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

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Abstract

We study the impact on the field of study and academic outcomes of a personalized online mentoring program connecting high school students with university students in quantitative fields. Our RCR shows that the likelihood of choosing the field of the mentor increases by 14 to 22 p.p. – a 25% to 45% increase from the baseline. The program shifts preferences towards STEM/Economics, enhancing prospective wages by 3.1-3.7%. Administrative data confirm the validity of survey-based evidence and show that the intervention does not negatively impact performance, even though treated students enroll in more competitive fields. These findings underscore the potential to guide undecided students toward more informed and beneficial educational choices.

Keywords: mentoring, university choices, RCT

JEL Classification Numbers: C93, I23, I26

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We are grateful to all the mentors who enthusiastically volunteered for this project. We thank Maria Bigoni and Lukas Kiessling for their valuable contributions in early stages of this project. We thank seminar and conference participants for useful comments and discussion. We thank Petru Calenici, Alessandro Pompa Pacchi, Federico Stronati, and Manuele Zannini for their research assistance at different stages of this project. Stefania Bortolotti gratefully acknowledges financial support from Diligentia Foundation. The research project received IRB approval from the University of Bologna (No. 109165, May 4th 2021).

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, the evidence overwhelmingly suggests that people 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 misperception of personal ability can derail educational trajectories (Avery et al., 2018; Bobba and Frisancho, 2022; Bobba et al., 2023). 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).

Here, we focus on the selection of the field of study, rather than the decision to enroll in tertiary education. This distinction is important not only because different fields of study have widely varying expected returns, but also because choosing the wrong field may lead to dissatisfaction or increase the likelihood of dropping out. In Italy, where we conduct our study, an astonishing 30% of graduates report that, if given the chance to go back in time, they would not enroll in the same degree program.[1] Failure to select the right degree program may also contribute to the dropout rate, which is approximately the 35% – a figure comparable to that in the USA.[2] The high dropout rate and dissatisfaction with chosen programs highlight 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. These information barriers are particularly significant for students from disadvantaged backgrounds or those with limited access to firsthand information and role models.

This paper presents findings from a field experiment assessing the effects of a one-to-one mentoring program on students' selection of university field of studies. Developed alongside the host institution's orientation office, the program facilitated encounters with

[1]AlmaLaurea, a large consortium of Italian universities, conducted this survey among virtually all students just before graduation. Statistics are sourced from www.almalaurea.it. Further details about AlmaLaurea can be found in Appendix C. Specifically, students were asked, "If you could go back in time, would you re-enroll again at any university?" On average, from 2013 to 2022, approximately 2% responded that they would not enroll at any university. Meanwhile, 30% expressed a desire to choose a different degree program, with 11% opting for a different field of study, 12% preferring a different university, and 7% desiring changes in both aspects.

[2]For Italy: ISTAT indicators for university education, accessible at dati.istat.it. For the USA: graduation within six years after entry at a four-year college, as shown in Table 326.10 in the Digest of Education Statistics at nces.ed.gov. More generally, Italy is one of the EU countries with the lowest rates of young adults holding university degrees. According to the OECD, in 2022, less than 30% of 25-34-year-olds attained tertiary education, compared to the EU average of 47% and 51% in the USA. The indicator "Share of population by educational attainment" is accessible at OECD.stat.

successful and motivated undergraduate and graduate students enrolled in a quantitative field and volunteering as mentors. The online meetings were designed to be open-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 met 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.

We consider this intervention particularly relevant in our setting for at least two reasons. First, in Italy, as in most European countries, students enroll in specialized programs, with relatively inflexible curricula. This implies that the system penalizes students who seek to transfer between fields. Most students who switch fields at the end of their first year must nearly start from scratch, a challenge that is especially pronounced when moving from a quantitative to a non-quantitative field. Given the long-term consequences of this choice and the costs associated with switching fields, we believe this is a highly consequential decision that warrants careful consideration and guidance. Second, in our reference population, most students remain highly undecided about their academic path even in the final months leading up to enrollment in a bachelor's program. Data from a large online orientation event organized by the host institution indicate that, about six months before the final enrollment deadline, approximately half of the participants are still exploring multiple fields of study. Among those interested in a quantitative field, roughly half are also considering a non-quantitative field. Furthermore, among students with at least one quantitative field of interest, 23% listed a non-quantitative field as their first choice. These figures clearly show that students are not only undecided among similar fields but often weigh vastly different subjects. This suggests significant potential for providing guidance in the decision-making process, particularly in encouraging students toward more quantitative and financially rewarding fields.

To evaluate the impact of the intervention, we conducted a randomized controlled trial (RCT) among 337 high school students in their final 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 10,000 students in their last year of high school. To enroll in the mentoring program, students had to complete a baseline questionnaire where we collected background information and their ranked list of fields of study they were interested in. We used this information to match each applicant with a mentor; randomization into Control and Treatment group relied on program oversubscription, and only students in the Treatment group were informed about the assignment. The observable characteristics of the pool of applicants align very well with national statistics on students who pursue further studies after high school graduation. 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 enrollment and performance from the host university.[3] 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.[4]

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 choosing 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%). Results based on actual enrollment data from the host institution confirm the sizable effect of the intervention; we estimate an increase with respect to the Control group ranging between 25% and 30%.

Second, we find that our mentors 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 in which field of study the treated mentees would have enrolled in absence of the intervention to make sure our program is not generating undesired negative effects. We observe an increase in the likelihood of choosing STEM/Econ fields, a decrease for Humanities, no effect for Medical professions. To gauge a better understanding about labour market prospects, we also test the effect of the intervention on future earnings, using average monthly wage of recent graduates in the same field. 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 en-

[3]We have access to administrative data for only those students who enrolled in the host institution (43% of the initial sample).

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

hanced 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 part of expensive 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), rigorous evaluations of pure mentoring interventions are scarce.[5] To the best of our knowledge, we are the first to study the causal impact of one-to-one mentoring on university choices and subsequent performances. In fact, previous mentoring intervention, have either focused on the transition to high school or to the labor market. Falk et al. (2020) find that mentoring programs offered during childhood or early adolescence substantially increase the probability of enrolling in an academic track after completing elementary school for children from low socio-economic backgrounds. Instead, Alfonsi et al. (2024) and Resnjanskij et al. (2024) focused on older students, and tested the efficacy of mentoring intervention on the transition from vocational training to the labor market. Resnjanskij et al. (2024) shows that mentorship improves the performance and labor market prospects of 14-year-old from similar backgrounds enrolled in vocational education. Complementary to these findings, Alfonsi et al. (2024) demonstrates that mentorship enhances the school-to-work transition of young adults from vocational schools in Uganda. Both studies confirm that mentoring programs can have substantial effects well beyond childhood. While programs targeting children and younger teens usually require in-person meetings, interactions in both Alfonsi et al. (2024) and our study took place remotely (online or over the phone), making these interventions easier to organize and scale up. Similarly, Biroli et al. (2024) provides evidence of the impact of online role-model interventions among middle school students in Italy. Treated students are more likely to enroll in an academic track and perform better on standardized tests.

Our intervention involved mentors from quantitative fields with relatively higher expected returns, where females tend to be underrepresented. In this respect, our study bears some resemblance to 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 through a one-time session positively affected their subsequent enrollment in economics courses. Similarly, Breda et al. (2023) found that classroom interventions could diminish gender stereotypes and encourage high-achieving females to pursue male-dominated fields. Our study differs from these role model interventions in several ways. First, we recruited mentors of both genders. Second, mentor-mentee interactions were one-on-one, and we encouraged mentees to ask

[5] See DuBois et al. (2002) for a review of non-experimental pure mentoring interventions.

questions themselves to receive personalized, ad-hoc answers. Although our program was relatively light-touch, with mentors and mentees meeting only a few times over approximately five months, the mode of interaction and customization of the program was markedly different. Third, we did not target a specific field for all participants but tailored the mentorship to the individual requests of our participants. By design, role model interventions often involve a large number of students, many of whom have no interest in pursuing a specific career, which limits the impact to a subset of participants. While gender was not the main target of our intervention, the findings are particularly compelling for female students (comprising 60% of our sample), because they suggest that one-on-one conversations with current students can effectively increase woman's enrollment in fields where they are underrepresented.

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 primary school, three years of middle school, and five years of high school. Education is compulsory from the ages of 6 to 16, with tracking occurring after the 8^{th} grade. At this point, students can choose between three types of high schools: academic (*licei*), technical, or vocational. Students are usually 18 or 19 when they conclude high school and can access tertiary education regardless of the type of high school diploma they hold. In 2022 about 54% of the students enroll in the academic track, 31% in the technical, and 15% in the professional one.[6] 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.[7] 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,

[6]Our elaboration on Invalsi data, see Table 1.

[7]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.

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 TOLC. Unlike other international tests such as the SAT, TOLC tests are not uniform across fields, 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 students, and the standardized test is used 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. Given the large number of degree programs and the specificity of some curricula, we will focus primarily on the field of study throughout the paper. This is important 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:

- **Fields:** they tend to overlap with the departments offering the program. In our intervention, we consider a total of 17 fields and offer mentors for 9 of them, as listed in Table A2 and detailed in Section 2.2.[8]
- **Degree program classes (as defined by MIUR):** each program offered in an Italian university must be approved by MIUR, which will assign a *degree program*

[8] 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.

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 of the bachelor program;

- **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.[9] In 2022, the host university offered 97 bachelor's degrees and 14 5-year master's degree. In some cases, the same degrees, with very similar curricula, are offered in multiple campuses.[10]

To better understand the relationship between the three measures – field, degree program class, and university-specific programs – let us 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 one specific degree (Mathematics), the program class of Physics include two (Physics, Astronomy), and the program class of Chemistry include five (Chemistry, Industrial chemistry, Chemical methodologies, Chemistry for the environment, the latter offered in two different campuses).

2.2 The mentoring program

We designed the intervention to study the effect of mentorship on university choices, particularly the choice of the field of study, 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,

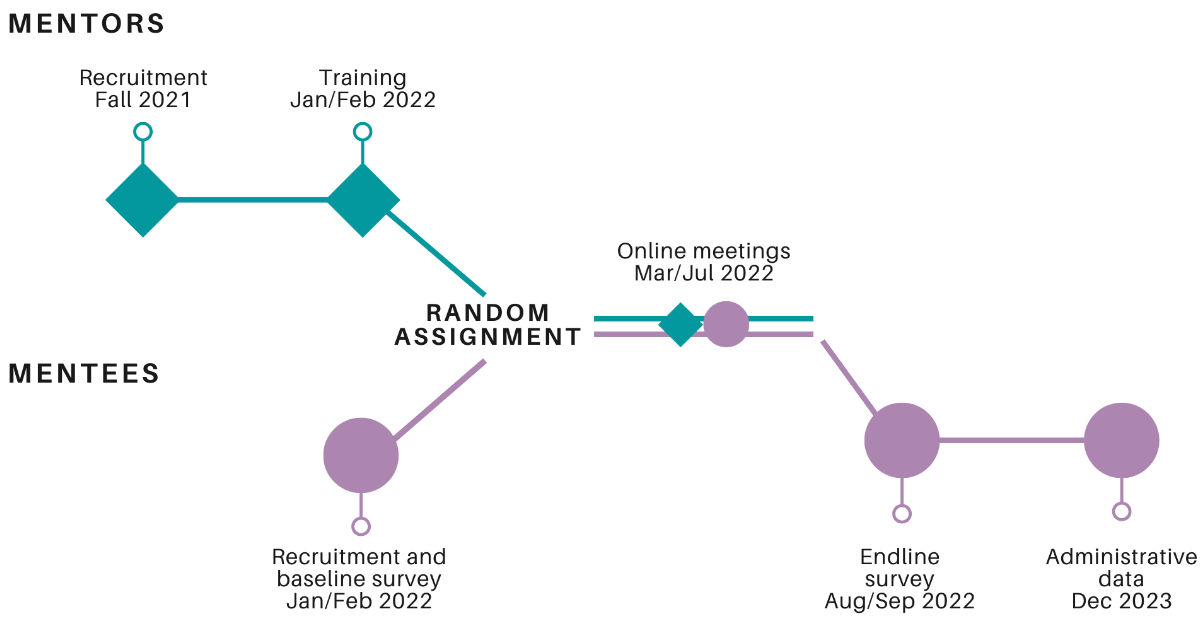
[9] There is only one exception. When utilizing administrative data from the host university, we rely on average performance at university-specific degree program level as controls.

[10] The host university has one main campus located in the main city of the region, and four other smaller campuses in nearby cities.

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.

Figure 1: Timeline of the intervention



2.2.1 Recruiting mentors and high school students

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 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 final-year high school students attending an Italian high school. We promoted the mentoring program on the homepage of the university's orientation office in the weeks prior to and during the main virtual orientation fair organized by the host institution. In 2022, over 10,000 students in their last year of high school from all Italian regions enrolled in one of the more than 1,800 events offered during the fair. Although we do not have official data, it is reasonable to assume that most of the students enrolled in the events must have visited the homepage of the orientation office to enroll. Despite the significant traffic on the website, our program was just one of the many initiatives that were advertised at that time. We also had the opportunity to promote the initiative in one virtual stand, but due to the high number of events, the average number of visitors is fairly low in this type of virtual spaces.[11] The program was also advertised 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 made aware that, due to capacity constraints, not everyone would be assigned a mentor.

Content of the online meetings. 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, interactions with classmates, and interactions with professors (see Table A1 in Appendix for the frequency of each topic). The satisfaction rate with the meetings was extremely high for both mentor and mentees.

[11] On average, 46 students for events dedicated to a specific degree program and 17 for other events.

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*).

- **Ranking 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.[12]

In the endline survey, we skipped the background information and we 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 consider our endline self-reported measure of enrollment to be a good proxy for the actual choice for several reasons. Even though the enrollment period was still ongoing, about half of the participants reported having completed the enrollment process. Of the remaining students, most indicated that they had already taken the test and were waiting for the intake results. The reliability of the self-reported data is discussed in Section 5.2, where we compare administrative data with self-reported data for a subset of our respondents and find that the two sets of data align for virtually all participants. Another concern, could be related to the possibility to enroll in the desired field, given the cap on the number of students admitted. In this respect, it is important to note that there is no centralized system, so being unable to enroll in a specific degree program at one university does not preclude enrollment in the same program at another university. Furthermore, within the same field, there are several degree programs. We have reason to believe that most students did not shift their interest away from a field due to a low score on the entrance test; only 6 respondents reported attempting to enter a program but failing.

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

In the second part of the endline survey we collected some information about the information-gathering process, motives and expectations. Finally, we asked respondents to guess the performance of fellow university students in two different fields; vouchers were awarded to the students who performed best in this task.

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).

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 in detail in the next paragraph. Subsequently, students matched with a particular mentor were randomly *assigned* to either the Treatment or the Control group.[13] Treated students were introduced to their mentors, 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, even the mentors were not informed about the matched students assigned to 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.[14] 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

[13]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.

[14]Some students ranked a non-quantitative field — for which we had no mentors — as their first choice. To ensure they could find a suitable match, we prioritized these cases in the sorting process.

for higher-ranked programs.[15] 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 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 applicants included in the randomization process. We excluded from the sample used for this study all applicants who declared that they were not interested in any of the fields included in the intervention.[16]

The first column of Table 1 presents descriptive statistics for our baseline sample. The second column provides data on high school students in Italy in their final year, while the third column reports figures for first-year university students in Italy. The fourth column shows statistics from a 2022 national survey (AlmaDiploma) of high school students in their final year, computed on the subsample of respondents who intend to pursue further studies. The last column offers a comparison with the students who registered for the online orientation fair where we advertised our intervention.

About 67% of our sample attended an academic track (i.e., licei), a slightly lower proportion than the one found in the actual Italian University population or at the orientation fair (75%), and remarkably similar to the AlmaDiploma figure in column (4). Not surprisingly, students from an academic track are overrepresented in the university population, as they account for about 54% of the overall high school population.

Math and Italian grades are slightly higher in our sample compared with the the general high school population, consistently with a positive selection into tertiary education.

[15] 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.

[16] 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. Furthermore, 8 students were the only match with their mentor; we let them participate in the program, but given the absence of a counterfactual control students they are not included in the analysis. For 7 of them we can confirm with either survey or administrative data that they enrolled in the same field of their mentor.

Table 1: Characteristics of the sample

	Our sample at baseline (1)	High school population (2)	University population (3)	Prospective uni. students (4)	Orientation virtual fair (5)
School track *(%)*					
Academic	66.5	53.6	75.0	69.8	74.7
School performance					
Italian grade *(max 10)*	8.02	7.39	-	-	-
Math grade *(max 10)*	7.83	7.06	-	-	-
Final grade *(max 100)*	84.18*	81.02	83.82	84.3	-
Background					
Female *(%)*	61.4	53.8	55.0	61.0	67.0
First gen. college *(%)*	59.1	69.6	-	62.6	-
From host region *(%)*	52.2	6.9	-	-	63.5

Notes. Data in columns (2)-(5) are based on the following sources. (2): Own computations from a representative sample of Italian students enrolled in the last grade of high school in 2021/2022 (we used Invalsi data publicly available at serviziostatistico.invalsi.it). (3): School track and background data from MIUR - Ufficio Statistica e Studi (report available at ustat.mur.gov.it), figures refer to students who enrolled in an Italian university program for the first time in the academic year 2016/2017; Final grade from MIM data for the school year 2012/2022 (available at www.mim.gov.it). (4) AlmaDiploma survey, statistics for a national sample of 19009 high school students who intend to pursue tertiary education in 2022; data includes their realized final grade (www.almadiploma.it). (5): Sample of 10556 high school students in their final grade who enrolled in the virtual fair organized by the host institution.
* Expected final grade at baseline, roughly 5 months before graduation

In fact, students' expected final grade at graduation align closely with both the national average among university students (col. (3)) and the average among high school students who intend to enroll in further studies (col. (4)). Females represents 61% of our sample, a proportion between the national average (55%) and the figure for the orientation fair (67%), and almost identical to the proportion of respondents to the AlmaDiploma survey.[17] 59% of the participants are first-generation college students, that is, neither parent attended university. This is an important demographic given their likely need for guidance, as they may have less access to direct information about university life. Once again, this proportion is similar to that observed in the national AlmaDiploma survey among students planning to enroll in further studies. As shown in column (2) the proportion is even higher (nearly 70%) among the full population of high school students. Consistent with the statistics for the host institution, roughly half of the students come from the region where the university is located (the percentage was slightly higher for the virtual fair).

Panel (a) of Table 2 presents summary statistics for our baseline sample, separately for Control and Treatment groups. All the relevant characteristics of our sample are balanced across Control and Treatment groups.

[17] Similarly to the national average, 56% of students enrolled in the host university in 2022 were females. However, all fields for which mentors were available have a female enrollment share lower than 50%. This may suggest a relatively higher interest of women in orientation activities that target quantitative fields.

Mentees' preferred fields of study. In the baseline questionnaire, we asked respondents 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 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 matched with a mentor from their second or third most preferred field. About 55% of the students were instead matched with a mentor from their first field of study at baseline. Table A2 in Appendix reports the distribution of first and second/third fields for our baseline sample.[18] To better understand the representativeness of our sample in terms of fields of interest, we rely on data from the virtual fair. To enroll in the event, students were required to indicate one or two fields of interest. Notably, about 7 out of 10 students reported more than one field of interest, suggesting that the majority were still exploring different academic paths at the time of the fair (which corresponds to the enrollment in our program).[19] Almost half of the students who enrolled in the event expressed interest in at least one quantitative field. Interestingly, roughly half of those interested in a quantitative field also sought information about a non-quantitative field. Among students with at least one quantitative field of interest, 23% listed a non-quantitative field as their first choice. This figures reinforces the idea that students are not only undecided among similar fields but often weigh very different subjects with potentially distinct labor market returns.

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 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).

[18] Accounting, Business & Management (31%) 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%).

[19] As mentioned in Section 2.1 the host university uses a coarser definition of fields than ours. Thus, it is likely that this proportion would have been even higher using our classification.

Table 2: 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	0.685	0.645	-0.046	-0.059
	(0.466)	(0.480)	(0.054)	
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	0.676	0.622	-0.098	-0.080
	(0.471)	(0.488)	(0.110)	
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	

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

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%. This take-up rate is not surprising given the context; we never interacted personally with the applicants, with most communication occurring via email. Higher take-up rates are typically observed when school teachers or caregivers are directly involved in the recruitment process, allowing messages to be delivered directly to students by familiar figures. We notified applicants about the availability of a mentor (or the lack thereof) through email. It is plausible that a non-negligible share of the students failed to open the email, either because it was diverted to their spam folder or due to their infrequent use of this communication method.

The take-up appears unrelated to students' or mentors' observable characteristics, except for previous grades in Mathematics, suggesting that higher-achieving students might be more likely to participate. Sharing the same gender or same prospective living situation with the mentor are also unrelated with take-up, while being assigned a mentor in the preferred field at baseline is associated with an increased likelihood of participation. Notably, students who submit their application towards the end of the recruiting period are significantly more likely to met their mentor, suggesting that early applicants may have experienced a waning interest over time.[20]

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. 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 of 148 students, equally distributed across the Control and Treatment groups. In this sample, we include 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).[21] Our attrition rate should not be surprising bearing in mind that we did not have any direct contact with students other then their email. For comparison, consider that Biroli et al. (2024) report a 24% attrition rate between baseline and endline, even

[20]Table A3 in the Appendix shows regression results. A higher math grade is associated with an 11-13 p.p. increase in the probability of take-up. Submitting the application one day later is associated with an increase of almost 1 p.p. Being assigned a mentor in the preferred field is associated with a 13-20 p.p. increase (significant at the 10% level in the specification including all regressors). No other variables have a significant effect consistent across specifications.

[21]This subsample of 148 students and the sample of 169 respondents do not differ along any observable characteristic. Balance tables are available upon request from the authors.

when data were collected during school hours, or that Carlana and La Ferrara (2024) document a 56% attrition rate among control participants, despite the intervention being developed with the support of teachers. As a further reference, general surveys administered to university students can also be considered. In a previous survey distributed via the official channels of the host institution to the entire student population, the response rate was only 12%, despite its visibility on all university webpages and strong advocacy from student representatives.

Panel b of Table 2 reports the summary statistics at endline for Control and Treatment groups. Crucially, none of the observable characteristics 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.

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:

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

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

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, μ_j and ν_j are mentor j's fixed effects, and η_i and ϵ_i are error terms. Given the random assignment, ϵ_i is uncorrelated with the regressors. Conversely η_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 students 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. This type or spillovers are likely to render the estimates conservative, given that they tend to equalize outcomes of treated and non-treated students. In any case, the online nature of our program and the fact that participants are spread out throughout the country make spillovers a minor concern in our settings. In fact, students come from 73 different provinces and 9 school tracks, with 163 "`province X school track`" combinations.[22] 30% of the students are the only one from their `province X school track` combination, and 44% belong to `province X school track` combinations with 2 to 5 students.[23] These figures suggest that the likelihood of having multiple students from the same school is quite low. In fact, the `province X school track` combination is a coarse classification; for instance, just in the province where the host institution is located, there are 30 geographically separated high schools offering just one specific school track (i.e., liceo scientifico) which we bundle together for lack or more detailed information.

Mentor's fixed effect and covariates. In our main analysis in Section 4, we 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 further diminishes if we only consider instances where at least one student from the Control and one from the Treatment group per mentor replied to the final survey (N=110). Additionally, we conduct robustness checks employing mentor's covariates as proxies for mentor's fixed effects, allowing us to include all 169 respondents to the endline survey in the analysis. In Section 5, we utilize various samples obtained by merging survey data with administrative data. Due to the small sample size, including mentor's fixed effects is empirically demanding

[22] 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.

[23] 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.

for some of these samples. Therefore, throughout the section, we present results including both mentor's fixed effects and mentor's covariates.

Selection into endline. As discussed in Section 2.3, observable characteristics of endline respondents are balanced across the Control and Treatment groups. Results from two-stage least squares estimates in the first two columns of Table A4 present further evidence that treated students are not more likely to answer the endline survey (first column) or belong to our main analysis sample (second column). In both cases the estimated treatment effect is not only insignificant, but also very close to zero. In Section 4.1 we discuss robustness checks to further confirm that our results are not driven by unobserved characteristics of endline respondents.

4 Results

4.1 Effect of the intervention on enrollment choices

We first analyze the enrollment choices as self-reported in the endline survey, examining the extent to which they align with the mentors' fields. Table 3 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 matched mentor, and 0 otherwise.[24] 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 is safe to assume that mentors had no influence on the 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 effect of meeting a mentor, significant at 10% level. 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.

[24] The results from the first stage are available in Table A5 in the Appendix.

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 3 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 respondent is a first-generation college student, attended an academic track, and a vector of dummies for their preferred field at baseline.[25] The results are qualitatively and quantitatively consistent with previous estimates. 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 specification in Model 2 as benchmark throughout the paper.[26]

Results from Table 3 are confirmed by the ITT estimates reported in Table A6 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.[27] This effect is larger than the minimum detectable effect (MDE) for this sample assuming a power of 0.8 and alpha level of 0.05.[28]

Robustness checks. Table A7 in the Appendix presents a series of robustness checks for our main result. Our preferred specification (Model 2 from Table 3) includes all instances in which at least two students per mentor responded at 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 the coefficient. Model 1 in Table A7 replicate the analysis restricting the sample to the 110 observations fulfilling this requirement. As expected, results are nearly identical.

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

[26]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.

[27]Results are qualitatively similar, although not always significant when considering the other specifications.

[28]To compute the MDE we used the tool "Powerup" for 2-Level Constant Effects Blocked Individual Random Assignment Design (Dong and Maynard, 2013). The MDE is 0.278 standard deviation, corresponding to approximately 13.9 p.p.

Table 3: Choice of mentor's field

	(1)	(2)	(3)
Treated	0.170^{+}	0.221^{**}	0.200^{*}
	(0.100)	(0.076)	(0.079)
Mentor in pref. field at baseline		0.601^{**}	0.643^{**}
		(0.086)	(0.121)
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 matched mentor according to the endline survey. The dummy "Mentor in preferred field at baseline" takes value 1 if the student ranked the mentor's field as their favorite option 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 macro-area 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. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

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 due to the small number of observations associated with each mentor, and forces us to drop some observations from the analysis. To address this potential concern, in Model 2 and 3 of Table A7 we replicate the results using mentor's covariates as a proxy for the mentor's fixed effect, including dummies for gender, campus, master student status, and a vector of dummies for their field of study. Model 2 is estimated on the original sample of 148 students; coefficients are very similar to our preferred specification, suggesting that mentors' personal characteristics effectively replace the fixed effects. Therefore, in Model 3 we include all 169 respondents to the endline survey. The treatment effect remain statistically significant in this specification, albeit slightly smaller in magnitude.

In Model 4, we construct a new dependent variable that takes 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 if the student ranked the degree program class attended by the mentor as their first choice. 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. There is no differential attrition among Treatment and Control groups: their response rate are identical and their observable characteristics are balanced (Table 2). However, 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. Let consider the following hypothetical scenario: (i) the likelihood of responding to the endline survey was orthogonal to the treatment effect among controls; (ii) treated students who were affected the most were also more likely to respond, for instance because having particularly appreciated the program they were more responsive to messages from the host university; (iii) other treated students were less likely to respond than the control groups, for opposite reasons. In this hypothetical case, one would overestimate the real effect of the intervention.

To address this potential concern, we estimate a "lower bound" for the true effect, assuming that the intervention had zero effect on all mentees in the Treatment group who did not answer to the endline survey. In other words, we assume that their likelihood of choosing the mentor's field is comparable to that observed among similar students in the Control group. We find that even under this very restrictive assumption, the average effect of the program is sizable.

To compute this 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.[29] 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 3) 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.[30]

[29] 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.

[30] 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.

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 and/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 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 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 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 4 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 mentor's covariates. 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 hence 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 4) 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.[31]

Table 4: 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
Treated + interaction	0.196	0.207		0.248	0.213
P-val (treated+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 matched mentor (as reported in the endline survey). The dependent variable in columns (3) - (5) is a dummy that takes value 1 if the student rank first the same field both at baseline and endline. The dummy "Mentor in preferred field" takes value 1 if the student ranked the mentor's field as their first choice at baseline. Mentor covariates include dummies for gender, campus (main campus vs other campuses), seniority (master vs bachelor), and a vector of dummies for their fields. 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. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Characteristics of mentors and students. One might wonder whether there is a heterogeneous treatment effect based on the characteristics of mentors and students. We first consider the mentors and focus on two dimensions that seem particularly relevant: career stage (e.g., bachelor's vs. master's students) and gender.

Table A9 reports the results of the heterogeneity based on mentors' characteristics. In models 1 to 2, we interact *Treated X Mentor master student* and in models 3 to 4 we

[31] ITT estimates are reported in Table A8 in the Appendix. Results are qualitatively aligned with the one reported here and, as usual, estimated effects are smaller in size.

consider the interaction between *Treated* and *Mentor female*. As in Table 4, the sum of *Treated* and the interaction estimates the effect of meeting a mentor enrolled in a master program (columns 1 and 2) or a female mentor (columns 3 and 4). While the results should be interpreted cautiously due to the small sample size, it is noteworthy that mentors enrolled in master's programs seem to be the primary drivers of the observed effects. We hypothesize that more experienced mentors, having completed their foundational coursework and now specializing in topics of personal interest, may offer a broader and more inspiring perspective on their field.

There is also some suggestive evidence that female mentors might be more effective. However, the insights we can gain from this result are limited, and we suspect it may largely be driven by the self-selection of mentors. Despite the male-dominated fields under consideration, we had a majority of female mentors. This could be attributed to the general tendency of women to volunteer more frequently than men, but it may also reflect an additional motivation stemming from their underrepresentation in their studies. This lack of representation might drive them to help others, as they may have faced similar challenges in adapting to their fields.

We also explore heterogeneity with respect to students' characteristics and find no significant differences by gender or first-generation college status. However, establishing any causal effect by exploiting within-mentor variation is challenging in this sample, as relatively few mentors are matched with students with differing characteristics.

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 fields 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 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 5, 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.

To gain a precise understanding of the monthly wages that students may anticipate when enrolling in a particular program, we leverage data from AlmaLaurea, which surveys university graduates in the years following their graduation (for further details, see Section C in the Appendix). While our estimates are necessarily based solely on the university choices made by our participants, it is well established that these choices can have profound and lasting effects on their labor market prospects. Additionally, we utilize a range of detailed, program-specific measures, including the returns experienced by former students of the same program and the actual likelihood of continuing studies beyond a bachelor's degree. For each program, we calculate the average wage 5 to 7 years after graduation, when respondents are typically in their late twenties or early thirties. The results are presented in Panel b) of Table 5. 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

Section 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.[32]

Table 5: 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)	(2)
Treated	64.360^{*}	58.003^{+}	51.708^{+}
	(28.156)	(30.412)	(26.422)
Control mean	1725.931	1629.238	1659.419

Notes. In both panels, the same sample and set of regressors as in column (2) of Table 3 are used.
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 at endline.
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.
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

[32] 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.

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. 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.

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.[33] 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 use data on the degree program in which they are enrolled for both December 2022 and 2023 (indicating enrollment in the first and second year of university, respectively). Changes in the program during the first months of the academic year are rare and do not affect our analysis.[34] 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).

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" part, 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, although 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.

[33] 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.

[34] 7 students "pre-enroll" in a different degree program and then switch before December 2022. Using the initial program, our main dependent variable of interest (enrolling in mentor's field) would only change for two treated students, from 0 to 1. Thus, if anything, using enrollment in December makes our results more conservative.

5.2.1 Sample selection and description

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.[35]

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. Second, 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 11 of the 13 students that declared that they already met all the administrative requirements (e.g., passing the admission test) but had not completed the enrollment process yet, and 14 of 18 who already submitted the application and took the required test but did not know their admission outcome yet.[36]

5.2.2 Replication of the main finding

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,

[35] 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 the field of Medicine at the endline, but enrolled in Pharmacy or Chemistry according to the administrative data. They took our survey before enrolling in the university, as stated in the questionnaire and confirmed by comparing the dates.

[36] 42 students declared that they wished to enrol in the host institution, but have not started the application process at the time of the survey. We retrieve 11 of them in the administrative data. They may have failed some of the legal requirements to enroll and eventually have chosen a different university.

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.[37] 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 A4 in the Appendix, 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 A10 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 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.[38] 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 A11 in the Appendix replicates the main analysis (Table 3) 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.[39] 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

[37] 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.

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

[39] 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%.

estimated, but confirms that the intervention had a large impact on treated students.

So far, we have focused on first-year enrollment, but our administrative data also allow us to examine whether the effect persists when considering enrollment in the second year—over a year after the end of our intervention. Of the 144 students identified in the administrative data for 2022, 17 did not enroll again in the host university for the academic year 2023/2024. Unfortunately, we do not have information about their outcomes: they either dropped out of university entirely or started a new degree program from scratch elsewhere.[40] Among those who remained enrolled at the host university in 2023, 10 switched to a different degree program. In only two of these cases did the change involve switching into or out of the mentor's field, that is, our dependent variable.[41] Overall, 27 students did not enroll in the same program in the second year (18.7% of our sample); this figure is comparable with the statistics from the host university (18.2%).

Table A12 in the Appendix presents the results for enrollment data in 2023. The first two columns report the likelihood of being enrolled in the same field as the mentor in 2023 for all students who appear in the administrative data for both years. Therefore, this analysis focuses on students who renewed enrollment in the host university for the second year. As before, the models in the (all) columns include all available information and control for mentor covariates, while the models in the (fe) columns use restricted samples to include mentor fixed effects. The results are both qualitatively and quantitatively consistent with those observed in the 2022 administrative data (see the first two models in Table A11 in the Appendix).

In the next two columns, we replicate the analysis, but this time we assign a value of 0 to all students who are not present in our 2023 dataset. In other words, the dependent variable is 1 if the student is still enrolled at the host university and in the same field as their mentor. Based on this specification, we find that the probability of being enrolled in the same field as the mentor during the second year of university is 22 percentage points higher for Treated students compared to Control students.

5.2.3 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

[40] More precisely, only two of them officially transferred to a different university and requested recognition of their exams; only for these students, we observe the new program in which they enrolled. While one of them moved during the first academic year, the other transferred right before the beginning of the second year. Including them in the following analysis would deliver very similar results.

[41] We remind the reader that, in the Italian system, transferring from one field to another at the end of the first year can be very costly, as few credits are typically recognized, and many first-year courses are prerequisites for second-year courses, leading to further delays in a student's academic progress.

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 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 slower completion rate—our goal is to ascertain that the intervention does not negatively affect their medium-term university outcomes.

Table 6: 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 obtained at least half of the credits 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 obtained at least 80% of the credits 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. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 6 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 A13 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 6 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.[42] Although students are expected to acquire 60 CFU to complete their first-year course load, it is common for them to fall behind.[43] 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 GPA, and once again, we find a positive sign for the treated, although the effect is not significant.

To better understand how our sample compares to other students, we analyze the difference between the number of CFUs acquired by each subject in our sample and the average CFUs for the specific degree program in which they are enrolled.[44] Figure B2 plots the distribution of this difference separately for the Treatment and Control groups. In the Control group, 58% of students exhibit performance above the average, but the difference between our control students and the average student in their degree program is not statistically significant. This provides further reassurance that our sample is not markedly different from the population of the host university. Interestingly, in the Treatment group, 69% of students outperform the average, and fewer display a significantly negative gap. This further supports the evidence that the program has been particularly beneficial for weaker students.

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

[42] 1 Italian CFU corresponds to 1 ECTS credit in the European Credit Transfer and Accumulation System.

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

[44] Data refer to 2021.

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.[45]

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 our context, it is not only costly to switch to a different field after the first year, but a sizable share of students report to be undecided among different field just months before enrollment. Analysis of data from the host institution's virtual fair indicates that approximately half of those interested in quantitative fields are also considering non-quantitative alternatives - and often rank those first. This highlights a substantial opportunity to guide students in their decision-making process, particularly in encouraging engagement with quantitatively and financially rewarding fields.

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.

We estimate that mentored students are between 14 and 22 percentage points more likely to choose the same field as their mentors, an increase compared to the Control group ranging from 25 to 45%. While estimates might change in size depending on the sample (self-reported or administrative data) or the specification, the effect size is remarkably large for a cost-effective, light touch intervention that has the potential for easy expansion on a larger scale.

The program notably shifts student preferences towards STEM and Economics fields, leading to an estimated increase in prospective wages by 3.1-3.7%, without negatively impacting academic performance at the university level. These findings underscore the significant role that mentorship can play in guiding students toward educational choices that are not only informed but also economically beneficial. However, it's important to acknowledge that these outcomes may be partially influenced by the specific design of our intervention, which exclusively offered mentors in quantitative fields. If mentors had been provided across all fields, regardless of their associated economic returns, we might not

[45] 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.

have observed the same positive impact on prospective wages.

Our decision to focus on mentorship in STEM and Economics fields can be seen as somewhat paternalistic, as it implicitly assumes that future monetary outcomes are an important outcome to be maximized. This approach aligns with numerous other studies aimed at increasing enrollment in higher education, particularly in more selective or high-demand fields. Nevertheless, it raises an interesting question: would a more universal mentoring program, which is commonly employed by many institutions and does not prioritize certain fields based on expected financial returns, be equally effective? Exploring the effectiveness of such broad-based mentoring initiatives could provide valuable insights into how best to support students in making educational choices that balance personal interests with long-term career benefits.

Our findings offer valuable insights for the development of effective one-to-one interventions. First, the online nature of the interaction did not seem to diminish the impact of the mentoring program. This is an important consideration, as the use of online platforms can enable access to a broader base of students, including those in geographically remote areas. The ability to reach students who might otherwise lack access to such resources highlights the potential of online mentoring to bridge gaps in educational opportunities. Interestingly, despite the decentralized recruitment process, our sample is strikingly similar to the general university student population across several key observable characteristics. The ability to attract a diverse range of students from various backgrounds without relying on school teachers or associations is a significant achievement. This suggests a genuine and widespread demand for the service, with students from different backgrounds actively seeking such opportunities.

Second, based on our experience managing the intervention, we observed an substantial supply of qualified mentors, whose response was overwhelmingly enthusiastic. This suggests that the project could be easily scaled. There is suggestive evidence that more experienced master's students are better equipped for the mentoring role than bachelor students and are more effective in guiding students toward their field of study. It would be interesting to explore whether this result is robust and it is due to their ability to provide more accurate and comprehensive information or whether the content of their discussions is more aspirational and inspiring for the mentees. Understanding the dynamics of these interactions could help refine the selection and training of mentors to maximize their impact.

Finally, one of the strengths of our program lies in its tailored approach. The algorithm was designed to match participants as closely as possible with mentors aligned with their preferences. Although we could not perfectly match every request, all students included in the analysis received a mentor from one of their most preferred fields. This personalized matching is crucial in a decentralized intervention where students are approached via email and participation in the mentoring program is not mandatory. The importance of this personalized approach is underscored by the fact that, out of the 158 students who completed the baseline survey and did not list any STEM or Economics/Business fields among their top three preferences, only 14 expressed continued interest in meeting with our mentors. This suggests that students are not merely seeking general information about university life; rather, they are eager to gain a deeper understanding of the specifics of each field. These insights emphasize the importance of personalized, well-targeted mentoring programs, particularly in decentralized and voluntary settings, where student engagement hinges on the perceived relevance and value of the mentorship being offered.

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7 Appendix (additional materials)

Note: all or part of the tables (Appendix A) and figures (Appendix B) in the Appendix can be moved to an online Supplemental Appendix. Similarly for Appendix C, which is a self contained section about AlmaLaurea data and our methodology to produce some of the variables used in the paper.

A Additional tables

Table A1: Topics discussed by mentor-student pairs

Topic	**Discussed** (% of pairs)
Curricula covered in the program/field	70.37
Admission tests and procedures	69.44
Study techniques	64.81
University exams	58.33
Social life as a college student	50.00
Job or study prospects after graduation	50.00
Mentor's satisfaction with their choice	48.15
Scientific topics related to mentor's field	40.74
Flat-hunting	39.81
Relationship with classmates	39.81
Interactions with professors	30.56

Table A2: Fields selected at baseline

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

Notes. The table is based on responses at baseline. Each row in the table shows the percentage and number of students who ranked a given field as their most preferred one ("1^{st}") or as their second or third choice ("2^{nd} or 3^{rd}"). The fields for which we have mentors are in bold.

Table A3: Take-up of the program

	(1)	(2)	(3)	(4)	(5)
Application day	0.009*	0.009*			0.009*
	(0.004)	(0.004)			(0.004)
Math grade	0.130**	0.131**			0.112**
	(0.026)	(0.032)			(0.034)
Italian grade		0.009			-0.004
		(0.045)			(0.044)
Female		0.040			0.072
		(0.081)			(0.081)
First gen. college		0.043			0.056
		(0.073)			(0.077)
From host region		0.008			0.022
		(0.076)			(0.069)
Academic track		-0.039			-0.009
		(0.104)			(0.094)
Mentor female			-0.066		-0.058
			(0.078)		(0.079)
Mentor bachelor			-0.140+		-0.112
			(0.079)		(0.077)
Mentor living away			0.138+		0.073
			(0.080)		(0.086)
Mentor in Science			0.169+		0.143
			(0.097)		(0.103)
Mentor in Engineering			0.105		0.064
			(0.094)		(0.100)
Mentor in pref. field				0.202**	0.130+
				(0.073)	(0.071)
Mentor same gender				0.064	0.041
				(0.071)	(0.076)
Mentor same living cond.				-0.045	-0.009
				(0.074)	(0.079)
Constant	-0.604**	-0.710*	0.540**	0.474**	-0.622+
	(0.197)	(0.342)	(0.080)	(0.076)	(0.342)
R^2	0.133	0.138	0.068	0.048	0.200
adj. R^2	0.123	0.100	0.039	0.031	0.121
N	169	169	169	169	169

Notes. The dependent variable is a dummy that takes value 1 if the student met the mentor at least once. Standard errors are clustered at the mentor level.

Table A4: 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 if the student 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 A5: Choice of mentor's field - first stage

	(1)	(2)	(3)
Assigned to treatment	0.745**	0.753**	0.733**
	(0.072)	(0.069)	(0.074)
Mentor in pref. field		0.119	-0.056
		(0.085)	(0.121)
Mentor FE	Yes	Yes	Yes
Other covariates	No	No	Yes
F-test	108.2	117.8	98.5
Take-up rate	0.59		
N	148	148	148

Notes. The table reports the first stage of the 2SLS regressions in Table 3. 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 macro-area at baseline. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table A6: Choice of mentor's field - ITT

	(1)	(2)	(3)
Assigned to treatment	0.127	0.166*	0.146+
	(0.099)	(0.073)	(0.077)
Mentor in pref. field		0.627**	0.631**
		(0.107)	(0.164)
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 macro-area at baseline. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table A7: 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. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table A8: 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
Assigned + interaction	0.157	0.171		0.195	0.171
P-val (assigned+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. Mentor covariates include dummies for gender, campus (main campus vs other campuses), seniority (master vs bachelor), and a vector of dummies for their fields. The row "Assigned + 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 A9: Heterogeneity by mentor's characteristics

	Career stage		Gender	
	(1)	(2)	(3)	(4)
Treated	0.085	0.044	0.096	0.072
	(0.135)	(0.099)	(0.104)	(0.089)
Treated X mentor master	0.226	0.291*		
	(0.158)	(0.126)		
Treated X mentor female			0.209	0.220+
			(0.149)	(0.128)
Mentor in pref. field	0.599**	0.582**	0.597**	0.594**
	(0.085)	(0.072)	(0.085)	(0.073)
Mentor FE	Yes	No	Yes	No
Mentor covariates	No	Yes	No	Yes
Control mean	0.486		0.486	
Treated + interaction	0.311	0.335	0.305	0.292
P-val (treated+interaction)	0.000	0.000	0.004	0.001
N	148	148	148	148

The dependent variable is a dummy that takes value 1 if the student chooses the same field of study of the assigned mentor. The row "Control mean" shows the mean dependent variable in the control group. The row "Treated + interaction" shows the sum of the first two coefficients; the following row shows the p-value of this sum. Coefficients are estimated using a two stage least square model. Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table A10: 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	0.797	0.733	-0.082	-0.106
	(0.405)	(0.445)	(0.080)	
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	0.789	0.689	-0.175	-0.161
	(0.413)	(0.468)	(0.125)	
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	0.713	0.682	-0.036	-0.047
	(0.454)	(0.468)	(0.060)	
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 A11: 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 3); "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 A12: Enrolled in mentor's field during 2023/24 - with administrative data

	Unibo '23		Everyone	
	(all)	(fe)	(all)	(fe)
Treated	0.165^{+}	0.136	0.229^{**}	0.215^{*}
	(0.086)	(0.099)	(0.086)	(0.097)
Mentor in pref. field	0.389^{**}	0.378^{**}	0.381^{**}	0.388^{**}
	(0.096)	(0.128)	(0.083)	(0.104)
Mentor FE	No	Yes	No	Yes
Mentor covariates	Yes	No	Yes	No
Control mean	0.544	0.574	0.449	0.466
N	127	96	144	111

Notes. In all specification, the dependent variable is a dummy that takes value 1 if the student is enrolled in the same field of study of the assigned mentor at the beginning of the academic year 2023/2024. In columns "Unibo '23", the sample includes students enrolled in the host university in 2022 who are still enrolled in 2023. In columns "Everyone", the sample includes students enrolled in the host university in 2022; the dependent variable is 0 if the student did not enrol again in 2023. 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. The coefficients are estimated using 2SLS, with "Treated" instrumented with "Assigned to treatment." Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

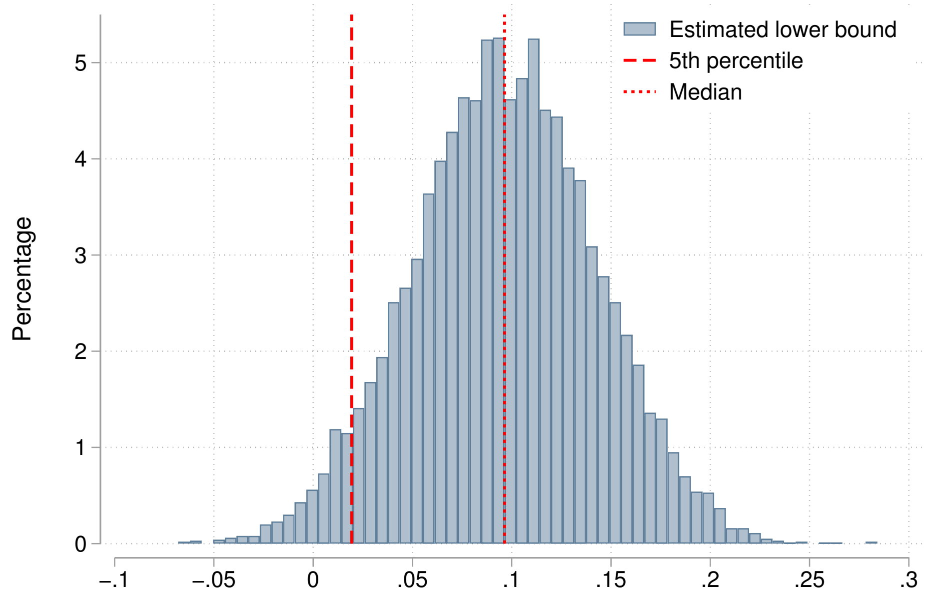
Table A13: Medium run effect on performance - students with endline survey

	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

Dependent variables and regressors are as in Table 6; the analysis are performed on the subset of students who answered the endline survey. The coefficients are estimated using 2SLS, with "Treated" instrumented with "Assigned to treatment". Standard errors clustered at the mentor level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

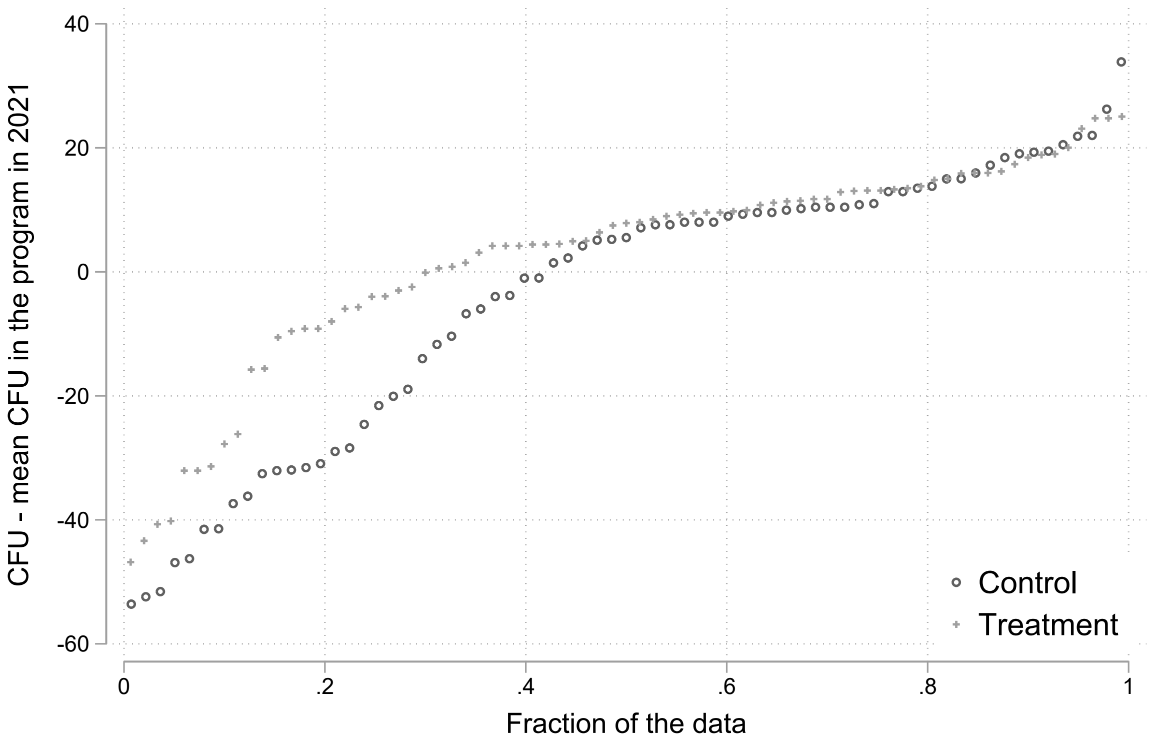
B Additional figures

Figure B1: Simulation results



Notes. The histogram plots 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 3. The histogram plots the distribution of the estimated coefficients.

Figure B2: Distribution of performance by treatment groups



Notes. The figure plots the quantiles of the variable "CFU - mean CFU in the program in 2021," by Control and Treatment group. CFU are the university credits acquired by students in our sample. Mean CFU are the average study credits obtained by students enrolled in the same program in 2021.

C AlmaLaurea data

AlmaLaurea is an interuniversity consortium established in 1994 and supported by MIUR 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.[46] Aggregated data are publicly available on the AlmaLaurea website.[47]

According to the 2022 survey, 67% of bachelor's graduates pursued further studies by enrolling in a master's program. Of these, 96% chose a master's program in the same field as their bachelor's degree.[48] Except for some vocational programs, particularly in the medical field, all bachelor's programs have a continuation rate above 50%, with peaks reaching up to 90% for programs such as Mathematics or Biotech. Only 25% of respondents are not enrolled in a university and are working; of the remaining 8%, roughly half are looking for a job and half are inactive. Less than 1 in 4 master's students is also working, while the others focus solely on their studies. Therefore, for most bachelor's graduates, labor market outcomes after completing their master's degree are the most relevant outcomes to consider.

For the analysis described in Section 5.1, we focus on a survey of master's graduates conducted five years after graduation, as we believe it provides the most informative data about labor market outcomes over the life cycle. We use data from the most recent wave of the survey, which was administered in the same year as the intervention, and refers to graduates from 2017. Most respondents are in their late twenties or early thirties when they respond to the survey, having completed their education and being in a more stable position compared to one or three years after graduation.[49]

We use enrollment statistics from AlmaLaurea to map bachelor's programs with their most commonly chosen master's programs.[50] 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.

[46] 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.

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

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

[49] For instance, some master's graduates pursue doctoral studies after graduation, receiving a relatively low stipend for a few years. This is relatively common 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).

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

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.[51] 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.[52] 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 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, we compute each bachelor's program growth rate as weighted average of the growth rates calculated for the associated master's programs.

[51] The website 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.

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