considerable overconfidence among job seekers regarding their prospects (Spinnewijn, 2015; Mueller et al., 2021; Potter, 2021). New research in low-income settings documents similar findings and warns that distorted beliefs can damp the effectiveness of active labor market policies (Abebe et al., 2023; Kelley et al., 2024; Chakravorty et al., 2024; Bandiera et al., 2023; Banerjee and Sequeira, 2023; Jones and Santos, 2022). Two previous attempts at correcting job seekers' overly optimistic beliefs are the work of Jones and Santos (2022) and Chakravorty et al. (2024), who rolled out targeted information interventions to university graduates in Mozambique and vocational students in India. The first study finds that information shared via SMS has no impact on employment outcomes, as optimistic expectations are barely affected. The second study shows that information sharing that corrects beliefs also reduces the accumulation of human capital, as optimistic students leave the program. Four recent studies accelerated natural learning, though treated job seekers did not achieve higher employment rates or wages in any study except that of Abebe et al. (2023). Kelley et al. (2024) found that high expectations led to voluntary unemployment as job seekers awaited better opportunities. Banerjee and Sequeira (2023) noted that a job search subsidy reduced expenses, prompting more intensive searches that led to impatience and a shift toward lower-paying local jobs. Similarly, Bandiera et al. (2023) observed that low callback rates led workers to become discouraged, lower their job expectations and search less and for lower quality jobs. This study presents the first successful debiasing method that avoids discouragement.

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

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 Context

2.1 The Ugandan Labor Market

We study three urban labor markets in Central and Eastern Uganda. Like many others across Sub-Saharan Africa, they are characterized by high rates of youth underemployment, job turnover, and job separation (Donovan et al., 2023). Most youths, including skilled ones, fail to climb the job ladder—their employment is characterized by transience and informality. The relative magnitudes of the supply- and demand-side imbalances are unclear. Firms often cannot recruit workers who satisfy their needs. Simultaneously, jobseekers are overly optimistic about their prospects; the frequency of their failures to obtain their ideal employment may lead to indefinite withdrawal from the job market (Bandiera et al., 2023).

2.2 Study Population

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 still fails to clear (Uganda Bureau of Statistics, 2018).

Students/Job seekers Our sample comprises vocational students about to enter the labor market. Specifically, we surveyed the 2019 cohort of students enrolled in the National Certificate Program at five VTIs across Eastern and Central Uganda.[3] The National Certificate 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, 40% are female, the majority are single and largely of Christian faith. The sample is relatively heterogeneous in terms of socioeconomic background—the distribution of households' assets and urbanity is wide. About 50% of the students worked before the treatment rollout, almost exclusively in casual occupations.

Mentors These are 158 workers who we identified as "successful," by which we mean that they held stable employment in their sector of training. We connected these workers to randomly selected students during their labor market transition. We assigned each mentor to between one and five treated students randomly by strata, where the strata are VTI of attendance and occupation. The mentors are 25 years old on average, 35% are female, and

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[3] 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.

they have an average tenure in the labor market of three years. One of our goals when designing the MYF program was to connect students with successful workers to whom they could relate and feel comfortable asking for help or advice. For this reason, we settled on mentors that graduated two to five years prior to the student's job market entry.[4] We also sought to minimize the probability of excessive recall bias. These individuals have substantive experience in the labor market without being too senior relative to current students. Like most others, our partner VTIs do not systematically track their graduates and do not keep organized and updated records of their contact information. To identify successful alumni, we collected and digitized hard copies of thousands of phone contacts and old registries. Online Appendix A describes the mentor selection process in detail.

2.3 The School-to-Work Transition and Associated Frictions

In Uganda, 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.[5] 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.

Similar to findings in other contexts, we document distorted beliefs among the entire cohort of students over their future labor market prospects. In Panel A of Figure 1, we document a striking optimism bias among job seekers with respect to entry-level jobs and specifically to the mean wage distribution of offers. (The panel structure of our data allowed us to provide monetary incentives that rewarded prediction accuracy at the individual level.) This upward bias held throughout the entire VTI training: expected first-job salaries at baseline were much higher than realized average salaries. On average, students realized earnings at first job were just 14% of their expectations. When compared to realized earnings after one year, the share raises to 65%, suggesting that optimism is pervasive and not only relevant to their first spell.[6] We tracked students' expectations over job offer arrival rates and the distribution of expected earnings. We did so at the start of their programs, a year into their

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

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

[6] Only marginally less striking patterns occur if we compare students' expectations to mentors' realizations five years prior, an exercise that that allows us to rule out any Covid-19 specific effect.

program, and twice in their second year.[7] This finding contributes to the recent evidence from other low-income settings (Banerjee and Sequeira, 2023; Bandiera et al., 2023) as well as high-income ones (Spinnewijn, 2015; Mueller et al., 2021) that labor market entrants are overly optimistic about their labor market prospects.

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 actual transition matrices of employment pathways from three months to one year after the school-to-work transition. In comparing the two, we learn that students (i) undervalue unpaid (or negatively paid) initial job spells, which they consider as likely to lead to stable wage employment as an initial spell of unemployment; (ii) underestimate the risk related to being unemployed at three months after graduation; and (iii) underestimate the overall unemployment prevalence at one year.

These beliefs are consistent with a model of thin labor markets, in which young job seekers are primarily encounter people with jobs but less frequently know starting salaries. If students' beliefs lead them to target jobs that are beyond their reasonable reach, they may have reservation wages that are too high for prevailing labor market conditions. The same holds true if they underestimate the future value of a low-paying first job. These "unicorns"—entry-level positions offering ample pay and opportunities for internal promotion—are simply not the median outcome for young job seekers, including skilled ones. Taken together, we interpret this as evidence of overoptimism regarding entry-level wages and a general lack of understanding of the path to stable wage positions.

3 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.

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

3.1 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 regressions. In all our choices, we followed the principles highlighted by Bruhn and McKenzie (2009) and Athey and Imbens (2017).[9] The identification strategy for 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 18% 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 (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

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[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] In Online Appendix C, we provide detailed information from our pre-analysis plan regarding the selection of "strata variables" and "balance variables"—the set of variables for which we require 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 available graduates. In such instances, students were paired with successful graduates from the VTI nearest to their own.

and after graduation, respectively (Figure A.3). These sessions, initiated by the mentor, were private conversations between the 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 (Figure A.4).

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 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 6.2. For most analyses, we pool T1 and T2 and refer to the effects as those of the MYF program.

3.2 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. Sixty-six percent 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. Enumerators' observations of student-mentor conversations also assessed ease and engagement. We observed high satisfaction rates across all indicators and student-mentor pairs.[13] Similarly, the identification and transportation indexes, adapted

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[11] Noncompliance was mostly due to the inability to contact some students. In Table A.6 we show that non-compliers (56 students) are no different at baseline on observables.

[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

from Banerjee et al. (2019), were consistently high.

We validate these findings using our exclusive data source: 512 audio recordings of the mentorship sessions, transcribed and translated when necessary.[14] Sentiment analysis with 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 link formation is primarily inhibited by students and mentors from different VTIs, age gaps, and common socioeconomic background.

4 Results

4.1 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

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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.

[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.

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).[16] λ_s are strata fixed effects. $\epsilon_{i,s,t}$ is the error term. We cluster errors at the strata level. 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 715 accredited VTIs in Central and Eastern Uganda (1,270 nation-wide), 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 render 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.

4.2 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 (either working or searching for a job). In other words, treated students are significantly less likely to have exited the labor market (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.

4.3 Transitions and Medium-Run Labor Market Outcomes

le 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

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

[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.

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 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 superior future employment opportunities. Control students do not take up apprenticeships as fast. They continue searching, and many of them become discouraged, resulting in a 27% greater likelihood of having left the labor market three months after graduation and subsequent depreciation in human capital. After one year, the 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 $8.57 (p-val .10) at the 50th percentile, $14.29 (p-val .06) at the 75th percentile and $20.00 (p-val .04) 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).

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[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, 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 6.2.)

5 Mechanisms

Can mentors' success be linked to their ability to correct distorted beliefs without causing discouragement? In this section, we present a stylized model to guide the interpretation of our results and derive testable predictions to learn through which mechanisms have the mentors improved young job seekers' labor market outcomes.

5.1 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 GPT-4o model developed by OpenAI to label the topic of each sentence within a conversation.

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 the GPT-4o model with natural language descriptions for each of the four categories plus two: information, search tips, encouragement, referrals, neutral, and backchannel. Sentences classified by the GPT model as backchannel are reclassified into the category of the non-backchannel sentence that immediately precedes them. Neutral is the residual category.[21] Although some researchers argue that fine-tuned transformer models can outperform generative large language models like GPT, we refrain from supervised learning, due to our relatively small corpus and to avoid subjective input during training data labeling. 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. Several things can be deduced from this figure. First, job referrals, including both the mention of current vacancies the mentor is aware of and the promise of future job referrals, were less frequent than we anticipated. Second, the majority of conversations discussed all three remaining categories of support, with information about

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[21] In a previous version of this paper we relied on the at the time state-of-the-art 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) have demonstrated that GPT 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).

entry-level jobs and encouragement having the highest correlation in terms of frequency. Lastly, no other major topic was discussed.[22] 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 greater for the encouragement category than for the search tips and information on entry requirements.

5.2 An Illustrative Model

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 finite-timed version of the seminal search model from McCall (1970) in which search occurs sequentially. We adapt this model to incorporate subjective beliefs about the labor market, following Cortés et al. (2023). 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 $F(w) \sim N(\mu, \sigma)$ 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 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 2.3, we assume that job seekers do not know μ, the mean wage offer they will receive, nor ω, the wage evolution

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[22] Manual reading of the content categorized as neutral suggests that (1) the vast majority of the neutral sentences consist of initial greetings, personal introductions, exchange of phone numbers, resolutions of issues related to the poor network quality in the call, or simply short sentences that are hard to classify, such as "yes, that completely makes sense"; (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.

given by the experience premium.[23] Instead, they form beliefs about μ and act based on a perceived probability distribution $F(\hat{\mu}, \sigma)$ of the entry-level wages. Likewise, they form beliefs about 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 permit wage growth, the value of employment will depend on the beliefs over the job ladder:

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

The value of unemployment instead can be written as:

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

and it depends on the job seeker's beliefs because the expectation is taken over the subjective offer distribution $F(w; \hat{\mu}, \sigma, \hat{\omega})$. Given a draw for search costs c, the job seeker must determine whether or not to search. If they choose not to search, they receive no offers, whereas if they do search, they face a probability λ of receiving an offer. By comparing the returns to search,

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[23] Our framework comprises of distorted beliefs and subsequent learning about the mean-wage offer distribution at entry. Alternatively, biases in beliefs about one's job search prospects have been modeled as biases in assumptions regarding the arrival rate of job offers, *lambda* (Spinnewijn, 2015; Bandiera et al., 2023). Students in our study appear to have a good grasp of the timing requirements for obtaining a first job. What they fail to account for is the type of position (traineeship versus temporary versus permanent) and earnings associated with the first job. Students reported seeking permanent, paid employment, despite the likelihood of obtaining such a position for an individual with their age and skill profile being extremely low. Similarly, Banerjee and Sequeira (2023) find that young job seekers in South Africa expect to earn nearly twice the median actual salary of individuals with similar profiles, primarily due to an overestimation of the likelihood of obtaining a high-wage job.

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:

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

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:

$$W(w_R(\hat{\mu}, \hat{\omega}), \sigma, \hat{\mu}, \hat{\omega}) - U(\hat{\mu}, \hat{\omega}) = 0 \tag{4}$$

5.3 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; $\lambda \uparrow$.
2. It can rectify beliefs over the mean offer distribution of their first job. As we saw in section 2.3, students are overly optimistic about the mean wage offer. The mentor can correct overly optimistic beliefs by sharing information about entry-level jobs' characteristics, therefore lowering $\hat{\mu} \downarrow$.[24]
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 $\hat{\omega} \uparrow$.

We derive predictions on the reservation wage behavior and discouragement effects, depending on which of these mechanisms prevail. The proofs for the propositions listed below 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 the cutoff search strategy ($c^*(\hat{\mu}, \hat{\omega}) \uparrow$).

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[24] 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.

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.[25]

> **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 ($\hat{\mu}$). 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. 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 graduates of heterogeneity in wage dynamics, including the fact that unpaid 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. When optimizing their lifetime income, we anticipate

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[25] 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.

that treated graduates who received encouragement emphasize wage growth rather than just starting wages.

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. Using our survey data, we now test empirically which dominates.

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

We start by examining the direct impacts the mentorship program had on job 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: treated students are 25% less likely to turn down a job offer while looking for their first job. While we did not collect information on the exact wages offered, we asked the reasons for why each rejected offer was turned down. The three primary reasons were lack of learning prospects, low wages, and long commutes. The remaining refusals were due to personal reasons, such as family obligations. With the caveat that the sample size decreases greatly when we condition on having declined an offer, we find that treated students were much more likely to decline a job offer because it did not provide sufficient learning potential. While the difference is not statistically significant at the standard levels (p-value .19), the magnitude of the effect is large (6.4 p.p. on a control mean of 10%), suggesting that power may be what prevents us from making definitive statements. On the contrary, we see no difference in treatment and control groups when comparing the likelihood of turning down a job offer because of distance to the workplace or any other reason. The heterogeneity panel of Table A.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 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 highlights the significance of the 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 and confidence, 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.[26] 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 tenacity, and eventually get retained (promoted) or transferred to a better job.

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

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 6, we carry out two validations exploiting additional randomization features of the design and data we gathered on the students, their network, and the mentors.

6 Validations and Extensions

6.1 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:

$$Y_{i,j,d} = \sum_{j} M_{ij}\gamma_j + \lambda_d + \mu_i \tag{5}$$

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 .00 and .03 for the short run labor market index and the career trajectory index, respectively). Although the overall sample is large, the sample cells are small within each mentor, leading to finite sample bias. Consequently, the $\hat{\gamma}$ obtained via equation 5 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).[27] 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, .670 for the short run index and .699 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 .670 and the medium run index by .699 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 the particular set of three channels for explaining this heterogeneity. Our three channels are exactly the three main types of support that emerged during the conversations, which map onto the mechanisms proposed in the illustrative model. What we are after is:

$$Y_i = \beta_0 + \beta_1 Info_i + \beta_2 Enc_i + \beta_3 Search_i + X_i'\delta + \epsilon_i \tag{6}$$

where Y_i is the outcome of interest for student i. We focus on the four standardized indexes described above. $Info_i, Enc_i$ and $Search_i$ are three indicator variables for whether the mentor provided mainly information on entry conditions, encouragement, or search tips, as measured by the students' main takeaway. However, running equation 6 would not necessarily give us the causal effects of conversation content on the outcomes of interest. Although different mentors are more likely to provide information versus encouragement versus search tips, conversations were non guided; i.e., mentees could affect them with their questions and level of engagement.

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

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[27] 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.

The first stage regressions are:

$$Info_{i,j,d} = \sum_{j} M_{ij}\gamma_{j1} + \lambda_{d1} + \mu_i \tag{7}$$

$$Search_{i,j,d} = \sum_{j} M_{ij}\gamma_{j2} + \lambda_{d2} + u_i \tag{8}$$

$$Enc_{i,j,d} = \sum_{j} M_{ij}\gamma_{j3} + \lambda_{d3} + \tau_i \tag{9}$$

where M_{ij} are the 158 mentor indicators and λ_{d1}, λ_{d2}, and λ_{d3} the VTI and course duals fixed effects. The second stage regression is:

$$Y_{i,d} = \beta_0 + \beta_1 \widehat{Info}_i + \beta_2 \widehat{Enc}_i + \beta_3 \widehat{Search}_i + \lambda_d + \epsilon_i \tag{10}$$

where Y_i are the same outcomes of interest in equation 1 and $\widehat{Info}_i$, $\widehat{Enc}_i$ and $\widehat{Search}_i$ the fitted values from the first stages. The validity of this strategy relies on two assumptions. The first is the relevance of the instruments, which is violated if the 158 mentor dummies are uncorrelated with the three endogenous variables representing the main conversation content. We rule out weak identification using Sanderson and Windmeijer (2016). The p-values on three first stage F-statistics for excluded instruments are reported at the bottom of Table 5. We reject the null hypothesis of weak identification for all three endogenous regressors. First-stage F-statistics are always between 11 and 27, suggesting there is sufficient variation to be exploited in our instruments, even after partialling out the predicted value of the other two endogenous variables. The second assumption is the exclusion restriction, i.e., the assumption that the instruments (mentor assignment) directly affect outcomes only through the three channels identified. This assumption is violated if, for example, conversational contents exist that we are not accounting for but that affect the outcomes of interest. To test this assumption, we leverage the many orthogonality conditions (158 to identify three endogenous variables) and conduct the Sargan-Hansen overidentification test, where the joint null hypothesis is that the instruments are valid ones. We cannot reject the null for two out of four outcomes of interest,and the third and fourth ones are rejected at marginal significance levels, suggesting that we have identified most of what mediates the heterogeneity.

Results Table 5 reports the results on the four indexes. We confirm the findings from our main analysis: mentors who provided information about entry-level jobs as well as encouragement were the most effective in the short run. In the medium run, the role of encouragement becomes even larger: the push to persevere and be patient pays persistent dividends toward students' career trajectories.

6.2 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.[28] 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.9 and A.8).

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 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).

6.3 Extensions

To inform the optimal design of mentorship programs, we explore how characteristics of the program design affected its success. First, we consider the impact of the number of mentees on a mentor's effectiveness, and we confirm that effectiveness decreases with each additional mentee after four. Additionally, we assess whether exposure to a more experienced mentor, who has conducted multiple sessions, differs from exposure to a first-time mentor. We not find evidence of any difference. Lastly, we test whether mentors who tailor conversations to each student are more effective than those who share similar information with all. By creating embeddings for each conversation and calculating the Euclidean distance between

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[28] 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 the direct observation of increased savings at endline 1, significantly higher among T2 students.

them, we find no conclusive patterns. Students who ask more questions and talk more appear to benefit more, but this may be their higher engagement and receptivity, not the mentor's higher personalization. We explain the lack of difference with the fact that these conversations are all relatively personalized, as all mentors graduated from the same course of study and work in the same labor markets that the students are trying to transition into.

7 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 \$6.15 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 mentor training and to thank them for participating in the mentor's check-in survey and mentoring sessions. Given that a mentor is connected to an average of 3.9 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 275%. The returns stay positive under extreme assumptions, reaching a positive minimum of 5% 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.

8 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, permit 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. Contrary to our expectations, neither direct job referrals nor the improvement of job seekers' search technology played a role. Students connected to experienced workers for personalized mentoring sessions become more realistic about their initial earnings and less pessimistic about wage growth opportunities and returns to experience. This shift in perception results in lower reservations wages and a greater willingness to accept unpaid work. As a result, they reject fewer job offers and start 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. Our findings highlight the role of distorted beliefs as an important channel by which information frictions decrease earnings and career advancement. They also emphasize the importance of balancing bad news with hope for better future outcomes to prevent *discouragement*, dropout from the labor force, and, particularly among skilled workers, human capital wastage. Finally, the program affordably increases the effectiveness of vocational training programs.

Main Tables

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

	Control		Treatment		
	N	Mean	N	Mean	P-value
Panel A: Socio-economic characteristics					
Age	466	19.87	645	19.84	.82
Gender (1=M)	466	.59	645	.60	.86
Christian	466	.83	645	.84	.64
Single	462	.90	642	.89	.33
Has Children	466	.02	645	.02	.97
Region of Origin: Central	464	.30	643	.32	.39
Region of Origin: Eastern	464	.54	643	.51	.40
HH Assets Index Above Mean	458	.42	643	.37	.11
HH Main Income Source Agriculture	464	.47	645	.47	.77
Panel B: Labor market history pre MYF					
Ever Worked Pre MYF	466	.53	645	.53	.82
Ever Worked in Training Sector	441	.07	614	.08	.39
Has Done Any Casual Work	464	.26	645	.25	.75
Has Done Any Wage Employment	464	.29	645	.30	.74
Has Done Any Self Employment	464	.08	645	.09	.65

Notes: The table reports means and robust standard errors from OLS regressions in parentheses. P-value on T-test of equality of means with the control group in Coulmn 5. 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 had an active working relationship, neither held any contractual obligations toward the other, and the principal requested agent on a need-based basis.

Table 2: ITT Estimates: Short Run Labor Market Outcomes

	Short Run					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***	1.747	19.227***	.150***
	(.019)	(.540)	(.026)	(2.232)	(4.872)	(.050)
	[.004]	[.008]	[.004]	[.078]	[.001]	[.004]
Control Mean	.21	16.15	.54	12.33	78.07	-.00
Treatment Effect (%)	-26.57	7.85	15.11	14.16	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, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). 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. 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.

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	6.149*	.135**
	(.019)	(.033)	(.022)	(.925)	(3.601)	(.057)
	[.072]	[.072]	[.152]	[.348]	[.074]	[.072]
Control Mean	.18	.37	.26	12.50	34.84	.00
Treatment Effect (%)	22.87	20.70	-9.53	2.12	17.65	-
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 the internship (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. 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: 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	-11.581***	.071**	-.045**	.029**	-.009	.002	-.086	-8.334**	-.027	-.236***
	(3.357)	(.031)	(.022)	(.014)	(.068)	(.065)	(.078)	(3.951)	(.075)	(.077)
	[.006]	[.058]	[.058]	[.058]	[.623]	[.623]	[.182]	[.058]	[.556]	[.010]
Control Mean	36.76	.54	.18	.93	-.00	.00	.00	27.69	.00	.00
Treatment Effect (%)	-31.50	13.09	-24.85	3.10	-	-	-	-30.10	-	-
N	737	739	890	934	934	934	936	887	936	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)
Entry Conditions	-.09	-.45***	.24**	.06
	(.11)	(.14)	(.11)	(.12)
Encouragement	-.04	-.17*	.19**	.24***
	(.07)	(.10)	(.08)	(.09)
Search Tips	.11	-.27**	-.05	-.05
	(.10)	(.13)	(.11)	(.12)
Control Mean	.00	.00	-.00	.00
N Mentors	158	158	158	157
N	936	668	934	844
F-Test of joint significance (pval)	.52	.01	.02	.03
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
Sargan (pval)	.39	.01	.07	.09

Notes: In this table, we report the 2SLS estimates of the effect of each type of support on our outcomes of interest, collected in four indexes. These are obtained by estimating equation 10 on our four main indexes.

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	10.837**	.256***
	(.024)	(.042)	(.030)	(1.132)	(4.186)	(.074)
T2 (MYF+Cash)	.025	.041	.006	-.523	1.954	.028
	(.025)	(.042)	(.031)	(1.034)	(3.795)	(.071)
Control Mean	.18	.37	.26	12.50	34.84	.00
T1 Effect (%)	32.69	30.93	-22.79	9.17	31.10	-
T2 Effect (%)	13.57	11.01	2.32	-4.18	5.61	-
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 four outcomes for which there are significantly different treatment effects. Below each coefficient estimate, we report the strata-level clustered standard errors. For each outcome, we report the mean outcome for the control group and each treatment effect. At the foot of each column, we also report the p-value from an F-test of the null hypothesis that the impact of MYF alone is equal to the impact of MYF + Cash. All regressions control for strata dummies, the balance variable *ever_worked* as well as control variables selected following the post-double-selection LASSO procedure set forth in Belloni et al. (2014). For a detailed description of the outcomes, please refer to Tables 2, 3 and 4.

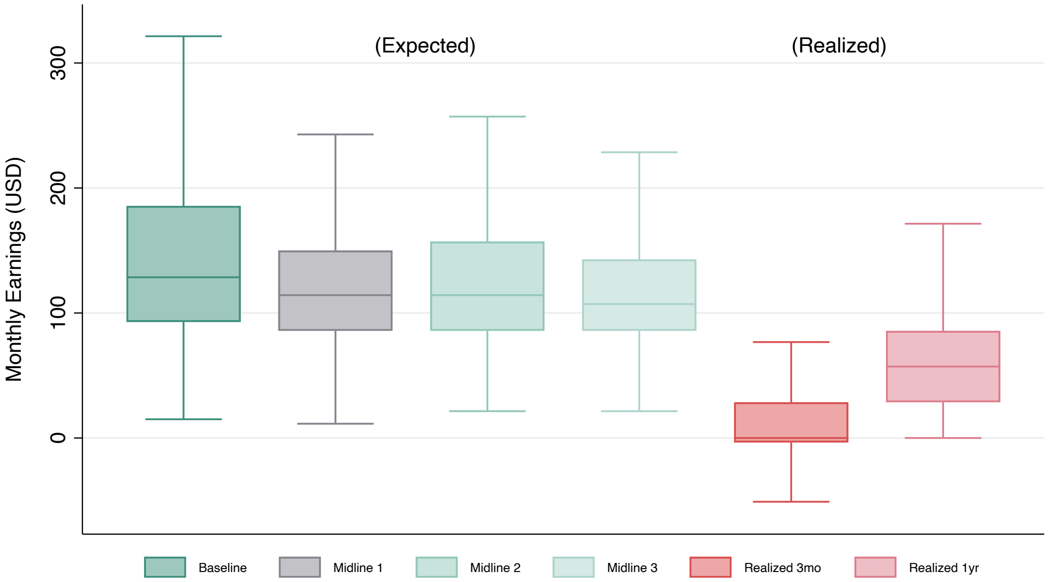
Table 7: IRR

	All students
Social discount rate	.05
Remaining expected productive life	10 years
Panel A. External parameters	
Total cost per individual	23.44
· Student opportunity cost (2 days of work)	4.99
· Alum opportunity cost (1 day of work, ext. interaction only)	3.45
· Program costs	15.00
Panel B. Estimated earning benefits	
Extra monthly earnings	6.15
NPV change in steady state earnings (from model estimates)	546.33
Benefits/cost ratio	24.31
IRR	3.00
Panel C. Sensitivity	
Sensitivity to different expected remaining productive life of beneficiaries	
Remaining expected productive life = 5 years	3.00
Remaining expected productive life = 2 years	2.76
Sensitivity to different earnings	
Opportunity costs = 90th percentile	2.20
Opportunity costs = max	0.24
Opportunity costs = double max	0.05
Sensitivity to different engagements	
5 days of work foregone	1.60
7 days of work foregone	1.19

Main Figures

Figure 1: Overoptimism

Panel A: Expected and Actual Monthly Earnings | Employment



Panel B: Expected and Actual Job Ladders

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

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

Figure 2: Project Timeline

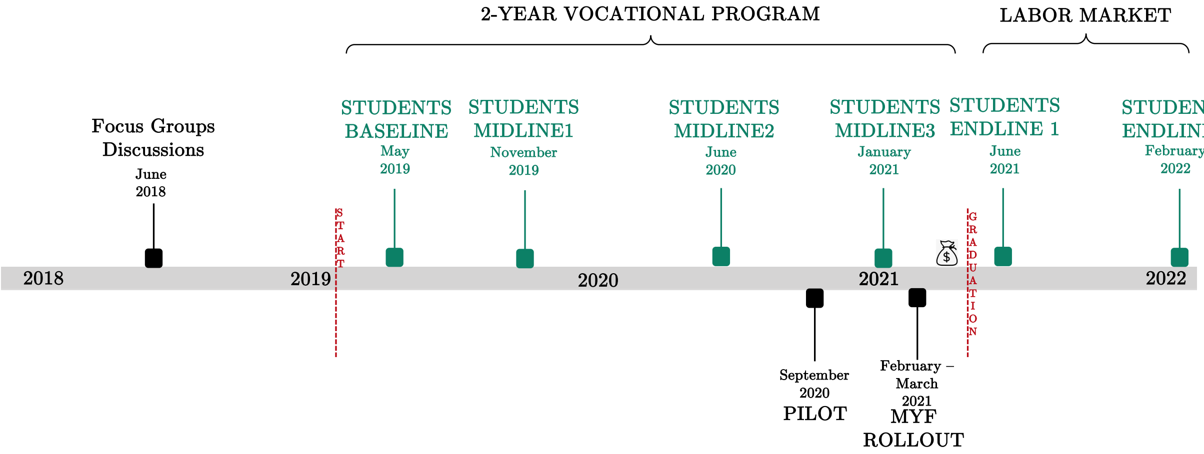


Figure 3: Experimental Design

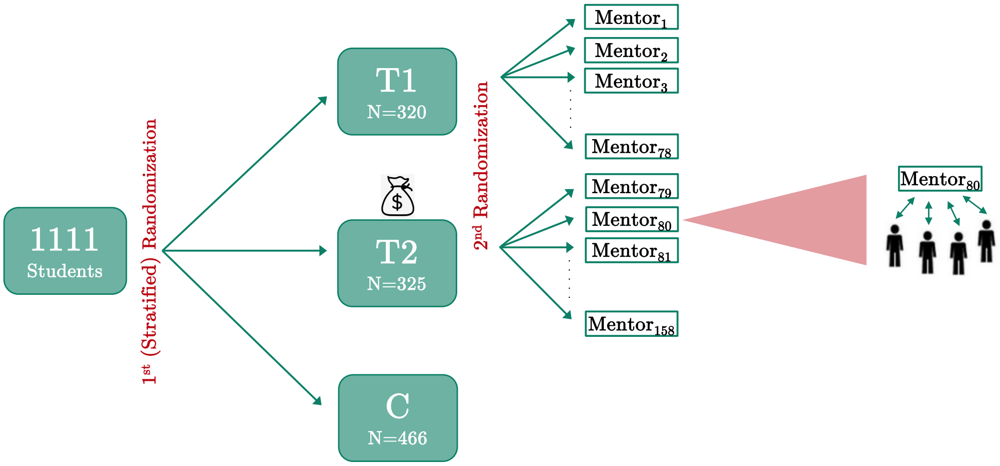
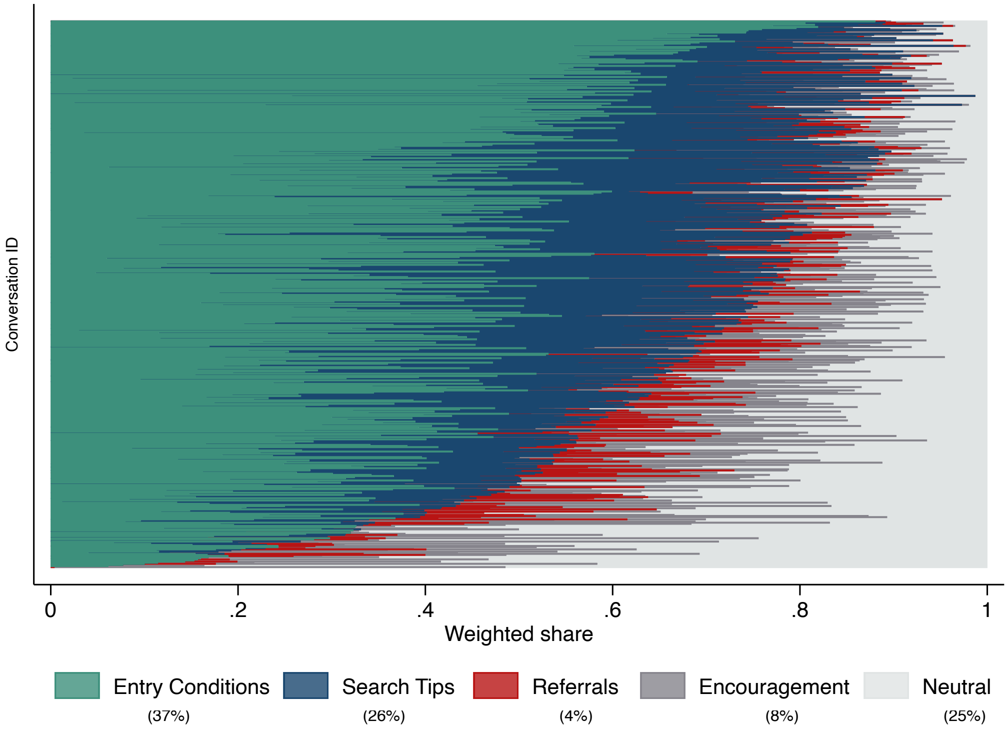
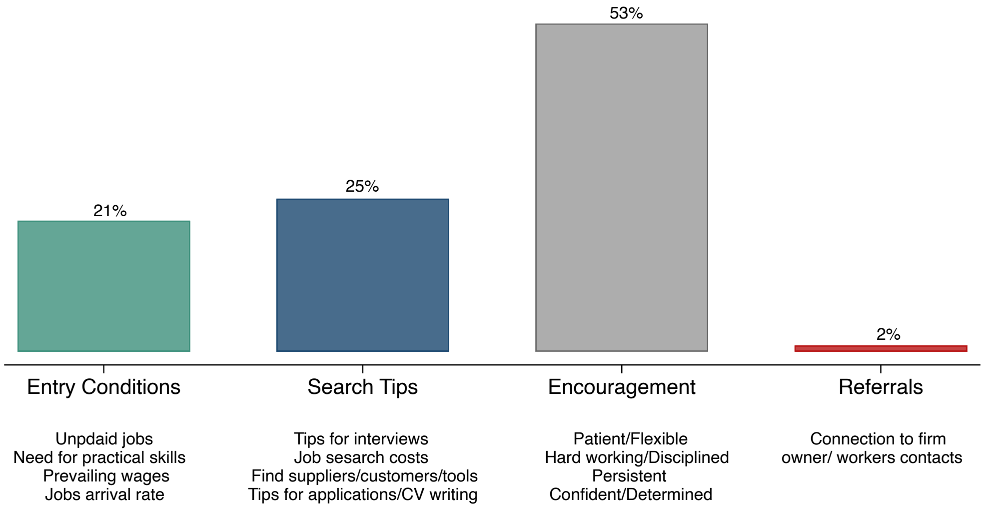


Figure 4: Conversations' Content and Takeaways

Panel A: Coded Conversation Content From Audio Recordings

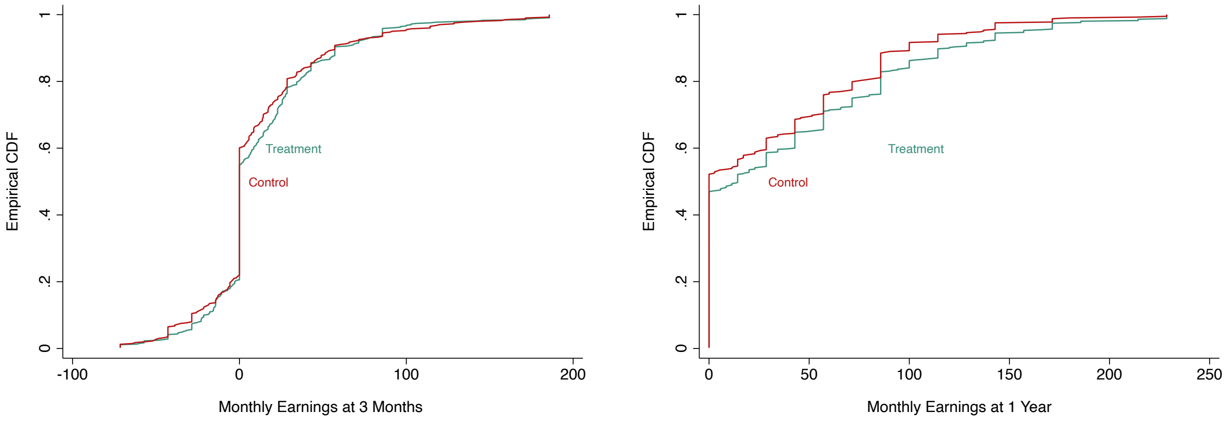


Panel B: Students' Main Takeaway



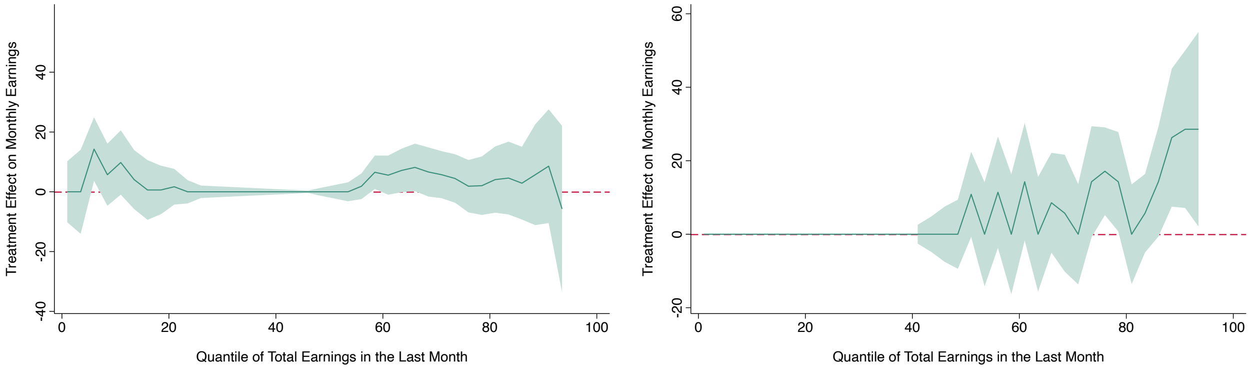
Notes: Panel A shows the raw conversation data from MS1: each observation represents a conversation, with sentences weighted by word count. Panel B displays the distribution of students' main takeaways from their mentor conversations. Each bar indicates the percentage of students whose takeaway falls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

Figure 5: Treatment Effects on Monthly Earnings



Panel A: Empirical CDF at 3 months

Panel B: Empirical CDF at 1 year



Panel C: QTE at 3 months

Panel D: QTE at 1 year

Notes: Panel A and B show the empirical distributions of monthly earnings 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. 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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A.1 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	-0.17	(-0.62)	-0.15	(-0.53)	-0.25	(-1.43)
Language	-0.29	(-1.01)	0.06	(0.18)	-0.27	(-1.23)
District of origin	0.05	(0.19)	0.07	(0.23)	0.36^{**}	(1.98)
VTI	0.67^{**}	(2.01)	0.67^{**}	(2.14)	0.38^{*}	(1.76)
Gender	-0.36	(-0.94)	-0.30	(-0.73)	-0.06	(-0.23)
Panel B - Sum of:						
Age	0.04	(1.18)	0.06^{*}	(1.90)	0.03	(1.18)
Household Asset Index	-0.14	(-1.64)	-0.08	(-0.92)	-0.04	(-0.73)
Panel C - Difference in:						
Age	-0.07^{*}	(-1.72)	-0.07	(-1.60)	-0.06^{*}	(-1.82)
Household Asset Index	-0.25^{*}	(-1.79)	-0.04	(-0.29)	-0.12	(-1.14)
Observations	601		600		601	

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

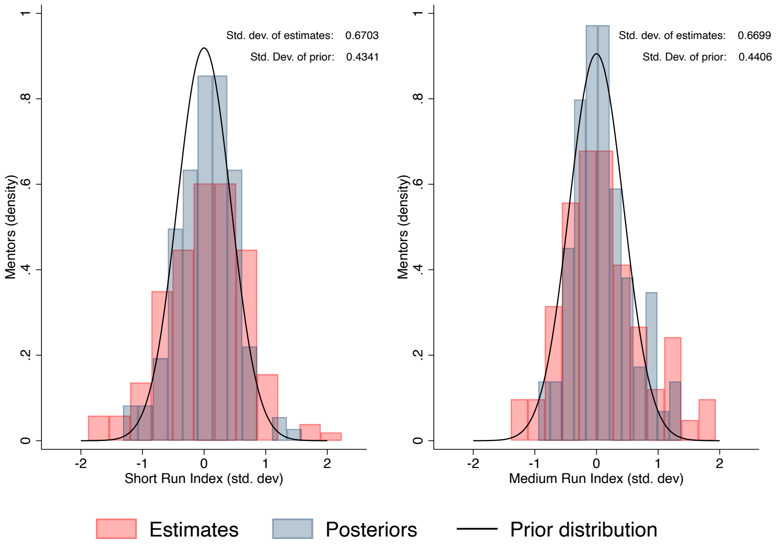
Notes: This table shows reduced-form estimates of the effects of MYF on various pathways to employment in year 1. There are nine possible pathways, although we only report those with a minimum of 5% of the total number of students (the treatment effects on the pathways we do not report are not statistically 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	-23.525***	.139**	-.114	.045	-4.521
	(5.985)	(.060)	(.069)	(.028)	(6.398)
× Pre-MYF Expected Earnings Below Median	1.426	.023	-.057	.023	-6.052
	(3.130)	(.046)	(.048)	(.028)	(5.711)
Difference	-24.951	.116	-.057	.022	1.530
P-Value	.000	.131	.384	.538	.863
N	737	739	890	934	887

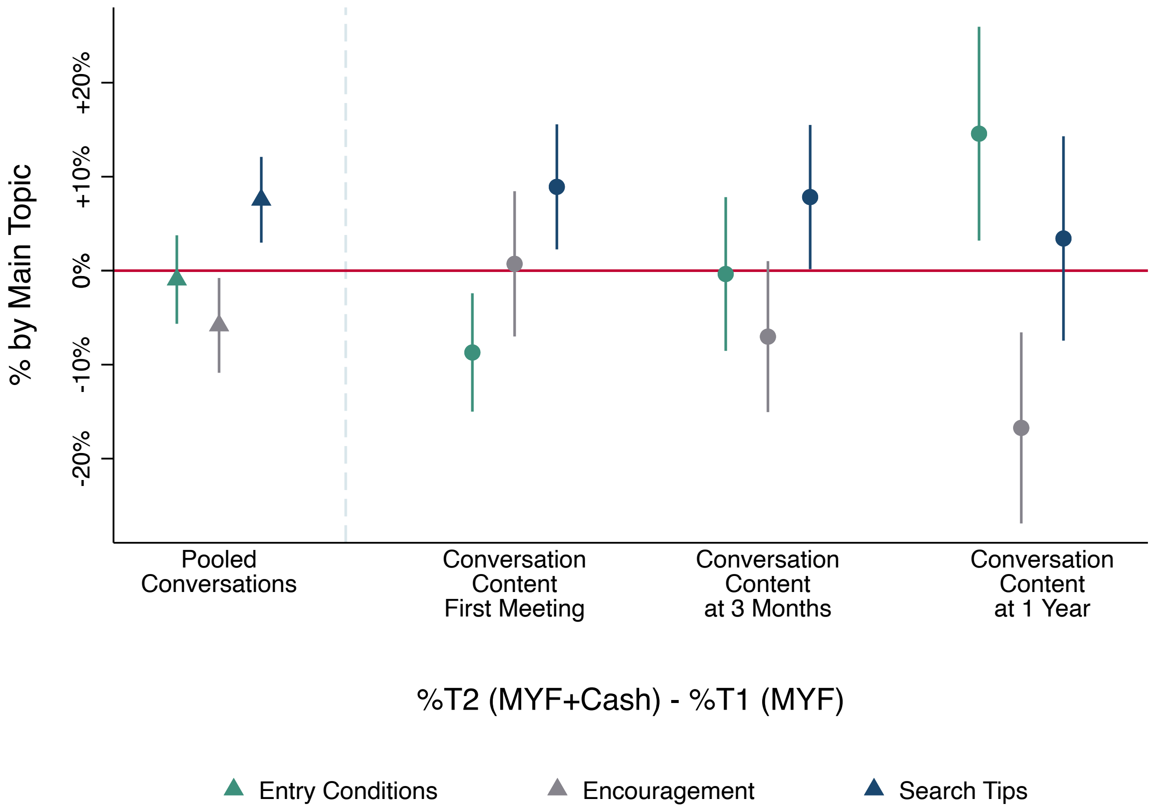
Notes: In this table, we report the intent-to-treat estimates of the direct effects of the MYF program on willingness to accept a job and job search outcomes. We do so for the overall sample (in the top panel) and in two different samples: those with pre-MYF above median and those with below median expectations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