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Response to Reviewers:	

Pre-analysis plan
Mentorship networks and the early career outcomes of college-educated women

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Abstract

Can mentorship programs improve women's job search and early career outcomes? We will run a randomized controlled trial to evaluate an online one-to-one mentorship program targeted at female college students and early career women, with the majority being from socially vulnerable groups. We hypothesize that structured one-to-one mentoring programs increase self-confidence, networks, and job search readiness and, consequently, improve labor market outcomes of college-educated women, particularly from socially disadvantaged backgrounds.

Pre-registration: research is registered at the AEA RCT Registry (AEARCTR-0012396).

Keywords: women; mentorship; early career; higher education.

JEL codes: J15, J16, J24, C93

Proposed timeline: Post-intervention data collection expected to start in the last week of September 2024 (endline) and be completed by December 2025 (follow up). Paper with complete analysis expected to be ready for submission in the Fall 2026.

1 Introduction

The labor market returns to college are large and heterogeneous (e.g., Rodríguez et al. 2016; Zimmerman 2014), with women faring worse than men in early career outcomes, especially in high-return fields (Aguirre et al., 2022). Explanations include differences in attitudes toward job search (Cortés et al., 2023), professional network availability (Cullen and Perez-Truglia, 2023; Hampole et al., 2021), willingness to negotiate (Biasi and Sarsons, 2022; Roussille, 2024), and expectations (Kiessling et al., 2024; Leibing et al., 2023). These factors may influence how individuals choose their first post-college occupation, apply for jobs, and build their career prospects during the early stages of the job market.

In Brazil, especially due to affirmative action policies, college enrollment of women from low-income backgrounds and black, Indigenous, and mixed-race women is increasing (Mello, 2022; Otero et al., 2020). However, intersectional differences among women in the labor market remain salient. The racial gap among female college-educated workers is larger than among their male counterparts, and a substantial portion of the racial gaps for college-educated women can be explained by a racial gap across establishments (Gerard et al., 2021). These documented differences highlight the importance of pushing non-white and low-income women to be employed in higher-paying firms. Relative to their more economically affluent and white peers, socially disadvantaged women and women of color have limited exposure to role models, people with experience navigating college-job transitions, and professional networks.

Can mentorship programs improve women's job search and early career outcomes? We will answer this question by evaluating an online one-to-one mentorship program targeted at female college students transitioning to the labor market in Brazil. We hypothesize that structured one-to-one mentoring programs increase socioemotional and employability skills, setting college-educated women on a path of higher employment, earnings, and higher-quality jobs. To the best of our knowledge, no previous study evaluates a one-to-one structured mentorship program that aims to reduce job search barriers faced by college women in their early careers or one that aims to boost self-confidence before women start their job search.

We anticipate that the mentorship program's effects are larger for women from socially disadvantaged backgrounds. For instance, using our baseline data, we document the existence of socioeconomic gaps in employment, job quality, labor market expectations, and job search readiness within the group of eligible women. Previous literature has shown the benefits of mentorship to women's educational trajectories (e.g. Dennehy and Dasgupta 2017; Carrell and Sacerdote 2017; Bettinger and Baker 2014). Less is known about the effectiveness of these programs on early-career labor market outcomes for college-educated women and if they reduce within-gender socioeconomic inequality in the labor market.

We have partnered with *Alumna*, an organization that provides a 6-month program on soft skills training and career development for female college students and early career women. The mentorship sessions are the core of the program. Mentors are composed exclusively of women with ten or more years of professional experience. Mentors and mentees are matched based on shared characteristics such as field of study, career interests, and race. The mentorship sessions occur once a month over five months. Each session has an expected duration of 60 minutes, with standard guidelines. In the sixth month, the participants are encouraged to participate in a final event with all the mentors and mentees to present a pitch of who they are and their future professional goals. Besides one-to-one mentoring, participants are also included in social media channels that connect all mentees from their cohort, providing them with a national network. The participants can also join monthly employability-related workshops and access racial literacy videos parallel to the one-to-one mentoring sessions.

Our evaluation will include two experimental cohorts. The first runs from January to June 2024, offering 300 program slots, and the second from July 2024 to December 2024, offering 200 vacancies. Eligibility to the program is restricted to individuals identifying as women or non-binary who are in the three final years of college or who graduated within the past five years. Our experimental design randomly allocates eligible applicants into receiving or not receiving an offer to participate in the program, stratified by race, region, and college graduation status. After the randomization, Alumna conducts their standard procedures to match mentees with mentors. We will estimate intention-to-treat effects by comparing eligible participants who ever received an offer to the mentorship with those who never received an offer; and local average treatment effect by two-stage least squares using the offer as an instrument for take-up.

We will collect data from the eligible pool at different points in time. At baseline, administered when prospective participants apply to the program, we will collect information on educational background, socioeconomic characteristics, networks, and job market expectations. Three months after the end of the program, we will run an endline survey with questions about professional plans, job search readiness, networks, self-confidence, and short-term labor market outcomes. We will also conduct one additional follow-up survey to capture labor market outcomes and career paths 12 months after the end of the program.

Our paper contributes to a recent and growing set of papers that study the impact of mentorship programs on labor market outcomes.[1] Kofoed et al. (2019) find that having a

[1] A larger set of studies investigate the effect of mentoring and role models on educational outcomes, such

same-gender or same-race mentor may influence the occupation choice of women or racial minorities. In our context, all mentors and mentees are women or non-binary, and pairing on race is a priority, which is achieved for the majority of the matched pairs. Blau et al. (2010) and Ginther et al. (2020) show that a mentorship program focused on female assistant professors in Economics improves the short and long-term labor market outcomes of the target group, including the number of publications and federal grants and the likelihood of receiving tenure and staying in academia. Our primary contribution to this literature is to evaluate the labor market effects of an intensive mentoring program targeting women from disadvantaged backgrounds.

More related to our study, Resnjanskij et al. (2024) and Alfonsi et al. (2024) study how mentorship programs improve school-to-work transitions in Germany and Uganda, respectively. Resnjanskij et al. (2024) find that a mentoring program that matches school-attending adolescents with a university-student mentor increases labor market prospects and the likelihood of participating in apprenticeships for low-SES students, with limited effects for high-SES individuals. Alfonsi et al. (2024) show that a mentorship program targeted at vocational students during their school-to-work transitions improved employment and earnings by 27 and 18 percent, respectively. Although very different in the mentorship focus and design when compared to Alumna,[2] Resnjanskij et al. (2024) and Alfonsi et al. (2024) test similar hypotheses as we do in our paper. Resnjanskij et al. (2024) test whether mentoring interventions are more successful for disadvantaged participants lacking family support for a successful transition to the labor market. Alfonsi et al. (2024) test whether mentors affect labor market outcomes through job referrals, actionable search tips, information about entry-level conditions, or encouragement, finding that their intervention works by correcting students' overoptimistic beliefs while avoiding potential discouragement effects.

Our paper differentiates from theirs in key ways by evaluating a program aimed at improving early career outcomes of college-educated women from various fields of study, targeted at women from vulnerable socioeconomic backgrounds, and thus, with the potential to close socioeconomic gaps in labor market outcomes among women. First, in contrast to Resnjanskij et al. (2024), which examines how mentorship programs influence the labor market

as students' major choices (Porter and Serra, 2020), retention (Dennehy and Dasgupta, 2017), and graduation (Bettinger and Baker, 2014). Another set of papers investigates the role of job search interventions (e.g., job application workshops (Abebe et al., 2021) or training workers to use LinkedIn (Wheeler et al., 2022)).

[2] The program of Resnjanskij et al. (2024) is focused on adolescents, lasts for at least one year, and does not follow a structured content. Mentors and mentees meet mostly in person for various activities involving leisure and informal interactions. Alfonsi et al. (2024)'s intervention pairs soon-to-be graduates of vocational training institutes with relatable and successful workers who have graduated in the same program. The program consists of three telephone sessions in which mentors and mentees can speak flexibly about their preferred topics.

prospects of adolescents from different socioeconomic backgrounds, our study is the first to explore how these interventions impact *actual* labor market outcomes, specifically by family income background. This distinction is crucial because many job search challenges disproportionately affecting low-income individuals — such as limited networks, lower job-search preparedness, and reduced self-confidence — tend to become more prominent during the actual job search process. As a result, these factors are unlikely to have been fully captured for the adolescent sample in Resnjanskij et al. (2024). Second, unlike Alfonsi et al. (2024), we test an alternative mechanism that Alumna works through increased confidence instead of moderating overoptimistic expectations, along with increased networks and improved job search readiness as potential mechanisms.

Our paper is also related to a flourishing literature that analyzes gender differences in confidence and the impact on labor market outcomes. Gender gaps in confidence are well-documented (Lundeberg et al., 1994; Möbius et al., 2022). These gaps might relate to differences in early career outcomes through mechanisms such as women having lower earnings expectations (Reuben et al., 2017), being less likely to apply for competitive fields and occupations (Buser et al., 2014; Coffman et al., 2023; Niederle and Vesterlund, 2007) or accepting early job offers due to risk-aversion (Cortés et al., 2023). Although we do not attempt to measure how mentorship programs might reduce *gender gaps* in confidence, since our intervention only targets women, we add to this literature by studying whether such interventions can boost women's confidence and improve their labor market outcomes.

Finally, our paper is related to a set of recent papers that study the importance of networks and social interactions for improving career outcomes (Cullen and Perez-Truglia, 2023; Michelman et al., 2022; Zimmerman, 2019). Zimmerman (2019) shows that peer ties formed between male college classmates of high socioeconomic status play an important role in raising leadership positions among students from elite colleges. Such networks are not effective among females or college classmates of low socioeconomic status (SES). Cullen and Perez-Truglia (2023) find that employees' social interactions with their managers can benefit their careers and that such interactions contribute to the gender pay gap. Hampole et al. (2021) show that a larger proportion of female MBA section peers increases the likelihood of entering senior management for women but not for men. We add to this literature by evaluating a mentorship program that has the potential to improve labor market outcomes by widening women's professional networks and by documenting how such interventions might particularly benefit women from vulnerable backgrounds.

2 Research Design

2.1 Implementing partner

Our randomized controlled trial will evaluate the mentorship program provided by our implementing partner, Alumna.[3] The female-led organization was born to empower Brazil's next generation of female leaders, prioritizing female college seniors and recent graduates from underrepresented backgrounds. The founders identified the demand for such a program from a survey of female undergraduates at their *alma mater*, the University of Brasília. They found that 70 percent of students considered dropping out at least once, 60 percent lacked the knowledge and skills to enter the labor market, and 50 percent did not know what to do after graduating.

Created in 2020, Alumna's program offers intensive online one-to-one mentoring, soft skills, and employability workshops, and a network of female professionals to guide and inspire young women in their early career journey. Conversations with program mentors and feedback from former mentees indicate that improving self-confidence is one of the program's key perceived short-term outcomes. From its start in 2020 to 2023, Alumna had graduated more than 1,000 mentees over eight cohorts.

2.2 Intervention

Our paper will evaluate Alumna's default mentorship program, which consists of a bundled intervention with three components: one-to-one mentorship, group training on leadership, and a network of senior professional women accessible through social media and group chats. All mentorship and training sessions are virtual.

The mentorship program is the core of the program and is standardized. Mentors and mentees receive guidelines on how to conduct and what to expect from each meeting. Mentees also receive an exercise booklet to work through the sessions. Each session has a scheduled duration of 60 minutes, covering the following topics, in recommended order: (i) empathy and goal setting, (ii) self-knowledge, (iii) opportunities' map, curriculum vitae, and LinkedIn profile; (iv) impostor syndrome; and (v) professional pitch, conclusion, and future steps. In appendix B, we describe in detail the 1:1 mentorship guidelines. In the sixth month, the participants are encouraged to participate in a final virtual event with all the mentors and mentees to present a pitch of who they are and their future professional goals. Mentors are

[3] Organization's website: https://www.alumna.com.br/

composed exclusively of women with ten or more years of experience. Each mentor is matched to a mentee based on shared characteristics such as field of study, career aspirations, location, race, and socioeconomic background. The five individual mentorship meetings occur once a month over five months, with the 6^{th} happening collectively.

The training component parallels the mentorship sessions and comprises soft skills and job search readiness workshops. There are monthly workshops, which are not mandatory. They are facilitated by Alumna's founders or a guest speaker. Every cohort has an onboarding session, a Q&A section that includes all mentees and mentors, and a workshop to train the participants on creating their professional pitch. In addition, they have asynchronous materials (videos) on racial literacy produced by a black collective. On top of standard meetings, they offer one or two additional workshops covering a mixture of soft skills and employability skills. The specific themes vary across cohorts, but examples of workshops previously offered include "imposter syndrome", "how to build your CV", and "leadership".

Participants are also included in private social media groups (LinkedIn) and group chats (WhatsApp). The LinkedIn group includes all mentees and mentors, summing up over a thousand professional women, exposing the participants to a national and experienced community. Mentees are also added to a WhatsApp group chat for general communication and to facilitate engagement between mentees of each cohort.

2.3 Eligibility and randomization

Our evaluation will combine two experimental cohorts. The first cohort has 300 mentorship spots. The second cohort has 200 mentorship spots. For the first cohort, sessions and workshops are scheduled from January through May, with the final group meeting in June. The second cohort is planned to start in July 2024. Sessions and workshops are scheduled from July through November, with the final group meeting scheduled for December. We are negotiating with the implementing partner to also evaluate the 2025 cohorts, but we have not included this possibility in this pre-analysis plan.

The experiment's eligible population comprises individuals who identify as women or non-binary, are 18 years old or older, and have been enrolled in the last three years of college, enrolled in a Masters or PhD program, or graduated in the past five years. Eligible applicants can be from any region in the country, field of study, or major. The recruitment of participants follows the standard practice of our implementing partner. Their main strategy is an intense online campaign through social media and Alumna's network of former mentees and mentors.

Randomization follows a stratified random allocation of mentorship offers among the final list of eligible applicants. A total of eight strata are defined based on college education status (college degree vs. not yet graduated), racial groups (black, mixed, or indigenous vs. white or Asian Brazilian), and regions (North or Northeast vs South, Southeast or Midwest). We randomly rank eligible participants in each stratum and allocate a pre-defined proportion of individuals to treatment to guarantee a minimum representation for minoritized groups as defined in partnership with our implementing partner: 50 percent for black, mixed, and Indigenous, 50 percent first-generation in college, and 25 percent from the North or Northeast regions. The non-selected individuals will compose the waitlist and control groups.

The waitlist serves two purposes: to replace non-consenting individuals randomized to the treatment cohort and those who did not accept the offer. First, consenting to participate in the study was defined in the application and did not alter the chances of acceptance into the mentorship program. The implementing partner agreed to expand capacity and invite individuals from the wailist until reaching the agreed experimental cohort size. Second, following standard protocol from the implementing partner, during the first month of the program, new invitations will be sent, following the randomized order within the respective strata, to replace invited participants who may decline or not respond to the offer to participate in the program.

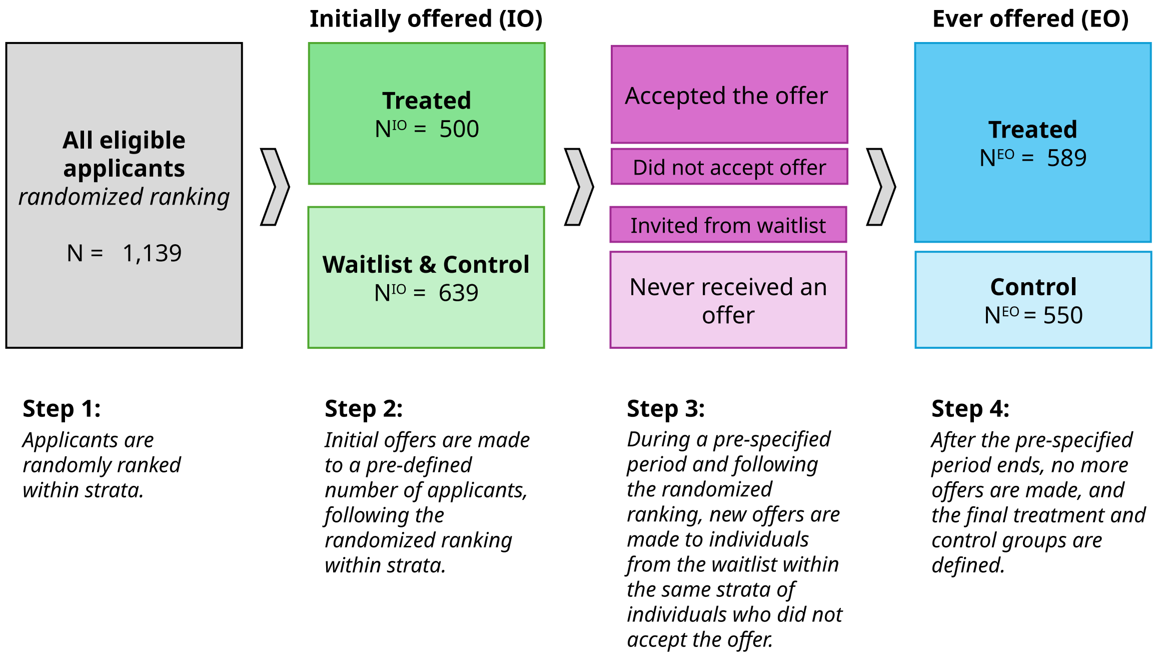
The treatment group is composed of all individuals who ever received an offer. The control group is composed of those who never received an offer. We will check for randomization balance by comparing treatment and control groups across the following characteristics collected in the baseline: first-generation, age, attended public high school, household income per capita, employment status, and the field of study in higher education. Figure 1 summarizes the randomization steps from the randomized list to the final set of offers.

3 Data

We will collect data from participants and mentors at baseline, endline, and one follow-up. The endline is scheduled for three months after the end of the program, and the follow-up for 12 months after the end of the program.[4]

[4]Pending funding and results from these planned follow-ups, we will conduct an additional follow-up three years after the end of the program when all participants are expected to have graduated from college. In this case, we will register in the project's AEA Registry an independent pre-analysis plan for that new set of outcomes.

Figure 1: Randomization procedure



3.1 Baseline data

Baseline data will be collected through an online questionnaire administered by our implementing partner as a requirement of their screening process. The first module of the questionnaire will include information on socioeconomic characteristics, such as gender, age, race, place of birth, family income, parental education, and educational attainment. The second module will cover data on the applicants' schooling trajectories and professional histories, such as higher education institution, field of study, graduation prospects, college GPA, professional plans, and current professional status. Finally, the third module will collect information on confidence, networks, and job market expectations.

3.2 Data on program attendance

The implementing partner closely monitors the participants' attendance in the mentorship program. This information is collected through a mentorship platform and individual contact with mentors and mentees. Our primary measure of attendance is the partner's monitoring data. They keep track of all offers, acceptance, matched pairs, and realization of the five mentoring meetings. This data will primarily provide information on program take-up. In parallel, we will also collect data on attendance in the endline surveys from mentors and participants where we ask about the number of sessions in which they participated, and the reasons why they did not participate in all the sessions. We will use these measures from mentors and mentees to cross-validate the information collected by the partner.

3.3 Three-months follow-up survey (endline)

We will administer an online survey for all eligible applicants included in the randomization who agreed to be part of our research three months after the conclusion of the mentorship program. We will ask about employment, job quality, job search behavior, earnings, expectations about new career possibilities, self-confidence, knowledge on how to search for a new job or showcase skills, salary expectations, professional networks, and her willingness to bargain. We will also ask participants to upload their most recent CV and their LinkedIn profiles. For the treatment group, we will also collect data on their evaluation of the mentorship program.

3.4 Twelve-months follow-up survey

We will administer one additional online survey for all eligible applicants 12 months after the end of the mentorship program. This survey will capture employment, earnings, job quality, job search efficacy, characteristics of their professional networks, self-confidence, bargaining power, human capital, and career satisfaction.

3.5 Data on mentors

Baseline data for the mentors was collected through an online questionnaire administered by our implementing partner as a requirement of their screening process and will be shared with the research team. We will also run an endline survey with mentors to collect data on the mentorship sessions and their perceptions regarding the mentees.

3.6 Attrition

We will account for an attrition rate of 10 percent in our power calculations. Differential attrition between the treatment and control groups is another possible concern. We will test for differential attrition by comparing the following baseline characteristics: first-generation, age, attended public-high school, household per capita income, and employment. If needed, we will report Lee bounds (Lee, 2009) for our estimates.

4 Theory of change, Outcomes, and Hypothesis

Alumna's mentorship program was designed to positively impact women's career outcomes, particularly for women from underrepresented social and racial groups. The program aims to directly boost women's socioemotional (particularly self-confidence) and employability skills, expand their professional network, build role models, and widen their career prospects.

The program's components - individualized mentorship, training, and professional networks - can directly and indirectly affect labor market outcomes. In the short term, mentoring and training directly affect socioemotional and employability skills by (i) focusing on individuals' self-awareness and self-confidence, (ii) offering practical information and practice on building a CV, social media presence, and a compelling professional pitch, (iii) expanding participants' professional networks and widening their career prospects through a senior mentor, social media communities, group chats, and group meetings. In the medium and long term, through improved soft and employability skills, and a solid national community, the mentorship program can boost individuals' career outcomes, particularly by increasing employment in higher-quality jobs.

We specify two families of hypotheses. Family A relates to the participants' potential to succeed in the labor market, dimensions the mentorship affects directly. Family B relates to labor market outcomes. Each family of hypotheses includes primary and secondary hypotheses and outcomes. For family A, we classify three mechanisms as primary outcomes, the ones the program directly affects: self-confidence, job search readiness, and professional networks. For family B, we classify as primary outcomes the set of labor market outcomes the program ultimately wants to affect: employment, job quality, and earnings. Table 1 summarizes the outcomes related to each hypothesis. In appendix C, we provide a complete description of the outcomes.

Multiple hypotheses: We will separately account for the Type 1 error probability for (i) the primary hypotheses and (ii) the secondary hypotheses. We will control for false discovery rate (FDR) by calculating Anderson's sharpened q-values (Anderson, 2008). As additional results, we will also report the effects, standard deviations, and confidence intervals for the specific outcomes but will not perform hypothesis testing.

Family A The mentorship program directly improves women's potential to succeed in the labor market.

Primary hypotheses and outcomes:

P.H1: The mentorship program improves participants' self-confidence.

P.H2: The mentorship program improves the participants' perceived ability to effectively find and apply for jobs or promotion opportunities.

P.H3: The mentorship program expands participants' professional networks.

Secondary hypotheses and outcomes:

S.H1: The mentorship program increases the number of people participants perceive as role models.

S.H2: The mentorship program induces participants to open up to new goals and professional possibilities.

S.H3: The mentorship program affects participants' salary expectations.

S.H4: The mentorship program improves participants' attitudes toward salary negotiation.

Family B: The mentorship program improves women's early labor market outcomes.

Primary hypotheses and outcomes:

P.H4: The mentorship program increases participants' employment.

P.H5: The mentorship program increases participants' job quality.

P.H6: The mentorship program increases participants' earnings.

Secondary hypotheses and outcomes:

S.H5: The mentorship program increases job search efficacy.

S.H6: The mentorship program increases the participants' use of their professional networks to succeed in the labor market.

S.H7: The mentorship program increases the participants' bargaining power, measured by effective job offer negotiation.

S.H8: The mentorship program affects participants' human capital.

S.H9: The mentorship program increases participants' satisfaction with their career path.

4.1 Outcomes with limited variation

One concern is *ex-post* realization that an outcome may have limited variability in the control group, limiting the ability to test for the program impact. We will exclude survey questions for which over 90 percent of observations in the control group have the same value. When the question is part of an index, the question will be excluded, and the index will be calculated based on the remaining questions. If the question is the outcome or all the questions within an index have low variability, we will exclude the outcome and not test the corresponding hypothesis.

5 Analysis

5.1 Statistical power

We performed the power analysis considering the sample size for both cohorts to calculate the minimum detectable effect (MDE) for a selected set of outcomes. We provide MDEs for wages and employment. However, we cannot perform power analysis for all of our outcomes due to data unavailability or the difficulty of benchmarking the MDE using other studies. We use data from the Brazilian Household Survey (PNAD-Contínua) as a reference for the mean and standard deviation for wages and employment. We restrict the sample to college-graduated women aged 19 and 49 years old to cover the experimental cohort's 1^{st} and 99^{th} age percentiles. We re-weighted observations to mimic the age distribution of the mentorship applicants.

We set the parameters for the calculation as follows. For cohort 1, the eligible cohort consists of 657 eligible applicants who consented to participate in the program. After the randomization and the wailist round, we ended up with 340 women who ever received an offer and 317 who never received one. For Cohort 2, we received 482 eligible applicants that consented. Among these, 249 were ever offered a spot, and 233 were never offered a spot.

Table 1: Summary of hypothesis and outcomes

	Outcome	Type	Specific measures
Family A *The mentorship program directly improves women's potential to succeed in the labor market*			
Primary hypothesis and outcomes			
P.H1	Self-confidence (3-month) Self-confidence (12-month)	Index	4-item self-confidence scale
P.H2	Job search readiness (3-month)	Index Constructed	Updated CV and LinkedIn profile Self-reported job search readiness Quality of CV and LinkedIn profile
P.H3	Professional networks (3-month)	Question	New allies for professional advancement
Secondary hypothesis and outcomes			
S.H1	Role models (3-month)	Question	New role model
S.H2	New goals and possibilities (3-month)	Index	Postgrad studies Job field or company Migration
S.H3	Salary expectations (3-month)	Question Question Constructed	Reservation wage Salary expectation Would accept salary below expectation
S.H4	Negotiation attitudes (3-month)	Index	Fear/Willingness to negotiate
Family B The mentorship program improves women's early labor market outcomes			
Primary hypothesis and outcomes			
P.H4	Employment (3-month) Employment (12-month)	Question	Employment (employee, self-employed or employer)
P.H5	Job quality (3-month) Job quality (12-month)	Index	Formality Valuable amenities Job fits qualifications Skill development opportunities Salary growth opportunities
P.H6	Earnings (3-month) Earnings (12-month)	Question	Earnings
Secondary hypothesis and outcomes			
S.H5	Job search efficacy (3-month) Job search efficacy (12-month)	Index	Ratio interview/applications in the past three months Ratio offers/applications in the past three months
S.H6	Use of networks (12-month)	Index	Number of allies asked for help Knew Employer Job through referral Job discovery source
S.H7	Bargaining power (12-month)	Index	Success negotiating offer
S.H8	Human Capital (12-month)	Index	Has a college degree Enrolled in additional education
S.H9	Career satisfaction (12-month)	Question	Career track satisfaction

For take-up rates, we follow Alfonsi et al. (2024) and Resnjanskij et al. (2024) and define take-up as having completed at least one 1:1 mentorship session during the program. We use the ongoing monitoring of cohort 1 to set up the expected take-up rate. The implementing partner shared their monitoring results, and by June 2024, 81 percent had completed at least one session. We use this take-up rate for cohort 1 to predict the take-up rate of cohort 2. Therefore, the take-up rate for both cohorts is set at 81 percent. In addition, we assume a 90 percent response rate to our online surveys. We set power at 80 percent and significance level at 5 percent. Our unit of analysis is the individual.

Our MDE calculations show that we can detect an effect of at least 16.9 percent for earnings and 11.8 percent for employment. Our calculations do not consider the randomized stratification, which improves our statistical power. We will also include covariates to increase the precision of our estimates. Therefore, these MDEs should be viewed as conservative. In addition, we are in negotiation with our implementing partner to randomize two more mentorship cohorts (between 300 and 400 vacancies in total), which would lead to lower MDEs through increased sample size.

These MDEs are reasonably aligned with evidence from other mentorship programs. For example, Alfonsi et al. (2024) found a 27 percent effect on employment and 18 percent on earnings for a sample of less formally educated people enrolled in vocational courses (for example, hairdressers).

5.2 Statistical model

Our main analysis leverages the random allocation of offers across treatment and control groups. We define treatment assignment as ever receiving an offer. We define take-up as mentees who completed at least one mentorship meeting. We will estimate two parameters of interest: the Intention-to-Treat (ITT) Effect and the Local Average Treatment Effect (LATE). Estimates include observations from both cohorts.

We will estimate Intention-to-Treat (ITT) effects using the following regression model:

$$Y_{is} = \beta_0 + \beta^{ITT} T_i + X_i' \delta + \mu_s + u_{is} \tag{1}$$

where Y_{is} is the outcome for person i of strata s. The variable T_i indicates if the person ever received an offer for the mentorship. Therefore, the initially assigned to the mentorship and those later invited from the waitlist are coded as $T_i = 1$. As alternative specifications, we include covariates X_i measured at the baseline to improve the precision of our estimates.

Controls include *per capita* household income, age, a dummy if the individual studied in a public high school, and a dummy for being a first-generation college student. We will also include baseline employment status (working, interning, or having own business) as a control for the specification measuring employment as an outcome. Strata fixed effects are represented by μ_s. Finally, u_{is} is the error term.

Due to imperfect compliance, we will also estimate the Local Average Treatment Effect (LATE) by two-stage least squares (2SLS) using the "ever receiving an offer", EO, as an instrument for take-up. We will estimate:

$$\textbf{First Stage}: T_{is} = \alpha_0 + \alpha_1 EO_i + X_i'\delta + \mu_s + u_{is} \tag{2}$$

$$\textbf{Second Stage}: Y_{is} = \beta_0 + \beta^{LATE}\hat{T}_i + X_i'\gamma + \rho_s + \epsilon_{is} \tag{3}$$

where $T_i = 1$ if the mentee participated in at least one mentorship session. As an alternative definition and specification, we will also provide estimates for T_i defined as participating in five mentorship sessions.

As an alternative specification, we will also estimate Equation (1), (2), and (3) including cohort fixed effects to account for potential time fixed effects. Covariates inclusion in (2) and (3) follow the same logic as in Equation (1).

Finally, as another alternative specification, we will also report results considering as treated those who received the initial offer (T^{IO}).

Multiple hypothesis testing: In all specifications, we will report robust standard errors and standard errors accounting for false discovery rate (FDR) for (i) the set of primary outcomes and (ii) the set of secondary outcomes. Specifically, we will calculate Anderson's sharpened q-values (Anderson, 2008) for FDR. As additional results, we will also report the effects, standard deviations, and confidence intervals for the specific outcomes but will not perform hypothesis testing.

Spillovers: Only 7.5 percent of individuals from Cohort 1 are enrolled or have graduated from the *same institution, major, and graduation cohort* of at least one individual in the treated group. We believe this is the most relevant group when thinking about spillover effects, because these individuals are more likely to be classmates and to know each other. Since they are a small share of the control group, our treatment effects are unlikely to be downward biased by spillover effects from the treated to the control group.

5.3 Heterogeneity

In addition to the main specification, we plan to estimate the heterogeneous effects of the program across the following characteristics:

Family income: Low-income students can face specific barriers transitioning from college to the labor market. For example, baseline data from the program reveals that low-income applicants are less likely to report having a job or an internship, and, conditional on working, women from the low-income group are also less likely to hold a job or an internship related to their field of study or interest. Moreover, we observe a socioeconomic gap in the expectation to receive a wage above average, and on job search readiness. Finally, low-income women are more likely to report having zero professional references. Taken together, these facts suggest that, due to financial and non-financial barriers, low-income applicants have access to fewer job opportunities and to lower-quality jobs when compared to their high-income counterparts. We will create a Low-income dummy equal to 1 if the applicant belongs to a family whose per capita income is lower than 1 minimum wage at the baseline. We will modify the main specification to include an interaction between treatment (defined for the ITT and LATE) and the Low-income dummy.

Race: Baseline data from our sample shows that non-white women are, on average, poorer, more likely to come from a family of lower parental education, and more likely to have attended a public school. However, even if they come from a poorer background, our baseline data also shows that non-white applicants are similarly likely to report employment and have similar labor market expectations compared to white applicants. One potential explanation for the similarity in employment and expectation outcomes, despite the substantial socioeconomic differences between white and non-white women, might be the differences in the field of study. Non-white women in our sample are more likely to study Business and Law, while white women are more likely to study Social Sciences and Humanities. White and non-white women might also differ in dimensions not observed in our baseline but collected at the endline, such as social-emotional skills and the presence of role models. Even though we do not observe racial gaps in employment and labor market expectations in our baseline, we believe it is still interesting and important to investigate whether our intervention has heterogeneous effects for white and non-white women. Non-white women are more vulnerable and, due to labor market discrimination, could experience additional barriers when transitioning from college to the labor market.

We will create a URM dummy to represent belonging to an underrepresented racial minority in college, which is equal to 1 if the person is black, mixed-race, or Indigenous. We will modify the main specification to include an interaction between treatment (defined for the ITT and LATE) and URM.

Timing of the intervention The program can have differential impacts depending on whether the participant has graduated from college or not. We will test if the impacts of the Alumna program vary across participants with and without a college degree. We hypothesize that mentorship could have a stronger impact on women who were still in college when receiving the mentorship and, therefore, lacked experience in searching for a job that requires higher qualifications.

6 Interpreting Results

Our main parameters of interest are β^{ITT} and β^{LATE}. We interpret β^{ITT} as the program's effect on all applicants that received an offer, regardless of take-up. Due to imperfect compliance, β^{ITT} is a lower bound for the program's average treatment effects. We also estimate β^{LATE}, which we interpret as the program's effect on the compliers.

We interpret positive effects on employment and job quality as the program successfully equipping its participants with the necessary soft and employability skills it aims to provide. Therefore, we also directly test the effects of the program on self-confidence,job search readiness, and professional networks.

If we do not detect effects of the program on our primary outcomes, one possibility is that the control group could have enrolled in alternative mentorship programs. Our follow-up data collection will ask whether the individual participated in any other mentorship program besides Alumna, and we will provide descriptive statistics.

We also consider the possibility that Alumna (i) may not significantly or even negatively impact employment in the short run if the intervention leads women to be more ambitious and target more difficult goals. To study this, we will provide evidence of these mechanisms by analyzing if mentorship impacts reservation wages or leads to a change in their human capital investment decisions (pursuing more education), which may further delay the labor market outcomes; (ii) may not significantly impact employment and earnings. Since the population served by the mentorship are college-educated women, their employment rates are generally high. We will measure outcomes related to job quality to interpret whether our findings could indicate that Alumna impacts the quality of the job rather than employment. For earnings, there may be a low variance in earnings in the short- and medium-term, with an effect more likely to be detected in the long-run, beyond the scope of our evaluation.

A positive impact of a mentorship program on female labor market outcomes is of the utmost relevance for policies that aim to reduce labor market gender inequalities. Although an NGO implements the program we evaluate, this evidence can inform universities and the government on implementing a mentorship program for senior college students. This is particularly relevant for Brazil, given its increased population of underrepresented groups following the recent expansion of inclusive admissions policies.

7 Mechanisms: Mediation Analysis

In our main analysis, we estimate the effects of the program on two sets of outcomes: what the program directly impacts (self-confidence, job search readiness, and professional networks) and what it intends to ultimately impact (labor market outcomes). What is the relative importance of the intermediate outcomes (potential mediators) in explaining participants' labor market outcomes?

We will explore the relative role of three potential mediators - self-confidence, job search readiness, and professional networks. We will follow the method proposed by Heckman and Pinto (2015) and its applications in Resnjanskij et al. (2024) and Oreopoulos et al. (2017) to decompose the overall effects of the Alumna program on employment, job quality, and earnings into those three potential mediators.

Following standard practice, we will provide a mediation analysis of the outcomes for which we find non-zero effects. We will also only include as potential mediators the intermediate outcomes for which we find non-zero effects. For the sake of interpretation, if the program has opposing effects by race, income, or graduation status, we will provide mediation analysis separately by group (e.g., low vs. high income).

The mediation analysis assumes that the outcomes can be expressed as a linear function of the mediators (m). We modify Equation 1 to include the potential mechanisms (θ_i^m).

$$Y_{is} = \beta_0 + \beta^{residual} T_i + \sum_m \alpha^m \theta_i^m + X_i' \delta + \mu_s + u_{is} \tag{4}$$

We interpret $\beta^{residual}$ as the proportion of β^{ITT} that cannot be explained by the effects of the program on the mediators. The effects of the mediators are captured by α^m, conditional on the mediator being directly affected by the program. The direct effect of the program on the mediator (β_m^{ITT}) is estimated from Equation 1, with each of the mediators as the outcome variable. We estimate the share of the treatment that can be explained by each mediator by calculating:

$$Share_m = \frac{\alpha^m \beta_m^{ITT}}{\beta^{ITT}} \tag{5}$$

The randomization guarantees the identification of β^{ITT} for the labor market outcomes and mediators. The identification of α^m depends on the assumption that the effects of the observed mediators are independent of the effects of other alternative and unobserved mediators. If this assumption does not hold, α^m will be biased. Therefore, $Share_m$ should be interpreted as upper bounds.

8 Additional analysis: understanding baseline salary perceptions

Pending authorization, we will use restrict-access administrative datasets to conduct additional analysis to further investigate baseline participant expectations. While the mentoring program in Alfonsi et al. (2024) aimed to correct students' overoptimistic beliefs, Alumna was specifically designed to address women's underconfidence, recognizing that women globally often hold more pessimistic expectations (Kiessling et al., 2024; Leibing et al., 2023).

This analysis must be conducted in-person in restricted-access rooms at Brazil's National Institute of Educational Studies and Research Anísio Teixeira (INEP). Upon authorization, we will use information on salary perception collected at the baseline and compare it with the actual average salary for graduates from the same college and major from administrative databases. We will merge individuals from the Brazilian Higher Education Census (CESUP) and the *Relações Anuais de Informações Sociais (RAIS)*, an employee-employer dataset covering all formal sector workers in Brazil. We will link all college graduates from CESUP with RAIS to calculate the average wages for each college-major combination. We will restrict this calculation to individuals who completed their degree five years after college enrollment. Finally, we will calculate the gap between individuals' perceived average monthly wage at baseline and the actual average wage for their specific college and major. We will report this analysis separately by socioeconomic status and race.

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A Administrative information

Ethics approval

This research was reviewed by IRBs at Insper (316/2023) and Princeton University (15829).

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B Mentorship guidelines

Alumna shares with the mentor a booklet to serve as guidelines for mentorship. Below, we describe the session's suggestions in detail:

1. Empathy and goal-setting

   The first meeting has three main goals: connect with the mentor, set their mentorship goal and participate in an empathy exercise, where mentors and mentees create their personal timeline. According to Alumna, they aim for mentors and mentees to share their successes and vulnerabilities, fostering a deeper connection between them.

2. Self-knowledge

   Alumna suggests a self-awareness exercise where mentees describe their strengths, points to improve, and goals/interests.

3. Opportunities map, curriculum vitae, and LinkedIn profile

   Alumna suggests an exercise where mentees write down their professional goals, areas of interest, and possible firms to apply for jobs. Mentors and mentees debate this exercise together, and mentors are encouraged to suggest other job possibilities. In addition, mentors share their expertise in building a CV. Also, mentors should talk about how to use LinkedIn to find job openings and how to set a good LinkedIn profile. Mentees are encouraged to send their CVs and LinkedIn profiles so the mentor can provide feedback.

4. Impostor syndrome

   Mentors should explain what the Impostor Syndrome is. Mentees are stimulated to talk if they have experienced this phenomenon. Mentors are encouraged to talk about how they overcome the Impostor Syndrome.

5. Professional pitch and application process

   Mentees are stimulated to ask questions about the job application process, while mentors are encouraged to talk about their experience. Mentees present their personal pitches to the mentors, and they give their feedback.

C Outcomes description

A note on the construction of indexes: a sub-set of our outcomes of interest are measured by aggregating different survey questions into an index. We will calculate two types of indexes: a simple index and a weighted index (Anderson, 2008). We will report the estimates for the two indexes, one as the main and the other as the alternative.

The summary indexes will be implemented as follows:

1. Switch the item direction sign to the direction that a higher value indicates a positive outcome.

2. Convert the measurements into effect sizes by demeaning the outcomes and dividing them by the control group's standard deviation.

3. Create the indexes:

   - Simple index: take the simple average across the items.
   - Weighted index: follow Anderson (2008) by weighting the converted items by the inverse of the variance-covariance matrix of the transformed items.

Family A

Primary outcomes

P.H1: Self-Confidence: We ask three questions to the eligible pool regarding self-confidence. The four questions are based on Rosenberg's Self-Esteem Scale (Rosenberg, 1989) composed of ten questions, but we select four as Alfonsi et al. (2024): (i) I have a number of good qualities; (ii) I feel like a failure; (iii) I am able to do things as well as most other people; (iv) on the whole, I feel satisfied with myself.

P.H2: Job search readiness: We will construct an index combining five survey questions.

> *Self-reported job search readiness* (3 items): We ask the eligible pool to rate, using a Likert scale, their ability to find job openings they are interested in applying for, prepare resumes, and deliver a job interview pitch.
>
> *Have an updated CV and LinkedIn profile* (2 items): In two separate survey questions, we ask the eligible pool if they have an updated CV and a LinkedIn profile.

Quality of materials: We ask the eligible pool to send us an updated version of their CV and LinkedIn profile. Since the Alumna program guidelines encourage mentors to help mentees improve their CV and LinkedIn, we hypothesize the mentorship will significantly and positively impact their quality. We will hire independent graders to evaluate the quality of LinkedIn and CVs.[5] We will not report this variable if there are more than 50 percent of missing values.

P.H3: Professional networks: We will ask the eligible pool how many professional allies they recognize that could help them achieve their professional goals.

Secondary outcomes

S.H1: Role models: We will ask our eligible pool how many professional role models they recognize.

S.H2: New goals and possibilities: We will construct an index combining three survey items asking if they intend to pursue alternative career options, post-graduate studies, and move to a different city or state.

S.H3: Salary expectations: This hypotheses encompasses three outcomes.

Salary expectation: We ask the eligible pool about their salary expectations for a job in their area of interest. We will construct this variable from different survey questions. For women in higher education, we ask about their salary expectations for their first job after graduation. We ask those not enrolled in a higher education degree for their expected salary relative to a potential new job or promotion.

Reservation wages: We ask the eligible pool the minimum salary they would accept for a job.

Would to accept a salary below expectation: We will create a variable that is the difference between reservation wage and salary expectations to measure whether the eligible pool is willing to accept a salary below expectation.

S.H4: Negotiation attitudes: We will combine three survey items to construct an index. We ask the eligible pool to rate, using a Likert scale, their fear of looking greedy if negotiating a wage increase, fear of losing a job opportunity because of trying to negotiate the salary, and if they are willing to negotiate their job offers.

[5] The inclusion of this variable is pending funding for hiring graders.

Family B

Primary hypothesis and outcomes

P.H4: Employment: We ask the eligible pool if they are employed. We will create a dummy variable equal to one if the person is employed, including if the person is an employee, employer, self-employed, or if the person is doing an internship.

P.H5: Job quality: We will construct an index by combining five survey questions.

> *Type of contract*: We ask the employed eligible pool which type of contract they hold if they are employed. We will aggregate options into formal and informal contracts.
>
> *Job fits qualifications*: We ask the employed eligible pool to rate, using a Likert scale, how their job fits their qualifications.
>
> *Job provides skill development opportunities*: We ask the employed eligible pool to rate, using a Likert scale, how their job contributes to their skill development, both in gaining new skills and enhancing existing ones.
>
> *Job with high salary growth potential*: We ask the employed eligible pool to rate, using a Likert scale, whether their job provides salary growth opportunities.
>
> *Job provides amenities*: We provide a list of amenities and ask the employed eligible pool to select all amenities available at their job. The outcome is the count provided amenities.

P.H6: Earnings: We ask the employed eligible pool about their base average monthly earnings from all their jobs. We also include bonus payments in our average monthly earnings measure. We will use log earnings to mitigate results being driven by outliers. We will report results on levels as an alternative.

Secondary hypothesis and outcomes

S.H5: Job search efficacy: We will construct an index of two outcomes:

> *Ratio between interviews and applications*: We ask the eligible pool that reported to have searched for jobs in the past 3 months how many jobs they applied for and how many interviews they had. We will construct the ratio by dividing the number of interviews per number of applications.

Ratio between offers and applications: We ask the eligible pool that reported to have searched for jobs in the past 3 months how many jobs they applied for and how many offers they had. We will construct the ratio by dividing the number of offers per number of applications.

S.H6: Use of networks: We will combine four survey questions in an index.

Number of allies: We will ask the eligible pool how many people they asked for help to achieve their primary goal.

Job through referral: We will ask the eligible pool who are employed if they got their jobs through referral.

Knew employer: We will ask the employed eligible pool if they already knew their employer/direct supervisor before starting the job.

Job discovery source: We will ask the employed eligible pool where they found out about their job. We are interested in evaluating if the treatment group discovered their job offers through private sources that are not friends or family, such as a mentor, WhatsApp group (excluding posts from family, friends, past job colleagues or professor), social media other than whatsapp (excluding posts from family, friends, past job colleagues or professor).

S.H7: Bargaining power: We combine two survey questions to construct an outcome capturing successful offer negotiation. We ask the employed eligible pool if they negotiated any aspect of their offer and if they succeeded in negotiating. We will create a dummy variable equal to one if they negotiated an offer and succeeded.

S.H8: Human Capital investment: We will construct and index based on two questions:

Additional education: We will ask our eligible pool if they are enrolled in another undergraduate program or postgraduate studies.

College degree: We will ask our eligible pool if they hold a higher education degree. This question will include college degree, masters degree, doctorate degree, and other degree.

S.H9: Career satisfaction: We ask the eligible pool to rate, using a Likert scale, their satisfaction with their career track (or if their education is on track for their intended career for those that are in college and have not started their career yet).

D Ongoing intervention status (Not for publication)

This section reports the intervention status as of October 2024. The first cohort, which started in January, has already ended. The cohort that started in July is ongoing.

D.1 Cohort 1 and 2

D.1.1 Applications and baseline

Applications for the first experimental cohort started in November 2023, with a 40-day campaign to disseminate information about Alumna's mentorship program. Alumna received 1,229 applications for the mentorship program, with 896 applicants completing the survey. We further restricted the pool of applicants to the 676 women who met the eligibility criteria: identified as women or non-binary, 18 years old or older, students with expected graduation dates no later than the end of 2026, and college graduates who have completed their studies no earlier than 2019.

Applications for the second experimental cohort started in May 2024, with a 34-day campaign to disseminate information about Alumna's mentorship program. Alumna received 944 applications for the mentorship program, with 693 applicants completing the survey. We further restricted the pool of applicants to the 506 women who met the eligibility criteria.

D.1.2 Randomization

For cohort 1, a total of 676 eligible participants were included in randomization, but only 657 eligible applicants consented to participate in the study. Consenting to participate in the study did not alter the chances of acceptance into the program to preserve fairness. For every non-consenting applicant randomized to treatment, the research team and the implementing partner agreed that a new applicant would be randomly invited to the program. This procedure would repeat until we reached 300 participants who consented to participate in the study, which will compose our experimental cohort. Therefore, the mentorship cohort will comprise the 300 participants in the experimental cohort plus those who did not consent.[6]

[6] Consent to the study was asked at the application stage, before they received the offer. Consenting or not did not alter applicants' chances of acceptance into the program. All non-consenting participants who received a randomized offer had the chance to join the mentorship in the same cohort, receiving the same program. Our research team will never contact them for any other research-related activity. The non-consenting participants in the control group were also excluded from any further communication from the research team.

Randomization was conducted on a virtual conference call with the implementing partner on January 10^{th}. Randomization consisted of randomly ranking eligible applicants per stratum and assigning an equal proportion to treatment, summing up to 301 randomly selected individuals assigned to treatment. The extra randomized applicant was due to the rounding to the nearest integer when proportions were applied to each stratum. We excluded this extra applicant from the treated cohort by randomly selecting one stratum, and excluding the last accepted from the randomized list. Seven of the 300 selected did not consent to participate in the study. We followed the randomized list in each stratum to invite seven more individuals to join the program, preserving the non-consenting person's stratum. We reached the targeted experimental cohort size, with 300 participants consenting to the study plus seven non-consenting. Of the 369 applicants who did not receive any offer, 357 consented to the study and integrated our control group.

For cohort 2, 506 eligible participants were included in the randomization. Among those, 482 consented to participate in the study. The same procedure as for Cohort 1 was adopted for Cohort 2.

D.1.3 Partial compliance

Following standard protocol from the implementing partner, new invitations were sent for a limited and pre-specified amount of time (one month) to fill the program vacancies. To preserve the experiment's integrity, we followed the randomized ranking for each stratum to assign more offers.

For cohort 1, of the 300 individuals participating in the study who received an initial offer for cohort 1, 40 did not respond, declined, or withdrew from the program in the early weeks. We followed the randomized waitlist and selected 40 applicants to receive an offer plus one extra applicant due to a non-consenting applicant among the first 40. By the end of the recall month, we had made 340 offers. There were 317 participants who never received an offer and compose the control group. Table 2 summarizes the offering rounds. The experimental cohort started with 297 mentees who agreed to participate in the program and were paired with a mentor. By the end of the mentorship, Alumna informed us that, according to their monitoring, 81 percent of the ever-offered completed at least one mentorship session.

For cohort 2, of the 200 individuals initially offered a spot in the mentorship, 49 did not answer or declined the offer. Therefore, they were replaced by other 49 eligible candidates following the randomized order. By the beginning of the mentorship period, 249 applicants had received an offer, and 233 applicants had never received an offer. Table 3 summarizes cohort 2's offering rounds.

Table 2: **Randomized offers Cohort 1**

	Consenting	Non-consenting	All
Initial offers	300	7	307
Declined/Never Answered/Dropout	40	2	42
Additional offers	40	3	43
Ever received an offer	340	10	350
Never received an offer	317	9	326
Total eligible participants	657	19	676

Table 3: **Randomized offers Cohort 2**

	Consenting	Non-consenting	All
Initial offers	200	12	212
Declined/Never Answered/Dropout	46	3	49
Additional offers	49	0	49
Ever received an offer	249	12	261
Never offered an offer	233	12	245
Total eligible participants	482	24	506

D.1.4 Experimental cohort and balance tables

Our experiment's first cohort includes 657 individuals, divided into treatment and control groups. Table 4 presents the balance tables for those receiving the initial offer and for those ever receiving an offer in cohort 1. It is possible to observe that those randomized to initially receive an offer only differ from their control group regarding the probability of studying Social Sciences and Communication. In addition, applicants who are ever offered a seat in the mentorship only differ from applicants who were never offered a spot in *per capita* household income. Both field of study and *per capita* household income will be included as a control in one of our statistical model specifications.

Table 5 presents the balance table for cohort 2. Those randomized to initially receive an offer only differ from the control group regarding the probability of studying a Health and Well-Being major.

Table 4: **Balance table, initial and final offers- Cohort 1**

	Initially-offered			**Ever-offered**		
	Control	**Treat**	**p-value**	**Control**	**Treat**	**p-value**
First generation	0.55	0.54	0.81	0.54	0.54	0.97
	(0.03)	(0.03)		(0.03)	(0.03)	
Age	26.17	26.35	0.71	26.14	26.36	0.67
	(0.37)	(0.34)		(0.39)	(0.33)	
Public high school	0.62	0.64	0.64	0.61	0.64	0.35
	(0.03)	(0.03)		(0.03)	(0.03)	
Household income *per capita*	2131.37	1980.19	0.34	2222.24	1913.25	0.05**
	(114.01)	(106.35)		(126.37)	(95.66)	
Employed	0.59	0.61	0.61	0.60	0.61	0.74
	(0.03)	(0.03)		(0.03)	(0.03)	
Field of Study						
Education	0.13	0.10	0.16	0.14	0.10	0.12
	(0.02)	(0.02)		(0.02)	(0.02)	
Business, Administration and Law	0.28	0.24	0.37	0.27	0.26	0.79
	(0.02)	(0.03)		(0.03)	(0.02)	
Engineering	0.17	0.16	0.63	0.17	0.16	0.69
	(0.02)	(0.02)		(0.02)	(0.02)	
Agriculture, Forestry and Veterinary	0.03	0.02	0.88	0.02	0.03	0.72
	(0.01)	(0.01)		(0.01)	(0.01)	
Health and Well-being	0.04	0.05	0.50	0.04	0.06	0.15
	(0.01)	(0.01)		(0.01)	(0.01)	
Natural Sciences, Math and Statistics	0.02	0.01	0.46	0.02	0.01	0.66
	(0.01)	(0.01)		(0.01)	(0.01)	
Social Sciences and Communication	0.23	0.31	0.02**	0.25	0.28	0.34
	(0.02)	(0.03)		(0.02)	(0.02)	
Arts and Humanities	0.03	0.04	0.40	0.03	0.04	0.51
	(0.01)	(0.01)		(0.01)	(0.01)	
Information and Technologies	0.07	0.06	0.61	0.07	0.06	0.69
	(0.01)	(0.01)		(0.01)	(0.01)	
Services	0.00	0.00	0.90	0.00	0.00	0.96
	(0.00)	(0.00)		(0.00)	(0.00)	
Observations	357	300	657	317	340	657

Notes: Data from Cohort 1. Reported p-values are for the pairwise t-test for the mean difference between the control and treatment means. *Per capita income* is an approximation using the midpoint values of household income ranges divided by number of people in the household. For the first range (until 2000 BRL) we use 2000 BRL and for the last (more than 30000 BRL) we use 30000 BRL. Variable "Employed" is defined as having a job, an internship, or being an entrepreneur. The graduation areas classification is based on the General Area classification by the Brazilian Minister of Education.

Table 5: **Balance table, initial and final offers- Cohort 2**

	Initially-offered			**Ever-offered**		
	Control	**Treat**	**p-value**	**Control**	**Treat**	**p-value**
First generation	0.56	0.56	0.91	0.57	0.55	0.59
	(0.03)	(0.04)		(0.03)	(0.03)	
Age	25.61	25.68	0.89	26.30	25.96	0.20
	(0.33)	(0.40)		(0.34)	(0.38)	
Public high school	0.63	0.63	0.83	0.63	0.63	0.99
	(0.03)	(0.03)		(0.03)	(0.03)	
Household income *per capita*	2121.07	2022.07	0.64	2185.53	1981.23	0.32
	(150.76)	(126.89)		(177.67)	(108.85)	
Employed	0.60	0.61	0.66	0.61	0.60	0.95
	(0.03)	(0.04)		(0.03)	(0.03)	
Field of Study						
Education	0.12	0.11	0.64	0.13	0.11	0.49
	(0.02)	(0.02)		(0.02)	(0.02)	
Business, Administration and Law	0.25	0.28	0.51	0.24	0.28	0.26
	(0.03)	(0.03)		(0.03)	(0.03)	
Engineering	0.14	0.15	0.80	0.14	0.15	0.63
	(0.02)	(0.03)		(0.02)	(0.02)	
Agriculture, Forestry and Veterinary	0.04	0.03	0.52	0.04	0.02	0.36
	(0.01)	(0.01)		(0.01)	(0.01)	
Health and Well-being	0.09	0.04	0.01**	0.09	0.05	0.15
	(0.02)	(0.01)		(0.02)	(0.01)	
Natural Sciences, Math and Statistics	0.02	0.02	0.62	0.03	0.01	0.27
	(0.01)	(0.01)		(0.01)	(0.01)	
Social Sciences and Communication	0.22	0.26	0.32	0.22	0.24	0.65
	(0.03)	(0.03)		(0.03)	(0.03)	
Arts and Humanities	0.04	0.05	0.75	0.04	0.04	0.88
	(0.01)	(0.02)		(0.01)	(0.01)	
Information and Technologies	0.08	0.09	0.64	0.08	0.08	0.91
	(0.02)	(0.02)		(0.02)	(0.02)	
Services	0.12	0.11	0.64	0.13	0.11	0.49
	(0.02)	(0.02)		(0.02)	(0.02)	
Observations	282	200	482	233	249	482

Notes: Data from Cohort 2. Reported p-values are for the pairwise t-test for the mean difference between the control and treatment means. *Per capita income* is an approximation using the midpoint values of household income ranges divided by number of people in the household. For the first range (until 2000 BRL) we use 2000 BRL and for the last (more than 30000 BRL) we use 30000 BRL. Variable "Employed" is defined as having a job, an internship, or being an entrepreneur. The graduation areas classification is based on the General Area classification by the Brazilian Minister of Education.

D.1.5 Descriptive Statistics of Baseline Variables by Socioeconomic Status and Race

Table 6 reports how low- and high-income applicants differ regarding some important dimensions. Low-income applicants are less likely to report having a job or an internship, and, conditional on working, women from the low-income group are also less likely to hold a job or an internship related to their field of study or interest. Moreover, we observe a socioeconomic gap in the expectation to receive a wage above the perceived average wage for their field and experience [7] and in job search readiness. Finally, low-income women are more likely to report having zero professional references. Taken together, these facts suggest that, due to financial and non-financial barriers, low-income applicants have access to fewer job opportunities and lower-quality jobs when compared to their high-income counterparts.

Table 6: **Differences in Baseline Characteristics by Socioeconomic Status**

	Low-income	Standard Errors	Observations
	Panel A: No controls		
Employment	-0.075***	(0.029)	1139
Employment in the field	-0.076***	(0.028)	687
Expect to receive wage above average	-0.074***	(0.028)	1139
Job Search Readiness Index	-0.132***	(0.047)	1139
Low expectation to obtain a job or promotion	0.029	(0.029)	1139
Expected to receive zero offers for first or next job	-0.003	(0.014)	1139
Report zero close professional references	0.059**	(0.028)	1139
	Panel B: Control for Age and College Graduation		
Employment	-0.070**	(0.030)	1139
Employment in the field	-0.070**	(0.029)	687
Expect to receive wage above average	-0.058**	(0.029)	1139
Job Search Readiness Index	-0.136***	(0.049)	1139
Low expectation to obtain a job or promotion	0.013	(0.030)	1139
Expected to receive zero offers for first or next job	-0.009	(0.015)	1139
Report zero close professional references	0.061**	(0.029)	1139

Notes: Using baseline data for cohorts 1 and 2, the table shows the results of regressions of several outcomes on a dummy for whether the individual belongs to a low-income group (family per capita income lower than 1 minimum wage) . Variable "Employed" is defined as having a job, an internship, or being an entrepreneur. Variable "Employed in the field" is conditional on employment. The "Job Search Readiness" Index combines responses for three separate questions: ability to find job vacancies, ability to impress interviewers and ability to prepare CVs.

Table 7 shows how white and non-white women from our sample compare. Non-white women are, on average, poorer, more likely to come from a family of lower parental education, and more likely to have attended a public school. Around 57% of the non-white women are from the low-income group, compared to 38% among the white women. Similarly, among the low-income women, 59% are non-white, compared to 41% among the high-income. The

[7] For women still in undergrad we ask related to perceived mean wage for their field after they graduate.

correlation between the low-income and non-white variables is 0.18. However, even if they come from a poorer background, Table 8 shows that non-white applicants are not more likely to have worse employment and expectations outcomes when compared to white applicants.

One potential explanation for the similarity in employment and expectation outcomes, despite the substantial socioeconomic differences between white and non-white women, might be the differences in the field of study. Table 7 shows that non-white women in our sample are more likely to study Business and Law, while white women are more likely to study Social Sciences and Humanities. White and non-white women might also differ in dimensions not observed in our baseline, such as social-emotional skills and the presence of role models.

Table 7: **Baseline Characteristics by Race**

	White	**Non-White**	**p-value**
First generation	0.45	0.65	0.00***
	(0.02)	(0.02)	
Age	25.20	26.79	0.00***
	(0.23)	(0.28)	
Public high school	0.50	0.76	0.00***
	(0.02)	(0.02)	
Household income *per capita*	2498.96	1635.35	0.00***
	(107.84)	(58.36)	
Employed	0.61	0.60	0.87
	(0.02)	(0.02)	
Field of Study			
Education	0.12	0.11	0.70
	(0.01)	(0.01)	
Business, Admin. Law	0.22	0.30	0.01***
	(0.02)	(0.02)	
Engineering	0.16	0.15	0.47
	(0.02)	(0.02)	
Agriculture	0.02	0.03	0.19
	(0.01)	(0.01)	
Health and Well-being	0.05	0.07	0.11
	(0.01)	(0.01)	
Natural Sciences	0.02	0.01	0.65
	(0.01)	(0.01)	
Social Sciences	0.28	0.22	0.03**
	(0.02)	(0.02)	
Arts and Humanities	0.05	0.03	0.04**
	(0.01)	(0.01)	
Info. and Technologies	0.07	0.07	0.96
	(0.01)	(0.01)	
Services	0.05	0.05	0.93
	(0.01)	(0.01)	
Observations	573	566	1139

Table 8: **Differences in Baseline Characteristics by Race**

	Non-white	Standard Errors	Observations
	Panel A: No controls		
Employment	-0.005	(0.029)	1139
Employment in the field	-0.021	(0.028)	687
Expect to receive wage above average	0.015	(0.028)	1139
Job Search Readiness Index	0.033	(0.047)	1139
Low expectation to obtain a job or promotion	-0.024	(0.029)	1139
Expected to receive zero offers for first or next job	-0.012	(0.014)	1139
Report zero close professional references	0.030	(0.028)	1139
	Panel B: Control for Age and College Graduation		
Employment	-0.001	(0.029)	1139
Employment in the field	-0.007	(0.029)	687
Expect to receive wage above average	0.025	(0.029)	1139
Job Search Readiness Index	0.021	(0.047)	1139
Low expectation to obtain a job or promotion	-0.024	(0.029)	1139
Expected to receive zero offers for first or next job	-0.010	(0.014)	1139
Report zero close professional references	0.037	(0.028)	1139

Notes: Using baseline data for cohorts 1 and 2, the table shows the results of regressions of several outcomes on a dummy for whether the individual is non-white . Variable "Employed" is defined as having a job, an internship, or being an entrepreneur. Variable "Employed in the field" is conditional on employment. The "Job Search Readiness" Index combines responses for three separate questions: ability to find job vacancies, ability to impress interviewers and ability to prepare CVs.

Declaration of interests

☒ The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

☐ The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: