Journal of Development Economics

Building Future-proof Skills for Youth Employment

--Manuscript Draft--

Manuscript Number:	DEVEC-D-23-00688R1
Article Type:	Registered Report Stage 1: Proposal
Section/Category:	Health, Education, gender, poverty
Keywords:	coding bootcamps; Employment; vocational training; youth; Gender; STEM; Technology; economic mobility
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Response to Reviewers:	

Pre-Analysis Plan: Building Future-proof Skills for Youth Employment

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Latest draft: August 29, 2023

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Keywords: coding bootcamps, employment, active labor market policy, vocational training, youth, gender, STEM, technology

JEL codes: M53, J21, J24.

Study pre-registration: AEA RCT Registry-0011362.

Proposed timeline: If accepted based on pre-results review, this study will be completed in June 2026.

Contents

1 Introduction

Youth unemployment and underemployment have become major challenges in the developing world. In the Latin America and the Caribbean (LAC), the youth unemployment rate is, on average, triple that of adults. Moreover, the proportion of youth not in education, employment, or training has been at least 20% for the past 15 years (ILO, 2022). Skill mismatch is one of the most popular explanations for youth labor market problems, especially in a labor market that continually evolves in response to technological changes. Indeed, surveys of firms throughout LAC countries suggest a substantial gap between the skills youth possess and those firms seek in potential employees. This is particularly salient in Brazil, one of the top 10 countries experiencing the most significant talent shortages (Manpowergroup, 2022). Vocational training programs are the government's most common active labor market policies to tackle such issues, and they are often the flagship programs for the country's youth policies.

Although there is relatively recent but extensive literature on the effects of vocational training programs, the evidence to date has been disappointing. A recurrent criticism of traditional training models is that they fail to adjust timely to labor market changes that shape the demand for different skill sets. In this context, an increasing number of developing countries see the Information and Communication Technology (ICT) sector as having the potential to generate significant expansion in well-paying formal jobs. However, while the ICT industry continues to grow at an accelerated pace, evidence suggests that many businesses struggle to find qualified workers with programming skills (Brasscom, 2021; ITU, 2021). To deal with this shortage, a new type of job training program has emerged: the coding bootcamp (ITU, 2016; World Bank, 2017). Coding bootcamps were designed to bridge the gaps in formal education in tech by providing people with no prior coding experience with an accelerated path to in-demand programming skills. In addition, they offer employment support to help workers overcome barriers they may face in the ICT labor market. Despite the growing popularity of coding bootcamps and the high levels of positive employment outcomes reported by training providers, the literature is still muted about the causal effects of this training model.

Our study uses an RCT to investigate the impacts of a coding bootcamp among disadvantaged young jobseekers in Brazil. To do so, we have partnered with an NGO that offers high-quality coding bootcamps worldwide. Their job placement rates in Brazil (measured six months after graduation) have been at least 80% over the past years, so the intervention has a lot of impact potential. Through 2023 and 2024, we will randomly offer applicants a spot in their oversubscribed courses in Brazil. Beneficiaries will receive 300-400 hours of programming skills training (Java, JavaScript, .Net, or Mobile) followed by job search assistance services. The program is free of charge and supports low-income individuals to pay for computer and internet costs. We will carry out primary data collection 6 months after the intervention to measure the program's impact on employment, earnings, skills, job search outcomes, job benefits and satisfaction, and other dimensions of well-being (enrollment in college, mental health, and perceptions about life success). Given the well-known underrepresentation of women in STEM (with the gender gap in computer science careers being the most salient), we are also particularly interested in measuring the treatment effects for women as well as measuring gender stereotypes about tech. Finally, using government administrative records, we will measure the program's effects on formal employment, earnings, and occupation up to 12 months after training.

There has been significant growth in the number of RCTs evaluating job training programs. Existing evidence suggests that these programs can enhance participants' career outcomes, although the impacts are generally modest (McKenzie, 2017; Card et al., 2018). Many of the programs studied in this literature target low-productivity jobs, which offer limited opportunities for economic mobility and may become obsolete as technology reshapes labor demand. Our study contributes by examining a training model that aims to help individuals with non-traditional backgrounds secure highly-paid technology jobs. In this sense, our research joins nascent work on how to prepare workers for the jobs of the future (J-PAL, 2020). Similar to our study, Aramburu et al. (2021) evaluate the impact of coding bootcamps in the LAC context. However, they test this training model only for women who are mostly employed at baseline and hold college degrees. We believe we are the first to investigate whether such programs could boost the labor market prospects of low-income young unemployed individuals. In this regard, our study complements the work by Atkin et al. (2021) in Sub-Saharan Africa. They study whether short-term digital skills training (50h) combined with a referral program help young jobseekers to thrive in the ICT sector. Our program's training is much denser, covering programming skills and preparing individuals to work as junior developers. Whether this model is adequate for our target population remains an open question and is crucial for policy. Our study is also related to recent research in the U.S. that examines sectoral employment programs providing skills training to disadvantaged workers for highly paid jobs in industries with high labor demand. Evidence suggests these programs generate substantial and persistent earnings gains following training (Katz et al., 2022). The program we evaluate has most of the features of the U.S. sectoral employment programs and targets a similar population. Hence, our results are key to the design of comparable training models in the developing country context. Finally, we contribute to this literature by bringing evidence from a randomized trial of a training program that happens entirely online. Despite the widespread popularity of online job training programs, the evidence so far is restricted to a few observational studies investigating Massive Open Online Courses (MOOCs).[1]

Our project also contributes to the literature interested in reducing the gender gap in the technology sector. Although female participation in the labor market has increased in the last decades, most ICT occupations –and, in particular, those that require coding skills– remain highly male-dominated. In Brazil, women are less than 30% of the workforce in these occupations (World Bank, 2018). Faced with this challenge, our partner implements many efforts to recruit women into the program. In particular, around half of the program participants are female. We will provide evidence on the effectiveness of providing them coding skills training combined with intermediary services to break down placement barriers. In this way, we complement Aramburu et al. (2021) who find a positive impact of coding bootcamps in Argentina and Colombia on women's probability of finding a job in tech. In contrast, we will evaluate a program not exclusively for women, and that targets women from completely different backgrounds. Athey and Palikot (2022) is the only other work that we are aware of that experimentally evaluates the effects of labor market interventions on women's employability in tech.[2] Unlike our study, they exclusively examine women who already possess advanced programming skills.

[1]Zhenghao et al. (2015) analyze administrative data from Coursera and report that the majority of learners perceive career benefits. In a difference-in-differences design, Castaño-Muñoz and Rodrigues (2021) assess the effects of MOOCs in Spain and find no impact on wages or employment, but a moderate effect on job retention.

[2]In particular, they evaluate a mentoring program and a program helping participants develop portfolios demonstrating capability for tech.

2 Research Design

2.1 Intervention

We partnered with an NGO providing ready-to-work coding bootcamps worldwide (refer to Appendix Section A for further details on the partnership). Our target population consists of unemployed Brazilian youth aged between 18 and 30 years who have completed high school in a public institution and reside in Recife, Campinas, São Paulo, and Rio de Janeiro[3]. Furthermore, the implementer is dedicated to ensuring a high participation rate among women, LGBTQIAP+ individuals, and non-white individuals. According to the available administrative data, in 2022, 45% of the students were female, 50% were non-white, 28% identified as LGBTQIAP+, and 45% had a monthly family income of less than 2,000 BRL (the minimum wage being 1,380 BRL), with an average age of 23.

The main aspect of the intervention entails 300–400 hours of intensive programming skills training over a three-month period, focusing on web development (Java or JavaScript), mobile development, or .NET development. The implementer establishes partnerships with private sector entities to ensure that the curricula align with the requirements of the ICT sector. The program demands full-time commitment, equivalent to approximately 8 hours per working day, and is delivered through online classes that combine synchronous and asynchronous components. Additionally, the program provides students with hands-on experience, enabling them to build a portfolio in their chosen field of study. Optional soft skills training courses and psychological support are also available to the students. Upon completing the training, students receive job placement services, including workshops on job interview skills and CV writing, newsletters featuring open vacancies, and job referrals to partner firms. The implementer maintains close contact with the students even after they secure employment, offering further training opportunities, professional guidance, and assistance in finding new employment if necessary. The program is offered free of charge, and the organization provides financial support to the most vulnerable individuals, including food and internet vouchers worth approximately 100 USD. Additionally, the organization lends laptops to these individuals.

Since 2020, the implementer has successfully trained over 2,000 individuals in Brazil, with an 82% job placement rate (measured six months after graduation) among the 2022 students. They conduct 1 to 4 classes per month, each accommodating 40 students. These are funded through private resources and strong partnerships with external companies, such as Microsoft. More recently, they have implemented their training model to governments as part of public policies[4]. The organization also operates in 16 other countries, having graduated nearly 80,000 students.

2.2 Randomization design

Individuals will be first screened using the implementer's standard screening process. First, they need to meet some eligibility criteria: (i) be between 18 and 30 years old, (ii) have completed high school, (iii) live within a 130 km radius from the city center where the program is running (Recife, São Paulo, Rio de Janeiro, or Campinas), (iv) have no employment relationship, and (v) have a per capita monthly

[3]These cities are situated in the southeast and northeast regions of Brazil. São Paulo and Rio de Janeiro are the country's largest cities, with estimated populations of 12.4 million and 6.8 million, respectively. Together, these four cities have a population of 22 million, accounting for 10.3% of Brazil's total population.

[4]For instance, as part of a public policy in Brazil, they will to train 5,000 students by 2025. The intervention follows the same format as their privately-funded programs, including the screening process. However, the government does not permit a lottery to select the final participants from the eligible pool, so we will not evaluate these cohorts.

family income of less than one minimum wage. Then, applicants go through logical and reading tests. Scores range from 0 to 100 and applicants have to score above 50 in both tests to move forward.[5] The last step of the screening process is a personal interview. In this stage, interviewers try to screen candidates from non-traditional backgrounds who have a good fit with technology and have available time and motivation to succeed in the course.

Individuals approved in these phases will be eligible to participate in the program. Due to a limited number of slots in the program and a large number of applicants, eligible students will be randomly selected to join the course. The randomization will occur in batches during 2023 and 2024 (over 20 classes). We expect to have 1,500 eligible students for 800 slots during a 14-month time window (the first cohort starting training in August 2023). Considering a compliance rate that is over 90% (based on baseline data), we plan to randomly offer a slot in the program to around 900 eligible individuals (treatment group).

2.3 Power calculation

Power calculations discussed next are based on a significance level of 5% and a power of 80%. We consider a scenario with 1,500 eligible students, of which 900 are randomly assigned to the treatment group, and assume a 90% compliance rate (based on baseline information). When using baseline data on formal employment from all program applicants to simulate our statistical power, we find a minimum detectable effect (MDE) of 6.5 percentage points (around 30% of the mean). We believe this is reasonable considering the high employment rate among the implementer's formal students (80%). For comparison, Atkin et al. (2021) found that a softer digital skills training program in Kenya increased overall employment by 10 p.p or 30% of the mean.

In this research, we are particularly interested in assessing the treatment effects on the likelihood of working in an ICT job. If we take an extremely conservative scenario by assuming the maximum variance of a dummy variable for this outcome, the MDE is 8.2. When investigating employment in the ICT sector, Atkin et al. (2021) found an effect of 42 p.p. Our MDE falls within the range reported in other studies as well. Katz et al. (2022) found that the impact of various U.S. sectoral employment programs on employability in the targeted sectors ranged from 12 to 40 p.p., depending on the program. Aramburu et al. (2021) found that women-only coding bootcamps in Argentina and Colombia increased participants' probability of finding a tech job by 9.2 p.p.

These calculations do not take attrition into account as we can use government administrative records to measure formal and tech employment, which are not affected by attrition. Nonetheless, when relying on survey data, we may expect some level of attrition. In the same scenario as mentioned above (binary variable with maximum variance), assuming a 10% (20%) attrition rate, the MDE increases to 8.6 (9.1) p.p.

An important aspect of this research is to evaluate the intervention's effect on women's labor market outcomes. Considering the share of women previously enrolled in the program, the MDE concerning formal employment stands at approximately 10 p.p. This falls within a reasonable range of potential effects. In terms of heterogenous effects, we can only identify large differential effects between women and men, 14 p.p.

Finally, the MDE on a standardized measure (e.g. coding skills) is around 0.16. We believe that

[5] Based on past data from more than 10,000 applicants, we know that approximately 70% score above the threshold.

the statistical power of our study will be higher than initially predicted by these calculations as we will include a rich set of baseline covariates in our main specification (age, gender, education, family income, experience in the formal labor market, and logical and reading skills).

2.4 Hypotheses

This section presents the hypotheses on (primary and secondary) outcomes and potential mechanisms through which the coding bootcamp could impact the job seekers. We will collect data on these outcomes 6 months after training through surveys. We will also collect data on the primary outcomes (restricted to the formal labor market) up to 12 months after training using government administrative records. We present detailed information about data collection in the next section.

2.4.1 Primary outcomes

The program we evaluate is designed to help participants gain employment in highly paid technology jobs by equipping them with in-demand skills and providing wraparound services. Therefore, we expect that the program will affect participants' employment, primarily in ICT, and earnings positively as the following hypotheses state.

A1. The treatment will have an effect different from zero on employment.

A2. The treatment will have an effect different from zero on earnings.

A3. The treatment will have an effect different from zero on the probability of working at highly paid tech jobs.

Measures: Our main measure to test ***A1*** takes value 1 if the individual reports to have had a job during the month before the interview, or 0 if unemployed or out of the labor force. Complementary employment measures include: number of months worked in the last semester, and hours worked in the last week. To test ***A2***, we will investigate total earnings from any wage or self-employment in the month before the interview.[6] We will also create a standardized "labor market" index of these five variables following Anderson (2008). In addition, we will ask workers to categorize their current work situation as (i) employment with a company under a formal labor contract, (ii) employment with a company lacking a formal labor contract, (iii) engagement in an internship/apprenticeship with a company, (iv) self-employment/ownership of a business with a registered number, or (v) self-employment/ownership of a business without a registered number.

Besides using self-reported survey data to measure ***A1*** and ***A2***, we will use government administrative records. However, these are restricted to the formal labor market. Moreover, we will measure the impacts 6 and 12 months after training. In particular, we will measure (i) whether the individual has a formal job, (ii) the number of months worked in the formal labor market in the past semester (or year), and (iii) total formal labor earnings. We will create a standardized index of these variables.[7] In addition, we will investigate whether individuals have a formal enterprise. Finally, we will directly compare the treatment effects on formal employment and earnings across survey and administrative data.

[6] We will express the treatment effects as percentage of the control group mean following Chen and Roth (2023).

[7] We will not assess the impacts on hours worked as there is barely any heterogeneity in this dimension in the Brazilian formal labor market.

To test ***A3***, we will investigate (i) whether the individual has had a tech job —there is, one requiring coding tasks (programming in Python, JavaScript, HTML, etc.)— during the month before the interview, and (ii) whether he or she has had a job paying above the median salary of young adults in Brazil. We will perform a similar analysis using government data. In this case, we will identify ICT jobs using the Brazilian Classification of Occupations (CBO) codes.

2.4.2 Mechanisms

We believe one of the primary reasons why the program should improve the labor market prospects of participants is through the enhancement of skills that are currently in high demand. In particular, the coding bootcamp represents an intensive, immersive learning experience providing students up to 400 hours of programming training. Therefore, we believe it should improve their coding skills as stated next. The other major program benefit that can help participants in the labor market is job search assistance. By providing individuals with search tips, information on open vacancies, and job referrals to employers with which the program staff has deep relationships, we expect the program to increase their job search efficacy and efforts (at least on the extensive margin).

B1. The treatment will have an effect different from zero on coding skills.

Measures: We will measure coding skills through an incentivized online questionnaire containing two easy questions about broad knowledge of programming and five questions where individuals see coding scripts and have to spot an error (one with a low level of difficulty, three medium, and one high). From these questions we will create a standardized measure and a binary outcome indicating whether the individual got one of the questions with medium or high difficulty. We will also ask if individuals have received any coding education and training last semester.

B2. The treatment will have an effect different from zero on job search efficacy and effort.

Measures: We will measure (i) whether the individual reports to have engaged in any job search in the past semester, (ii) the ratio between the number of interviews and the number of applications submitted in the past semester, and (iii) the ratio between the number of job offers received and the number of applications submitted.[8] In addition, we will investigate whether individuals accessed formal placement support services. We will create a standardized "job search" index of these outcomes.

To gain further insights into mechanisms, we will gather a few additional data from program participants, which we will analyze in supplementary analysis. One of the primary reasons why we believe the program should affect participants' job search effectiveness is by improving their social network —especially, through connecting them to potential employers. To delve deeper into this channel, we will leverage data from individuals' LinkedIn profiles, which they provide in the baseline questionnaire (see Section 3.1.1). In particular, we will scrape public data from their profile and estimate treatment effects on the number of connections, which we interpret as a measure of social

[8]Treatment effects on (ii) and (iii) may be confounded by selection effects if the treatment increases the proportion of people starting a job search spell. We believe that, if anything, this should bias our results toward zero. We will also evaluate the treatment effects on the number of applications, interviews, and job offers separately, in supplementary analyses. It is important to note, though, that treatment effects on these outcomes may have a different interpretation as they can mechanically reflect longer unemployment spells and/or worse job search skills.

network strength. The program also includes optional soft skills training, so another reason why it could enhance primary outcomes is by cultivating market-valued soft skills among students. We will not measure soft skills directly, but will investigate the relative importance of this component as perceived by the students. More specifically, we will investigate what program benefits students believe were important for their results on the job market (coding skills training, soft skills training, connection with potential employers, network with students, database of job vacancies, job search tips, and food and internet vouchers) and which of these they believe was the most important. We will also investigate the method that employed students used to find their job (including connections made by the bootcamp). Finally, we will investigate reasons for unemployment and turning down jobs.

2.4.3 Secondary outcomes

While the most direct effects of the program are on employment and income, we anticipate it will also affect other important life outcomes. Specifically, the technology sector is known for offering appealing amenities such as improved work arrangements and job flexibility. Moreover, the intervention we study aims to help individuals gain employment in formal jobs, which inherently comes with numerous benefits. Hence, we expect the program to result in increased job benefits and satisfaction. We believe that the program's benefits can extend to improvements in other dimensions of well-being, including mental health, subjective well-being, and optimism about future prospects. In addition, the program can foster a person's interest in pursuing tertiary or college education on a computer science-related field. We also posit that studying coding in a supportive environment where nearly half of the students are women can reduce the perception about the ICT industry not being appropriate for women to work in. Finally, we expect the program to increase one's decision-making power through increased access to economic resources and opportunities.

C1. The treatment will have an effect different from zero on job benefits and satisfaction.

Measures: We will measure whether the individual's job offers (i) flexible hours arrangement, (ii) work-from-home arrangements, (iii) paid time off, (iv) health care plan, (v) meal vouchers, and (v) retirement plan. We will create an index based on these questions. We will also measure job satisfaction on a likert scale (1–10). To further understand job satisfaction, we will ask individuals about the prevalence of potential stressors in the workplace (including long work hours and intensive time pressure).

C2. The treatment will have an effect different from zero on general well-being

Measures: We will investigate different aspects of well-being: (i) mental health, using the five-item Mental Health Index (MHI-5) (Damásio et al., 2014), (ii) subjective well-being, using a 10-step Cantril ladder, and (iii) optimism about the future, measured by survey questions on expected wealth, probability of having the desired job, living in a better neighborhood, and that children will have a better life (0–10 scale).

C3. The treatment will have an effect different from zero on tech studying.

Measures: Through survey information, we will create a binary outcome that takes value 1 if the individual is studying a computer science related degree (Information Technology, Data Science, Computer Science, Computer Engineering, and Software Engineering), 0 otherwise. This will be measured approximately one semester after the completion of training. One concern is that if the impact exists, it may take more time to manifest. Hence, we will also get information on the individual's expectation of having started a college degree in tech five years ahead (1–10 scale). We will also collect descriptive data on the reasons why program participants believe the intervention could contribute to their college enrollment.

C4. The treatment will have an effect different from zero on gender stereotypes in the ICT domain.

Measures: This is an index based on participants' perceptions regarding the suitability of the coding profession for women, their views on whether the sector carries a masculine connotation, and whether the successful programmer they envision is female.

C5. The treatment will have an effect different from zero on decision-making power.

Measures: We will consider an index based on individuals' answer to questions on who in their household makes decisions regarding expenditures on food, the purchase of large items (such as refrigerators and TVs), the time they spend socializing outside the house, how the money they earn is spent, and their educational/training pursuits.

3 Data

3.1 Data collection

Here we discuss our key data sources and the data collection design. The proposed schedule of this project is available in the Appendix Section B . In particular, follow-up data collection is planned to start in July 2024.

3.1.1 Baseline data

Baseline data will be collected through online questionnaires administered by the implementer as part of their screening process, approximately one month before the intervention (for each cohort). These questionnaires include socio-demographic characteristics of the applicants, such as age, race, gender, family income, and educational attainment. Additionally, the questionnaires capture an individual's taxpayer identification number (referred to as *Cadastro da Pessoa Física* or CPF), as well as contact details including their cell phone number (along with those of two close friends/family members), email address, and social media accounts. Furthermore, we will gather the participants' scores from online exams assessing logical reasoning and reading skills, as well as scores from an interview evaluating their fit with technology.[9] We interpret one's test scores in the logical reasoning test and fit with technology as proxies for coding readiness. Finally, we will collect information on whether individuals have friends participating in the program's screening process and who they are.

[9]In this interview, individuals are asked about general knowledge in programming and past experiences in the field. Based on their answers, students are assigned a score ranging from 1 to 4.

3.1.2 Program implementation data

A necessary condition for our intervention to positively affect outcomes, as described in Section 2.4, is that students access the program components. One might be worried that such compliance might not be as high since the intervention is entirely online. Therefore, we will collect administrative data from the implementer on several dimensions of program engagement. In particular, we will present descriptives on the number of classes students watch, number of coding projects students complete, proportion of students who receive job referrals, and the proportion of referrals that resulted in a job matching. Against the previous concern, our data indicate that around 90% of students complete the whole program and 97% of them finish all proposed coding projects. These data may also be relevant to understand mechanisms and potential heterogenous effects across groups of students. Finally, we will collect detailed data on the program's costs to do a cost benefit analysis.

3.1.3 Follow-up data: Survey

To capture the short-term impacts of the program and understand its mechanisms, we have designed a survey that will be administered six months after the end of the training program for each cohort (starting in April 2024). In this follow-up survey, we will capture information on all the outcomes described in the Section 2.4. The bulk of the survey will be conducted by phone and will last approximately 25 minutes. We will complement the phone survey with an online questionnaire to measure programming skills. A fieldwork team will track individuals' contact information from when they are approved in the screening process until the survey. In particular, they will monitor individuals' WhatsApp to check for changes in their cell phone numbers. They will also engage study participants through communication materials designed and delivered by them (through social media and WhatsApp). Moreover, they will follow a field protocol designed to maximize the response rate, including spacing contact attempts evenly and exhausting different days of the week and times of the day. They will use alternative approaches to contact those they cannot reach after exhausting these possibilities, such as social media and family members' phones. A gift card worth BRL 50.00 will be given to all respondents who complete the survey. Individuals must fill out a simple online form to receive the gift card. We will take advantage of this step to add a few questions about coding that require visual elements to the form. These questions are incentivized. We will enroll participants who answer more than half of the test questions correctly into a lottery, where a prize of BRL 200.00 will be drawn. A highly reputable survey firm (Oppen Social) will be responsible for all the fieldwork, collection, and construction of the database.

3.1.4 Follow-up data: Administrative records

In addition to using surveys, we will track study participants' formal labor market outcomes using government administrative data merged with our sample using CPF. In particular, we will use *Relação Anual de Informações Sociais* (RAIS), the Brazilian matched employer-employee dataset. Every formal firm in Brazil is obliged to fill out RAIS yearly. Firms must submit the full name and the CPF for every listed worker, their socioeconomic characteristics, and labor earnings. We can use RAIS data to determine whether study participants have a formal job, their wages, job tenure, and occupation. We have access to this dataset from cooperation agreements between Insper and the federal government. We will also use public data about formal entrepreneurial activity in Brazil, collected by the Brazilian

Internal Revenue Service (*Receita Federal do Brasil*, RFB). The dataset contains information about all firms registered in the country. Every firm has a tax identification number and a corresponding legal representative or a set of business partners. For each firm owner or partner listed in the RFB data, there is information about her full name and six intermediary digits of CPF. We can use this data to determine whether study participants have a formal enterprise.

3.2 Variations from the intended sample size and other challenges

3.2.1 Number of eligible students

Based on our partner's plans for the next year and the returns of recruitment investments from past years, we predict 1,500 eligible participants (i.e., in the experimental sample) for 800 slots between August 2023 and September 2024. We plan to run the randomization within this time window. It is possible that, in practice, this number will be lower than predicted. We have already agreed with our partner that if this is the case, we will prolong the experiment until reaching the desired sample. Moreover, the organization has raised extra funds to scale recruitment above normal to ensure the required sample size.

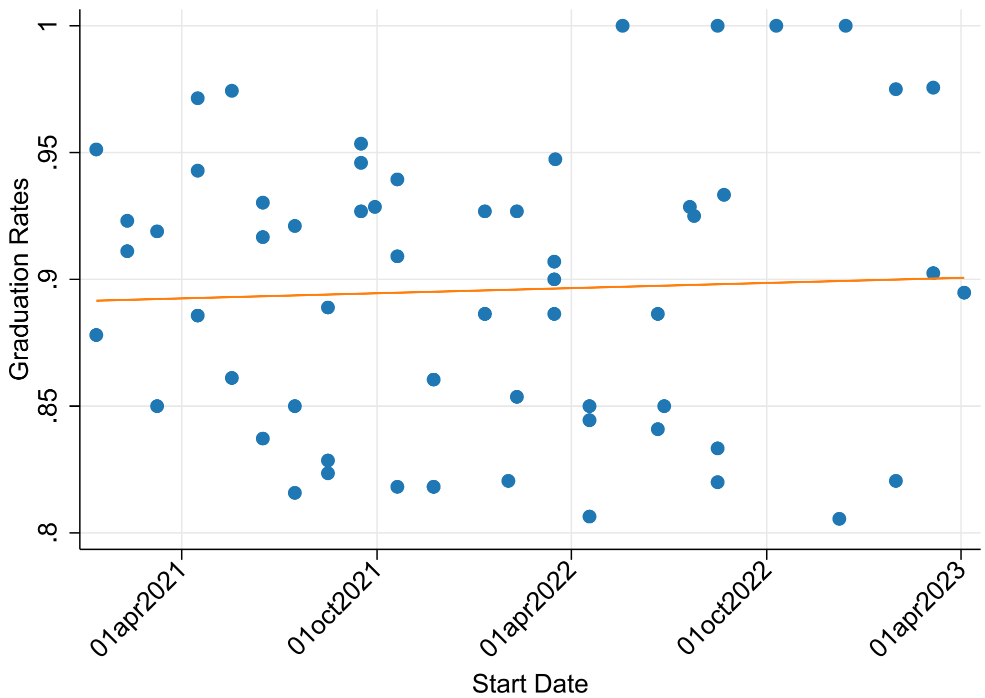
3.2.2 Atrittion

High attrition rates in the follow-up survey may reduce our power significantly. To prevent that, we will hire a high-quality survey firm with experience in cell phone data collection. The fieldwork team hired and trained by them will follow a research protocol designed to minimize attrition (see Section 3.1). Moreover, we will give a gift card to all respondents who complete the survey. Enumerators will be required to log in all contact attempts so we can use this information to correct for potential attrition using Behaghel et al. (2015). We might also report Lee Bounds (Lee, 2009) in case we find differential attrition rates between treated and control groups. Most important, we will also use government administrative records to track the outcomes of study participants in the formal labor market. These records are not subject to attrition.

3.2.3 Partial compliance

The effectiveness of the intervention we are studying hinges on students actually engaging with the program components. During the screening phase, our partner try to select individuals who have available time and motivation to succeed in the course. Even so, they may experience negative shocks that cause them to drop out of the program. This aspect becomes particularly pertinent in our context, where a substantial proportion of students come from disadvantaged backgrounds, and the course demands a significant daily time commitment (8 hours). A notably low compliance rate has the potential to considerably diminish our statistical power. To mitigate this issue, the most vulnerable individuals —most likely to dropout— will receive financial and material incentives to attend the program. As previously discussed, the organization lends them laptops and pay food and internet vouchers worth approximately 100 USD. In addition, the program will provide individuals with psychosocial support delivered by psychologists and social workers. These measures aim to support and motivate individuals to remain engaged in the program and may explain the low drop out rates the NGO has achieved in the past. Indeed, Figure 1 shows that the program's graduation rates have consistently remained high over the past 61 program classes, spanning from 2021 to the present. Specifically, the graduation rate has always been above 80%, and for half of the classes, it exceeds 90%. Importantly, we have not observed a downward trend in this rate during the recent period. Overall, we believe that low take-up may be less of a concern in our setting.

Figure (1) Graduation rates, 2021-2023 (61 classes)



Note: This figure plots the graduation rates of 61 program classes starting from January 2021 until April 2023 (blue dots). The orange line represents a linear fit.

3.2.4 Spillovers

Spillovers are often a concern in research designs with individual-level randomization, particularly when individuals are likely to interact. However, we believe that this issue is not pertinent to our particular context. Our experiment unfolds across four distinct, non-contiguous cities, collectively encompassing over 10% of Brazil's population. The program's reach extends to youth living within a radius of 130 km from the center of these cities, rather than being limited to specific communities. These factors, coupled with the relatively modest scale of the intervention, significantly reduce the probability of substantial interactions between individuals in the treatment and control groups. Furthermore, certain key facets of the program cannot be shared between these groups, such as access to synchronous classes and job referrals. In any case, the baseline questionnaire investigates whether participants have friends involved in the program's screening process, and if so, who they are. With these data, we can determine whether the existence of friendships between participants in the treatment and control groups is a recurring phenomenon. We can also investigate potential spillover effects for individuals in the control group who have friends in the treatment group.

4 Analysis

4.1 Statistical model

Assignment to the bootcamp will be defined randomly among the eligible applicants of each program's cohort. The causal impact of being offered a chance to participate in the bootcamp is thus identified and can be studied by comparing the outcome of eligible applicants randomly selected for the treatment and control groups. The intention to treat (ITT) effects of the program will be estimated based on the regression:

$$Y_i = \tau_{ITT} Z_i + \mathbf{X_i'}\mathbf{\Lambda} + \gamma_s + \varepsilon_i, \tag{1}$$

where Y_i is some outcome of interest (see Section 2.4) of individual i. Z_i is an indicator that takes value 1 if i received an offer to join the coding bootcamp, 0 otherwise. The vector $\mathbf{X_i}$, added to improve the precision of the estimates, includes a constant, age, gender, race, family income, education, experience in the formal labor market, and logical and reading test scores. γ_s represents strata fixed effects, so comparisons are within the same lottery. ε_i stands for the error term.

We will also report the local average treatment effects (LATE) of the bootcamp, estimated using

$$T_i = \beta Z_i + \mathbf{X_i'}\mathbf{\Gamma} + \mu_s + \rho_i, \tag{2}$$

and

$$Y_i = \tau_{LATE} \widehat{T_i} + \mathbf{X_i'}\mathbf{\Phi} + \lambda_s + \epsilon_i, \tag{3}$$

where T_i is an indicator variable that takes value 1 if i enrolls in the course, 0 otherwise. The remaining parameters follow analogous definitions of Equation (1).

4.2 Inference

We will use hetroskedasticity-robust standard errors. When evaluating multiple outcomes capturing a common dimension, we will adjust standard errors for multiple hypothesis testing. In particular, we will report q-values obtained using the sharpened procedure of Benjamini et al. (2006). When presenting the effects on each component of a family of outcomes, we will report both uncorrected p-values and q-values. In some cases, we will also aggregate the outcomes into an index following Anderson (2008). When presenting the effects on such an index, we will only report uncorrected p-values.

The families of outcomes we consider map into our hypothesis and are further discussed in Section 2.4. The first includes all the survey information on the primary results ***A.1*** and ***A.2***. The second includes all the outcomes collected through administrative data to measure similar hypotheses. For both these families, we will aggregate the outcomes into *labor market indexes*. The third and fourth include survey and administrative measures (respectively) to test ***A.3***. The fifth includes two coding skills measures (***B.1***). The sixth includes all the job search outcomes (***B.2***). We will aggregate these into a *job search index*. The seventh includes multiple outcomes on job benefits and one about job satisfaction (***C.1***). We will create an index based on the job benefits measures. The eighth includes multiple outcomes on different dimensions of well-being (***C.2***). We will aggregate them into a *well-being index*. The ninth includes two outcomes related to studying tech in a tertiary or college level (***C.3***). The tenth includes different measures capturing gender stereotypes in the tech sector (***C.4***). We will aggregate them into an index. The final family includes several outcomes indicating one's decision-making power within the household, which we will aggregate into an index (***C.5***).

4.3 Balance tests

To investigate the validity of our research design, we will test whether the randomized assignment mechanism generated statistically comparable groups of individuals at baseline. In particular, we will test for adjusted differences between control and treatment groups by reporting the estimates from a regression for each baseline covariate on the treatment assignment indicator (Z_i) and strata fixed effects alongside robust standard errors. Additionally, we will present p-values from a joint test that all covariates are balanced across treatment arms following Young (2019). The set of covariates we will test for balance include (i) sociodemographics gathered from a baseline questionnaire —age, race, gender, family income, and educational attainment—, (ii) scores on the screening tests —logic, reading, and fit with tech—, and (iii) experience in the formal labor market gathered from administrative records.

4.4 Heterogeneous effects

We will consider heterogeneity with respect to the following dimensions.

Basic skills. A common feature of coding bootcamps (and sectoral employment programs, more generally) is upfront screening for basic skills. A crucial question for policy is the extent to which these programs can be effective if expanded to cover a broader population of young job seekers by weakening the upfront screening criteria. To shed light on this dimension of external validity, we will consider heterogeneous treatment responses with regard to logical thinking and reading skills. In particular, we will present the treatment effects for individuals with above- and below-the-median scores in the logic and reading tests.

Gender. We are particularly interested in understanding the effects of our intervention for women. The core idea of our program is to help workers gain employment at ICT jobs through improvements in employment-related skills and provision of intermediary services to break down placement barriers. These benefits might be particularly important for women, who are underrepresented in the sector and may face additional barriers in transitioning into tech jobs. Moreover, the returns of thriving in ICT may be particularly strong for women, who could otherwise be trapped in the wrong sectors. Past research in Uganda has shown that women who cross over into male-dominated industries have remarkably high gains relative to those remaining in female-dominated industries (Campos et al., 2015). Finally, reinforcing our hypothesis, vocational training programs providing wraparound services have been shown to be particularly effective for women (J-PAL, 2022).

Race. The Brazilian labor market is marked by major racial disparities. People of color face higher unemployment rates and are underrepresented in high-paying jobs. For these, our program's benefits may have particularly high returns. We will estimate the program's effect for two groups: (i) white and Asian individuals, and (ii) black, mixed-race (*Pardo*), and indigenous people.

5 Interpreting Results

5.1 Primary results

Our main interpretation of positive treatment effects on employment, particularly in the ICT sector, is that the program assists individuals in overcoming barriers to acquiring coding skills that are highly valued by the market, as well as barriers to successful job search and matching (such as limited access to job referral network). We will further investigate these channels when assessing mechanisms.

It is possible that the program positively impacts the probability of finding a job in tech through the channels mentioned above, but does not affect total employment. Our interpretation of this outcome is that even though the program helps individuals address obstacles to thriving in the ICT industry, the level of labor demand in this market may not be sufficiently high, and some treated individuals wait for a tech job instead of migrating to jobs they would be just as capable of achieving as the less skilled people in the control group (Atkin et al., 2021). Such an explanation is particularly relevant in our context given the large informal sector in Brazil, which acts as an employment buffer for low-skill job seekers (Ulyssea and Ponczek, 2023). We can investigate these possibilities by exploring potential heterogeneity across cohorts in the conditions of the ICT labor market graduates face (job creation rates) and by investigating whether some treated individuals disperse to non-tech jobs over the unemployment spell (using more granular data from RAIS). We will also investigate reasons for unemployment and turning down jobs.

A more distant outcome to consider is that the program has no effect on employability in the tech sector (having zero or even negative effects on total employment). One potential interpretation of this outcome is that the intervention fails to effectively address the barriers that hinder the target population's progress in the tech labor market. For example, the training provided may not align with the specific needs of employers. Another related explanation is that participants do not fully engage with the program. However, we find these explanations less likely based on the baseline data, which indicates that the majority of graduates secure employment within six months of completing the training, and there is high engagement with the program. Our primary interpretation of this outcome is that people in the control group use alternative programs for coding training and employment services. To gain further insights into this matter, we will measure treatment effects on access to these programs. This will also be key to interpret the results on mechanisms, as discussed soon.

We interpret positive effects on earnings as a result of increased employment rates and/or hourly wages (through employment in highly paid jobs). Although it is challenging to disentangle these two explanations, examining the magnitude of the treatment effects on earnings, employment, and employment in higher-wage jobs can provide insights into their relative importance. For instance, if we observe only modest effects on overall employment but substantial effects on high-wage employment and earnings, it suggests that the earning gains are primarily attributed to a higher proportion of program participants securing well-paid jobs (Katz et al., 2022). Conversely, if we do not find any effects on earnings, our interpretation is that ICT jobs offer no substantial advantage over the employment opportunities individuals would have obtained regardless of their participation in the program. To shed further light on this, we will present descriptive evidence on the characteristics of jobs in the tech and non-tech sectors.

5.2 Mechanisms

Finding positive effects on coding skills indicates that the program successfully equipped participants with the necessary skills for employment in the technology sector. Our interpretation if these results are not found is that people in the control group may have acquired similar skills through alternative courses. We interpret positive treatment effects on job search outcomes as a result of the program's job placement services. Specifically, by connecting students to a greater number of job opportunities and providing them with search tips, the program should increase their job offer arrival rate, encouraging job search (McCall, 1970). These services, particularly job referrals, should also enhance the effectiveness of job search efforts. If we do not find such effects, one possible interpretation is that individuals in the control group also receive job search support through other programs. Another interpretation arises from treatment changing the firms individuals direct their search towards (Bandiera et al., 2021). In particular, the estimated impact on job search effectiveness may be mitigated if matching with tech firms naturally poses greater challenges. For this reason, we will also explore the extent to which individuals value the job placement services.[10]

5.3 Secondary results

By law, formal firms in Brazil must provide workers with a range of benefits. In addition, tech jobs are known for their positive amenities, particularly concerning work arrangements and job flexibility (Aramburu et al., 2021). Therefore, we interpret the results on job benefits in light of the program's effects on formal and tech employment. The effects on job satisfaction should largely reflect the impacts on job benefits and earnings. Our interpretation if the impacts on job satisfaction go in other direction is the existence of other job characteristics that individuals weight negatively. In particular, work demands that may be experienced as excessive by the target population (long work hours, intensive time pressure, task requiring complex decision-making, etc.). We can examine these factors in our data. We can also investigate whether individuals who were initially employed in tech jobs transition to non-tech positions over time.

We interpret the results on well-being as a direct consequence of the program's effects on economic outcomes (Ridley et al., 2020), as well as job amenities and satisfaction (Joyce et al., 2010). We interpret positive effects on college enrollment (or willingness to enroll) as the result of enhanced skills and self-confidence gained through completing the bootcamp. It can also result from access to economic resources to finance education. We can investigate these in our data. Our interpretation in case we find no effects is that participants do not perceive the need to pursue additional education to achieve their employment goals, particularly if the program facilitates access to high-quality jobs (World Bank, 2017). We also interpret the results on decision-making power as a consequence of the program's effects on access to economic resources and opportunities. Finally, we discuss how we interpret the results on participants' perceptions of the ICT sector not being appropriate for women to work. More generally, the program can challenge and reduce such stereotypes by exposing participants to a gender-diverse tech environment, wherein women have a high probability of success. Moreover, the program should equip women with the tools needed to excel in tech, enhancing their confidence and fostering a greater sense of belonging in this field.

[10] Other interpretation is that the job search services make individuals hold more accurate perceptions over their labor market prospects, which could mean reducing the expected job offer arrival rate –given the evidence that job seekers are generally overly optimistic about the job offer arrival rate (e.g. Bandiera et al., 2021; Banerjee and Sequeira, 2020)– and, hence, effort.

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A Partnership Details

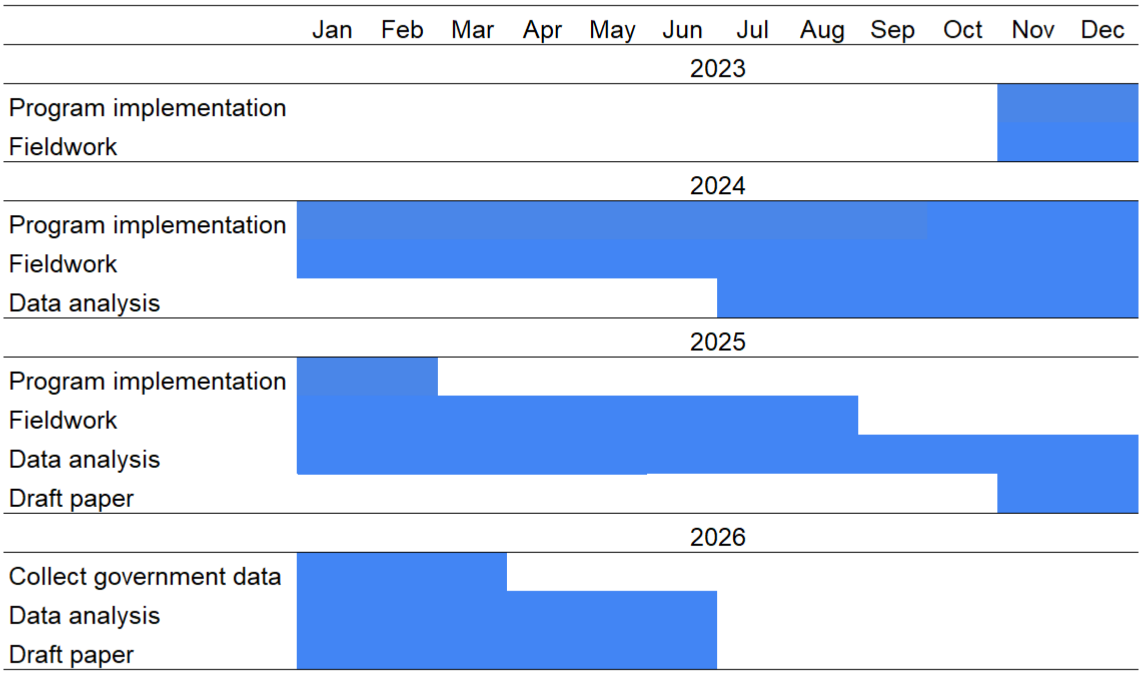
The history of this partnership began in early 2022 after members of the implementer participated in a workshop about impact evaluation organized by J-PAL, where we taught classes. Since then, we have worked closely with them to design an RCT to evaluate their innovative training model. We held multiple meetings with various teams within the organization to gain a comprehensive understanding of the intervention. Furthermore, we provided them with technical guidance on several aspects of the evaluation and worked closely with their Recruitment and Evaluation teams to define the research design. We analyzed their administrative data during this process to determine the most effective randomization design and run power calculations. We have also signed a confidentiality and non-disclosure agreement to access their identified data and formalize the possibility of writing a paper based on the data, regardless of the program's results. Finally, the implementer is also investing extra resources to guarantee the sample size of the RCT. Thus, our partnership is collaborative, formal, and well-established.

B Proposed Timeline

Below we present the schedule of this research. Here is a description of the items cited in the schedule:

- Program implementation: We plan to start the RCT in October 2023. It starts with randomizing the eligible participants from the first RCT cohort to the program and is followed by three months of training (starting in November 2023). Such a process repeats monthly for each RCT cohort.
- Fieldwork: Fieldwork starts with program implementation and continues until 6 months after the end of the training of the last cohort. It includes:
  - Tracking study participants
  - Piloting the survey
  - Training enumerators
  - Collecting data 6 months after the end of the training of each cohort. It is expected to start in July 2024.
  - Quality control of data
  - Preparing the database
- Data analysis: As soon as the survey database from the first cohort is completed (August 2024), we will start analyzing it. This will extend until close to the end of the project.
- Collect government data: We expect to collect data on the formal labor market from government records in the beginning of 2026.
- Draft paper: After analyzing the datasets and evaluating the impact of the intervention, we will write a working paper.

Research’s schedule



Administrative information

Funding. The primary data collection will be funded by the Jobs and Opportunity Initiative Brazil from J-PAL Latin America and the Caribbean (Grant N. 300275).

Institutional Review Board (ethics approval). This project was approved by Insper's IRB (Opinion N. 284/2023).

Declaration of interest. The authors declare that they have no relevant or material financial interests that relate to the research described in this plan.

Acknowledgments. We would like to thank J-PAL LAC staff, particularly the Jobs and Opportunity Initiative Brazil team, for supporting the development of the research partnership with the implementer and for funding.

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