ISSN 2282-6483

Alma Mater Studiorum - Università di Bologna
DEPARTMENT OF ECONOMICS

# The Impact of a Peer-to-Peer Mentoring Program on University Choices and Performance

Stefania Bortolotti
Annalisa Loviglio

*Quaderni - Working Paper DSE N°1192*

# The Impact of a Peer-to-Peer Mentoring Program on University Choices and Performance

Stefania Bortolotti*
*University of Bologna & IZA*

Annalisa Loviglio†
*University of Bologna & IZA*

March 4, 2024

**Abstract**

We study the impact of a personalized mentoring program on university enrollment choices and academic outcomes. Conducting a randomized controlled trial among 337 high school students, we find that the program significantly influences students' decisions, increasing the likelihood of choosing a field aligned with their mentor's by 22 percentage points, representing a 45% increase from the baseline. Notably, the program also shifts preferences towards STEM/Economics fields, enhancing prospective wages by 3.1-3.7%, without negatively impacting university performance. These findings underscore the mentorship's potential to guide students towards more informed and beneficial educational choices.

**Keywords:** mentoring, university choices, RCT.

**JEL Classification Numbers: A20, C93, I23, I26**

*University of Bologna, Department of Economics, Piazza Scaravilli 2, 40126, Bologna, Italy; e-mail: stefania.bortolotti@unibo.it

†University of Bologna, Department of Economics, Piazza Scaravilli 2, 40126, Bologna, Italy; e-mail: annalisa.loviglio@unibo.it

# Non-technical summary

Selecting a university degree and field of study plays a crucial role in shaping individuals' career trajectories and earnings potential. Despite this, choices made by students often do not solely aim to maximize income, facing numerous decision-making frictions. Italy, with one of the lowest percentages of young adults holding university degrees in the EU, sees only a third of university enrollees graduate, and a significant portion of graduates express regret in their program choice. This dissatisfaction and high dropout rate highlight the need for better information and guidance in transitioning from high school to higher education.

This paper explores the impact of a mentorship program aimed at aiding students in choosing university majors, with mentors from quantitative fields facilitating open-ended online discussions. This personalized approach seeks to address individual student needs, potentially mitigating the information gaps and decision-making frictions present in the Italian educational context.

The study found that the mentorship intervention increased the probability of students enrolling in the same field as their mentor by 22 percentage points, a 45% increase over the control group. Mentors plaid both a reinforcement and an attraction role. On the one hand, mentees matched with a mentor from their preferred field at baseline are more likely to stick with their initial choice at endline. On the other hand, mentees matched with a mentor from a field ranked second or third at baseline, are more likely to change their preferences and choose the field of the mentor at endline.

Finally, the intervention shifted student preferences towards STEM/Economics, leading to a prospective wage increase of 3.1-3.7%, or 52 to 64 euro per month. Importantly, it did not adversely affect academic performance, even among students who opted for more selective degrees.

# 1 Introduction

Decisions about investing in human capital, including the choice of tertiary education enrollment and field of study, significantly influence future job prospects, career paths, and earnings (Kirkebøen et al., 2016). However, evidence overwhelmingly suggests that individuals do not strictly maximize income in these decisions (Heckman et al., 2018). Even high-achieving students may avoid applying to selective programs due to inadequate information (Hoxby and Avery, 2013), while misperceptions of personal ability can derail educational trajectories (Avery et al., 2018; Bobba and Frisancho, 2022; Bobba et al., 2023). Furthermore, perceived non-pecuniary benefits—such as work-life balance, academic environment, and family approval—often play a crucial role in tertiary education decisions (Zafar, 2013; Wiswall and Zafar, 2015; Boneva and Rauh, 2017).

We focus on Italy, one of the EU countries with the lowest rates of young adults holding university degrees. The national statistics institute (ISTAT) reports that while half of the youth enroll in university, only a third graduate. Furthermore, satisfaction among those completing tertiary education is notably low; nearly one in three graduates would choose a different program if given the chance.[1] The high dropout rate and dissatisfaction with chosen programs signal concerns about insufficient information or guidance during the transition from high school to university. Contributing factors may include the absence of a centralized entrance system, complex enrollment procedures, a vast array of degree options, and inadequate counseling at the high school level. Information barriers are particularly impactful for students from disadvantaged backgrounds or those with limited access to firsthand information and role models.

This paper presents the findings from a field experiment assessing the effects of a one-to-one mentoring program on students' selection of university majors. Developed alongside the host institution's orientation office, the program facilitated encounters with successful and motivated undergraduate and graduate student enrolled in a quantitative field and volunteering as mentors. These online meetings were designed to be open-

[1] Data are based on the AlmaLaurea questionnaire administered by a large consortium of universities. See 5.1 in the Appendix for further details.

ended, allowing mentors to encourage mentees to pose questions. This approach aimed to personalize the mentorship experience, ensuring it met the individual needs of the students. Mentor-mentee pairs were encouraged to meet two or three times in the months leading up to the selection of a university degree program. The most common topics of discussion included: the curricula covered in the field, the admission tests and enrollment procedures, study techniques, and the exams, as well as social life, job prospects, and the mentor's satisfaction with their academic path.

To evaluate the impact of the intervention, we conducted a randomized controlled trial (RCT) among 337 high school students in their last year of high school from all over Italy. The mentoring program was mostly advertised during large online orientation events organized by the host institution, with a participation of roughly 20,000 students. To enrol in the mentoring program, students had to complete a baseline questionnaire where we collected background information, as well as students' most preferred field of studies. We use this information to match high school students with mentors; randomization into Control and Treatment group relied on program oversubscription. A few months after the intervention, and right before the start of the academic year, we run the endline survey where we collected the enrollment choices of our participants. We complement and validate these self-reported measures with administrative data about student's performance at the end of the fist year of university. Our primary interest lies in the field of study chosen by participants, rather than university enrollment per se, as all respondents opted to pursue university degrees.[2]

We report four main results. First, we analyze enrollment choices, as declared in the endline survey, and find a large and significant effect of our mentorship intervention. In our preferred specification, with mentor's fixed effects and controlling for the preferred field at baseline, the probability of enrolling in the same field as the mentor is 22 percentage points higher for treated students compared to those in the control group (an increase of 45% compared to the baseline). Results based on administrative data from the host institution confirm the sizable effect of the intervention; we estimate an increase with

[2]More precisely, all respondents opted to enroll in tertiary education. One respondent chose a two-year vocational education program, all the others chose university degrees.

respect to control ranging between 25% and 30%.

Second, we find that out mentor can play both a reinforcement and an attraction role. Mentees matched with a mentor from their preferred field at baseline are more than 20 percentage points more likely to confirm their choice at endline. Yet, being matched with a mentor from a field ranked second or third at baseline, significantly increases the chances of changing preferences and choosing the field of the mentor at endline.

Third, we document that we are not pulling students away from fields with better labour prospects. It is important to test which field of study the treated mentees would have majored in absence of the intervention to make sure our program is not generating undesired negative effects. We observe an increase in the likelihood of majoring in STEM/Econ fields, a decrease for Humanities, and no effect for Medical professions. To gauche a better understanding about labour market prospects, we also test the effect of the intervention on expected future earnings, based on the field of study. Our estimates suggest a sizable increase in the prospective wage of treated students, ranging from 52 to 64 euro per month depending on the specification. This corresponds to an increase of 3.1-3.7% in the average prospective wage compared to the control group.

Finally, we demonstrate that the intervention clearly did not negatively affect university performance, as measured by the end of the first year. This is an important finding and should not be overlooked, particularly since our mentees chose more selective degrees. Although a medium-term positive effect is not conclusively proven, evidence hints at an improved average completion rate among treated students, particularly due to enhanced performance in weaker students. The intervention notably decreased the number of students failing to achieve half of the required credits without significantly boosting the completion of the majority or entirety of their workload.

Our paper contributes to the existing literature on the impact of mentoring on educational outcomes. While mentoring is a common component in comprehensive educational programs that typically blend many components, such as incentives, tutoring, and mentoring (e.g., Rodriguez-Planas, 2012; Oreopoulos et al., 2017; Lavecchia et al., 2020), pure mentoring interventions are scarce. Beside non-experimental pure mentoring interventions (for a review, see DuBois et al., 2002), some recent studies have assessed mentoring's influence on proxies for labour market success among low-SES German students(Resnjanskij et al., 2024) and participation in the labour market among students from vocational schools in Uganda (Alfonsi et al., 2023). Falk et al. (2020) focuses on younger pupils (approximately 10 years of age) finding that mentorship increases the likelihood of choosing an academic track. Our participants are of similar age to the ones of Alfonsi et al. (2023); Resnjanskij et al. (2024), but we do not focus on the transition to the labour market. Instead, we are interested in educational choices like Falk et al. (2020).

Our mentoring program was briefer compared to previously described programs, with mentors and mentees meeting only a few times over approximately five months. Our intervention shares similarities with role model programs that briefly expose large groups to female role models in science or economics. Porter and Serra (2020) demonstrated that exposing students to successful women in economics for a one-time session positively affected their subsequent enrollment in economics courses. Similarly, Breda et al. (2021) found that classroom interventions could diminish gender stereotypes and encourage high-achieving females to pursue male-dominated fields. Despite the brevity and lack of personalization in these interventions, they report substantial effects, aligning with our findings. However, our intervention's focus was on the informational value of personalized mentorship rather than on gender.

The remainder of the paper is organized as follows. Section 2 describes the institutional background, the mentoring program, the data, and the characteristics of the sample. Section 3 outlines the empirical strategy. Section 4 reports the main findings based on responses to the baseline survey. In Section 5, we explore the medium- and long-term effects of degree program selection, utilizing data on prospective labour market outcomes and administrative records of university performance. Section 6 concludes.

# 2 Intervention and Experimental Design

## 2.1 Institutional setting

The Italian school system consists of five years of elementary education, three years of middle school, and four or five years of high school. Education is compulsory from ages 6 to 16, with tracking occurring after the $8^{\text{th}}$ grade. At this point, students can choose between three types of high schools: academic (*licei*), technical, or vocational. Students in Italy can access tertiary education regardless of the type of high school diploma they hold. According to the data provided by the Italian Ministry of Education (MIUR), roughly 50% of the students enroll in the academic track, 30% in the technical, and 20% in the professional one. Both secondary and tertiary educations are mostly provided by public institutions.

**University Entry.** After completing their high school diploma, students can enroll in a 3-year bachelor's degree or a 5-year single-cycle degree.[3] Upon completing a bachelor's degree, students can enroll in a two-year Master of Science program. The majority of students proceed to enroll in a master, after completing the 3-year bachelor. There is no centralized admission system, and each degree program has a separate acceptance process, often organized in multiple selection rounds from April to September. The only formal requirement common to all degree programs is that students must have graduated from high school. However, a standardized test is commonly required for entry, and the most widespread one is called the TOLC. Unlike other international tests such as the SAT, TOLC tests are not uniform for all majors, and different programs may require different TOLC tests focusing on specific topics. The test can be taken from February of the year before the actual enrollment to few weeks before the start of the program.

Virtually all degree programs fall under one of three categories: (i) free access with TOLC; (ii) limited access with TOLC; (iii) limited access with a national test or program-specific test. Programs with free access do not have a cap on the number of enrolled

[3] A five-year degree is limited to some specific fields, such as architecture, dentistry, law, pharmacy, and veterinary science. Medicine, however, is a 6-year degree.

students, and the standardized test is used only to assess the entry level of students. Limited access programs rank students based on the entry test and admit students based on the ranking until all available slots are filled. This means that the minimum score to successfully enter a course, depends on applicants' performance and vary from one intake to the other.

**The host university.** The mentoring program was hosted at one of the largest public universities in Northern Italy, attracting students from across the country (roughly half of the enrolled students come from regions different from the one where the university is located). The host university offers more than 100 bachelor or single-cycle programs and almost 150 master programs across all fields. The wide range of options available to students is due to the fact that there might be several programs within a single field (e.g., there are about 20 programs in engineering and 5 in economics). Bachelor's programs within the same field tend to have a significant overlap in terms of mandatory courses in the first two years. For the sake of simplicity, we will mostly focus on the field of study (e.g., architecture, economics, engineering, management, medicine, etc.) and not on specific programs.

Given the large number of degree programs and the specificity of some curricula, we will look at more aggregate levels of analysis throughout the paper. This is crucial since not all participants enroll in the host institution, and we need to find more aggregate measures that can be valid also for other institutions. Below we report the different levels of aggregation, starting from the broader and moving toward a finer definition:

- **Fields:** they tend to overlap with the departments offering the program. In this program, we consider a total of 17 fields and offer mentors for the following 9: Agricultural and Food Sciences, Architecture and Industrial Design, Biology and Environmental Sciences, Chemistry Physics and Mathematics, Computer Sciences, Economics and Finance, Management and Accounting, Engineering, and Statistics;[4]

[4]The host university uses a coarser definition for orientation purposes, referred to as macro-areas. However, we preferred to define a more precise unit of observation, since some macro-areas encompass very heterogeneous programs. These programs differ significantly both in terms of their curricula and in relation to their prospective labor market outcomes.

- **Degree programs class (as defined by MIUR):** each program offered in an Italian university must be approved by MIUR, which will assign a *degree program class* (classe di laurea). This code is assigned based on the study plan and the type of exams. In 2022, the host university offered degree programs belonging to 52 program classes (44 being for 3-year bachelor's program and 8 for 5-year master's program). The degree program class is useful to compare programs from different universities, which might have a similar study plan but different names. It is also key to enroll in master's programs, as access is commonly defined based on the program class;

- **University-specific degree programs:** while the main content of a degree program can be very similar across universities, names tend to differ from one institution to another. We used this measure only in the survey instrument, but in the analysis, we always consider either the field or the degree program class, as defined by MIUR, to ensure comparability across universities.[5] In 2022, the host university offered 97 bachelor's degrees and 14 5-year master's degree. In some cases, the same degrees, with virtually identical curricula, are offered in multiple campus.

To better understand the relationship between the three measures – field, degree program class, and university-specific programs – let consider the field of Chemistry, Physics and Mathematics which includes 3 program classes (L-27 Chemistry, L-30 Physics, and L-35 Mathematics). The program class of Mathematics only include 1 specific degree (Mathematics), the program class of Physics include 2 of them (Physics, Astronomy), and the program class of Chemistry include 5 of them (Chemistry, Industrial chemistry, Chemical methodologies, Chemistry for the environment, the latter offered in two different campuses).

[5] There is only one exception. When utilizing administrative data from the host university, we rely on average performance at university-specific degree program level as controls.

## 2.2 The mentoring program

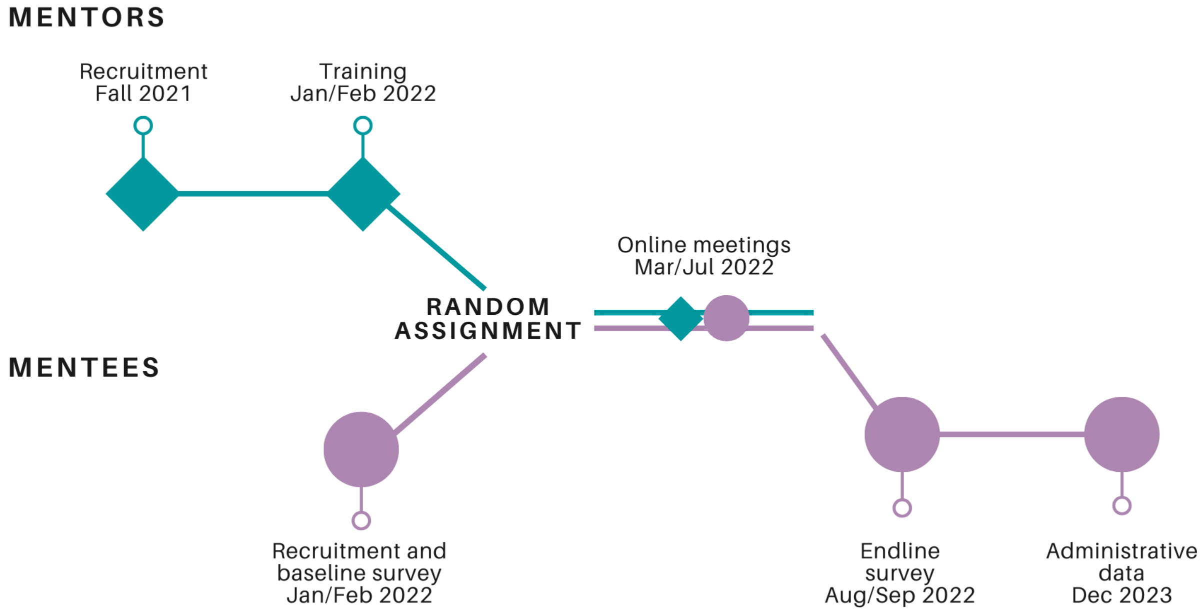
We designed the intervention to study the effect of mentorship on college major choices among last year high school students. We matched motivated and successful undergraduate and graduate students from the host university (mentors) with high school students (mentees) for one-on-one online orientation mentorship sessions. High school students had the opportunity to ask questions during the sessions, and the interactions were unrestricted to ensure that the mentorship was tailored to the needs of the mentees. While we encouraged mentors to prompt their mentees to ask questions, we also provided university students with guidance to facilitate discussions and cover a wide range of topics. Meetings were scheduled via a dedicated platform, where mentors could indicate their availability and mentees could book meetings. One-to-one meetings were mostly conducted via the MS-Teams platform, and we encouraged participants to use only official channels (the dedicated platform, institutional email, and MS-Teams), especially for the first meeting. Each mentor-mentee pair was encouraged to schedule 2 or 3 half-hour meetings.

High school students applied to the program between January and February 2022, a few months prior to the closure of the first intake. The final intake available for enrollment in a degree program was in September 2022. Personalized meetings were conducted between March and July 2022, with a higher frequency of meetings occurring in the initial months of the intervention while high school was still in session. We contacted all students for an endline survey just before the start of the academic year 2022/23. To account for the medium-term effects of the intervention, we collected administrative data regarding credits and grades at the end of the first year of university (December 2023). These data are only available for students who enrolled at the host university. Figure 1 illustrates the timeline of the evaluation program.

### 2.2.1 Recruiting mentors and mentees

The intervention was conducted in partnership with the orientation office of the host university, as part of a pilot project. This allowed us to use the official channels of the

Figure 1: Timeline of the intervention




university and to promote the initiative during the online orientation fairs. The study received ethical clearance from the board of the host university.

**Recruitment and training of the mentors.** Mentors were recruited from second and third-year bachelor's students and first and second-year master's students across nine fields: Accounting Business and Management; Agricultural and Food Sciences; Architecture and Industrial Design; Biology and Environmental Sciences; Chemistry Physics and Mathematics; Computer Sciences; Economics and Finance; Engineering; and Statistics. We specifically targeted quantitative fields which are known for having higher returns in the labour market. These fields encompass approximately 50 bachelor's and 70 master's programs. All mentors were required to be proficient in Italian and could mentor up to 4 students. We distributed a call for mentors to all participating degree programs and requested program directors to disseminate it to their students. Interested mentors could volunteer by completing a brief survey and had to attend two one-hour training sessions. We selected mentors mostly based on their academic performance.

**Recruitment of high school students.** Participants were recruited from among last-year high school students attending an Italian high school. We promoted the mentoring during virtual open days and orientation fairs organized by the host institution, which attract roughly 20,000 students from every region of Italy. The program was advertised on the homepage of the university's orientation office, as well as through emails sent to principals of Italian high schools.

To enroll in the program, students had to complete an online survey (see section 2.2.2). They were informed about the fields for which mentors were available and were made aware that, due to capacity constraints, not everyone would be assigned a mentor.

**Content of the online meetings.** Most mentor-mentee pairs reported having had long meetings (much longer than the suggested 30 minutes), and the satisfaction rate was extremely high on both sides. In a post-meeting questionnaire, we asked mentors which topics were discussed during the online meeting. The most common topics of discussion included: the curricula covered in the field, the admission tests and enrollment procedures, study techniques, and the exams. Half of the pairs also discussed social life, job prospects, and the mentor's satisfaction with their academic path. Other less common topics included: scientific topics related to the mentor's field, flat-hunting, relationships with classmates, and interactions with professors.

### 2.2.2 Survey instrument and administrative data

**Baseline and endline survey.** Both surveys were administered using Qualtrics and lasted about 15 minutes each. Participation in the endline survey was incentivized with 2 vouchers worth €300 each, and 10 vouchers worth €100 each. The prizes were awarded based on accuracy in a guessing task. We first detail the content of the baseline survey:

- **Background information:** We collected information on gender, year of birth, education level of parents, type of high school, county of the high school, mathematics and Italian grades (in the previous school year), and expected graduation grade (*voto di maturità*), along with the subjective expectation of enrolling in a university degree.

- **Choice of fields and degree programs:** Each prospective participant had to choose two or three fields of interest and rank them from the most preferred to the least preferred. For each chosen field, they could select up to 4 specific degree programs. The list of courses was based on the degrees available at the host university. After selecting all programs of interest, they were asked to rank them from the most to the least preferred.[6]

In the endline survey, we skipped the background information, with the exception of the final graduation grade (this time we asked for the actual grade, not the expected one). We then inquired about the university and degree program in which they had enrolled or planned to enroll. By the time we administered the survey, the last intake was still open, so we asked our participants about their enrollment status (e.g., enrolled, admitted but enrollment in process, awaiting an answer, etc.). We also asked for up to two alternative plans. Besides some information about the information-gathering process, we collected data on the weights for the 6 motives behind university choice and the subjective expectations for two fields, one of them being the field of the matched mentor. Finally, we asked them to guess the performance of fellow university students in two different fields; the vouchers were awarded to the students who performed best in this task. Completion of each survey took about 15 minutes.

**Administrative data.** We received permission from all participants to use their social security number or their temporary institutional email from the host university to gather administrative-level data about their academic performance. We have access to records only for those participants who enrolled at the host institution. Specifically, we have the following information: the degree program in which they are enrolled for the academic years 2022/23 and 2023/24; the number of class credits obtained by the end of the first academic year; and the average grade (GPA).

[6] The survey included also questions about the motives that might drive the choice of a university program and their subjective expectations following Boneva and Rauh (2017). This part of the questionnaire is not discussed in this manuscript.

#### 2.2.3 Assignment to treatment

The treatment assigned is stratified at the mentor level using a serial dictatorship mechanism to form mentor-mentee pairs. Initially, all eligible students were matched with a mentor following the algorithm described below. Subsequently, students matched with a particular mentor were randomly assigned to treatment or control.[7] Treated students were introduced to their mentor, while control students did not receive any communication about the matching procedure. They were simply notified that due the high volume of applications only a subset of students could join the program, and participants were selected randomly. This methodology allows us to identify which mentor a student in the control group would have been matched with had they participated in the program. This enables us to investigate whether treated students are more likely to pursue the same field of study as their mentors. Importantly, not even mentors were informed about the matched students from the Control group.

Here we detail the serial dictatorship mechanism used for pairing high school students with mentors. Students are randomly sorted and sequentially matched with the most affine available mentor. Mentors, upon registering for the program, are assigned between four to eight slots based on their availability; they are removed from the pool once these slots are filled. Matching quality hinges on academic affinity, initially seeking to pair students with mentors from programs the students listed in their baseline survey, with a preference for higher-ranked programs.[8] If no ideal mentor is available, the algorithm seeks mentors from related sub-fields, then within the same field. Ties are broken by matching students with mentors who share similar residential backgrounds, favoring mentors who would replicate the student's potential living situation at the host university. For example, a mentor living away from home is preferred for a student from a different region over a local mentor. Any unresolved ties are settled randomly. Therefore, depending on their

[7] Specifically, students were randomly sorted within each mentor group. The first half was assigned to the treatment group, the second half to the control group. In case of an odd number of students in the group, a further draw was conducted to assign the student in the middle.

[8] For mentors in master's programs, a related bachelor's program—often their own—is considered for pairing. If mentors have changed fields from bachelor's to master's, the bachelor's program feeding the most students into their master's program at the host university is preferred.

position in the randomly sorted list and the availability of the mentors, students may be matched with a mentor from the first, second or third preferred field. Students who ranked a field not included in the project as their top choice are inevitably matched with a mentor from a lower-ranked field.

## 2.3 Characteristics of the sample

**Characteristics of the participants.** A total of 495 last-year high school students completed the baseline survey, for a final sample of 337 requests considered for the program. We excluded from the sample used for this study all the applicants who declared that they were not interested in any of the fields included in the intervention.[9]

Panel (a) of Table 1 presents summary statistics for our baseline sample, categorized by Control and Treatment groups. Approximately 60% of our sample comprises females, a proportion consistent with the overall statistics of the host university (where 56% of enrolled students in 2022 were females), but higher than the average for the fields for which we had mentors.[10] The majority of participants are first-generation college students, an important demographic given their likely need for guidance, as they may have less access to direct information about university life. Consistent with the statistics for the host institution, half of the students come from the region where the university is located. In terms of their academic background and performance, over 75% of the sample attended an academic track (i.e., licei) and achieved good grades in both mathematics and Italian language (the highest grade in the Italian system is 10).

Turning to the characteristics of students regarding their most preferred field of study at the time of enrollment, in the baseline questionnaire, we asked prospective participants to identify their top fields of study from all available options. We did not restrict their responses to fields for which mentors were available; instead, we encouraged them to report their most preferred options regardless of the intervention. Among all eligible

[9]We contacted all students who completed the survey but were not interested in any of the 9 fields for which we had mentors, offering them the opportunity to speak with a mentor from our fields. However, only 14 students agreed to do so. These additional participants are not included in our main sample.

[10]We selected all macro-areas with a female enrollment share lower than 50%.

students, approximately 1 in 5 ranked a field that was not included in the nine of the program as their top choice, meaning they were assigned a mentor from their second or third most preferred field. More than 55% of our mentees were matched with a mentor from their top choice field of study at the time of enrollment. Importantly, all the aforementioned characteristics are balanced across Control and Treatment groups. Table A1 in Appendix reports the distribution of first and second/third choices for our baseline sample. Accounting, Business & Management (21%) is the field ranked first most often, followed by Engineering (13%), Architecture and Industrial Design (8%), and Computer Sciences (7%). As for the second/third field, Economics and Finance is the most common (21%), Accounting, Business, Management (20%), Political Science and Sociology (14%), Engineering (13%).

**Characteristics of the mentors.** We had 82 university students who served as mentors for the 169 mentees assigned to the Treatment group. Among these mentors, 48 (58.5%) were enrolled in a master's degree program, 44 were females (53.7%), and 43 (52%) were from a region different from that of the host institution. The distribution of mentors across the three macro-areas was as follows: 33 (19 females) from Economics, Management, and Statistics, 26 (12 females) from Sciences, and 23 (13 females) from Engineering and Architecture. Volunteers from all areas had a GPA well above the average of their peers (28.28 out of 30). Mentors were matched with 2 to 8 students (with a mean of 4.1 and a median of 4) and were put in contact with 1 to 4 mentees (with a mean of 2.0 and a median of 2).

### 2.3.1 Compliance and attrition

Overall, 99 out of 169 students assigned to the Treatment group met their mentor at least once, for a take-up rate of 59%. Data about the first meeting came from the dedicated platform where mentees could book their slot with their mentor. To ensure that the meeting was completed and not just booked, and to monitor the progress of the project, we had a short questionnaire for both parties involved who had to confirm the meeting.

Table 1: Balance tables

(a) Baseline survey

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.631 | 0.598 | -0.028 | -0.048 |
| | (0.484) | (0.492) | (0.063) | |
| First gen. college | 0.565 | 0.615 | 0.053 | 0.072 |
| | (0.497) | (0.488) | (0.070) | |
| From host region | 0.542 | 0.503 | -0.051 | -0.055 |
| | (0.500) | (0.501) | (0.055) | |
| Academic track | 0.774 | 0.757 | -0.023 | -0.027 |
| | (0.420) | (0.430) | (0.049) | |
| Math grade | 7.820 | 7.838 | 0.017 | 0.011 |
| | (1.168) | (1.179) | (0.157) | |
| Italian grade | 7.976 | 8.060 | 0.092 | 0.060 |
| | (0.981) | (0.986) | (0.139) | |
| Field 1 not STEM/ECON | 0.179 | 0.213 | 0.036 | 0.061 |
| | (0.384) | (0.411) | (0.042) | |
| Mentor in preferred field | 0.607 | 0.550 | -0.051 | -0.081 |
| | (0.490) | (0.499) | (0.059) | |
| Observations | 168 | 169 | 337 | |

(b) Endline survey

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.676 | 0.622 | -0.074 | -0.080 |
| | (0.471) | (0.488) | (0.144) | |
| First gen. college | 0.527 | 0.568 | 0.039 | 0.057 |
| | (0.503) | (0.499) | (0.130) | |
| From host region | 0.581 | 0.459 | -0.092 | -0.172 |
| | (0.497) | (0.502) | (0.118) | |
| Academic track | 0.757 | 0.770 | -0.044 | 0.022 |
| | (0.432) | (0.424) | (0.120) | |
| Math grade | 7.919 | 8.135 | 0.297 | 0.129 |
| | (1.156) | (1.220) | (0.283) | |
| Italian grade | 8.122 | 8.270 | 0.041 | 0.104 |
| | (0.979) | (1.038) | (0.240) | |
| Field 1 not STEM/ECON | 0.270 | 0.270 | 0.009 | 0.000 |
| | (0.447) | (0.447) | (0.095) | |
| Mentor in preferred field | 0.635 | 0.568 | -0.063 | -0.097 |
| | (0.485) | (0.499) | (0.105) | |
| Observations | 74 | 74 | 148 | |

*Note.* Differences are computed accounting for mentor dummies and clustering the errors at the mentor level. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Subsequent mentor-mentee interactions could also happen outside the platform, and we have less control over the exact number of meetings for all pairs.

At endline, a total of 169 high school students answered the final survey and stated their choice of degree program. We include in our main analysis a total 148 students, balanced across the Control and Treatment groups. In this sample we included all those instances in which at least two students per mentor responded to the endline survey to include mentor's fixed effects (see Section 3 for a discussion of the empirical strategy). The sample of 169 and 148 students do not differ along any observable characteristic.[11] Panel b of Table 1 reports the summary statistics at endline for both groups. None of the observables differ across the two groups of respondents, and all variables align perfectly with those at baseline. Among the mentees in the Treatment group who responded to the endline survey, 57 out of 74 had met with their mentor at least once. Looking at mentees for whom we retrieved data from the administrative records of the hosting institution, we find that there are 52 out of 75 compliers.

# 3 Empirical Strategy

## 3.1 Estimation strategy

We estimate the program's local average treatment effect (LATE) by using Treatment Assignment ($Z$) as an instrument for Treated ($T$). This is a standard practice to deal with imperfect compliance (Angrist et al., 1996). Specifically, we estimate the following model using two-stage least squares:

$$Y_i = \alpha T_i + X_i \beta + \mu_j + \eta_i \tag{1}$$

$$T_i = \pi Z_i + X_i \gamma + \nu_j + \epsilon_i, \tag{2}$$

[11] Balance tables are available upon request from the authors.

where $Y_i$ is student $i$'s outcome of interest, $T_i$ equals 1 if the student met mentor $j$, $Z_i$ equals 1 if the student was assigned to Treated group, $X_i$ are individual predetermined characteristics, $\mu_j$ and $\nu_j$ are mentor $j$'s fixed effects, and $\eta_i$ and $\epsilon_i$ are error terms. Given the random assignment, $\epsilon_i$ is uncorrelated with the regressors. Conversely $\eta_i$ may be correlated with $T_i$ given that students assigned to the treatment decide whether to actually participate or not. The estimated parameter $\widehat{\alpha}$ quantifies the effect of the treatment on compliers, namely students who take-up the intervention when they are offered it. To ensure consistency, Treatment Assignment (used as an instrumental variable) must satisfy the exclusion restriction, implying that the effect on the outcome of the treatment works only via the treatment itself. Although this assumption cannot be tested directly, it appears reasonable in this context. Our main outcome of interest is the choice of the field of study, and, in particular, if mentees choose the same field as their mentor. The mere fact of being offered the treatment, appears highly unlikely to affect such choice. This is especially true considering that the existence of a mentor and their field of study was unknown to control students.

Furthermore, the Stable Unit Treatment Value Assumption (SUTVA) should be satisfied. SUTVA essentially states that the potential outcomes for any individual do not depend on the specific treatment assignments of other individuals. In other words, the treatment of one student does not directly affect the outcomes of other students. A typical concern in the framework of RCTs is the presence of spillover effects, where the treatment assigned to one unit indirectly affects the outcomes of other units. Concretely, in a program like ours, this issue may arise if a group of friends applies together and ends up with different treatment statuses. Treated mentees may share what they learned during the meetings with both control students and with other students assigned to the treatment but who did not meet with the mentor, possibly affecting their outcomes. However, the online nature of our program and the fact that participants are spread out throughout the country make spillover a minor concern in our settings. In fact, students come from 73 provinces and 9 school tracks, with 163 "province X school track" combinations.[12] A large

[12]While there are exceptions, usually, different school tracks are offered by different schools on separate premises, thereby decreasing the chances that students know each other.

fraction (30%) of the students are the only one from their province X school track" group, and 44% belong to groups with 2 to 5 students.[13] These figures suggest that the likelihood of having multiple students from the same school is quite low. In fact, the "province X school track" is a coarse classification; for instance, just in the province where the host institution is located, there are 30 high schools offering one specific school track (i.e., liceo scientifico).

**Mentor's fixed effect and covariates.** In our main analysis we will include all instances in which at least two students per mentor responded to the endline survey (N=148), hence allowing for mentor's fixed effects. The sample size is further reduced if we consider instances in which at least one student from the Control and one from the Treatment group per mentor replied to the final survey (N=110). Given the small sample size, we also present robustness checks in which we use mentor's covariates as proxies for mentor's fixed effects.

# 4 Results

## 4.1 Effect of the intervention on enrollment choices

We first analyze the enrollment choices as reported in the endline survey, examining the extent to which they align with the mentors' fields. Table 2 presents results from a series of two-stage least squares estimates, where the dependent variable is a dummy that takes the value 1 if the field chosen by the student matches that of the assigned mentor, and 0 otherwise.[14] It is important to note that all students, whether in the Control or Treatment group, were matched with a mentor. However, those in the Control group never met their mentor or received any information about their characteristics. Similarly, mentors were never informed about the existence of high school students who were matched but not assigned to them. It can be reasonably assumed that mentors had no influence on the

[13] Another 14% belong to groups with 6 to 9 students. The remaining students are divided into three groups of 10, 11, and 22 individuals and are located in the province of the host institution.

[14] The results from the first stage are available in Table A3 in the Appendix.

decision-making of students assigned to the Control group.

Program participation (e.g., meeting the mentor at least once) is captured by the dummy variable *Treated*, which is instrumented with a dummy variable taking the value of one if the participant was assigned to the treatment and zero otherwise (Treatment Assignment). In all models, we include mentor fixed effects, given that the treatment assignment is stratified at the mentor level. When not controlling for other covariates (Model 1), we observe a sizable but only marginally significant effect of meeting a mentor. In subsequent models, we incorporate additional covariates. In Model 2, we include the lag of the dependent variable, that is the dummy *Mentor in preferred field at baseline*, which takes a value of 1 if the preferred field at baseline matches the mentor's field. Overall, 3 out of 4 of our respondents choose at endline the preferred field of study at baseline. This stability is reasonable, given the relatively short interval between the two surveys (7 months) and some consistency in educational preferences. Therefore, both treated and control students assigned a mentor in their preferred field are more likely to choose that field at endline. Given that the dummy explains a large part of the variation in the outcome, it appears important to control for it to improve the precision of the estimates.

Model 2 in Table 2 shows a large and significant effect of our intervention on enrollment choices, even after controlling for whether the mentor's field was the most preferred at baseline. The probability of enrolling in the same field as the mentor is 22 percentage points (p.p.) higher for treated mentees compared to those in the control group, an increase of 45%. As expected, the coefficient for *Preferred field at baseline* is sizable and significant. In Model 3, we control for additional covariates: gender, whether the student is a first-generation college student, attened an academic track, and a vector of dummies for their preferred field at baseline.[15] The results are qualitatively and quantitatively consistent with previous estimates. Furthermore, none of the additional covariates have a significant effect on the dependent variable. Given the relatively small sample size, we prefer to be conservative with the number of additional regressors, and we will use the

[15]We aggregated the 17 fields in 5 macro-areas: Humanities, Medicine and Pharmacy, Economics and Business, Science, Engineering and Architecture.

specification in Model 2 as benchmark for the rest of the paper.[16]

Results from Table 2 are confirmed by the ITT estimates reported in Table A4 in the Appendix; if we consider the specification with *Preferred field at baseline* and mentor fixed effect (Model 2), the effect of the intervention decreases compared to the LATE estimates but remains sizable (16.6 p.p.) and statistically significant. Results are qualitatively similar, although not always significant when considering the other specifications.

Table 2: Choice of mentor's field

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated | $0.170^{+}$ | $0.221^{**}$ | $0.208^{**}$ |
| | (0.100) | (0.076) | (0.078) |
| Mentor in pref. field at baseline | | $0.601^{**}$ | $0.636^{**}$ |
| | | (0.086) | (0.120) |
| Mentor FE | Yes | Yes | Yes |
| Other covariates | No | No | Yes |
| Control mean | 0.486 | | |
| N | 148 | 148 | 148 |

Notes. The dependent variable is a dummy that takes value 1 if the student chooses the same field of study of the assigned mentor according to the endline survey. The dummy "Preferred field at baseline" takes value 1 if the student ranked the mentor's field as their favorite choice in the baseline survey. Other covariates include student predetermined characteristics (dummies for gender, first generation college, academic track) and a vector of dummies for their preferred field at baseline. Coefficients are estimated using a two stage least square model, with program participation ("Treated") instrumented with program assignment ("Assigned to treatment"). The row "Control mean" shows the mean dependent variable in the control group. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

**Robustness checks.** Table A5 in the Appendix presents a series of robustness checks for our main result. Our preferred specification (Model 2 from Table 2) includes all instances in which at least two students per mentor responded to the endline, but in practice the estimation of the effect of the treatment rely on variation of the variable "Treated" within mentor. Thus, only observations of mentors who had at least one mentee in the Control group and one in the Treatment group contribute to the estimation of its coefficient. Model 1 in Table A5 replicate the analysis restricting the sample to these 110 observations. As

[16] A regression of the dependent variable on students' individual characteristics confirm that they have very low explanatory power. In the interest of space, coefficients of the additional covariates are not shown; they are available upon requests.

expected, results are nearly identical.

In our main specifications, we always add mentor fixed effects; while this is the most rigorous approach given the nature of our data, it is also quite demanding given the small sample size. To address this potential concern, in Table A5 we replicate the results using mentor covariates as a proxy for the fixed effects. In Model 2, we analyze the original sample of 148 students and include mentors' covariates: dummies for gender, campus, master student status, and a vector of dummies for their field of study. In Model 3, we consider yet another sample; this time, we include all 169 respondents to the endline survey, regardless of whether their mentor was assigned to at least two students. Similar to the previous model, we replace mentor fixed effects with personal characteristics of the mentors. The effect of the mentoring intervention remains statistically significant in this specification, albeit slightly smaller in magnitude. In Model 4, we construct a new dependent variable that takes the value 1 if the student chooses the same degree program (as defined by the degree program class set by MIUR) as the mentor, and 0 otherwise. This represents a stringent test, given the large number of degree programs and the fact that any field may encompass multiple degree program classes. In this specification, we include both *Pref. field at baseline* and *Pref. program at baseline.* The latter variable takes value 1 the student ranked first the degree program class attended by the mentor. Once again, our main result is validated.

**Lower bound estimates.** As discussed in Section 2.3, approximately half of the initial sample participated in the endline survey. While the response rate for both the treatment and control groups are identical, and their observable characteristics are perfectly balanced, we cannot entirely rule out the possibility that students in the two groups self-selected differently into the endline survey based on some unobservable characteristics. For instance, if treated students who appreciated the program the most were more likely to respond at the endline and, at the same time, the likelihood of responding was orthogonal to the treatment effect among controls, one would overestimate the real effect of the intervention. To address this concern, we estimate a "lower bound" for the true effect, assuming the the most challenging scenario for our findings. We assume that the intervention had zero effect on all treated mentees who did not answer to the endline survey. In other words, their likelihood of choosing the mentor's field is comparable to that observed for similar control students. Even under this very restrictive assumptions, the average effect of the program is sizable.

To estimate the lower bound for the true effect, we use control group responses at endline to estimate individual-specific probabilities of choosing the mentor's field absent any treatment.[17] For each student $i$ who did not participate in the endline survey, we compute the probability $p_i$ that the outcome of interest (i.e., choosing the mentor's field) occurs. We then estimate our usual specification (column (2) in Table 2) on the full sample of 337 students from the baseline survey, imputing the choice for students who did not answer at endline. Each choice $c_i$ is drawn from a Bernoulli distribution with probability $p_i$. This final step is replicated 10.000 times in a Montecarlo simulation. Figure B1 in the Appendix shows the distribution of estimated coefficients. The median estimated coefficient is 9.6 p.p. (with a mean of 9.7 p.p.), indicating a substantial increase of 20% compared to the control group. Furthermore, 98.2% of the estimates are above 0. This suggests that the true effect is likely positive and sizable, even in the presence of some positive sorting of the treatment group into the endline survey.[18]

### 4.1.1 Heterogeneity analysis

We now consider the heterogeneous effects of the treatment based on assignment. In particular, we want to test if mentors *reinforce* baseline preference or *attract* mentees toward their field, even though it was not the most preferred one at baseline. More specifically, we aim to determine if the effect arises from receiving a mentor in one's

[17] We regress the outcome on the dummy "mentor in preferred field" and a vector of mutually exclusive dummies for the field ranked first at baseline. We tried alternative specifications with different set of regressors, particularly individual characteristics (e.g., gender, first generation student, academic track,...), and results are robust. Given that these additional regressors do not improve the fit of the model, we use the more parsimonious specification.

[18] We also implement a simple alternative exercise. We assume that students who did not participate in the endline survey keep the same preferences that they reported in the endline, and therefore impute their outcomes. In this case we estimate a 8.4 p.p. effect, with a p-value of 0.068.

preferred field at baseline. That is, mentees assigned to the Treatment group and matched with a mentor from their most preferred field of study are more likely to confirm that field even at endline, compared to mentees matched to a mentor from their first-choice field but in the Control group. If we were to observe this effect, the mentor acts as a reinforcement of the baseline preferences. However, the main effect may also be driven by a higher proportion of mentees changing their minds in the Treatment group than in the Control group, leading them to revise their baseline choice in favor of the mentor's field. In this case, the mentor acts as an attractor, shifting mentees' preferences from one field to another.

Table 3 reports the results of the heterogeneity analysis based on assignment. Model 1 replicates our main analysis by adding an interaction between *Treated* and *Mentor in preferred field*. In Model 2, we replace mentor fixed effects with the characteristics of the mentors. In both models, the coefficient for Treated has similar magnitude to our preferred specification, although is less precisely estimated (p-values are 0.06 for Model 1 and 0.112 for Model 2 respectively). Conversely, the estimated coefficient of the interaction term is small in size and highly insignificant. The sum of the two coefficients, which gives the effect of meeting a mentor in the preferred field, is always significant. Results indicate that mentors can act both as reinforcers and as attractors, and their effect is equally important for the final choice of their mentees.

As a complementary way of studying the same question, we also investigate the treatment effect on the probability of choosing at endline the field ranked first at baseline. The dependent variable in Models 3 to 5 (Table 3) is a dummy taking value 1 if the student confirms their baseline choice at endline. Model 3 suggests that, overall, treated students may be slightly more likely to confirm their initial preference, but the difference is modest in size and not significant. Model 4 and 5 show that this is due to two large effects going in opposite directions. As for the first two models, we included both the dummy *Treated* and the interaction term *Treated X Mentor in preferred field*. In both specifications the coefficient of Treated is negative and sizable, albeit imprecisely estimated. This suggests that treated students who met a mentor from a field they initially found less appealing are more likely than similar control students to change their mind and shift into a different field at endline (i.e., attraction effect). Conversely, for both models the interaction term has a positive and very large coefficient (significant at 5% and 10% respectively). The sum of the two coefficients show that students who met a mentor in their preferred field at baseline are more than 20 p.p. more likely to chose the same field at endline (i.e., reinforcement effect). Overall, this additional analysis confirms that mentors serve both as attractors for students that initially preferred a different field, and as reinforcers for students whose preferences were already aligned with the mentor's field.[19]

# 5 Labour market prospects and university performance

So far, we have demonstrated the impact of mentors in shaping mentees' enrollment choices; one might question whether this is a desirable outcome. We will tackle this issue using a two-pronged approach. First, we assess whether we are inadvertently steering students away from more lucrative fields. Implicitly, this approach assumes that our ultimate goal targets prospective labour market outcomes, and it is important to nudge mentees toward degrees with better employment prospects (see Section 5.1). Second, we leverage administrative data to ensure that the nudge does not lead to unintended negative consequences. Although some majors offer better labour prospects, they are often more demanding. In Section 5.2, we will provide evidence regarding the academic performance of our participants at the end of their first year at university.

## 5.1 Selection into quantitative fields and prospective wages

The mentors in our intervention are enrolled in STEM, Economics, or Business. These fields typically offer higher quantitative contents and better labour market prospects than the fields not covered by the intervention, with potential exceptions for some programs in

[19]ITT estimates are reported in Table A6 in the Appendix. Results are qualitatively aligned with the one reported here and, as usual, estimated effects are slightly smaller in size.

Table 3: Heterogeneity by assignment type

| | mentor's field | | preferred field at baseline | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treated | 0.268$^{+}$ | 0.202 | 0.087 | -0.222 | -0.121 |
| | (0.143) | (0.127) | (0.090) | (0.180) | (0.166) |
| Treated X mentor in pref. field | -0.072 | 0.004 | | 0.470$^{*}$ | 0.333$^{+}$ |
| | (0.177) | (0.157) | | (0.212) | (0.196) |
| Mentor in pref. field | 0.626$^{**}$ | 0.589$^{**}$ | 0.094 | -0.068 | -0.060 |
| | (0.109) | (0.088) | (0.119) | (0.118) | (0.103) |
| Mentor FE | Yes | No | Yes | Yes | No |
| Mentor covariates | No | Yes | No | No | Yes |
| Treatment + interaction | 0.196 | 0.207 | | 0.248 | 0.213 |
| P-val (treatment+interaction) | 0.036 | 0.009 | | 0.012 | 0.010 |
| Control mean - mentor in pref. field | 0.723 | | 0.723 | | |
| Control mean - mentor not in pref. field | 0.074 | | 0.741 | | |
| N | 148 | 148 | 148 | 148 | 148 |

Notes. The dependent variable in columns (1) and (2) is a dummy that takes value 1 if the student chooses the same field of study of the assigned mentor (as reported in the endline survey). The dependent variable in columns (3) - (5) is a dummy that takes value 1 if the student chooses at endline the field that they ranked first at baseline. The dummy "Mentor in preferred field" takes value 1 if the student ranked the mentor's field as their favorite choice in the baseline survey. Other covariates include student predetermined characteristics (dummies for gender, first generation college, academic track) and a vector of dummies for their preferred field at baseline. Coefficients are estimated using a two stage least square model, with program participation and its interaction ("Treated" and "Treated X mentor in pref. field" ) instrumented with program assignment ("Assigned to treatment" and "Assigned to treatment X mentor in pref. field"). The row "Treatment + interaction" shows the sum of the first two coefficients (that is, the effect of treatment on students with a mentor from their preferred field at baseline); the following row shows the p-value of this sum. The rows "Control mean" show the mean dependent variable in the control group, among students matched with a mentor in their preferred field or in another field. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

the fields of Medicine and Pharmacy. Thus, selecting the mentor's field could significantly impact the future labour market outcomes of students whose alternatives at baseline had lower returns. Conversely, there would be little or no impact for students already considering only fields with high returns at baseline.

In panel a) of Table 4, we examine whether the treatment changes the probability of choosing a field included in the project (column "STEM/Econ"), and whether it influences the selection of less quantitative fields (column "Humanities") or fields related to the medical profession (column "Medicine"). Results suggest that enrollment in STEM/Economics increases (+13.8 p.p., significant at 10%), driven by a decrease in enrollment in less quantitative fields (-12.5 p.p., significant at 10%), while the medical fields remain unaffected.

In panel b) of Table 4, we assess the effect of the intervention on the monthly wages that students may anticipate with a degree from their chosen bachelor's program. To estimate the future wages associated with each degree program, we leverage data from AlmaLaurea, which surveys university graduates in the years following their graduation (for further information about AlmaLaurea and the data, see C in the Appendix). We compute the average wage 5-7 years after graduation for each program, when respondents are in their late twenties or early thirties. In the first column, the dependent variable is the average wage among students who have completed 5 years of tertiary education, typically obtaining a 3-year bachelor's degree followed by a 2-year master's degree. The wage associated with each bachelor's program is calculated as the weighted average of the wages of master's degree holders in the same field, weighted by the share of students enrolling in each master's program after completing their bachelor's degree. In subsequent columns, the dependent variable is the average of the wage after 5 years of studies (as used in the first column) and an estimated wage for individuals who did not pursue further studies after their bachelor's degrees. This average is weighted by the proportion of graduates in the program who either enroll or do not enroll in a master's degree afterwards. Since bachelor's graduates are surveyed only 1 year after graduation, we impute their wage 7 years after graduation to make it comparable with the data for master's graduates. In column (1), we assume a 40% growth rate for all programs, while in column (2), we use program-specific growth rates inferred from data on master's students (for details, see C in the Appendix). Results in panel b) suggest a sizable increase in the prospective wage of treated students, ranging from 52 to 64 euro per month depending on the specification. This corresponds to an increase of 3.1-3.7% in the average prospective wage compared to the control group.[20]

[20] We also assess the treatment effect on prospective employment, using master's graduates data. Results suggest a modest increase of 1.2 p.p. (significant at 10%). Given that the average employment rate is 90.9%, we believe that wage is more relevant in this setting.

Table 4: Prospective outcomes

(a) Type of field chosen

| | STEM/Econ | Humanities | Medicine |
|---|---|---|---|
| Treated | $0.138^{+}$ | $-0.125^{+}$ | -0.013 |
| | (0.080) | (0.069) | (0.056) |
| Control mean | 0.662 | 0.243 | 0.095 |

(b) Prospective wage in the chosen program

| | Studying 5 years | Studying 3 or 5 years (1) | Studying 3 or 5 years (2) |
|---|---|---|---|
| Treated | $64.360^{*}$ | $58.003^{+}$ | $51.708^{+}$ |
| | (28.156) | (30.412) | (26.422) |
| Control mean | 1725.931 | 1629.238 | 1659.419 |

*Panel a).* The dependent variable in column "STEM/Econ" is a dummy that takes value 1 if the student chooses at endline a field related to STEM, Economics or Business. The dependent variable in column "Humanities" takes value 1 if Humanities, Laws, Sociology or Political Science are chosen. The dependent variable in column "Medicine" takes value 1 if the student chooses Medicine or Pharmacy. *Panel b).* The dependent variable in column "Studying 5 years" is the average wage 5 years after graduation among graduates from a master degree that is a natural prosecution of the chosen bachelor degree; data are retrieved from the 2022 AlmaLaurea survey. The dependent variable in columns "Studying 3 or 5 years" is a weighted average between prospective wage 5 years after obtaining a master degree and 7 years after obtaining a bachelor degree (and not pursuing further studies); weights are given by the share of graduates from the bachelor program who enrolled or did not enroll in a master program. Wage 7 years after obtaining a bachelor degree is inferred from the wage one year after graduation (from the 2016 AlmaLaurea survey); in column (1) a growth rate of 40% is assumed, while in column (2) the growth rate is program specific and it is inferred from the wage growth of master graduates in the same field. Further information can be found in Appendix C.

In both panels, the same sample and the same set of regressors as in column (2) of Table 2 are used. The row "Control mean" shows the mean dependent variable in the control group. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

## 5.2 Administrative data and university outcomes

In the previous section, we ascertained that the intervention nudges students towards university degrees with higher labor market returns. This reasoning, of course, applies only if our participants are successful in their university careers. In other words, it is crucial to ensure that treated students perform at least as well as untreated participants. In this section, we will present evidence of this based on university outcomes during the first year of the bachelor's program.

To enroll in the mentorship program, high school students were required to provide either their social security number or the institutional email of the host institution.[21] All participants also signed an informed consent form, allowing the researchers to match the data from the intervention with administrative data from the host institution regarding their academic careers. This enables us to access administrative data for those students who subsequently enrolled in the host institution. More precisely, we have data on the degree program in which they are enrolled for both December 2022 and 2023 (indicating enrollment in the second year of university). We do not have data on pre-enrollment and cannot track changes in the program that occur during the year. We also have information on the number of study credits obtained at the end of the first year of university (maximum 60 CFU) and the average GPA (on a scale from 0 to 30, where 30 is the highest grade). We first use the administrative data to validate the survey responses, and then we assess the academic outcomes at the end of the first year of university.

While all provided social security numbers and institutional emails are formally correct, there is a possibility that not all respondents accurately reported their information. For instance, institutional emails follow the format "name.surnameN@university.it". While we can verify the accuracy of the "name.surname portion", we cannot confirm the correctness of the appended number, N. Similarly, a digit in the social security number may be misspelled. 80% of the participants provided both their email and social security number, facilitating the merging process with university administrative data. Thus, al-

[21] High school students can activate a temporary institutional email from the host institution via an online platform. This email is used for gaining access to online orientation events and becomes permanent if a student decides to enroll at the host institution.

though there is a possibility that the merge might overlook some students who enroll in the host university, we consider this issue to be minor. Finally, it is important to acknowledge that the emails of treated students were verified before inclusion in the platform, potentially easing their retrieval in the administrative dataset compared with students who did not participate. In next section, we will test whether there is a differential selection of treated students in the administrative data.

### 5.2.1 Sample selection and replication of the main finding

According to our data, approximately 43% of students in the baseline sample (144 out of 337) enrolled at the host university in the academic year 2022/2023. We first use administrative data to validate survey answers. We can compare the endline answers and the administrative records for 98 students, 83 of whom belong to the main analysis sample. For 95 out of these 98 students the field recorded in the administrative data coincides with the field chosen at the endline. For 93 of them, the degree program is also the same.[22]

To validate the survey answers, we can also check another dimension: whether self-reported intention to enroll in the host university or in a different institutions are confirmed by administrative records. In fact, students had to declare in the endline survey whether they planned to enroll in the host university or in another university. First, we examine all students who declared they choose a university other than the host university; none of them are available in our administrative sample, indicating that they indeed enrolled in a different university. Of the 135 students who reported intending to enroll in the host institution, we find 73% (98 individuals) in the administrative records. Moreover, all 62 students who declared they already enrolled in the host university were retrieved in the administrative data. We also retrieved 9 of the 11 students that declared that they already met all the administrative requirements (e.g., passing the admission test) but had

[22]Similarly, only 3 (5) students out of the 83 in the analysis sample have a different field (program). The two students with identical fields but different programs ended up in different types of Engineering courses. The other three selected Medicine at the endline, but enrolled in Pharmacy or Chemistry according to the administrative data.

not completed the enrollment process yet.[23]

Having provided evidence of the reliability of our data sources, we move to assess whether there is any differential selection of Control and Treatment group, and then we replicate our main analysis with the administrative data. We will use three different samples and verify that results are consistent across them. First, we consider all 144 observations retrieved in the administrative data (69 in the Control and 75 in the Treatment group). Second, we consider the subsample comprising students from the main sample used throughout the paper (148 students) for whom we also have administrative data; this sample includes 83 individuals (38 in the Control and 45 in the Treatment group). Third, we consider the union between students who responded the endline (169 individuals) and students retrieved in the administrative data (the 144 in the first sample). This third sample allows us to define the main dependent variable of interest (choosing mentor's field) for 215 individuals (108 in the Control and 107 in the Treatment group), using either their endline data or their administrative data.[24] Specifications using mentor fixed effects require to focus only on restricted samples for which the fixed effect can be added (i.e. the mentor is matched with at least two students in the sample). This is particularly demanding for the first and second samples, whose size is already small. Therefore, we always estimate the model of interest both on the full sample, including mentor covariates, and on the restricted sample, including mentor fixed effects.

As shown in Table A2, treated students do not have a significantly higher probability of being retrieved in the administrative data. The difference is somewhat sizable in magnitude (up to 9 percentage points), but it disappears completely when focusing on restricted samples and including mentor fixed effects. Balance tables in Table A9 in the Appendix show that individual characteristics are well balanced in all samples. However, in the first and second samples fewer students in the Treatment group were matched

[23]Out of the remaining 60 students, 25 were retrieved in the administrative data. According to the endline responses, those students did not complete the application procedure when taking the survey, thus they may have failed some of the legal requirements to enroll and eventually have chosen a different university.

[24]When both information are available, we use the endline survey. As discussed above, the chosen field is the same for almost all students observed in both sources. Moreover, using the admin data would change the value of the dummy "Choose the mentor's field" for only 1 individual.

at baseline with a mentor from their preferred field. This difference is significant when using all the available observations and controlling for mentor covariates; when including mentor fixed effects, the size remains similar but the difference is not significant.[25] While we cannot completely rule out a differential sorting of treated students, these results are reassuring about the comparability of students in the Treatment and Control groups who enrolled in the host university. If anything, they suggest that it is important to control for the dummy "Mentor in preferred field," as already planned for consistency with previous analysis.

Table A10 in the Appendix replicates the main analysis (Table 2) on the samples described above. The models in columns (all) include all available information and control for mentor covariates, while the models in columns (fe) use restricted samples and include mentor fixed effects. The results are qualitatively aligned with previous results and significant in most specifications.[26] The estimated coefficients range from 0.14 to 0.19 p.p. While these effects are still substantial (representing an increase of 25% - 30% with respect to the Control group), they are slightly smaller than those previously found. This suggests that the coefficients in our preferred specification may be somewhat imprecisely estimated, but confirms that the intervention had a large impact on treated students.

#### 5.2.2 Effect on performance

We now turn our attention to academic performance, evaluating both the number of exams passed and the GPA at the end of the first year of university. The aim of this analysis is to ensure that we have not influenced students's choices towards a direction that could be detrimental to them. Specifically, we want to know whether, after meeting with their mentor, mentees opt for degree programs in which they perform worse compared to the programs they would have chosen in the absence of a mentor. While we do not necessarily

[25] In the interest of space, the Appendix show the former set of tables, not the latter. Results are available upon request.

[26] Coefficients of the fixed effect regressions on the sample of admin data (111 observations) and intersection with endline (58 obs) have p-values of 0.13 and 0.14 respectively. Their magnitude is similar or greater than the coefficients from the corresponding regressions on larger samples, which are always significant at 5%.

expect an improvement in mentees' performance—given our earlier findings that they tend to select more quantitative fields, which are often associated with lower grades and fewer credits—our goal is to ascertain that the intervention does not negatively affect their medium-term university outcomes.

Table 5 reports the effects of the mentorship programs on these dimensions. For each dependent variable, we estimate the model both on the 144 students retrieved in the administrative records, including mentor covariates, and on the restricted sample with mentor fixed effect. Similar analyses on different subsamples are available in Table A11 in the Appendix. In all models, we include a dummy variable for mentees who met their mentor (Treated), instrumented with *Assigned to treatment*. We also include a dummy for *Mentor in preferred field*, and controls at the degree program level. Specifically, we control for two aggregate measures of the previous cohort's performance: average number of university credits acquired in the first year, and proportion of students who did not continue in the same program in the following academic year. These controls are important to ensure that the estimated effects do not solely reflect a differential sorting of students across programs with varying difficulty levels.

The first two columns of Table 5 display the number of exams passed during the first year, measured by a standardized measure called *Crediti Formativi Universitari* (CFU, henceforth), which captures the required amount of effort for each course.[27] Although students are expected to acquire 60 CFU to complete their first-year course load, it is common for them to fall behind.[28] Control students in our sample obtained, on average, slightly less than 40 CFU during the first year. The estimated effect of the Treatment is quite sizable, being larger than 7 CFU in both specifications, although only marginally significant. We also created dummies for students who passed at least 50% and 80% of exams, corresponding to acquiring at least 30 and 48 CFU, respectively. The treatment effect is large and significant for the first threshold, while it is positive but small and not significant for the second one. In the last two columns, we consider the weighted

[27] 1 Italian CFU corresponds to 1 ECTS credit in the European Credit Transfer and Accumulation System.

[28] According to AlmaLaurea, only 62% of bachelor graduates nationwide had completed their studies in 3 years. The other takes one or more additional years.

GPA, and once again, we find a positive sign for the treated, although the effect is not significant.

In summary, it is safe to say that at the very least the intervention did not affect performance negatively. While we cannot definitively claim conclusive evidence of a medium-term positive effect on performance, the results suggest that the intervention may have improved the average completion rate among treated students, and that this was driven by improved performance among weaker students. In fact, the intervention appears to have reduced the proportion of students who failed to acquire half of the required credits, while it did not significantly increase the proportion of students who completed most or all their workload. According to post-meeting questionnaires, two out of three mentors discussed study techniques and exam management with their mentees. This exchange likely benefited less prepared students lacking effective study methods, while it may have had less impact on high-performing students.[29]

# 6 Conclusion

Choosing a university degree and field of study is critical in shaping individuals' career paths and potential earnings. However, students' decisions often encompass more than just income optimization and are subject to various decision-making frictions. In a country grappling with high university dropout rates and low student satisfaction, we conducted a randomized controlled trial to assess the impact of a personalized mentorship program on university major selection. This program paired students with mentors from quantitative disciplines and facilitated open discussions online, aiming to bridge the informational gap students face when making these complex and consequential decisions.

Mentored students are 22 percentage points more likely to choose the same field as their mentors, a 45% increase from the baseline. The program notably shifts preferences towards STEM/Economics fields, potentially increasing prospective wages by 3.1-3.7%,

[29] Anecdotally, mentors often shared during the training that one of the main challenges during their first year was to acquire a good study method and keep up with the exams, and that at the time they would have appreciate some guidance on that matter.

Table 5: Medium run effect on performance

| | CFU | | ≥50% exams | | ≥80% exams | | wGPA | |
|---|---|---|---|---|---|---|---|---|
| | (all) | (fe) | (all) | (fe) | (all) | (fe) | (all) | (fe) |
| Treated | $7.895^{+}$ | 7.091 | $0.263^{*}$ | $0.211^{+}$ | 0.027 | 0.032 | 3.413 | 2.977 |
| | (4.634) | (5.604) | (0.104) | (0.123) | (0.111) | (0.122) | (2.170) | (2.559) |
| Mentor in pref. field | 3.875 | 3.120 | 0.033 | -0.048 | $0.219^{*}$ | $0.273^{*}$ | 1.728 | 1.222 |
| | (4.056) | (5.176) | (0.082) | (0.097) | (0.088) | (0.113) | (1.853) | (2.253) |
| Program: mean CFU | $0.555^{+}$ | 0.291 | 0.008 | 0.004 | 0.013 | 0.006 | $0.240^{+}$ | 0.052 |
| | (0.314) | (0.529) | (0.006) | (0.011) | (0.009) | (0.014) | (0.144) | (0.231) |
| Program: % dropout | $-69.129^{**}$ | -60.497 | $-1.301^{**}$ | $-1.255^{+}$ | $-1.235^{+}$ | $-1.672^{+}$ | $-33.701^{**}$ | $-34.974^{*}$ |
| | (23.809) | (37.289) | (0.504) | (0.730) | (0.644) | (0.874) | (11.440) | (17.048) |
| Mentor FE | No | Yes | No | Yes | No | Yes | No | Yes |
| Mentor covariates | Yes | No | Yes | No | Yes | No | Yes | No |
| Control mean | 39 | 38.1 | .681 | .69 | .536 | .517 | 16.9 | 16.5 |
| N | 144 | 111 | 144 | 111 | 144 | 111 | 144 | 111 |

Notes. The dependent variable "CFU" is the number of university credits acquired in the first academic year (from 0 to 60). "≥50% exams" is a dummy variable that takes value 1 if the student passed at least half of the exams in the first year (that is, they acquired 30 CFU or more). "≥80% exams" is a dummy variable that takes value 1 if the student passed at least 80% of the exams in the first year (that is, they acquired 48 CFU or more). "wGPA" is the weighted average of exam grades in the first year; passed exams received a grade from 18 to 30, failed exams or those not taken are counted as 0. Columns (all) include mentor covariates: dummies for gender, campus (main campus *vs* other campuses), seniority (master *vs* bachelor), and a vector of dummies for their fields. Columns (fe) include mentor fixed effects and only groups with two or more students per mentor are included in the analysis. The coefficients are estimated using 2SLS, with "Treated" instrumented with "Assigned to treatment". Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

without adversely affecting university performance. These results highlight the potential of mentorship to steer students towards educational choices that are both informed and advantageous. This cost-effective, light touch intervention has the potential for easy expansion on a larger scale. Future analysis will explore the mechanisms behind these positive outcomes, with an emphasis on mentors' characteristics, students' subjective expectations, and their inclination towards competitiveness and confidence.

# References

Alfonsi, L., M. Namubiru, and S. Spaziani (2023). Meet your future: Experimental evidence on the labor market effects of mentors. Technical report. Available online or specific location.

Angrist, J. D., G. W. Imbens, and D. B. Rubin (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association 91*(434), 444–455.

Avery, C., O. Gurantz, M. Hurwitz, and J. Smith (2018). Shifting college majors in response to advanced placement exam scores. *Journal of Human Resources 53*(4), 918–956.

Bobba, M. and V. Frisancho (2022). Self-perceptions about academic achievement: Evidence from mexico city. *Journal of Econometrics 231*(1), 58–73.

Bobba, M., V. Frisancho, and M. Pariguana (2023). Perceived Ability and School Choices: Experimental Evidence and Scale-up Effects. IZA Discussion Papers 16168, Institute of Labor Economics (IZA).

Boneva, T. and C. Rauh (2017). Socio-Economic Gaps in University Enrollment: The Role of Perceived Pecuniary and Non-Pecuniary Returns. Technical report.

Breda, T., J. Grenet, M. Monnet, and C. Van Effenterre (2021). Do female role models reduce the gender gap in science? evidence from french high schools.

DuBois, D. L., B. E. Holloway, J. C. Valentine, and H. Cooper (2002). Effectiveness of mentoring programs for youth: A meta-analytic review. *American Journal of Community Psychology 30*(2), 157–197.

Falk, A., F. Kosse, and P. Pinger (2020). Mentoring and schooling decisions: Causal evidence. Discussion Paper Series 13387, IZA Institute of Labor Economics.

Heckman, J. J., J. E. Humphries, and G. Veramendi (2018). Returns to education: The causal effects of education on earnings, health and smoking. *Journal of Political Economy 126*(1), 197–246.

Hoxby, C. and C. Avery (2013). The missing "one-offs": The hidden supply of high-achieving, low-income students. *Brookings Papers on Economic Activity 44*(1 (Spring)), 1–65.

Kirkebøen, L., E. Leuven, and M. Mogstad (2016). Editor's choice field of study, earnings, and self-selection. *The Quarterly Journal of Economics 131*(3), 1057–1111.

Lagakos, D., B. Moll, T. Porzio, N. Qian, and T. Schoellman (2018). Life cycle wage growth across countries. *Journal of Political Economy 126*(2), 797–849.

Lavecchia, A. M., P. Oreopoulos, and R. S. Brown (2020). Long-run effects from comprehensive student support: Evidence from pathways to education. *American Economic Review: Insights 2*(2), 209–224.

Oreopoulos, P., R. S. Brown, and A. M. Lavecchia (2017). Pathways to education: An integrated approach to helping at-risk high school students. *Journal of Political Economy 125*(4), 947–984.

Porter, C. and D. Serra (2020, July). Gender differences in the choice of major: The importance of female role models. *American Economic Journal: Applied Economics 12*(3), 226–54.

Resnjanskij, S., J. Ruhose, S. Wiederhold, L. Woessmann, and K. Wedel (2024). Can mentoring alleviate family disadvantage in adolescence? a field experiment to improve labor market prospects. *Journal of Political Economy*.

Rodriguez-Planas, N. (2012). Longer-term impacts of mentoring, educational services, and learning incentives: Evidence from a randomized trial in the united states. *American Economic Journal: Applied Economics 4*(4), 121–139.

Wiswall, M. and B. Zafar (2015). Determinants of college major choice: Identification using an information experiment. *The Review of Economic Studies 82*(2), 791–824.

Zafar, B. (2013). College major choice and the gender gap. *Journal of Human Resources 48*(3), 545–595.

# 7 Appendix

## A Additional tables

Table A1: Fields selected at baseline

| | $1^{st}$ | | $2^{nd}$ or $3^{rd}$ | |
|---|---|---|---|---|
| | (%) | (N) | (%) | (N) |
| Pharmacy and biotech | 1.8 | 6 | 6.2 | 21 |
| Medicine and veterinary | 4.5 | 15 | 4.2 | 14 |
| Sports sciences | 0.3 | 1 | 2.1 | 7 |
| Humanities | 4.2 | 14 | 5.9 | 20 |
| Psychology and education | 1.2 | 4 | 6.5 | 22 |
| Foreign languages | 3.0 | 10 | 2.7 | 9 |
| Law | 2.4 | 8 | 5.3 | 18 |
| Political science and sociology | 2.4 | 8 | 13.6 | 46 |
| Architecture and industrial design | 8.0 | 27 | 8.3 | 28 |
| Accounting, business, management | 30.6 | 103 | 19.6 | 66 |
| Economics and finance | 5.9 | 20 | 21.4 | 72 |
| Statistics | 3.3 | 11 | 6.5 | 22 |
| Agricultural sciences | 3.3 | 11 | 3.0 | 10 |
| Biology and environmental sciences | 3.3 | 11 | 8.3 | 28 |
| Chemistry, physics, mathematics | 5.6 | 19 | 8.9 | 30 |
| Computer sciences | 7.1 | 24 | 5.0 | 17 |
| Engineering | 13.4 | 45 | 13.1 | 44 |

Notes. Each row in the table shows the percentage and number of students who at baseline ranked the field as their preferred choice ("$1^{st}$") or as their second or third best ("$2^{nd}$ or $3^{rd}$").

## B Additional figures

Table A2: Samples used in the analyses

| | Endline | | Admin data | | Endline & Admin | | Endline or Admin | |
|---|---|---|---|---|---|---|---|---|
| | (all) | (fe) | (all) | (fe) | (all) | (fe) | (all) | (fe) |
| Treated | 0.000 | 0.016 | 0.087 | -0.019 | 0.100 | 0.005 | 0.000 | -0.016 |
| | (0.110) | (0.100) | (0.083) | (0.062) | (0.064) | (0.041) | (0.110) | (0.104) |
| Control mean | 0.506 | 0.440 | 0.411 | 0.345 | 0.226 | 0.179 | 0.643 | 0.607 |
| Obs in sample | 169 | 148 | 144 | 111 | 83 | 58 | 215 | 201 |
| N | 337 | 337 | 337 | 337 | 337 | 337 | 337 | 337 |

Notes. In each column, the dependent variable is a dummy that takes value 1 if the student belongs to the sample indicated by first and second rows. "Endline" is the sample of students who took the endline survey; "Admin data" is the sample of students retrieved in the administrative data; "Endline & Admin" is the intersection of the two previous samples; "Endline or Admin" is the union of the two. In columns (all), the dependent variable is 1 is the students belongs to the sample. In columns (fe), only groups of two or more students in sample with the same mentors are classified as 1. The coefficient is estimated using 2SLS, with "Treated" instrumented with "Assigned to treatment". Regressions include the dummy "Mentor in preferred field at baseline" and mentor fixed effects. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A3: Choice of mentor's field - first stage

| | (1) | (2) | (3) |
|---|---|---|---|
| Assigned to treatment | 0.745$^{**}$ | 0.753$^{**}$ | 0.734$^{**}$ |
| | (0.072) | (0.069) | (0.074) |
| Mentor in pref. field | | 0.119 | -0.057 |
| | | (0.085) | (0.120) |
| Mentor FE | Yes | Yes | Yes |
| Other covariates | No | No | Yes |
| F-test | 108.2 | 117.8 | 98.3 |
| Take-up rate | 0.59 | | |
| N | 148 | 148 | 148 |

Notes. First stage of the 2SLS regressions in Table 2. The variable "Assigned to treatment" takes value 1 if the student is randomly assigned to the treatment group. The variable "Treated" takes value 1 if a student assigned to treatment takes-up the intervention, that is, meets with the mentor once or more. Other covariates include student predetermined characteristics (dummies for gender, first generation college, academic track) and a vector of dummies for their preferred field at baseline. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A4: Choice of mentor's field - ITT

| | (1) | (2) | (3) |
|---|---|---|---|
| Assigned to treatment | 0.127 | $0.166^{*}$ | $0.153^{+}$ |
| | (0.099) | (0.073) | (0.077) |
| Mentor in pref. field | | $0.627^{**}$ | $0.624^{**}$ |
| | | (0.107) | (0.161) |
| Mentor FE | Yes | Yes | Yes |
| Other covariates | No | No | Yes |
| Control mean | 0.486 | | |
| N | 148 | 148 | 148 |

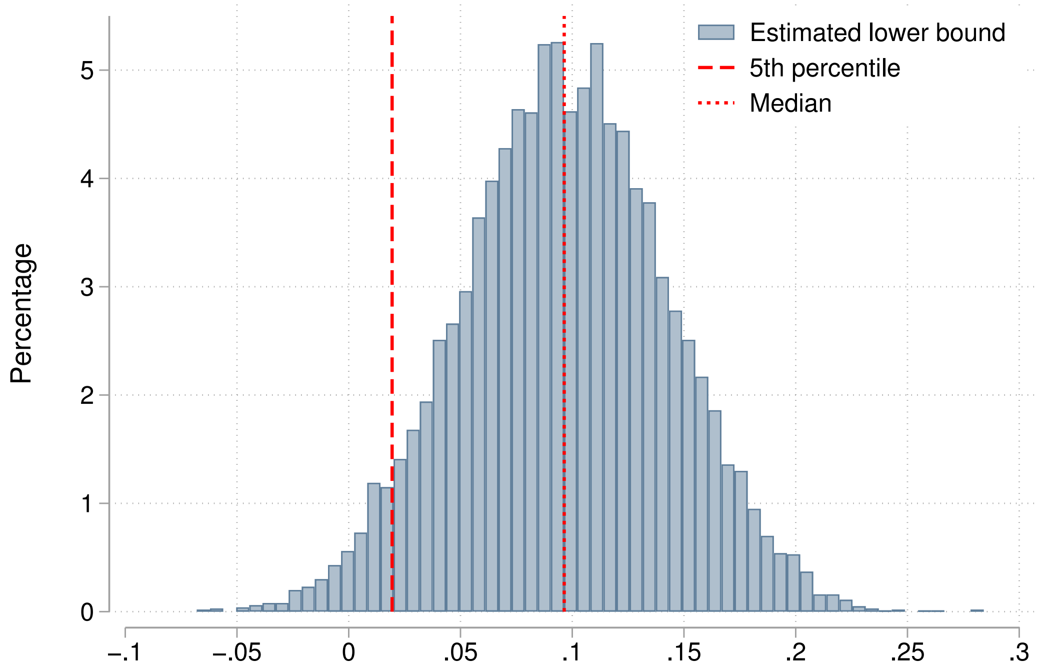
Notes. The dependent variable is a dummy that takes value 1 if the student chooses the same field of study of the assigned mentor according to the endline survey. The dummy "Assigned to treatment" takes value 1 if the student is randomly assigned to the treatment group. The dummy "Preferred field at baseline" takes value 1 if the student ranked the mentor's field as their favorite choice in the baseline survey. Other covariates include student predetermined characteristics (dummies for gender, first generation college, academic track) and a vector of dummies for their preferred field at baseline. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A5: Robustness checks

| | mentor's field | | | mentor's program |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated | $0.220^{**}$ | $0.205^{**}$ | $0.161^{**}$ | $0.184^{*}$ |
| | (0.076) | (0.065) | (0.062) | (0.084) |
| Mentor in pref. field | $0.593^{**}$ | $0.591^{**}$ | $0.602^{**}$ | $-0.284^{+}$ |
| | (0.108) | (0.074) | (0.069) | (0.150) |
| Mentor in pref. program at baseline | | | | $0.850^{**}$ |
| | | | | (0.137) |
| Mentor FE | Yes | No | No | Yes |
| Mentor covariates | No | Yes | Yes | No |
| Mean control | 0.509 | 0.486 | 0.506 | 0.432 |
| N | 110 | 148 | 169 | 148 |

Notes. In columns (1) -(3), the dependent variable is a dummy that takes value 1 if the student chooses the same field of study of the assigned mentor according to the endline survey. In column (4), the dependent variable is 1 if the student chooses the same program of the assigned mentor (a field may contain more than one program). The dummy "Preferred field (program) at baseline" takes value 1 if the student ranked the mentor's field (program) as their favorite choice in the baseline survey. Mentor covariates include dummies for gender, campus (main campus *vs* other campuses), seniority (master *vs* bachelor), and a vector of dummies for their fields. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Figure B1: Simulation results




Notes. The histogram plot estimated "lower bound" effects from a simulation with 10.000 repetitions. In each iteration, we simulate the outcome of students that are not observed in the endline; more specifically, they choose the field of studies of their assigned mentor with probability $p_i$; $p_i$ is predicted using coefficients of a regression of the outcome variable on predetermined characteristics of control students who answered the endline survey. In each iteration, we estimate the treatment effect on the entire sample of students using the same approach as in column (2) of Table 2. The histogram plots the distribution of the estimated coefficients.

Table A6: Heterogeneity by assignment type - ITT

| | mentor's field | | preferred field at baseline | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Assigned to treatment | 0.181 | 0.142 | 0.066 | -0.149 | -0.082 |
| | (0.123) | (0.093) | (0.088) | (0.164) | (0.124) |
| Ass. treat. X mentor in pref. field | -0.024 | 0.029 | | $0.344^{+}$ | $0.252^{+}$ |
| | (0.161) | (0.116) | | (0.204) | (0.150) |
| Mentor in pref. field | $0.639^{**}$ | $0.588^{**}$ | 0.105 | -0.064 | -0.062 |
| | (0.143) | (0.094) | (0.154) | (0.155) | (0.110) |
| Mentor FE | Yes | No | Yes | Yes | No |
| Mentor covariates | No | Yes | No | No | Yes |
| Treatment + interaction | 0.157 | 0.171 | | 0.195 | 0.171 |
| P-val (treatment+interaction) | 0.109 | 0.015 | | 0.070 | 0.020 |
| Control mean - mentor in pref. field | 0.723 | | 0.723 | | |
| Control mean - mentor not in pref. field | 0.074 | | 0.741 | | |
| N | 148 | 148 | 148 | 148 | 148 |

Notes. The dependent variable in columns (1) and (2) is a dummy that takes value 1 if the student chooses the same field of study of the assigned mentor (as reported in the endline survey). The dependent variable in columns (3) - (5) is a dummy that takes value 1 if the student chooses at endline the field that they ranked first at baseline. The dummy "Mentor in preferred field" takes value 1 if the student ranked the mentor's field as their favorite choice in the baseline survey. Other covariates include student predetermined characteristics (dummies for gender, first generation college, academic track) and a vector of dummies for their preferred field at baseline. The row "Treatment + interaction" shows the sum of the first two coefficients (that is, the effect of treatment on students with a mentor from their preferred field at baseline); the following row shows the p-value of this sum. The rows "Control mean" show the mean dependent variable in the control group, among students matched with a mentor in their preferred field or in another field. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A7: Balance tables - administrative data

(a) All students in the admin data

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.536 | 0.573 | 0.019 | 0.052 |
| | (0.502) | (0.498) | (0.116) | |
| First gen. college | 0.493 | 0.547 | 0.033 | 0.076 |
| | (0.504) | (0.501) | (0.127) | |
| From host region | 0.638 | 0.573 | -0.044 | -0.093 |
| | (0.484) | (0.498) | (0.101) | |
| Academic track | 0.870 | 0.800 | -0.080 | -0.132 |
| | (0.339) | (0.403) | (0.087) | |
| Math grade | 7.942 | 7.987 | -0.071 | 0.026 |
| | (1.247) | (1.145) | (0.280) | |
| Italian grade | 8.072 | 8.160 | 0.055 | 0.061 |
| | (1.019) | (1.014) | (0.229) | |
| Field 1 not STEM/ECON | 0.145 | 0.200 | 0.033 | 0.103 |
| | (0.355) | (0.403) | (0.080) | |
| Mentor in preferred field | 0.725 | 0.573 | -0.140 | -0.225 |
| | (0.450) | (0.498) | (0.103) | |
| Observations | 69 | 75 | 144 | |

(b) At least 2 students per mentor

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.500 | 0.566 | 0.069 | 0.093 |
| | (0.504) | (0.500) | (0.147) | |
| First gen. college | 0.466 | 0.547 | 0.072 | 0.115 |
| | (0.503) | (0.503) | (0.165) | |
| From host region | 0.655 | 0.604 | -0.043 | -0.075 |
| | (0.479) | (0.494) | (0.120) | |
| Academic track | 0.862 | 0.755 | -0.105 | -0.193 |
| | (0.348) | (0.434) | (0.115) | |
| Math grade | 7.845 | 7.868 | -0.036 | 0.014 |
| | (1.182) | (1.225) | (0.344) | |
| Italian grade | 8.052 | 8.113 | 0.043 | 0.043 |
| | (1.033) | (0.993) | (0.287) | |
| Field 1 not STEM/ECON | 0.121 | 0.208 | 0.072 | 0.165 |
| | (0.329) | (0.409) | (0.091) | |
| Mentor in preferred field | 0.759 | 0.623 | -0.156 | -0.208 |
| | (0.432) | (0.489) | (0.124) | |
| Observations | 58 | 53 | 111 | |

(c) All students in the admin data and in the main analysis

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.526 | 0.600 | 0.039 | 0.104 |
| | (0.506) | (0.495) | (0.173) | |
| First gen. college | 0.421 | 0.489 | 0.078 | 0.095 |
| | (0.500) | (0.506) | (0.164) | |
| From host region | 0.632 | 0.511 | -0.158 | -0.171 |
| | (0.489) | (0.506) | (0.127) | |
| Academic track | 0.842 | 0.778 | -0.092 | -0.115 |
| | (0.370) | (0.420) | (0.130) | |
| Math grade | 8.000 | 8.133 | 0.235 | 0.078 |
| | (1.252) | (1.179) | (0.335) | |
| Italian grade | 8.211 | 8.400 | 0.251 | 0.147 |
| | (0.935) | (0.889) | (0.262) | |
| Field 1 not STEM/ECON | 0.132 | 0.267 | 0.121 | 0.240 |
| | (0.343) | (0.447) | (0.119) | |
| Mentor in preferred field | 0.816 | 0.578 | -0.244* | -0.375 |
| | (0.393) | (0.499) | (0.120) | |
| Observations | 38 | 45 | 83 | |

Notes. Differences are computed accounting for mentor dummies; in panel a) and c), students without a pair are pooled together in the baseline category. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A8: Balance tables - administrative data

(a) All students in the admin data

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.536 | 0.573 | 0.037 | 0.052 |
| | (0.502) | (0.498) | (0.091) | |
| First gen. college | 0.493 | 0.547 | 0.050 | 0.076 |
| | (0.504) | (0.501) | (0.104) | |
| From host region | 0.638 | 0.573 | -0.014 | -0.093 |
| | (0.484) | (0.498) | (0.079) | |
| Academic track | 0.870 | 0.800 | -0.112 | -0.132 |
| | (0.339) | (0.403) | (0.069) | |
| Math grade | 7.942 | 7.987 | -0.074 | 0.026 |
| | (1.247) | (1.145) | (0.210) | |
| Italian grade | 8.072 | 8.160 | 0.054 | 0.061 |
| | (1.019) | (1.014) | (0.178) | |
| Field 1 not STEM/ECON | 0.145 | 0.200 | 0.064 | 0.103 |
| | (0.355) | (0.403) | (0.058) | |
| Mentor in preferred field | 0.725 | 0.573 | -0.144+ | -0.225 |
| | (0.450) | (0.498) | (0.078) | |
| Observations | 69 | 75 | 144 | |

(b) Admin data & survey data

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.526 | 0.600 | 0.052 | 0.104 |
| | (0.506) | (0.495) | (0.148) | |
| First gen. college | 0.421 | 0.489 | -0.016 | 0.095 |
| | (0.500) | (0.506) | (0.142) | |
| From host region | 0.632 | 0.511 | -0.149 | -0.171 |
| | (0.489) | (0.506) | (0.103) | |
| Academic track | 0.842 | 0.778 | -0.135 | -0.115 |
| | (0.370) | (0.420) | (0.118) | |
| Math grade | 8.000 | 8.133 | 0.086 | 0.078 |
| | (1.252) | (1.179) | (0.279) | |
| Italian grade | 8.211 | 8.400 | 0.276 | 0.147 |
| | (0.935) | (0.889) | (0.230) | |
| Field 1 not STEM/ECON | 0.132 | 0.267 | 0.131 | 0.240 |
| | (0.343) | (0.447) | (0.083) | |
| Mentor in preferred field | 0.816 | 0.578 | -0.287** | -0.375 |
| | (0.393) | (0.499) | (0.094) | |
| Observations | 38 | 45 | 83 | |

(c) Admin data or survey data

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.630 | 0.607 | -0.019 | -0.032 |
| | (0.485) | (0.491) | (0.070) | |
| First gen. college | 0.546 | 0.570 | 0.027 | 0.034 |
| | (0.500) | (0.497) | (0.077) | |
| From host region | 0.593 | 0.533 | -0.030 | -0.085 |
| | (0.494) | (0.501) | (0.065) | |
| Academic track | 0.796 | 0.794 | -0.038 | -0.003 |
| | (0.405) | (0.406) | (0.059) | |
| Math grade | 7.944 | 8.019 | -0.012 | 0.044 |
| | (1.191) | (1.173) | (0.176) | |
| Italian grade | 8.056 | 8.150 | 0.038 | 0.064 |
| | (1.012) | (1.062) | (0.143) | |
| Field 1 not STEM/ECON | 0.231 | 0.215 | 0.009 | -0.028 |
| | (0.424) | (0.413) | (0.054) | |
| Mentor in preferred field | 0.639 | 0.570 | -0.057 | -0.099 |
| | (0.483) | (0.497) | (0.068) | |
| Observations | 108 | 107 | 215 | |

Notes. Differences are computed accounting for mentor covariates. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A9: Balance tables - administrative data

(a) Students in the admin data

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.500 | 0.566 | 0.069 | 0.093 |
| | (0.504) | (0.500) | (0.147) | |
| First gen. college | 0.466 | 0.547 | 0.072 | 0.115 |
| | (0.503) | (0.503) | (0.165) | |
| From host region | 0.655 | 0.604 | -0.043 | -0.075 |
| | (0.479) | (0.494) | (0.120) | |
| Academic track | 0.862 | 0.755 | -0.105 | -0.193 |
| | (0.348) | (0.434) | (0.115) | |
| Math grade | 7.845 | 7.868 | -0.036 | 0.014 |
| | (1.182) | (1.225) | (0.344) | |
| Italian grade | 8.052 | 8.113 | 0.043 | 0.043 |
| | (1.033) | (0.993) | (0.287) | |
| Field 1 not STEM/ECON | 0.121 | 0.208 | 0.072 | 0.165 |
| | (0.329) | (0.409) | (0.091) | |
| Mentor in preferred field | 0.759 | 0.623 | -0.156 | -0.208 |
| | (0.432) | (0.489) | (0.124) | |
| Observations | 58 | 53 | 111 | |

(b) Admin data & survey data

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.467 | 0.643 | 0.160 | 0.250 |
| | (0.507) | (0.488) | (0.245) | |
| First gen. college | 0.500 | 0.464 | -0.074 | -0.050 |
| | (0.509) | (0.508) | (0.228) | |
| From host region | 0.633 | 0.607 | -0.105 | -0.038 |
| | (0.490) | (0.497) | (0.160) | |
| Academic track | 0.800 | 0.786 | -0.026 | -0.024 |
| | (0.407) | (0.418) | (0.197) | |
| Math grade | 8.067 | 8.107 | 0.145 | 0.023 |
| | (1.258) | (1.257) | (0.451) | |
| Italian grade | 8.133 | 8.536 | 0.519 | 0.306 |
| | (0.973) | (0.881) | (0.349) | |
| Field 1 not STEM/ECON | 0.100 | 0.250 | 0.157 | 0.280 |
| | (0.305) | (0.441) | (0.153) | |
| Mentor in preferred field | 0.833 | 0.643 | -0.228 | -0.308 |
| | (0.379) | (0.488) | (0.152) | |
| Observations | 30 | 28 | 58 | |

(c) Admin data or survey data

| Variable | Control | Treatment | Difference | Std. diff. |
|---|---|---|---|---|
| Female | 0.637 | 0.606 | -0.043 | -0.045 |
| | (0.483) | (0.491) | (0.098) | |
| First gen. college | 0.529 | 0.576 | 0.031 | 0.066 |
| | (0.502) | (0.497) | (0.103) | |
| From host region | 0.598 | 0.515 | -0.042 | -0.118 |
| | (0.493) | (0.502) | (0.088) | |
| Academic track | 0.794 | 0.778 | -0.061 | -0.028 |
| | (0.406) | (0.418) | (0.084) | |
| Math grade | 7.843 | 7.960 | 0.039 | 0.071 |
| | (1.141) | (1.186) | (0.232) | |
| Italian grade | 8.049 | 8.091 | -0.099 | 0.029 |
| | (1.028) | (1.041) | (0.196) | |
| Field 1 not STEM/ECON | 0.235 | 0.232 | 0.013 | -0.005 |
| | (0.426) | (0.424) | (0.069) | |
| Mentor in preferred field | 0.647 | 0.556 | -0.091 | -0.132 |
| | (0.480) | (0.499) | (0.091) | |
| Observations | 102 | 99 | 201 | |

Notes. Differences are computed accounting for mentor dummies. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A10: Choice of mentor's field - with administrative data

| | Admin data | | Endline & Admin | | Endline or Admin | |
|---|---|---|---|---|---|---|
| | (all) | (fe) | (all) | (fe) | (all) | (fe) |
| Treated | $0.137^{+}$ | 0.142 | $0.190^{*}$ | 0.190 | $0.149^{*}$ | $0.155^{*}$ |
| | (0.083) | (0.094) | (0.095) | (0.129) | (0.065) | (0.073) |
| Mentor in preferred field | $0.428^{**}$ | $0.453^{**}$ | $0.438^{**}$ | $0.578^{**}$ | $0.549^{**}$ | $0.506^{**}$ |
| | (0.085) | (0.107) | (0.128) | (0.195) | (0.064) | (0.077) |
| Mentor FE | No | Yes | No | Yes | No | Yes |
| Mentor covariates | Yes | No | Yes | No | Yes | No |
| Control mean | 0.565 | 0.603 | 0.632 | 0.733 | 0.500 | 0.490 |
| N | 144 | 111 | 83 | 58 | 215 | 201 |

Notes. In all specification, the dependent variable is a dummy that takes value 1 if the student chooses the same field of study of the assigned mentor. The sample used in the analysis varies according with what is indicated in the top rows: "Admin data" is the sample of students retrieved in the administrative data; "Endline & Admin" is the intersection of this sample with the sample of students used in the main analysis (Table 2; "Endline or Admin" is the union of these two samples. Columns (all) include mentor covariates: dummies for gender, campus (main campus *vs* other campuses), seniority (master *vs* bachelor), and a vector of dummies for their fields. Columns (fe) include mentor fixed effects and only groups with two or more students per mentor are included. The coefficients are estimated using 2SLS, with "Treated" instrumented with "Assigned to treatment". Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

Table A11: Medium run effect on performance - further analysis

(a) Subsample of students who answered the endline survey

| | CFU | | ≥50% exams | | ≥80% exams | | wGPA | |
|---|---|---|---|---|---|---|---|---|
| | (all) | (fe) | (all) | (fe) | (all) | (fe) | (all) | (fe) |
| Treated | 4.002 | 3.762 | 0.118 | 0.134 | 0.052 | 0.095 | 2.047 | 2.336 |
| | (5.616) | (7.322) | (0.118) | (0.142) | (0.140) | (0.179) | (2.721) | (3.583) |
| Mentor FE | No | Yes | No | Yes | No | Yes | No | Yes |
| Mentor covariates | Yes | No | Yes | No | Yes | No | Yes | No |
| Control mean | 42.2 | 40.8 | .763 | .767 | .605 | .567 | 18.2 | 17.6 |
| N | 83 | 58 | 83 | 58 | 83 | 58 | 83 | 58 |

*Panel a).* Dependent variables and regressors are as in Table 5; the analysis are performed on the subset of students who answered the endline survey.
*Panel b).* The dependent variable "Stay in program" is a dummy that takes value 1 if the student stays enrolled in the same program in the academic year 2023/2024, while "Stay and ≥50% exams" takes value 1 if the student is enrolled in the same program and passed 50% or more of the first year exams. "GPA" is the average grade in the exams that the student passed (passing grades range from 18 to 30); if the student did not pass any exam this variable is missing. Regressors and subsamples used are as in Table 5.

The coefficients are estimated using 2SLS, with "Treated" instrumented with "Assigned to treatment". Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$

## C AlmaLaurea data

AlmaLaurea is an interuniversity consortium established in 1994 and supported by the Ministry for University and Research and its member universities. Currently, it includes 81 Italian universities — representing approximately 90% of the graduates in Italy. Every year, AlmaLaurea conducts census surveys on the Profile and Employment status of graduates. Bachelor's graduates are surveyed 1 year after graduation, while master's graduates are surveyed 1, 3 and 5 years after graduation.[30] Aggregated data are publicly available in the AlmaLaurea website.[31]

According to the 2022 survey, 67% of bachelor's graduates pursued further studies and enrolled in a master's program. 96% of them enrolled in a master's program in the same field of studies than the bachelor degree.[32] Besides some vocational programs, particularly in the medical field, all bachelor's programs have a continuation rate above 50%, with peaks of up to 90% for programs such as Mathematics or Biotech. Only 25% of respondents are not enrolled in University and are working, out of the remaining 8%, roughly half are looking for a job and half are inactive. Less than 1 out 4 master's students is also working, while the others only focus on studying. Therefore, for most bachelor's graduates labor market outcomes after the master's degree are the most relevant outcomes to consider.

For the analysis described in Section 5.1, we focus on master's graduates survey 5 years after graduation, because we believe that it provides the most informative data about labor market outcomes over the life cycle. We utilize data from the most recent wave of the survey, which was administered in the same year as the intervention, with the data referring to graduates in 2017. Most respondents are in their late twenties or early thirties when they respond to the survey, having completed their education and being in

[30] Most master's program are 2 years long and require a bachelor's degree for admission. Exceptions are the so called "Lauree a ciclo unico": Law, Primary teacher education, Architecture, Pharmacy, Veterinary, Dentistry, Medicine, which can be accessed after high school and typically last for 5 years, with Medicine being 6 years long.

[31] See https://www.almalaurea.it

[32] Specifically, 76% of students declared that their master's program represents the natural continuation of their previous studies; 20% declared that it is closely related to their previous studies; 4% said that it is not closely related.

a more stable position than 1 or 3 years after graduation.[33]

We use enrollment statistics from AlmaLaurea to map bachelor's programs with their most commonly chosen master's program.[34] Specifically, the website lists for each bachelor's program the most frequently chosen master's programs, along with their respective share of enrollment out of the total number of graduates who pursued a master degrees. 35% of programs are mapped with just one master's program (for instance, Mathematics - bachelor is associated with Mathematics - master), 33% with two master's program (for instance, Economics is associated with Management and Business, and with Economics) and the remaining with 3 to 5 master's programs.[35] Therefore, we compute prospective outcomes for a given bachelor's program as a weighted average of the outcomes for the associated master's programs, with weights given by the proportions of enrolled students.[36] In particular, our analysis focuses on prospective wage.

While pursuing further studies after a bachelor's degree is fairly common in Italy, there are relevant variation across programs. To the extent that master's graduates usually earn more than bachelor's graduate in the same field, our approach may overestimate returns for programs with a relatively low share of students who continue with a master's degree. Ideally, we would average outcomes with a master's degree and a bachelor's degree only, weighting by the proportion of students in the program who pursue further studies. However, for a fair comparison, we would need to observe bachelor's graduates outcomes 7 years after graduation, while AlmaLaurea surveys them only 1 year after graduation. Therefore, we use the survey administered in 2016 (to respondents who graduated in 2015) and project the average wage for each program in 2022. To do so, we use two alternative approaches. First, we simply assume a growth rate of 40% for all programs. This figure

[33] For instance, some master's graduates pursue doctoral studies after graduation, with a relatively low stipend for a few years. This is relatively commons in some fields, especially in Science (e.g. 54% of physics graduates, 32% of chemistry graduates, and 22% of mathematics graduates enroll in a PhD program according to the survey).

[34] We manually collected data from https://www2.almalaurea.it

[35] The website shows shows master's programs up to covering 70% of the enrolled. Thus, rarely chosen programs are not displayed. For instance, if 50% of students from a given bachelor's program enroll in master A, 30% enroll in master B, and 10% enroll in master C, only A and B and their respective percentages are displayed on the page. In the analysis, we rescale the shares so that they sum to 100.

[36] We directly use outcomes from the master's graduates survey for the 5-year master programs ("Lauree a ciclo unico").

is aligned with finding in Lagakos et al. (2018) regarding wage growth in other countries. Second, we compute program-specific wage growth, under the assumption that the growth profile for bachelor's graduates is similar to that for master's graduates. More precisely, for each master's program we compute the wage growth rate from the first to the fifth year after graduation from survey data. To project to the seventh year, we assume that the growth rate from year 5 to year 7 is identical to the growth rate from year 3 to year 5. Finally, for both approaches we compute each bachelor's program growth rate as weighted average of the growth rates calculated for the associated master's programs.

Alma Mater Studiorum - Università di Bologna

DEPARTMENT OF ECONOMICS

Strada Maggiore 45
40125 Bologna - Italy
Tel. +39 051 2092604
Fax +39 051 2092664
http://www.dse.unibo.it