Effective and scalable programs to facilitate labor market transitions for women in technology *

Susan Athey  Emil Palikot†

November 22, 2022

Abstract

We describe the design, implementation, and evaluation of a low-cost and scalable program that supports women in Poland in transitioning into jobs in the information technology sector. This program, called “Challenges,” helps participants develop portfolios that demonstrate capability for relevant jobs. We conduct two independent evaluations, one focusing on the Challenges program and another on a one-to-one mentoring program. We exploit the fact that both programs were oversubscribed to randomize access among applicants and measure the impact of the programs on the probability of finding a job in the technology sector within four months. We estimate that the mentoring program increases the probability of finding a job in technology by 13 percentage points and the Challenges program by 9 percentage points. The benefit of Challenges can be compared to the program cost of approximately $15 per person. Next, we show that treatment effects vary with individual characteristics, and we estimate gains from optimally assigning applicants across the two programs. We find that optimal assignment increases participants’ average probability of finding a job in technology by approximately 13% compared to random assignment. Finally, we analyze the counterfactual impact of expanding the available spots in Challenges from 15% to 50% of applicants, while assigning applicants to programs using the proposed targeting rule. Considering the entire applicant pool as the baseline, this generates a 30% increase in technology sector jobs.

*We thank Aleksandra Bis and Natalie Piling from Dare IT for support and collaboration. We thank Keshav Agrawal for excellent research assistance. The Golub Capital Social Impact Lab at Stanford Graduate School of Business provided funding for this research. This research has been subject to review and approval by Research Compliance Office at Stanford University, protocol number IRB-62530 and registered at AEA RCT registry with numbers 0010044 and 0010045.
†Both Stanford Graduate School of Business.
1 Introduction

Identifying and implementing effective programs to support workers transitioning into occupations and industries that offer attractive jobs is an important concern for labor policymakers. The information technology (IT) sector has experienced rapid growth in recent decades. However, not all demographic groups have benefited equally from opportunities in this sector; in particular, women remain underrepresented. Despite a number of programs and initiatives aimed at supporting women in the technology sector\(^1\) in 2021, women held only 30% of technology jobs worldwide (United Nations, 2021). The shortage of empirical evidence evaluating the effectiveness of specific approaches in addressing this issue is one of the hurdles in designing a successful policy response.

In this paper, we describe how we designed, implemented, and evaluated a scalable and low cost program called “Challenges” that increases the likelihood of successfully transitioning to a technology sector by approximately 45% within four months of graduation\(^2\). Challenges helps participants develop portfolios demonstrating capability relevant to the jobs they seek. The program is delivered online and primarily involves interactions between program participants, which makes it cheap to operate (approximately $15 per person) and suitable for scaling up. The program is also highly transferable across domains of the technology sector and languages; the first edition, evaluated in this paper, had specializations in User Experience (U/X) and front-end programming. The program’s second edition was offered in Ukrainian instead of in Polish.

We designed the Challenges program as a scalable alternative to traditional in-person mentoring. To develop it, we partnered with Dare IT, a Polish organization that has been operating a non-profit mentoring program since 2018. Dare IT’s mentoring program serves women who, despite already having invested time into acquiring IT skills, still have not been successful in obtaining a technology job. There is a high demand for the program, as evidenced by the fact that it has been repeatedly oversubscribed, but scaling it up to meet the demand faces several challenges. First, it is difficult to find enough mentors. Mentors operate only on a volunteer basis and tend to be women in mid-

\(^1\)For example, the United Nations organizes an annual event Girls in ICT Day; The European Commission carries out an annual study: Women in Digital Scoreboard. There are various NGOs dedicated to increasing women’s participation in the technology sector; For instance, Women in Technology International is a community of 160,000 women in the technology sector; Girls Who Code operates a network of 10,000 clubs providing support for women in technology. Finally, many major technology companies have initiatives aimed at supporting women in technology, including Women in Tech from Salesforce, The Women at Microsoft, or Accenture’s Women in TS&A.

\(^2\)Throughout this paper, we consider technology jobs as all jobs in firms in the technology sector other than those in finance, regulatory, legal, accounting, or human resources or jobs outside of the technology sector that involve software development and testing, IT support, and data analytics. Specifically, all jobs in U/X and front-end programming are considered technology jobs.
seniority technology jobs. Given that few women are in these positions recruiting more mentors is difficult. Second, the one-to-one format of the mentoring program makes it operationally demanding. Ensuring satisfaction, remediating conflicts, and tracking everyone are complicated tasks that require regular supervision by Dare IT staff.

The development of Challenges proceeded in two steps. First, together with Dare IT, we set up a series of interviews with HR experts and hiring managers of Polish technology companies to gain their perspectives on ways to support women candidates. Each of our interviewees mentioned the shortage of practical experience or clear signals of practical skills in female applicants’ profiles. Second, along with Dare IT and industry partners, we used the insights from the interviews to create Challenges. The program’s core consists of bi-weekly assignments that, if completed, result in a job-relevant skill signal that can be added to participants’ resumes or mentioned during an interview. The program is designed based on three principles: (i) it is delivered online, which lowers the costs; (ii) it primarily involves interactions between participants, addressing the shortage of mentors; and (iii) it is focused on projects that are directly implementable in a business setting. To operationalize the last principle, industry partners were recruited to help design the assignments and provide domain experts to review and critique selected submissions. We launched the registration to the program in December 2021 with 300 available spots; the program became oversubscribed within four hours.

We conducted two concurrent randomized experiments to evaluate the effectiveness of Challenges and the mentoring program. Both experiments exploit the fact that the two programs were oversubscribed, so we were able to randomize access and evaluate the impact of participation in a program on the probability of having a technology job. To evaluate the mentoring program, we asked mentors to select two mentee candidates (instead of the usual one), and within these pairs, we randomized into treatment and control. In the case of Challenges, we randomly assigned access to 200 spots in the program. Four months after the end of each program, we collected data on the outcomes. We estimate that the mentoring program increased the probability of having a job in technology from 29% to 42%, while the Challenges program increased this probability from 20% to 28%.

The two programs focus on different mechanisms that might support participants. Mentoring can positively impact the chances of obtaining employment in the technology sector by reinforcing self-efficacy, improving interviewing skills, or extending professional networks. In contrast, creating

---

3The difference in the outcomes of the two control groups is likely because, in the mentoring program, mentors selected applicants that were later randomized into the control group while the control group of Challenges is a random draw from the pool of applicants. We document that the outcomes of non-selected applicants are substantially lower than those of the applicants selected by mentors and later randomized into the control group.
portfolio items that constitute signals of specific, job-relevant skills can counter a pessimistic belief (e.g., based on stereotypes) held by hiring managers, recruiters, or the applicants themselves. We find that the two programs exhibit different patterns of treatment effect heterogeneity. Mentoring is particularly beneficial for women from smaller towns that had already spent substantial time acquiring skills in their selected domain. Working with mentors with managerial experience and a longer tenure in technology also leads to high treatment effects. In the case of Challenges, participants from larger cities have higher treatment effects than those from smaller towns.

We exploit heterogeneity in the treatment effects to propose prioritization policies. We propose simple rules that create priority groups based on observed characteristics (e.g., applicants from smaller cities are prioritized for mentoring). We then evaluate these rules using predicted treatment effects where we account for the selection by mentors into the mentoring program. We find that prioritization can substantially increase the two programs’ effectiveness. Furthermore, we show that even after expanding the capacity of Challenges so that every applicant would be able to obtain a spot, selecting some applicants based on their characteristics for the mentoring program improves their outcomes.

Finally, we estimate optimal targeting policies. We use the methodology of Athey and Wager (2021) to estimate non-parametric tree-based policies that optimally assign the applicants between the mentoring program, Challenges, and the group that receives no services. We then evaluate these policies using observed outcomes reweighed by propensity weights, to adjust for the impact of selection by mentors to the mentoring program. We start by comparing two policies: a status quo policy in which users are assigned randomly to the programs and only 28% of applicants receive services with an optimal policy that increases the number of spots in Challenges so that the program can accept 50% of applicants (while keeping the mentoring unchanged - 13%) and assigns applicants following the estimated targeting rule. We find that moving from the status quo to the optimal policy results in a 30% increase in the average probability of finding a job in technology (across all applicants). We decompose these gains by whether they are attributable to the optimal targeting or the additional capacity. We find that by keeping the capacity of Challenges fixed at 50% of applicants and changing the assignment rule from random to targeted, the average probability of programs’ participants of getting a technology job increases by 13%.
2 Literature review

This paper relates to several strands of literature. First, the paper contributes to the literature on the effectiveness of mentoring in improving career outcomes. Several influential papers focus on the effectiveness of mentoring in the academic context. Dennehy and Dasgupta (2017) employ a randomized experiment to show that assigning a female mentor to a female engineering student increases the sense of belonging in the discipline, self-efficacy, motivation, and retention for women majoring in engineering in college. Resnjanskij et al. (2021), also in a randomized experiment, document a positive impact of a mentoring program for disadvantaged high-school youth on their math grades and career plans. A number of papers focus on the impact of mentoring on the careers of academic faculty members. Gardiner et al. (2007) provide empirical evidence that mentored individuals are more likely to stay at a university, receive more grant income, and receive promotions more frequently. In more recent studies, Ginther et al. (2020) and Blau et al. (2010) analyze the effectiveness of a mentorship workshop to support women in research careers. In a randomized access design similar to ours, they show that individuals in the treatment group are more likely to have received tenure at their institution, to have more publications and citations, and to have received more federal grants. We contribute to this literature by providing evidence from a randomized trial of the efficacy of mentoring outside of the context of education or academic jobs.\footnote{The impact of mentoring on career outcomes has also been studied using surveys and other techniques based on observational data. For example, Ragins and Cotton (1999), in a survey of over 600 individuals, find that mentored respondents have better career outcomes than non-mentored respondents. They also find that the gender composition of the mentoring pair influences the effectiveness of mentoring. Similarly, Aryee et al. (1996) survey a group of workers in Hong Kong and found that mentored respondents reported a higher number of promotions and were more satisfied with their work than individuals who are not mentored. Kammeyer-Mueller and Judge (2008) carry out a meta-analysis of this literature and concludes that mentoring improves job and career satisfaction.}

Second, there is rich evidence evaluating the efficacy of offline labor market training programs. Card et al. (2018) carry out a comprehensive meta-analysis of the estimates from over 200 recent studies. They find that the analyzed training programs have, on average, close to zero short-term effects on employment and some small positive impact in 2-3 years after completion. Programs evaluated in Card et al. (2018) are generally aimed at unemployed individuals looking for blue-collar jobs. In this setting, programs that are focused on the development of skills on-the-job tend to be more effective (Sianesi, 2008; Lechner and Gerfin, 2000). On-the-job training programs do not necessarily need to take the form of long apprenticeships to be effective: Biewen et al. (2014) compare a long apprentice-
ship with a short-term on-the-job training, combined with a job search workshop, and find that the short-term program is equally effective or more effective than the much longer program. Fein and Hamadyk (2018) use a randomized experiment to evaluate YearUp, a sectoral training program, and find a 40% increase in quarterly earnings in the medium term. We contribute to this literature by bringing evidence from a randomized trial of a training program that happens online, is focused on training individuals joining a highly paid technology sector, and is very cheap to operate. Our program has elements of on-the-job training as participants develop content that is designed by domain experts to closely resemble the type of content they would produce on the job.

Third, several papers studying the effectiveness of online training focused on MOOCs. Zheng-hao et al. (2015) study survey responses of Coursera learners; they conclude that the vast majority of respondents report career benefits. They also point out that respondents from economically- and academically-disadvantaged populations are reporting particularly large gains. In a difference-in-differences setting, Castaño-Muñoz and Rodrigues (2021) analyze outcomes of learners taking MOOCs in Spain. They find no effect on wages or employment and a moderate effect on job retention. Hadavand et al. (2018) compare participants in a Data Science MOOC that were just above the threshold to graduate with those just below the threshold to graduate and argue that graduation from this MOOC leads to a 30% increase in the probability of finding a job. We contribute to this literature by providing evidence from an experiment randomizing access. Additionally, we compare an online training program to an in-person mentoring program and show that there are patterns of heterogeneity, suggesting groups that should be prioritized for online versus offline learning.

Fourth, the online program that we evaluate is targeted at highly-skilled individuals who want to develop a portfolio item that signals their skills. Thus, we relate to the literature on skills signaling (Spence, 1978). Tyler et al. (2000) show that signaling skills based on GED certification substantially increases annual earnings. Hadavand et al. (2018) argue that the mechanism through which MOOCs increase chances of finding a job is the ability to signal skills with a certificate. We contribute to this literature by showing evidence from a randomized trial of the effectiveness of a program focused on the development of labor market signals. Our population are women trying to get a job in technology where they might face a negative stereotype; thus the signal of skills is a way for them to counter that prior belief. However, since the programs we evaluate incorporate both learning and signaling, we cannot separate out the signaling effect. This would be a fruitful topic for further research.

---

6MOOC stands for Massive Open Online Course.
7GED stands for General Educational Development and is a certificate of high-school skill equivalency.
Finally, this paper contributes to the literature studying the gender gap in the technology sector. Various reasons for low women participation in the technology sector have been studied. For example, Murciano-Goroff (2022) examines the behavior of job seekers and recruiters in the labor market for software engineers and shows that women are more likely to under-report their programming skills, which diminishes their chances of getting hired. Several papers propose various strategies to counter this phenomenon. Correll and Mackenzie (2016) argue that women need more visibility in the form of role models. Cheryan et al. (2013), in a laboratory experiment, show that reducing the perception of technology jobs as male jobs can be effective in increasing women’s interest in the field. Similarly, Del Carpio and Guadalupe (2022), using a randomized experiment, show that targeted messages reducing the perception of software coding as a male field can increase application rates by women to a programming course. This paper contributes to this strand of literature by showing the effectiveness of mentoring and job-training programs in increasing women’s success rate in getting technology jobs. Notably, our work focuses on the primary outcome of finding a job.

3 The Dare IT Mentoring and Challenges Programs

The technology sector in Poland has grown over the last several decades and is currently valued at approximately USD 20 billion (Strzelecki, 2021). The growing demand for tech workers in Poland has contributed to salary growth within the technology sector (approximately 9% in 2021), resulting in a median monthly tech salary of USD 2,600 (Gawlowska-Bujok and Bujok, 2021). According to an industry report, women make up approximately 30% of the technology sector workforce in Poland (Gawlowska-Bujok and Bujok, 2021). In a report by the European Commission (European Commission, 2021), 1.1% of Polish women that are active in the labor market work in the technology industry compared to 5.2% of men.

Dare IT is a social impact organization with the goal of reducing the gender gap in the technology sector by helping qualified women transition from other careers. Dare IT offers a variety of services for women looking for jobs in tech or making their first steps in the profession, including a mentoring program, career fair, networking events, and an online social network.

Mentoring program. Since Dare IT launched its mentoring program in 2018, the number of applicants and mentors has increased each year. The program served five cohorts between 2018 and spring 2021, just before our evaluation began.

8The median salary in Poland amongst full-time employed individuals is around USD 950 per month (GUS, 2022).
The operation of the program proceeds in several steps. Before each cycle of the program begins, Dare IT recruits potential mentors through referrals from past mentors, social media channels, and corporate partnerships. All mentors are women that are currently working in the technology sector, and they are not paid for their participation in the program. Mentors are screened and selected to participate. Applicants interested in participating as a mentee are recruited mainly through social media channels and word-of-mouth recommendations.\textsuperscript{9} The program is funded by grants and donations, which allows participation in the programs to be free of charge. In recent years, demand for the program has been far greater than supply. For example, for spring 2021 cohort Dare IT received over 1,000 applicants for 150 spots.

Dare IT selects participants based on several criteria. They only consider complete applications from women that are currently not employed in a technology job. Additionally, the program is targeted at individuals that already have the skills necessary – measured by the reported number of hours spent learning in a given domain – to find a job in the technology sector but need help with the job search. Dare IT does not consider applicants below a given threshold of existing skills.

After applicants have been screened, mentors receive access to the database of eligible applicants, where they can browse and select the person with whom they want to work.

After participants are matched, mentors and mentees typically meet on a weekly basis in order to support the mentee in their job search. Dare IT provides mentors with guidelines and best practices to increase the likelihood of a successful experience, but the mentors are free to adjust the content to fit the needs of the mentee. As a result, mentoring takes various forms ranging from coaching in job search and interviewing, resume preparation, reviewing code, and developing technical skills.

\textit{Challenges program}

Given the excess demand for the mentoring program, Dare IT considered whether they could increase the number of spots in the program. Unfortunately, scaling up the program is difficult. First, recruiting and selecting mentors is challenging because of the shortage of women having appropriate positions. Second, carrying out a 1:1 mentoring program is operationally challenging.

We partnered with Dare IT to develop a more easily scalable approach. First, we identified impediments to women obtaining jobs in the technology sector by conducting interviews with hiring managers and human resources experts from a dozen major technology companies in Poland. Each of

\textsuperscript{9}At the time we carried out this research, Dare IT was not recruiting participants through paid advertising.
our interviewees mentioned a lack of practical experience or signals of practical skills on job application materials provided by female candidates. They argued that clear proof of a practical experience, even in the form of a short project, can change the dynamic of the interview by allowing focus on this experience, which generally benefits candidates. They also highlighted low trust in skill credentials that are available online. Many of our interviewees described online credentials as needing to be more informative about the capability to carry out real-world business tasks. Motivated by these findings, we developed a new program called Challenges designed to address these specific problems.

The Challenges program takes the form of six biweekly mini-projects which, if fully completed, lead to a portfolio item, e.g., a U/X design for a mobile application, which can be interpreted as proof of practical experience in the chosen domain. The program has a strong on-the-job training aspect: Assignments are developed by domain experts and designed to be directly implementable in a business setting. Each weekly mini-project is discussed and critiqued by domain practitioners, who are either existing mentors or work in the industry, so that participants can keep increasing their skills and knowledge.

The Challenges program is designed to maximize interactions between program participants. First, participants are encouraged to work in teams. In the first edition, evaluated in this paper, participants received guidelines on how to work in teams, and online events for team formation were organized. Starting from the second edition, Dare IT matches people into teams. Second, participants received access to online channels for collaboration on individual assignments. Throughout the program, we have observed high activity on these channels.

We announced the program in December 2021, with the starting date for the first cohort set in mid-January 2022. Dare IT advertised the program on its social media channels to encourage people to apply. Enrollment was set to a maximum of 300 participants. The first 100 places were given to the 100 first applicants (removed from this analysis), and the remaining 200 spots were assigned at random to the remaining applicants. The program was oversubscribed within four hours, and we closed the registration form after that. The applications received after we closed the registration were not included in the randomization, but we nonetheless collected the outcomes of these late applicants as a comparison group.
4 Experimental design and data

4.1 Experimental design

Mentoring experiment. We evaluate the mentoring program using the cohort of Autumn 2021. As in previous cohorts, Dare IT received substantially more applications than the number of mentors. Even after screening for eligibility, the ratio of applicants to mentors was 4:1. There were many more eligible applicants than mentors, which enabled us to randomize access. To ensure that the mentor-mentee matches remained high quality and that access was randomized, we asked mentors to choose two applicants as their mentee candidates instead of one. Within these pairs, we randomized who got into the program (treatment) and who did not (control). Thus, we created a paired experiment.

In the experiment, we had 156 mentor-mentee pairs and an equal number of mentor-control pairs. Four pairs did not start the program because the mentee pulled out before the start. As a result, we included 152 pairs in the analysis. Additionally, we collected data for about 300 eligible applicants whom mentors did not select.

Challenges. We closed the recruitment to the program when we received 500 complete applications. The first 100 applicants were granted a spot in the program, while the other 400 were randomized between treatment and control. Randomization was stratified on the variables collected from the registration survey. Additionally, we collected data on 196 applicants whose application forms came in after we closed the registration.

4.2 Data

We have three main sources of data: registration surveys, outcomes surveys carried out right after the end of the program and also four months later, and data collected from participants’ public LinkedIn profiles.

Applicant registration survey. Applicants in both programs were asked to complete a simple registration survey. Data from the survey included the city of residence, age, and time spent, which measures how much time the applicant had spent developing their IT skills. The mentoring application form also had several open-ended questions related to the applicant’s motivation and the possibility of attaching a short introductory video, but we do not use either in this analysis. Additionally, we collected information on participants’ preferred domains: the main categories were U/X design, manager
Mentors registration survey. Mentors were asked a number of questions before the start of the program. Mentors came from different backgrounds and had various levels of experience. Thus, it is plausible that different types of mentors varied in their impact they had on participants. Mentors were asked a number of questions before the start of the program. We collected the following variables: first-time mentor, career changer (which indicates whether the mentor herself has always worked in the technology sector or not), managerial experience (which indicates whether the mentor has any managerial experience or not), and years of experience (years of experience in the tech sector).

Outcomes survey. We surveyed participants of each program and those in control groups four months after the end of the program. In the survey, we were primarily interested in the salaries in the new jobs: we asked the question whether the respondent found a new job and in which range the salary was. We also inquired about the number of job offers and whether the participant negotiated the proposed offer or accepted the initial one. Finally, we carried out a shorter survey right after the end of mentoring, the “short survey,” where we asked for salaries for the new jobs. Altogether we conducted three surveys: the main mentoring survey in which we received 162 responses (with 111 responded from treatment group subjects), the initial survey of the mentoring program (with 194 responses), and the Challenges survey, for which we received responses from 68 subjects in the control group and 61 from the treatment group.

LinkedIn data. The primary source of outcomes data are LinkedIn profiles of the subjects. These data were collected four months after the end of each program by Dare IT using a web-crawling algorithm prepared for this purpose. The key outcome is whether the participant found a technology job after the beginning of a program. As new technology jobs, we consider jobs with a starting date of January 2022 or later for mentoring and February 2022 or later for Challenges that fall into one of the following two categories:

1. All jobs in technology companies other than positions finance, regulatory, legal, accounting, and HR, where technology companies include firms in software development, testing, and sales; data analytics; IT services; digital marketing; and online platforms (including peer-to-peer platforms, and online shops).
2. Jobs in non-technology companies that involve software development and testing, IT support, and data analytics. In our context, this category includes jobs in banks and management consulting agencies.

Based on these categories, we define two outcomes: new job - which is any job (in technology or not) that started in January 2022 or later, tech job - which is a job that falls in category (1) or (2) above. Additionally, we use LinkedIn to collect data on the highest education level (the top entry in the education section of the profile), which we translated into the following categories: high school, bachelors degree, masters degree, postgraduate degree, nontraditional credential, and other. In the last category, we include profiles with no education information or information that we were not able to classify as any of the other categories. Next, we collected information on college majors to construct two binary variables: STEM and social science or business. The first variable includes all STEM degrees, excluding social sciences. The second one includes degrees in either social science, business, finance, or management. Finally, when available, we infer a person’s age from the start date of the latest educational degree and the length of professional experience as the difference between 2022 and the first job recorded on LinkedIn in the professional experience section.

Table 1 presents summary statistics of the main variables for the treatment and control groups for the two programs.

\[\text{In Appendix B, we compare salary estimates from Glassdoor of salaries in group (1) and group (2). We do not find a statistically significant difference.}\]
Table 1: Summary statistics of the main variables: treatment, control, and applicants

| Variables                          | Treated group |  |  | Control group |  |  |
|-----------------------------------|---------------|---|---|---------------|---|---|
|                                   | N  | Mean | St. Dev. | Min | Max | N  | Mean | St. Dev. | Min | Max |
| **Mentoring: mentor characteristics** |    |      |         |     |     |    |      |         |     |     |
| First time mentor                 | 152 | 0.467 | 0.501   | 0   | 1   | 145 | 0.434 | 0.497   | 0   | 1   |
| Managerial experience             | 152 | 0.520 | 0.501   | 0   | 1   | 145 | 0.510 | 0.502   | 0   | 1   |
| Career changer                    | 152 | 0.691 | 0.464   | 0   | 1   | 145 | 0.710 | 0.455   | 0   | 1   |
| Years of tech experience          | 152 | 5.856 | 3.777   | 2   | 35  | 145 | 6.021 | 3.794   | 2   | 35  |
| **Mentoring: participant characteristics** |    |      |         |     |     |    |      |         |     |     |
| New job                           | 152 | 0.467 | 0.501   | 0   | 1   | 147 | 0.422 | 0.496   | 0   | 1   |
| Tech job                          | 152 | 0.421 | 0.495   | 0   | 1   | 147 | 0.293 | 0.456   | 0   | 1   |
| Social science                    | 152 | 0.132 | 0.339   | 0   | 1   | 147 | 0.136 | 0.344   | 0   | 1   |
| STEM                              | 152 | 0.487 | 0.501   | 0   | 1   | 147 | 0.531 | 0.501   | 0   | 1   |
| Family friends IT                 | 152 | 0.697 | 0.461   | 0   | 1   | 147 | 0.701 | 0.460   | 0   | 1   |
| Warsaw                            | 152 | 0.276 | 0.449   | 0   | 1   | 147 | 0.259 | 0.439   | 0   | 1   |
| Mother                            | 152 | 0.257 | 0.438   | 0   | 1   | 147 | 0.259 | 0.439   | 0   | 1   |
| Applied before                    | 152 | 0.243 | 0.431   | 0   | 1   | 147 | 0.231 | 0.423   | 0   | 1   |
| Years of professional experience  | 152 | 7.053 | 5.049   | 0   | 31  | 147 | 7.204 | 4.816   | 0   | 23  |
| **Challenges: participant characteristics** |    |      |         |     |     |    |      |         |     |     |
| New job                           | 183 | 0.333 | 0.473   | 0   | 1   | 225 | 0.293 | 0.456   | 0   | 1   |
| Tech job                          | 183 | 0.284 | 0.452   | 0   | 1   | 225 | 0.196 | 0.398   | 0   | 1   |
| Social science                    | 183 | 0.208 | 0.407   | 0   | 1   | 225 | 0.267 | 0.443   | 0   | 1   |
| STEM                              | 183 | 0.421 | 0.495   | 0   | 1   | 225 | 0.391 | 0.489   | 0   | 1   |
| Years of professional experience  | 183 | 7.645 | 4.976   | 0   | 31  | 225 | 7.671 | 6.237   | 0   | 31  |
| **Applicants both programs: participant characteristics** |    |      |         |     |     |    |      |         |     |     |
| New job                           | -   | -    | -       | -   | -   | 496 | 0.317 | 0.466   | 0   | 1   |
| Tech job                          | -   | -    | -       | -   | -   | 496 | 0.152 | 0.359   | 0   | 1   |
| Social science                    | -   | -    | -       | -   | -   | 496 | 0.258 | 0.438   | 0   | 1   |
| STEM                              | -   | -    | -       | -   | -   | 496 | 0.359 | 0.480   | 0   | 1   |
| Years of professional experience  | -   | -    | -       | -   | -   | 496 | 7.905 | 4.885   | 0   | 35  |

Note: Summary statistics of selected variables. The bottom panel, Applicants both programs, includes applicants that applied to the program and were not included in the randomization between treatment and control. Career changer takes the value of one when the mentor reported in the registration survey having changed occupations. Years of tech experience is reported by mentors in the survey. STEM excludes social sciences and architecture. Social science includes business, management, finance, and accounting. Years of professional experience is measured based on LinkedIn profiles. Appendix A shows covariate balance between treatment and control.
5 Effectiveness of DareIT programs

Analytical framework

Let each DareIT applicant be characterized by \((X_i, Z_i, W_i, Y_i, S_i)\), where \(i\) is the applicant’s index \(i = 1, \ldots, I\), \(X_i \in \mathcal{X}\) is a matrix of characteristics of applicants that are observed by mentors and recorded in data, \(\zeta_i \in \mathcal{Z}\) are characteristic of applicants that are not observed in the data (though some might be observed by mentors prior to treatment assignment), and \(S_i \in \mathcal{S}\) is a random variable indicating whether the applicant has been selected by a mentor. Finally, \(W_i^P \in \{0, 1\}\) is the treatment assignment into program \(P\) and \(Y_i \in \mathcal{Y}\) is the observed outcome.

Let \(Y_i(P)\) and \(Y_i(0)\) be potential outcomes when applicant \(i\) participates in program \(P\) and when she does not; \(P \in \{M, C\}\), where \(M\) stands for Mentoring and \(C\) for Challenges. We are primarily interested in estimating the magnitude of the average treatment effect, defined as \(\tau^P := E[Y_i(P) - Y_i(0)]\), and testing whether average treatment effects are higher than zero. We are also interested in comparing the treatment courses to one another, although our estimates of such contrasts are noisy due to the limited sample size in each experiment.

Selected sample in Mentoring. Mentors were asked to choose two applicants. Let \(G_j\) denote the pair of applicants selected by mentor \(j\), and let \(S_i = 1\) when the applicant has been selected by a mentor while \(S_i = 0\) otherwise. Let \(J\) be the total number of mentors. Dare IT asks mentors to choose applicants who, in the mentor’s opinion, would benefit the most from the program. Thus, selected applicants are not a random draw from the pool. The average treatment effect of mentoring that we estimate is defined as

\[
\tau^M := E[Y_i(M) - Y_i(0)|S_i = 1].
\]

Table 1 includes summary statistics about subjects in the experimental groups of the mentoring program as well as characteristics of selected applicants; the control group is different from the mentoring applicant group in a variety of dimensions, most notably the probability of getting a job in technology. In Challenges, participants were randomly selected from eligible applicants.

\[\text{To select mentees, mentors received information from the application form; the important part of the application that mentors observed, which we did not, are short introductory videos recorded by the applicants.}
\]

\[\text{See Section 3 for eligibility criteria.}\]
5.1 Average treatment effects

Table 2 shows the average treatment effects of mentoring and Challenges. We present results for two outcome variables: tech job – our primary outcome variable – takes the value of 1 when the subject found a job in the tech sector since the start of the program and 0 otherwise. We also include new job, which takes the value of 1 if the subject started any new job after the start of a program and 0 otherwise.

Rows one, two, and three show the estimates of the average treatment effect. The first row is the estimate of $\tau^P$, computed using a difference-in-means estimator: Below the estimates, we present standard errors. The second row shows the estimate of the average treatment effect and its standard error estimated using the Augmented Inverse Propensity Weighing (AIPW) estimator (Robins et al., 1994). AIPW is a doubly-robust method which means that it adjusts for covariates in the outcome model and the propensity score. We use the grf implementation of the AIPW estimator, as in Athey et al. (2019). The third row is the estimate of the average treatment effect divided by the mean outcome in the control group.

The results presented in Table 2 show that the average treatment effects on the primary outcome are high and the difference from zero is statistically significant in both programs. We find that mentoring increases the probability of finding a tech job by 44% (S.E. 17%) and that Challenges increases this probability by 45% (S.E. 14%). Comparing the effectiveness of the two programs, we conclude that both are similarly impactful, though due to the wide confidence intervals, we cannot exclude moderate differences.

Columns (2) and (4) of Table 2 present results with new job as the outcome variable. We find that the average treatment effect is much lower and the difference from zero is statistically insignificant. These results indicate that the subjects in the control group found new jobs, but that these jobs were in different sectors.

In Appendix D, we show the results based on the outcomes survey. We find that the survey respondents in both treatment groups report higher number of job offers and higher salaries than the

---

$\text{V}(\hat{\tau}^M) = \frac{1}{J \times (J - 1)} \sum_{g=1}^{J} (\hat{\tau}^M_j - \hat{\tau}^M)^2$. (2)

Athey and Imbens (2017) show that this is a conservative estimator of variance in a paired experiment.

In Appendix C, we show how the treatment effect evolved over time. We find that approximately half of the impact of the mentoring program can be attributed to the time period during which the program was running and the other half to the three months following its completion.
Table 2: Average treatment effect estimates.

|                | Mentoring           | Challenges        |
|----------------|---------------------|-------------------|
|                | Tech job            | New job           | Tech job | New job         |
| ATE            | 0.129 (0.05)        | 0.045 (0.05)      | 0.089 (0.04) | 0.040 (0.05) |
| ATE AIPW       | 0.129 (0.05)        | 0.037 (0.06)      | 0.088 (0.04) | 0.0437 (0.04) |
| ATE % baseline | 43.941 (17.09)      | 10.749 (14.23)    | 45.306 (14.07) | 13.636 (12.00) |
| Mean treatment | 0.421 (0.04)        | 0.467 (0.04)      | 0.284 (0.03) | 0.333 (0.03)   |
| Mean control   | 0.293 (0.04)        | 0.422 (0.04)      | 0.196 (0.03) | 0.293 (0.03)   |
| No. of obs. treatment | 152 | 152 | 183 | 183 |
| No. of obs. control     | 147   | 147   | 225   | 225   |

Note: The estimates of the average treatment effects of both the mentoring program and Challenges on the probability of finding a new job in technology and finding any new job. The first row shows estimates of the average treatment effect using the difference-in-means estimator. The second column shows the estimates using the augmented inverse propensity weighting (AIPW) estimator. The third row is the difference-in-means estimate of the average treatment effect divided by the mean outcome in the control group. Rows four and five present mean outcomes in treatment and control groups and rows six and seven the number of observations per experimental group. The first and second columns present the results of the mentoring intervention on the probability of finding a job in technology and the probability of finding any new job. Columns three and four show the results of the impact of participation in Challenges on the probability of finding a job in technology and any new job. Standard errors are presented below the average treatment effect estimates. The variance of the difference-in-means estimates the average treatment effect of mentoring estimated using equation 2. The variance of the difference-in-means estimate of the Challenges intervention is estimated using the classical Neyman-Splawa estimator. AIPW variance estimated using grf implementation of AIPW estimator (see Athey et al. (2019) section 4 for details).
control group. For both outcomes, the differences between treatment and control are greater for the mentoring experiment.

5.2 Heterogeneous treatment effects

To study heterogeneity in the treatment effects we subdivide the covariate space into disjoint sets based on a given characteristic and compare average outcomes in the two sets. We test two types of hypotheses: first, whether the average treatment effect for the specific group equals zero:

$$H_0^1 : E[Y_i(P) - Y_i(0)|X_i = x] = 0.$$  \hspace{1cm} (3)

and second, we test whether the two groups have different treatment effects:

$$H_0^2 : E[Y_i(P) - Y_i(0)|X_i = x] \neq E[Y_i(P) - Y_i(0)|X_i = x'].$$  \hspace{1cm} (4)

**Mentor characteristics.** We consider two groups of covariates: participants’ observed characteristics and mentors’ observed characteristics. Figure 1 presents the most important characteristics of mentors.

**Figure 1:** Heterogenous treatment effects of mentoring: Mentors’ characteristics.

Note: We estimate conditional average treatment effects (CATE) using the difference-in-means estimator. The height of the plot shows the difference in the mean probability of finding a job in tech between treatment and control for subjects in the group and the whiskers show confidence intervals. Long experience takes the value of one when the mentor has experience in her domain of 6 years or more (mean of the sample) and zero otherwise. Short experience analogously takes the value of one when the mentor has less than 6 years of experience. See Appendix for additional variables and details.

Two characteristics of mentors are associated with large, statistically significant treatment effects: mentees working with mentors who have managerial experience and those who have long experience
in the tech industry. Mentees who work with mentors without managerial experience and short tenure in the technology sector have treatment effects that are statistically insignificant. In contrast, we find that the distinction between mentors who had prior experience mentoring and those who did not is inconsequential.

**Participants characteristics.** Figure 2 presents the estimates of conditional average treatment effects across participants’ characteristics. We focus on comparing the impact of mentoring (in blue) with the impact of Challenges (in red) along several important characteristics. In several groups, the conditional average treatment effects are very similar: participants without a social science background or with a STEM degree had similar outcomes across the two programs. In contrast, participants from smaller cities benefited substantially from mentoring but reaped small benefits from Challenges. We find that the mentoring program has more heterogeneity in the treatment effects than Challenges. In fact, as we show in Appendix E after adjusting for multiple hypotheses testing we find heterogeneity in the treatment effects only in the mentoring experiment.

**Figure 2:** Heterogeneous treatment effects program comparison.

Note: Blue bars represent mentoring and red bars represent Challenges. Conditional average treatment effects are estimated using the difference-in-means estimator. The height of the plot shows the difference in the mean probability of finding a job in tech between treatment and control for subjects in the group; whiskers show confidence intervals. “Small city” excludes the six largest cities in Poland. See Appendix E for additional variables and details.

### 5.3 Applicants

In addition to collecting data on applicants in the treatment and control groups, we also collected data on remaining applicants. In the case of the mentoring program, these are applicants who were
screened for eligibility by Dare IT but not selected by mentors, while in Challenges, these are last-minute applicants who were not considered in the randomization. There were 300 additional applicants in the mentoring group and 193 in Challenges. Comparing the data from participants and non-participants, we document two points: First, we show that mentors chose more promising candidates, as measured by candidates’ eventual outcome; Second, we document that applicants to Challenges were similarly successful to the control group. We use these observations to carry out an additional treatment effects heterogeneity analysis.

**Mentoring program**

In Figure 3, we compare the average outcomes across the three groups. We find that applicants to the mentoring program were much less successful in finding a job in tech than people in the control group.

![Figure 3: Probability of finding a job in technology across the three groups.](image)

Note: This figure shows the shares of subjects in treatment, control, and applicant groups of the mentoring intervention who found a new job in technology. Whiskers show confidence intervals. The variance is estimated using the Neyman-Splawa estimator.

The statistics shown in Figure 3 show that, in this context, the ability to carry out a randomized trial is crucial for obtaining reliable estimates of the program’s effectiveness.

**Challenges program**

For the Challenges program, we accepted applications for a few days after places for the experiment had already filled. As a result, we received additional 193 late applications and collected data on their outcomes. Unlike in the mentoring program, there was no selection process by mentors to consider for the program. This meant that essentially there was no difference between the control group and the late applicants. As such, their outcomes are similar: 0.26 vs. 0.20, with a standard error of the
difference of 0.04.

We use this additional data from the Challenges program for robustness checks to the estimates of the heterogeneous treatment effects. First, we estimate the propensity scores of being treated for the three groups using regression forest. Figure 4 shows the distribution of predicted propensity scores. The supports of the propensity scores across the three groups are similar.

**Figure 4:** Propensity scores for the two experimental groups and applicants.

![Histogram of propensity scores](image)

*Note: Histograms of the estimates of the propensity to be treated in the Challenges intervention by experimental group. In green for the applicants’ group, in orange for the control group, and in purple for the treatment group. Propensity was estimated using regression forests with a full set of covariates.*

With this expanded data set, we first estimate the treatment effect using the AIPW estimator and obtain the average treatment effect estimate of 0.08 (standard error of 0.037), which amounts to a 38% increase.

Second, we estimate AIPW scores and compare them across groups based on characteristics. Figure 5 presents the results. We find the same patterns of heterogeneity as those reported in Figure 2. The subjects without a social science degree, those from larger cities, and those pursuing paths other than in U/X have large, statistically significant treatment effects.
Figure 5: Conditional average treatment effects of the Challenges program.

Note: Estimates of the conditional average treatment effect of Challenges using the AIPW estimator. The sample includes three experimental groups: treated, control, and applicants. Whiskers show confidence intervals.

6 Targeting of programs and capacity constraints

The mentoring program has a capacity constraint due to the availability of mentors. In the case of Challenges, we capped the number of spots at 300 to account for unforeseen operational complexities and logistical Challenges. Given the large treatment effects of each program, it is natural to hypothesize that removing capacity constraints has the potential to create substantial value. However, capacity constraints are likely to be a long-term concern for the mentoring program, and they may persist to a lesser extent for the Challenges program. Consequently, Dare IT will continue being faced with the question of which applicants to accept for each program.

In this section, we undertake an analysis of three questions. First, in an environment of limited capacity, are there benefits to prioritizing admission to each program by targeting applicants with particular characteristics? In a survey, 23% of applicants to the Challenges program reported that they were interested in applying to or had previously applied to Mentoring. This suggests that some applicants are interested in both programs, and Dare IT could recommend participating in one of them. Second, who benefits the most from different treatments? This question can be useful in determining the designs of future programs. Third, what are the benefits of relaxing capacity constraints in each program?
6.1 Gains from prioritization.

To study gains from prioritization, we start by considering simple admission rules based on estimated heterogeneous treatment effects and our understanding of the mechanisms through which the two programs impact job outcomes. We focus on two cases: (i) we keep the existing capacity limits in each program and propose an admission rule that increases the overall effectiveness of Dare IT programs; and (ii) we investigate whether there are gains in selecting some participants for the mentoring program instead of Challenges, even when everyone can be offered a spot in Challenges.

Prioritization to Dare IT. Let an admission rule be a mapping from applicants’ characteristics to admission priority levels, $\pi : X \rightarrow \Gamma$, where $\Gamma$ is a set of labels for priority groups. In this section, we consider simple parametric rules. In Section 6.2, we estimate a non-parametric assignment policy that maximizes expected treatment effects.

To evaluate an admission rule, we use AIPW estimates of the average treatment effect in a priority group, $E [Y_i(P) - Y_i(0)|\Gamma_i]$. We consider the full data set including applicants to both programs and the treatment is the participation in any of the Dare IT programs. We are interested in testing the following hypothesis for various groups $g$:

$$H^0 : E [Y_i(P) - Y_i(0)|\Gamma_i = g] \geq E [Y_i(P) - Y_i(0)],$$  \hspace{1cm} (5)

where $E [Y_i(P) - Y_i(0)]$ is the average treatment effect across all applicants. In other words, we want to test whether applicants from the proposed priority groups have higher treatment effects than the average treatment effect. Consider an admission rule described by Algorithm 1:

**Algorithm 1 Prioritization rule**

1. Admit the priority group 1: applicants without a graduate degree who are not from Warsaw, and pursue other paths than U/X,

2. If spots are remaining, admit the priority group 2: applicants without graduate degrees who do not pursue U/X, that come from Warsaw,

3. If spots are remaining, admit the priority group 3: applicants without a graduate degree who want to pursue U/X,

4. If spots are remaining, admit the priority group 4: applicants with graduate degrees.
Algorithm 1 creates four priority groups, so $\Gamma_i \in \{1, 2, 3, 4\}$. This simple admission rule is based on the results presented in Section 5.2. Depending on the number of available spots, Dare IT admits applicants from different priority groups. The group with the highest priority is admitted first, and if there are available spots left, lower-priority groups are admitted. In the evaluated programs, Dare IT could admit approximately 28% of applicants. In the proposed groups, 15% of applicants fall into priority group, 22% into priority groups 1 and 2. Groups 1, 2, and 3 make up 38% of applicants; the rest of them fall into Group 4.

**Figure 6:** Gains from prioritization in admissions to Dare IT.

We present the results of applying this admission rule in Figure 6. We can observe that higher priority groups exhibit higher treatment effects, which is consistent with the patterns of heterogeneity we documented in Section 5.2. The estimate of the average treatment effect for priority group one amounts to over 35 pp (S.E. 8) and for priority groups one and two to 27 pp (S.E. 7). We estimate that, given the existing capacity of admitting approximately 28% of applicants, the proposed admission prioritization rule leads to substantially higher treatment effects.

\[15\] In Appendix F we present the results where instead of using AIPW estimates of the average treatment effect in the group, we use observed outcomes weighted using treatment-propensity weights.
Prioritization to the mentoring program. The two programs have similar average treatment effects and the Challenges program is easier to scale and cheaper to operate. As such, it is interesting to ask whether there are substantial gains from having the mentoring program at all. To investigate this question, we propose admission rules to Mentoring and compare the resulting average treatment effects of mentoring in the priority groups with the average treatment effect of Challenges. We consider a prioritization rule based on applicants’ characteristics (in Appendix F we include an alternative rule that has both mentors’ as well as applicants’ characteristics).

Algorithm 2 Prioritization rule to mentoring: Applicants’ characteristics

1. Admit priority group 1: applicants who are not from Warsaw, are mothers, and spent over 150 hours studying in the domain,

2. If spots are remaining, admit priority group 2: applicants who are not from Warsaw, are mothers, and spent less than 150 hours studying in the domain,

3. If spots are remaining, admit priority group 3: applicants who are not from Warsaw, are not mothers, and spent less than 150 hours studying in the technology domain,

4. If spots are remaining, admit priority group 4: applicants from Warsaw.

The prioritization rule presented in Algorithm 2 focuses on giving priority to applicants that are from smaller cities, are mothers, and have spent a considerable amount of time studying content in the selected domain. We have four priority groups. These groups are consistent with the results presented in Section 5.2.

We are interested in the scenario in which all applicants could be granted a spot in Challenges. Thus, we want to compare the average treatment effect of mentoring per priority group with the average treatment effect of Challenges. We test the following hypothesis:

\[ H^0 : \mathbb{E}[Y_i(M) - Y_i(0)|\Gamma_i] \geq \mathbb{E}[Y_i(C) - Y_i(0)] \]  \hspace{1cm} (6)

Figure 7 presents the results. We estimate that Priority Group 1 has an average treatment effect of 44 pp (S.E. 15) and Priority Group Two of 30 pp (S.E. 8). Both of these estimates are substantially higher than the average treatment effect of Challenges. This means that participants in the mentoring

\footnote{We also compare the estimated average treatment effects of mentoring per group to the estimated average treatment effects of Challenges per group (instead of the average over the entire sample). In the Challenges data, we do not observe whether the applicant is a mother or not. Thus, we aggregate Priority Groups 1 and 2. The resulting priority group one has an average treatment effect of Challenges of -2 pp (S.E. 13) and group two of 5 pp (S.E. 7).}
program gain from being assigned to it, even when they could participate in Challenges.

**Figure 7:** Gains from prioritization in admissions to mentoring: applicants’ characteristics.

![AIPW estimates of ATE in group](image)

Note: Evaluation of admission rule from Algorithm 2. The sample includes the treatment and control groups of the mentoring experiment and the treatment refers to being admitted to the mentoring program. Per each group defined in Algorithm 2, we estimate the average treatment effect using the AIPW estimator:

\[
E[Y_i(M) - Y_i(0) | \Gamma_i].
\]

Whiskers around point estimates present confidence intervals. The red dotted line is the average treatment effect of Challenges for the entire sample.

### 6.2 Optimal assignment policies

The admission rules proposed in the previous section show that prioritization based on applicants’ characteristics can be beneficial. In this section, we focus on estimating an optimal assignment rule.

Let an assignment rule be a mapping from applicants’ characteristics and capacity limits to programs, \( \pi : (X, Q) \rightarrow P \), where \( Q \) are capacity limits in each program, and \( P \) is a set of available programs, \( P \in \{0, M, C\} \). The optimal assignment policy is given by,

\[
\pi^* = \arg \max_{\pi \in \Pi} E[Y(\pi(X_i))],
\]

where \( \Pi \) denotes the class of permissible policies. We follow the methodology developed in Athey and Wager (2021) and restrict our attention to policies generated using regression trees. In Figure 8, we visualize an example of an assignment rule estimated using grf.\(^{17}\)

Although a depth-3 tree is easy to examine and describe, simple trees may not provide the most effective policy. Here, we estimate an optimal policy non-parametrically. We describe our estimation

\(^{17}\)We use a gradient-boosted tree as an outcome model because of better performance in the test set. The results based on a regression forest outcome model are similar.
**Figure 8:** An example of tree-based assignment rule.

Note: The figure presents a depth-3 policy tree based on a gradient-boosted tree outcome model. Example of an optimal assignment rule. Several characteristics play a role in determining which program an applicant should be assigned to: education, with STEM degree splitting the tree first and the level of education with a master’s degree determining in the third layer. Second, professional circumstances play a role; the length of the professional experience matters, with younger applicants generally assigned to Challenges. Additionally, selecting U/X as the intended domain impacts assignment. Finally, the type of residence matters too.

strategy in Algorithm 3

**Evaluated policies.** We consider several ways in which Dare IT programs can be further developed. The evaluated admissions policies can be summarized by capacity limits $Q = (Q^M, Q^C)$ and an assignment rule $\pi$. Specifically, we consider increasing $Q^C$ since the motivation behind the development of Challenges was the low cost of scaling up the program. To evaluate this potential policy, we consider an expansion of the program and compare it to the random and targeted assignment policies.

First, we consider an optimal policy in which the capacity constraint on Challenges is increased to 50% of applicants, so $Q = (0.13, 0.5)$, and participants are assigned to programs using an assignment policy $\pi^*$ obtained using Algorithm 3. Second, we consider the case in which the capacity limits are increased to $Q = (0.13, 0.5)$ but the assignment to programs is done at random. We refer to this policy as the random policy. Finally, as the benchmark, we have the status quo policy, in which $Q = (0.13, 0.15)$ and assignment is done at random.

We start by evaluating optimal policy. We define groups using the assignment in Algorithm 3, $A \in \{0, M, C\}$, and compare outcomes of these groups under alternative assignments. The group assigned to 0 does not participate in any of the Dare IT programs. We label their assignment as “Nothing”. Thus, we are interested in testing:
Algorithm 3: Non-parametric policy estimation

1. Select outcome model based on test set predictive performance.
2. Estimate multi-arm causal forest using `grf` implementation of [Athey et al. (2019)].
3. Obtain $\tau_{p}^{i}$ using out-of-bag predictions from the forest: $\tau_{p}^{i} = \mathbb{E}[Y(P) - Y(0)|X = x_i]$, where $Y(P)$ is the outcome under program $P$ and $Y(0)$ is the outcome under not participating in any program.
4. Assign treatment to maximize treatment effects subject to capacity constraint. Let $Q^p$ be the capacity limit of program $p$ and $z_{ip}$ an indicator variable taking the value of one when applicant $i$ is assigned to program $p$ and zero otherwise. We solve the following constrained optimization problem:

$$\max_{z_{ip}} \sum_{i=1}^{I} \sum_{p=1}^{P} z_{ip}\tau_{p}^{i} \text{ s.t. } \sum_{i=1}^{I} z_{ip} \leq Q^p \forall p \text{ and } \sum_{i=1}^{I} z_{ip} = 1 \forall i.$$ 

The first constraint ensures that the capacity constraints are not violated. The second one that every applicant is assigned to one program. There is no capacity limit on being out of the program ($P = 0$).
5. Adjust observed outcomes using propensity weights estimated using multinomial logistic regression. Obtain $\hat{Y}$.

```
return (A, \hat{Y})
```

Table 3: Gains from the optimal assignment.

| group                      | Share | Optimal Assignment | Challenges | Mentoring | Nothing | Mentoring - Challenges | Mentoring - Nothing |
|----------------------------|-------|--------------------|------------|----------|---------|------------------------|---------------------|
| Group assigned to mentoring| 0.131 | 0.499              | 0.170      | 0.499    | 0.147   | 0.329                  | 0.352               |
| Group assigned to Challenges| 0.500 | 0.330              | 0.330      | 0.344    | 0.197   | 0.034                  | 0.147               |
| Group assigned to nothing  | 0.369 | 0.186              | 0.315      | 0.409    | 0.186   | 0.094                  | 0.223               |
| All applicants             | 1     | 0.299              | 0.304      | 0.388    | 0.187   | –                      | –                   |

Note: The rows describe groups defined under the optimal policy. Columns two to five describe the mean value of the group under various assignments. Groups are defined on the basis of $A$ from Algorithm 3 and the estimated values are $\mathbb{E}[\hat{Y}|A = p, A = p]$ where $A$ is the treatment group in the experiment. The 5th column is the value under mentoring less the value under Challenges and the last column is the value under mentoring less the value under nothing. Assigning all applicants to the mentoring program or to Challenges would violate capacity constraints. We do not report the benefit of doing that. Standard errors are in parentheses below point estimates.

$$H : E[\hat{Y}|A = p & A = p] \geq E[\hat{Y}|A = p & A = p'] \forall p' \in \{M, C, 0\},$$ (8)

where $A$ is the evaluated assignment; for example, $E[\hat{Y}|A = M & A = C]$ is the expected probability of finding a technology job of individuals in a group that, under optimal policy, is assigned to the mentoring program when they are assigned to Challenges. Thus, we want to evaluate whether the optimal assignment leads to better outcomes than the alternative assignments. Table 3 shows the results.

We find that the group that is assigned to the mentoring program benefits substantially from this...
Table 4: Comparison of counterfactual policies.

|                        | Share Optimal & Random | Optimal | Random | Optimal - Random | Share Status quo | Status quo | Optimal - Status quo |
|------------------------|------------------------|---------|--------|------------------|------------------|------------|----------------------|
| Value mentoring        | 0.131                  | 0.499   | 0.390  | 0.108            | 0.131            | 0.390      | 0.109                |
|                        | (0.102)                | (0.037) | (0.047) |                  | (0.037)          | (0.047)    |                      |
| Value challenges       | 0.500                  | 0.330   | 0.304  | 0.0257           | 0.150            | 0.304      | 0.257                |
|                        | (0.063)                | (0.040) | (0.048) |                  | (0.040)          | (0.048)    |                      |
| Value Dare IT          | 0.63                   | 0.365   | 0.322  | 0.043            | 0.281            | 0.344      | 0.021                |
|                        | (0.041)                | (0.001) | (0.007) |                  | (0.013)          | (0.017)    |                      |
| Value all              | 1                      | 0.299   | 0.272  | 0.027            | 1                | 0.230      | 0.069                |
|                        | (0.041)                | (0.001) | (0.010) |                  | (0.013)          | (0.017)    |                      |

Note: The rows describe average values of participants in programs. The composition and size of groups change across policies. Column one shows the share of participants in each program under random and optimal policies. Columns two and three describe values (mean-adjusted outcomes) under the optimal policy and random policy, with standard errors below. Column four is the differences between outcomes under optimal and random policies. Column five is the share of applicants under the status quo in each program. Column six shows average adjusted outcomes under status quo policy; the last column is the difference between optimal policy and status quo. The bottom row, value all, includes all applicants: those in the mentoring program, those in Challenges, as well as the group that is left out of the programs.

The key comparison for this group is that of their outcomes when they would be assigned to the mentoring program versus Challenges. We estimate the benefit of assigning them to the mentoring program to be 0.329 (S.E. 0.09). These are substantial differences in the probabilities of finding a job in the technology sector. The group assigned to Challenges benefits from participation in the program as compared to nothing. The gain from assigning them to the mentoring program, on the other hand, is statistically insignificant. Finally, the group assigned to nothing would benefit from being assigned to the mentoring program estimate of this benefit amounts to 0.223 (S.E. 0.03), which is lower than for the mentoring group. The group assigned to nothing would not benefit from Challenges.

The last row of Table 3 presents the outcomes for all applicants. We find that the optimal assignment does worse than sending everyone to Challenges or to mentoring. However, assigning everyone to Challenges or the mentoring program would violate capacity constraints, so we do not report the benefit of this policy option.

In Table 4, we compare the three policies. We define the value of Dare IT programs as the mean probability of finding a job in the technology sector across participants in the program: \( \text{value} P = E[Y(P) \mid Q, \pi(X)] \). For the group all, we consider all applicants, both those taking part in a program as well as those who do not participate.

First, comparing optimal and random policies we see that there are substantial gains from the optimal assignment. In the last row of column 4, we see the difference in estimated average (across all applicants) probabilities of finding a job in technology between the two policies. We find that un-
nder the optimal policy the probability of finding a technology job is higher by 2.7 (S.E. 1.0) percentage points, or 10% of value under random policy. Comparing the outcomes of the Dare IT program participants only we find that the targeted policy increases their outcomes by 4.3 (S.E. 0.7) percentage points or 13% of the value under random policy.

Second, the gains from implementing the optimal policy are even higher when we also consider the change in value due to increased capacity. We do that by comparing value under optimal to value under status quo. We find a 6.9 (1.7) percentage points difference, which corresponds to 30% of the value under status quo.

In Figure 9, we show average covariates values and standard errors per assignment group. Several assignment patterns are consistent with the results of the analysis of the heterogeneous treatment effects reported in Section 5.2. Applicants from smaller cities are prioritized to Dare IT programs. The same applies to applicants with degrees in social sciences. Applicants without a graduate degree are assigned to Challenges.

Figure 9: Average covariate values in assignment group.

Note: Each tile corresponds to a covariate value per assignment group from Algorithm 3. Tiles show means and standard errors. We present values for selected covariates.

The comparison of the three policies shows that there are high gains both from optimal targeting...
as well as from increasing capacity limits. When we compare the outcomes under the existing policy to outcomes under a counterfactual scenario in which the capacity of Challenges increases from 15% to 50% of applicants, and the applicants are assigned following the estimated policy, we find a 30% increase in the probability of finding a job in the technology sector across all applicants.

7 Conclusion

Changing labor markets push workers to want to transition into high-growth sectors, and facilitating these transitions is an important goal for labor policy. However, effectively designing such policies requires principled evaluation of various training programs in terms of their overall impact and their heterogeneity. In this paper, we provide evidence of the high effectiveness of two programs supporting women transitioning into the technology sector.

This paper identifies a class of interventions that can effectively narrow the gender gap in technology. We show that a mentoring program and an online training program aimed at developing labor market signals increase the probability of having a job in the technology sector by more than 40% 4 months after the end of the program. Furthermore, the online training program developed during this research project is cheap to operate, approximately $15 per person, and suitable for scaling up, suggesting that it can have an impact extending beyond the current setting.

We evaluate policies to improve the proposed programs by analyzing the heterogeneity of treatment effects and existing capacity limits. We estimated that expanding the capacity of the online training program and optimally assigning applicants across the two offerings substantially increased the chances of landing a job in the technology field across all applicants.

Both studied programs involved relatively small groups of participants, which was a limiting factor to the type of analyses we could carry out. Specifically, we studied simple approaches for the possible development of the programs. We plan to pursue this research further by scaling the programs and evaluating richer strategies for developing them.
References

Aryee, S., Wyatt, T., and Stone, R. (1996). Early career outcomes of graduate employees: The effect of mentoring and ingratiation. *Journal of management studies*, 33(1):95–118.

Athey, S. and Imbens, G. W. (2017). The econometrics of randomized experiments. In *Handbook of Economic Field Experiments*, volume 1, pages 73–140. Elsevier.

Athey, S., Tibshirani, J., and Wager, S. (2019). Generalized random forests. *The Annals of Statistics*, 47(2):1148–1178.

Athey, S. and Wager, S. (2021). Policy learning with observational data. *Econometrica*, 89(1):133–161.

Barnow, B. S. (1987). The impact of CETA programs on earnings: A review of the literature. *Journal of Human Resources*, pages 157–193.

Biewen, M., Fitzenberger, B., Osikominu, A., and Paul, M. (2014). The effectiveness of public-sponsored training revisited: The importance of data and methodological choices. *Journal of Labor Economics*, 32(4):837–897.

Blau, F. D., Currie, J. M., Croson, R. T., and Ginther, D. K. (2010). Can mentoring help female assistant professors? Interim results from a randomized trial. *American Economic Review*, 100(2):348–52.

Bloom, H. S., Orr, L. L., Bell, S. H., Cave, G., Doolittle, F., Lin, W., and Bos, J. M. (1997). The benefits and costs of JTPA Title II-A programs: Key findings from the National Job Training Partnership Act study. *Journal of Human Resources*, pages 549–576.

Card, D., Kluve, J., and Weber, A. (2018). What works? A meta analysis of recent active labor market program evaluations. *Journal of the European Economic Association*, 16(3):894–931.

Castaño-Muñoz, J. and Rodrigues, M. (2021). Open to MOOCs? Evidence of their impact on labour market outcomes. *Computers & Education*, 173:104289.

Cheryan, S., Plaut, V. C., Handron, C., and Hudson, L. (2013). The stereotypical computer scientist: Gendered media representations as a barrier to inclusion for women. *Sex roles*, 69(1):58–71.

Correll, S. and Mackenzie, L. (2016). To succeed in tech, women need more visibility. *Harvard Business Review*, pages 2–6.
Del Carpio, L. and Guadalupe, M. (2022). More women in tech? Evidence from a field experiment addressing social identity. *Management Science*, 68(5):3196–3218.

Dennehy, T. C. and Dasgupta, N. (2017). Female peer mentors early in college increase women’s positive academic experiences and retention in engineering. *Proceedings of the National Academy of Sciences*, 114(23):5964–5969.

European Commission (2021). Women in digital scoreboard 2021.

Fein, D. and Hamadyk, J. (2018). Bridging the opportunity divide for low-income youth: Implementation and early impacts of the Year Up program. Pathways for Advancing Careers and Education. OPRE Report 2018-65. *Office of Planning, Research and Evaluation*.

Gardiner, M., Tiggemann, M., Kearns, H., and Marshall, K. (2007). Show me the money! An empirical analysis of mentoring outcomes for women in academia. *Higher Education Research & Development*, 26(4):425–442.

Gawlowska-Bujok, M. and Bujok, T. (2021). IT job market in Poland in 2021. Salaries, technologies and requirements in job ads.

Ginther, D. K., Currie, J. M., Blau, F. D., and Croson, R. T. (2020). Can mentoring help female assistant professors in economics? An evaluation by randomized trial. In *AEA Papers and Proceedings*, volume 110, pages 205–09.

GUS (2022). Structure of wages and salaries by occupations in October 2020.

Hadavand, A., Gooding, I., and Leek, J. T. (2018). Can MOOC programs improve student employment prospects? *Available at SSRN 3260695*.

Heckman, J. J., LaLonde, R. J., and Smith, J. A. (1999). The economics and econometrics of active labor market programs. In *Handbook of Labor Economics*, volume 3, pages 1865–2097. Elsevier.

Kammeyer-Mueller, J. D. and Judge, T. A. (2008). A quantitative review of mentoring research: Test of a model. *Journal of Vocational Behavior*, 72(3):269–283.

Lechner, M. and Gerfin, M. (2000). Microeconometric evaluation of the active labour market policy in Switzerland. *Available at SSRN 233906*. 

32
Murciano-Goroff, R. (2022). Missing women in tech: The labor market for highly skilled software engineers. *Management Science, 68*(5):3262–3281.

Ragins, B. R. and Cotton, J. L. (1999). Mentor functions and outcomes: a comparison of men and women in formal and informal mentoring relationships. *Journal of applied psychology, 84*(4):529.

Resnjanskij, S., Ruhose, J., Wiederhold, S., and Woessmann, L. (2021). Can mentoring alleviate family disadvantage in adolescence? A field experiment to improve labor-market prospects.

Robins, J. M., Rotnitzky, A., and Zhao, L. P. (1994). Estimation of regression coefficients when some regressors are not always observed. *Journal of the American Statistical Association, 89*(427):846–866.

Romano, J. P. and Wolf, M. (2005). Exact and approximate stepdown methods for multiple hypothesis testing. *Journal of the American Statistical Association, 100*(469):94–108.

Sianesi, B. (2008). Differential effects of active labour market programs for the unemployed. *Labour Economics, 15*(3):370–399.

Spence, M. (1978). Job market signaling. In *Uncertainty in Economics*, pages 281–306. Elsevier.

Strzelecki, R. (2021). Not just the Pope, Lech Walesa and vodka: Poland’s technology sector is booming. *Forbes*.

Tyler, J. H., Murnane, R. J., and Willett, J. B. (2000). Estimating the labor market signaling value of the GED. *The Quarterly Journal of Economics, 115*(2):431–468.

United Nations (2021). Measuring digital development. facts and figures.

Zhenghao, C., Alcorn, B., Christensen, G., Eriksson, N., Koller, D., and Emanuel, E. J. (2015). Who’s benefiting from MOOCs, and why. *Harvard Business Review, 25*(1):2–8.
Appendix

A Covariate balance

In Figure 10 we show the balance of covariates in the mentoring experiment: each dot corresponds to the absolute difference of means in treatment and control divided by the square root of the sum of variances. The only difference that is statistically significant is the graduate degree, where 66% of treated subjects have a graduate degree and 55% of control (p-value 0.05).

Figure 10: Covariate balance mentoring.

In Figure 11 we show the balance of covariates in the Challenges experiment: each dot corresponds to the absolute difference of means in treatment and control divided by the square root of the sum of variances in treatment and control. None of the differences is statistically significant.
Figure 11: Covariate balance Challenges.

Note: Balance across treatment and control of the main variables. Points correspond to the absolute difference in means between treatment and control divided by the square root of the sum of variances in treatment and control.

B Comparison of salary levels from Glassdoor

We consider two types of jobs as a job in technology:

1. All jobs in tech companies other than finance, regulatory, legal, accounting, and HR, where tech companies include firms in software development, testing, and sales; data analytics; IT services; digital marketing; and online platforms (including peer-to-peer platforms, and online sales).

2. Jobs in non-tech companies that involve software development and testing, IT support, and data analytics. In our context, this category includes jobs in banks and management consulting agencies.

We analyze whether the two categories of jobs pay similar salaries. To do that we searched for salary estimates of all unique job titles that fall into the two categories using Glassdoor.com. We considered only exact matches and salary estimates for jobs in Poland. In total, we have 265 unique salary estimates. We find that the mean monthly salary for tech jobs in tech firms (category 1) is PLN 8634 and for tech jobs in non-tech firms it is PLN 8866. The difference is statistically insignificant (p-value 0.9). Figure 12 shows histograms of salary estimates.
Figure 12: Histograms of salary estimates from Glassdoor.

Note: Histograms of salary estimates from Glassdoor. In green, we show tech jobs in tech firms (category 1), in blue tech jobs in non-tech firms (category 2).
C Average treatment effect of mentoring over time

In Figure 13 we estimate the average treatment effect on the probability of finding a job in tech over time. The analysis is cumulative, that is it presents the results up to the month given on the horizontal axis. We find that there has already been a positive, albeit statistically insignificant, effect during the program. However, the difference between treatment and control increased after the program has finished.

Figure 13: The estimate of the average treatment effect over time.

Note: Difference-in-means estimated average treatment effect considering outcomes up to the month given on x-axis - cumulative.

D Outcomes measured in surveys

We use data from outcomes surveys to provide additional evidence of the effectiveness of the two programs that give insights into the mechanisms through which participation boosts outcomes. There were three surveys: two main ones carried out approximately four months after the end of each program, and a shorter survey right after the end of the mentoring program from which we obtained information on salaries in jobs found during or right after the mentoring program. As discussed in Section 4, results based on the outcomes survey suffer from a selection problem. Thus, we interpret the findings based on this data as suggestive evidence.

The number of job offers. Figure 14 shows the number of job offers received by survey respondents broken by experimental groups. We find that subjects in the control group (in green) were more likely to report receiving zero job offers than the subjects in the treated group (in orange); for any non-
zero range, there is a higher share of the treatment group rather than the control group. The largest difference between the experimental groups is amongst subjects reporting more than ten job offers.

**Figure 14:** Reported numbers of job offers: Mentoring.

![Figure 14](image)

*Note: The number of job offers received by subjects in the mentoring experiment that responded to the survey. Each bar corresponds to the share of respondents from an experimental group. Orange represents the treated group, and green represents the control group.*

In Figure 15, we present evidence of the differences in the number of job offers across treatment and control groups of the Challenges experiment. We find results analogous to those from the mentoring program. The lowest category, here less than two job offers, has a higher share of control group respondents than treated group respondents. In contrast, treated subjects are more likely to report a higher number of job offers. The difference between treatment and control is more pronounced in the mentoring program.

**Figure 15:** Reported numbers of job offers: Challenges.

![Figure 15](image)

*Note: The number of job offers received by subjects in the Challenges experiment that responded to the survey. Each bar corresponds to the share of respondents from an experimental group. Orange represents the treated group, and green represents the control group.*
**Salary ranges.** Figure 16 presents the share of respondents to mentoring outcomes surveys declaring a salary within a salary range (ranges were pre-defined in the survey). Orange bars indicate the treatment group, and the green ones are the control group. The right panel is based on the survey carried out right after the end of the program, while the left panel is from the survey conducted four months after the end of the program.

**Figure 16: Salary ranges: Mentoring.**

![Salary ranges: Mentoring](image)

*Note: Reported salary ranges by subjects who found new jobs after the start of the mentoring program. Orange represents the treated group, and green represents the control group. The length of a bar represent the share of respondents declaring a salary within the range. The right panel survey was conducted right after the end of the program; the left panel survey was conducted four months after the program.*

Evidence presented in Figure 16 suggests that subjects in the treated group of the mentoring experiment that found a new job are more likely to have a salary in a high salary range.

In Figure 17, we report analogous results from the Challenges outcome survey. We find that a higher share of the survey respondents in the control reported salary in the lowest range as compared to the treatment group. We also find that the opposite is true for the highest salary range.

**Figure 17: Reported salary range Challenges.**

![Reported salary range Challenges](image)

*Note: Reported salary ranges by subjects who found new jobs after the start of the Challenges experiment. The length of a bar represent the share of respondents declaring a salary within the range. Orange represents the treated group, and green represents the control group.*
Negotiation. Respondents that reported finding a job were asked whether they negotiated the conditions of their employment. Amongst the mentoring group, 33% of treated subjects that found a new job negotiated the terms of the new job, while only 24% of the control group did so. The difference is statistically insignificant.

E Heterogeneous treatment effects

Mentoring experiment. In Table 5 we show the estimates of the heterogeneity in treatment effects of the mentoring intervention. We find that after adjusting for multiple hypotheses testing using the methodology of Romano and Wolf (2005) participants from Warsaw have lower treatment effects, participants from smaller cities that spent a lot of time learning in the domain have higher gains (at the 10% level). Additionally working with mentors with long experience in the technology sector is associated with higher treatment effects. Other characteristics are insignificant.

Challenges experiment. In Table 6 we show the estimates of the heterogeneity in treatment effects of the Challenges intervention. After adjusting for multiple hypotheses testing using the methodology of Romano and Wolf (2005) none of the covariates lead to statistically significant heterogeneity in treatment effects.
### Table 5: Heterogeneous treatment effects of the mentoring intervention.

| variable           | ATE  | CI low | CI high | Romano Wolf p-value |
|--------------------|------|--------|---------|---------------------|
| Social science edu | 0.10 | -0.21  | 0.41    | 0.96                |
| STEM edu           | 0.10 | -0.06  | 0.26    | 0.55                |
| Warsaw             | -0.06| -0.27  | 0.15    | 0.05                |
| Small town         | 0.33 | 0.11   | 0.55    | 0.06                |
| Mother             | 0.25 | 0.06   | 0.45    | 0.31                |
| Age low            | 0.12 | -0.04  | 0.28    | 0.31                |
| UX path            | 0.03 | -0.14  | 0.20    | 0.20                |
| No graduate degree | 0.20 | 0.03   | 0.38    | 0.51                |
| Lot of time spent  | 0.23 | 0.09   | 0.38    | 0.08                |

Note: Conditional average treatment effects are estimated using the difference-in-means estimator. The top panel shows mentee characteristics and the bottom panel mentors’ characteristics. Columns two and three show 5% confidence intervals. The last column shows the p-value adjusted for multiple hypotheses testing using methodology of Romano and Wolf (2005). “Small town” excludes the six largest cities in Poland. “Age low” includes participants between 18 and 24 years old. “Lot of time spent” takes the value of one when the participant reported spending over 150 hours learning in the domain.

### Table 6: Heterogeneous treatment effects of the Challenges intervention.

| group              | ATE  | CI low | CI high | Romano Wolf p-value |
|--------------------|------|--------|---------|---------------------|
| Social science edu | -0.02| -0.16  | 0.13    | 0.31                |
| STEM edu           | 0.14 | 0.00   | 0.29    | 0.39                |
| Small town         | 0.04 | -0.07  | 0.16    | 0.87                |
| Age low            | 0.13 | 0.00   | 0.25    | 0.64                |
| UX path            | 0.09 | -0.04  | 0.22    | 0.80                |
| No grad degree     | 0.18 | 0.03   | 0.32    | 0.80                |
| Lot of time spent  | 0.01 | -0.24  | 0.26    | 0.19                |

Note: Conditional average treatment effects are estimated using the difference-in-means estimator. Columns two and three show 5% confidence intervals. The last column shows the p-value adjusted for multiple hypotheses testing using methodology of Romano and Wolf (2005). “Small town” excludes the six largest cities in Poland. “Age low” includes participants between 18 and 24 years old. “Lot of time spent” takes the value of one when the participant reported spending over 150 hours learning in the domain.
F Gains from prioritization

Figure 18: Gains from prioritization to Dare IT programs.

Prioritization to mentoring. We additionally evaluate a prioritization rule that is based on both applicants’ and mentors’ characteristics.

Algorithm 4 Prioritization rule to mentoring: Applicants’ and mentors characteristics

1. Admit Priority Group 1: applicants that are not from Warsaw, do not have a graduate degree, and mentor have a managerial experience,

2. If there are spots remaining, admit Priority Group 2: applicants that are not from Warsaw, do not have a graduate degree, and mentor does have a managerial experience,

3. If there are spots remaining, admit Priority Group 3: applicants that are not from Warsaw, have a graduate degree, and mentor does have a managerial experience,

4. If there are spots remaining, admit Priority Group 4: applicants from Warsaw.

In Figure 19 we show the results.
Figure 19: Gains from prioritization in admissions to mentoring: Applicants’ and mentors’ characteristics.

Note: Evaluation of admission rule from Algorithm 4. The sample includes the treatment and control groups of the mentoring experiment and the treatment refers to being admitted to the mentoring program. Per each group defined in Algorithm 4, we estimate the average treatment effect using the AIPW estimator: $E[Y_i(M) - Y_i(0)|T_i]$. Whiskers around point estimates present confidence intervals. The red dotted line is the average treatment effect of Challenges for the entire sample.