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Technical Education, Noncognitive Skills and Labor Market Outcomes: Experimental Evidence from Brazil*

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Abstract

This paper describes the results from the evaluation of the Student Training Scholarship ("Bolsa Formação Estudante"), a public policy that offers scholarships to current and former high school students of the public educational system in Brazil so that they can attend technical and vocational education courses free of charge. We base our analysis on a waiting list randomized controlled trial in four municipalities and use survey and administrative data to quantify the effects of the program on educational investments, labor market outcomes, noncognitive skills and self-reported risky behaviors. Our intention-to-treat estimates suggest substantial gender heterogeneity two years after program completion. Women experienced large gains in labor market outcomes and noncognitive skills. In particular, those who received the offer scored 0.63σ higher on an extraversion indicator, but, surprisingly, reported more frequently that they were involved in argument or fights and binge drinking. We find no effects of the program on the male sub-sample. The findings corroborate the evidence on gender heterogeneity in the literature on technical and vocational education programs, and also extend it to additional dimensions.

JEL Codes: C21, I38, J24
Keywords: Vocational Education; Human Capital; Noncognitive Skills; Labor Market Outcomes; Risky Behavior.

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1 Introduction

Low skill levels are usually seen as a central policy concern affecting youth employment in developing countries. The skills identified by employers for which there is unmet demand go beyond occupational-specific skills, also encompassing a broader set of life "soft" skills (Fazio, 2011; Kuhn and Weinberger, 2005; FGV/CLEAR, 2017). Investing in skills development is often seen as a policy option that acts upon filling in the skills gaps, smoothing the school to work transition for vulnerable youth. Vocational educational (VET) programs are one appealing alternative that has been proven effective to promote youth employment (Attanasio et al., 2011; Kugler et al., 2015; Attanasio et al., 2017; Card et al., 2011). One potential mechanism through which this could have been accomplished is through building the necessary skills and mitigating the mismatches between the abilities developed in regular school and those demanded by the labor market.

While VET programs are traditionally designed to develop job-related skills to specific occupations, through bringing the student closer to the reality of the labor market, these programs may also potentially impact other general skills, such as the so-called "soft" skills (Carneiro et al., 2010). Also, some of these programs combine occupational in-classroom training with components oriented at developing skills such as communication capacity, perseverance, self-control, responsibility, cooperation and job-readiness. Acquiring these skills becomes particularly relevant in the context of technological change, in which occupational-specific skills may become obsolete too quickly. Although specific skills may facilitate an early transition into the labor market, Hanushek et al. (2017) shows evidence that general human capital skills are more likely to bring benefits at later stages in life. The empirical evidence on whether training programs targeting youth can affect noncognitive and general life "soft" skill formation, either by explicitly teaching and exercising these skills or by providing an environment where they can be developed, is relatively scant.

Fostering successful job trajectories for vulnerable youth has been an increasingly important policy issue in developing countries, not only because of youth's welfare but also because vulnerable youth is associated with criminal and risky behaviors, which has adverse effects on the entire society. The development of youth's skills is expected to be associated with better labor market prospects and, consequently, with an increase in risk aversion and lower probability of involvement in criminal activities (Deming, 2011; Machin et al., 2011). Different policy alternatives focused at widening choices and opportunities for youth or remedying and compensating their vulnerabilities have suggested that these associations might occur (Chioda et al., 2016; Heller et al., 2017), but these links are

relatively under-explored in the VET literature.

This paper addresses these questions in the context of a VET program targeted at current and former high school students of the public educational system in Brazil. We describe the results from the evaluation of the Student Training Scholarship, a policy under a large public program — the National Program for Access to Technical Education and Employment (Pronatec Program)— that offers scholarships so that eligible youth can attend VET courses free of charge. We base our analysis on a waiting list randomized controlled trial in four mid-sized municipalities in Santa Catarina State that arose in the subscription process to scholarships for courses at two of the main providers in Brazil. Faced with excess demand, these institutions chose to select their candidates through a randomization criteria in 2012, 2013 and 2014. These scholarships allowed interested eligible individuals enrolled in high school or high school graduates to attend to two years or 1,200 hours of in-classroom training on a variety of occupations, including mechanics, workplace safety, information technology, among others. The development of "soft" skills— such as job readiness, organization and the ability to work in teams — was among the programs' goals, although there were no explicit modules aimed at developing them.

The main purpose of the program is to generate better opportunities for beneficiaries in the labor market. We describe the causal effect of assignment on employment, job formality and earnings and find that men and women were affected very differently by the offer. The impacts on women are both large and strongly significant. Employment rose by 19 percentage points (p.p.s), formal employment rose by more than 30 p.p.s and work earnings among employed rose by roughly 20%. Considering the unconditional distribution of work earnings, we estimate an impact of approximately 400 Brazilian Reais or almost 50% of the no offer group mean. Point estimates for men are small in magnitude when positive, and frequently are negative, but always statistically indistinguishable from zero. Administrative data on formal workers from the Brazilian Ministry of Employment and Labor's Annual Social Information Report (RAIS) corroborates these findings and provides further evidence of the large magnitude of the impact on earnings.

The difference in effects between men and women we observe on labor market outcomes is also found in most of the standardized indicators of noncognitive skills. In particular, the extraversion index rose sharply and significantly for women, by 0.63σ. Once again, estimates for women tend to be large in all dimensions we look at, and effects on men are not only small but frequently wrong-sided. For instance, women assigned to treatment had 0.29σ and 0.27σ higher indicators of agreeableness and neuroticism while the estimates for men are -0.002σ and -0.017σ. All in all, this suggests that noncognitive skills in women may be malleable to the investments channeled through the program in

dimensions that could play an important role in explaining the results found in the labor market. Surprisingly, we find adverse effects of the offer assignment on self-reported risky behaviors for women, in particular engagement in argument or fight and binge drinking in the survey reference period. We speculate that the large gains on income and on the extraversion dimension — which encompasses self-confidence — could be associated with an increase in women's bargaining power for control over their lives, which in turn led to interpersonal conflicts and to more autonomy in leisure choices.

The gender heterogeneity we find on labor market outcomes has been pointed out in systematic reviews about technical and vocational education in developing countries (Blattman and Ralston, 2015) and other active labor market policies in general (Bergemann and Van den Berg, 2008; Card et al., 2010, 2015; Caliendo and Künn, 2015)[1]. We find strong evidence of heterogeneous effects in other dimensions, namely noncognitive skills and risky behaviors. Even though we are not able to gauge the specific mechanisms that underlie our results, we hypothesize that important traits related to sociability, organization and responsibility could have played a big role in explaining the results observed in the labor market dimension.

Our analysis comes with some caveats related to the external validity of our findings. First, our experiment opportunity is from four mid-sized municipalities in Brazil, and the public policy we evaluate is a national one. In this sense, the setting we analyze could be very specific and differ from other locations in ways that may limit our ability to generalize the results[2]. Arguing against this, one could note that, although training courses are administered locally, quality is regulated by a set of federal guidelines. Also, the courses offered through the program are always provided by the same institutions in Brazil, which are part of a nationwide network and follow similar standards and overall curricula for provision across the country. Second, our analysis is limited to various short- or mid-term outcomes, averaging two years after program completion[3]. Following samples for a longer time to track whether the gains observed in noncognitive skills can be persistent provides an interesting avenue for future research. Finally, our sample sizes are small, especially when compared to the most recent literature on VET. The

[1] To our knowledge, Alzúa et al. (2013) describe the only intervention where gender differences point to another direction, documenting that the Argentinian program "entra21" in Córdoba had positive and significant impacts on formal employment and earnings of men approximately 2 years after course completion, but not for women.

[2] In particular, it has been argued by previous non-experimental evaluations (Assunção and Gonzaga, 2015) that the impacts of the program and technical education in general vary according to region.

[3] Few studies provide impact estimates for the period beyond two or three years after completion of training, but this shortcoming has been recently addressed in the case of the Colombian program "Jóvenes en Acción" (Attanasio et al., 2017). Their results using administrative data suggest that the effects of the program are persistent.

fact that randomization inference p-values are not different from the ones that rely on usual inference procedures nevertheless indicates that large-sample approximations are not relevant for the main qualitative results.

The remainder of the paper is divided as follows. Section 2 provides background information on the public school system in Brazil and on the design and implementation of the program. Section 3 describes the data (lottery, survey and administrative), the experimental design and the empirical strategy. Section 4 presents the intention-to-treat estimates of the offer on educational investments, labor market outcomes, noncognitive skills and self-reported risky behaviors. Section 5 concludes with a discussion of our findings. Appendix A includes a detailed description of how the outcome variables were constructed.[4]

2 Background and Program

Basic education in Brazil's formal educational system after preschool is divided in preprimary (4-5 years old), primary (6-10 years old), lower secondary (11-14 old) and upper secondary or high school (15-17 years old)[5]. All educational levels can be completed either in private or in public institutions and one of the main features of the Brazilian school market is its segmentation in terms of income and quality indicators. Students with lower family income are predominantly in public schools, which accounted for 86-90% of the enrollments in secondary school from 2000 to 2015. For instance, in 2014, only 2%, 3% and 4% of high school students in families in the first, second and third income quintile were in private schools, respectively (Almeida et al., 2017). Also, repetition and evasion rates are substantially higher in public schools (Costa, 2013) and students therein perform considerably worse than private high school students in national standardized tests that are used for admission at the tertiary level (26% worse in the National Secondary Education Exam, "Exame Nacional do Ensino Médio", in 2005, cf. Camargo et al., 2017). In this sense, policies targeted at public school students in Brazil are usually based on the principle that they approach a large share of vulnerable and unskilled adolescents and young adults in the population.

It is mandatory to complete basic education in Brazil. Thus, all individuals are

[4] In previous versions of this draft, we also analyzed the results for a sub-sample of individuals aged 36 or below. We eliminated 63 subscribers who were above this age threshold at the time of subscription to concentrate on impacts on individuals who takes the decision to enroll as an investment in human capital in early stages of the life cycle. The results are qualitatively similar to the main sample ones and are available upon request.

[5] After completing basic education, one can enroll in tertiary education which consists of undergraduate, master, doctoral and postdoctoral programs.

required to complete regular high school degree. Secondary technical education must be obtained over and above regular high school. The share of students enrolled in technical and vocational education programs during high school is relatively low in Brazil, as compared to other countries (at 14%, compared to 33% in Chile and 60% in Italy, cf. Almeida et al., 2015). As a result of the federal government's decision to massively scale up policies to increase the offer of professional training both for students of and individuals that had left the formal public educational system, the National Program for Access to Technical Education and Employment ("Programa Nacional de Acesso ao Ensino Técnico e Emprego", PRONATEC) was instituted in 2011, in collaboration with the Ministry of Education (MEC). The program's main goal is to expand the supply of training — specifically to non-metropolitan, non-central areas of the country — and it provided an umbrella for several overlapping policies that were already in place across the country (Almeida et al., 2015). It explicitly targets current or former students of public schools and beneficiaries of other federal transfer programs, addressing quality gaps in the public educational system through its integration with training[6]. In part as a result of PRONATEC, investments in technical and vocational education programs and training increased from 0.04 to 0.2% of the GDP and enrollments have also increased sharply, around 89% between 2007 and 2013 and, in 2014, year in which the program reached its maximum number of enrollments, PRONATEC offered 646 short qualification courses and 227 technical courses across 4,300 municipalities in Brazil (out of approximately 5,500, cf. Amorim et al., 2015).

Through PRONATEC, public and private providers offer course tracks for different age profiles and with different duration, which can be separated into three groups. Figure 1 illustrates the options available to potential beneficiaries. First, initial and continuing training courses [*formação inicial e continuada*, FIC], which are short-term and provide training for a predefined occupation, with duration of 160 to 400 hours[7]. These courses do not count toward the completion of a formal general education level, and thus they do not qualify students to take courses at the next level. Second, after completing the first year of high school, one can enroll in technical education [*ensino técnico*], which is offered in three modalities based on the sequence of general and technical education within the course track and the registration for each: (i) concomitant [*concomitante*], by

[6] In the law that created PRONATEC, one can read that one of the goals of the program is to "contribute to the improvement of high school education through the articulation with technical and vocational education and training" (Lei 12.513/2011).

[7] Available FIC courses are organized in conformity to the Brazilian Classification of Occupations ("Classificação Brasileira de Ocupações", CBO). A full list of courses offered can be found in http://portal.mec.gov.br/index.php?option=comdocmanview=downloadalias=41261-guia-pronatec-de-cursos-fic-2016-pdfItemid=30192.

which general education is taken independently of the technical education components, in an alternative shift and often in a different institution; (ii) integrated [*integrado*], in which the student completes only one program that includes both general and vocational education; (iii) sequential [*subsequente*], which are attended by individuals who have already graduated secondary education, thus being only enrolled in the technical component. The concomitant and sequential modalities are typically completed in two years, divided in four semesters, while the integrated course takes a longer period of four years. Finally, students who completed high school can attend technological education [*cursos tecnológicos*], which have two to three years and are equivalent to a tertiary education diploma.[8]

The focus of this paper is on the Student Training Scholarship ("Bolsa Formação Estudante") program, an initiative under PRONATEC that offers scholarships to students enrolled in the public high school system and to former students of public schools so that they can attend FIC courses or the technical education course tracks free of charge. These targeting guidelines have led MEC to establish partnerships with other ministries (social development, employment, among others) to identify and advise potential students for pre-enrollment. The scholarships are not provided directly by the government to beneficiaries. Through the program, MEC provides financial support to providers in exchange of vacancies to be filled by eligible individuals. The resources cover for all the expenses that would be paid by students to attend, including the amount that would be paid for the course *per se*, the learning materials, food and transportation.

We evaluate the effects of scholarships for the sequential and concomitant modalities in the technical education course track at two of the most important private providers within the so-called S System[9], the National Service of Industrial Training ("Serviço Nacional de Aprendizagem Industrial", SENAI) and the National Service of Commercial Training ("Serviço Nacional de Aprendizagem Comercial", SENAC). These scholarships allowed interested eligibles to attend to two years of in-classroom training, divided in four semesters (or modules) with a course load of 1,200 hours in a variety of subjects, such as mechanics, workplace safety, computer networks, electrotechnology, food technology, and informatics, among others. Unlike other programs targeted at similar beneficiaries, there were no explicit partnerships with firms for a period of on-the-job training. Nonetheless, the reputation and importance of the providers may have played some role in facilitating knowledge about job vacancies and, ultimately, access to firms. The technical courses

[8] In 2007, program enrollments in each of the course tracks as a percentage of total enrollments were 79%, 19% and 2%, respectively (Almeida et al., 2015).

[9] The S System is a group of 10 "autonomous social services", non-profit private entities that exert private activities of public interest within specific economic sectors (industry, commerce, agriculture).

offered in the localities and schools we study provided trainees with practical occupational skills. Although the goals stated in the enrollment forms involved skills that one would consider "soft" — such as job readiness, organization and the ability to work in teams —, there was no explicit modules aimed at developing them.

3 Data and Experimental Design

In the cases in which specific providers face excess demand, they are allowed to choose which selection criteria to apply. They can, for example, select candidates through an entrance exam given to potential students, or use age, low-income, or quotas for students from public schools as a priority criteria. They can also use ordering and randomization criteria[10].

Our study explores events of over-subscription to the program at SENAI and SENAC in four municipalities in the southern State of Santa Catarina: Chapecó, Itapiranga, São Miguel do Oeste and Xanxerê. Enrollments were collected by the Santa Catarina Secretariat of Education and the institutions chose to select their candidates through a randomization criteria in 2012 (Chapecó), 2013 (all municipalities) and 2014 (Chapecó). Thus, for each class (course and time period in a given school) offered, when the number of candidates was bigger than the number of vacancies available, the candidates were randomly selected through a lottery. They offered 34 classes and 29 of them had over-subscription in the period of the analysis. The schools generated a random order of classification of subscribers for each class separately[11]. We had access to provider administrative data with offer assignments, which contained the names of individuals[12] selected at some point after random assignment to enroll and the position on a randomly generated waiting list.

Survey Data The primary dataset was based on the application of a survey from June to August of 2016, using a team of undergraduate students as enumerators, trained and

[10] According to the federal guidelines for PRONATEC management (MEC, 2011, p. 31), providers can call students in the waiting list to fulfill the remaining vacancies, as long as they follow its order of classification.

[11] In general, each class had 35 slots to be filled and the schools kept a list of around twice the class size (around 70 subscribers per class) and followed the order of the list to fill all the open slots. Thus, this is a typical over-subscription waiting list experimental design. It is possible that in some cases more than 70 individuals could have subscribed to a given class. However, information was kept only for the first randomly allocated 70 subscribers. To the extent that this acts as random sampling it should not compromise the main results.

[12] In principle, an individual could subscribe for more than one class and thus increase her chances to be treated. However, in practice, very few of them actually had more than one class subscription.

supervised by the authors[13]. The survey included questions on demographic and socioeconomic characteristics (age, sex, color, education degree, educational level of the parents and household condition), labor market variables and a self reported risky behavior module.

In our analysis of labor market attachment and work earnings, we focus on five outcome variables, which are constructed following the literature on the impacts of technical and vocational education programs (Card et al., 2011; Attanasio et al., 2011; Alzúa et al., 2013; Ibarrarán et al., 2015; Hirshleifer et al., 2016): (i) whether the respondent had a job in the month before the survey; (ii) the lenght of duration in months in the current most important job; (iii) whether the respondent claimed to have a signed work card (formal job); (iv) monthly work earnings in the survey reference month; (v) the last variable in logarithm. All outcomes were set to zero before transformations for respondents who reported being either unemployed or out of the labor force in the survey reference period.

Our measures of risky behavior are: (i) self-reported engagement in an argument or fight in the last four years; (ii) whether the individual claimed using marijuana in the survey reference week; (iii) whether the individual claimed using other drugs in the same period; (iv) binge drinking, which is defined as 5 or more alcoholic drinks for males and 4 or more alcoholic drinks for females on the same drinking occasion; (v) whether the frequency of events where the individual drink and drives is different from "never" or "almost never".

We also applied a nationally validated instrument[14] named SENNA (Social and Emotional or Noncognitive Nationwide Assessment) to measure individuals noncognitive skills. The creators of the instrument, developed by the Ayrton Senna Institute ("Instituto Ayrton Senna", IAS), adopted the method proposed by John and Srivastava (1999) to frame the traits collected by tests within the Big Five framework, which groups personality traits in five basic dimensions: agreeableness, conscientiousness, extraversion, neuroticism and openness to experience. They also added a sixth dimension to these dimensions, locus of control. These are latent constructs obtained by factor analysis performed on the answers to extensive questionnaires with questions about behaviors that are representative of characteristics that an individual could have. The constructs used in our analysis of noncognitive skills and the definitions are:

- *Agreeableness*: "tendency to act in a cooperative and unselfish manner [...] the

[13]The interviews occurred at SENAI's schools in the municipalities of Chapecó and Xanxerê. For the municipalities of Itapiranga and São Miguel do Oeste, due to budget constraints, we made the survey through telephone interviews. Answering the survey was not mandatory, but the majority of candidates who we could contact agreed to participate in the study.

[14]The next paragraphs draw heavily on Santos and Primi (2014).

agreeable or cooperative individual is characterized as being tolerant, altruistic, modest, likeable, flexible and objective (direct when dealing with people)" (Santos and Primi, 2014, p. 20);

- *Conscientiousness*: "tendency to be organized, hard working and responsible [...] the conscientious individual is characterized as being efficient, organized, autonomous, disciplined, lacking impulse and guided towards his objectives (a fighter)"(Santos and Primi, 2014, p. 19);

- *Extraversion*: "orientation of interests and energy towards the external world, people and things (instead of the internal world of subjective experience) [...] the extravert individual is characterized as being friendly, sociable, self-confident, energetic, adventurous and enthusiastic"(Santos and Primi, 2014, p. 20);

- *Neuroticism*: "the predictability and consistence of emotional reactions, without quick mood swings [...] the emotionally unstable individual is characterized as being short-tempered, introspective, impulsive and lacking in self-confidence, with a tendency towards depression and anxiety disorders" (Santos and Primi, 2014, p. 21);

- *Openness to experiences*: "tendency to be open to new aesthetic, cultural and intellectual experiences [...] the individual who is open to new experiences is characterized as being imaginative, artistic, excitable, curious and unconventional, whilst having a wide range of interests" (Santos and Primi, 2014, p. 18);

- *Locus of Control* "how much individuals attribute current experiences either to decisions and attitudes they have taken in the past" (Santos and Primi, 2014, p. 22).

Administrative Data We augment our in-person survey data with administrative data from the Brazilian Ministry of Employment and Labor's Annual Social Information Report ("Relação Anual de Informações Sociais", RAIS), a confidential longitudinal data set of compulsory administrative records reported by every employer in the *formal* market. It omits workers without signed work cards ("carteira assinada"), including interns, the self-employed, elected officials, domestic workers and other smaller categories. This information must be sent each year to the Labor Ministry by all government controlled and private companies operating, and an employer's failure to report can result in fines proportional to the firm size. There are also reasons to believe that workers have an incentive to ensure that their employers are indeed providing the correct information,

since having a signed work card entitle the worker to all social security benefits. The data set compiles records identified by a worker ID number and a firm or establishment number ("Cadastro Nacional da Pessoa Jurídica" CNPJ). Wages are reported as average monthly wage over the months worked in a given year, and include all taxable income and worker payments to Brazilian social security contributions. We constructed five indicators of formal labor market attachment after the matching procedure using the sample of individuals who had an identifier: (i) an indicator of whether the individual could be matched to some record with a positive wage in the same year; (ii) tenure in the establishment or firm where the individual was matched; (iv) establishment or firm size; (v) yearly mean work earnings in the formal labor market; (v) the last variable in logarithm. For workers with multiple employment spells in a given year, we keep the observation with the highest wage.

Empirical Strategy The main challenge in estimating the impact of the scholarships is that enrollment might be correlated with unobservable characteristics (like family background, motivation or grit) that limit the ability to attribute future labor market success, noncognitive development and risky behaviors to participation. Excess demand in the period and municipalities analyzed for many courses provided the possibility for an oversubscription design, which we leverage for causal identification. We use variations of the following specification to compare outcomes of individuals across treatment offer arms:

$$y_{is} = \alpha + \tau_{\text{ITT}} W_i + \mu_s + \epsilon_{is} \tag{1}$$

where y_{is} denotes an outcome for individual i in lottery strata (class) s, W_i is an indicator variable that takes on the value one if individual i received an offer to participate in the program, μ_s are strata fixed-effects and ϵ_{is} is an error term. The intent-to-treat effect captures the effect of being offered the chance to participate in the program and is given by the ordinary least squares estimate of τ_{ITT} in equation (1). In the setting of a stratified randomized experiment, the ordinary least squares estimator $\hat{\tau}^{\text{ols}}_{\text{ITT}}$ is consistent for τ_{ITT}, the weighted average of within-stratum average intention-to-treat parameters (Bruhn and McKenzie, 2009; Imbens and Rubin, 2015).

We present p-values for inference using heteroskedasticity-robust standard errors. Standard errors are not clustered because randomization was conducted at the individual level (Abadie et al., 2017). Nevertheless, a natural question is whether our statistical inferences are sound, given the relatively small number of observations. As an alternative to standard t-tests to determine statistical significance, throughout the discussion of our experimental findings, we also report results from randomization inference per-

mutation tests. These tests are nonparametric in the sense that inference does not rely on distributional assumptions about the error term in a given sample nor on asymptotic approximations as the sample size grows to infinity. In practice, they rely on generating a series of placebo offer assignment vectors that mimic the underlying assigment process and computing the empirical distribution of average intention-to-treatment effects. The p-values are then obtained using the rank of the actual offer assignment vector. Since we rely on individual level randomization within classes, we use 1,000 randomly drawn offer vectors with fixed within-strata probabilities taken from the realized assignment and obtain p-values in the same way.

One can also estimate the impact of actually completing training by replacing the offer variable W_i with a graduation dummy and instrumenting this with treatment assignment. Under the assumptions that the offer has no impact on outcomes for those who do not graduate in the course and there are no individuals who would take courses only if assigned to the control group, the instrumental variables estimator is consistent for the local average treatment effect — the impact of graduating with a scholarship for an individual who takes up training when selected in the course-site lottery, and does not take it up otherwise. In a given sample, the estimate will be an "inflated" version of the intent-to-treat estimate. Table 4 shows that the offer affected the likelihood of graduating by 45 percentage points (std. err.=0.042), and thus, for each outcome, the causal effect of graduating on compliers is approximately two times the effect of the offer on the full sample. In fact, it increased not only the probability to graduate in PRONATEC but also the likelihood to graduate in any secondary technical education by 20 p.p.s.

Samples, Attrition and Balancedness We started with the full set of 29 oversubscribed lists with random orders of classification for $1,435$ individuals. We were able to use valid phone numbers and interview 735 individuals successfully (237 women and 498 men). Thus, we were able to obtain complete set of information on 51% of the applicants. The main source of attrition was outdated contact information, not refusal to participate in the interview. Table 1 shows that there is no significant differences in the attrition rates of treatment and control groups. The dependent variable is an indicator whether the individual in the provider administrative list was not interviewed. We run regression for all subscribers as well as for women and men separately. There is no difference in the attrition rates of treatment and control groups within gender sub-samples either. Additionally, among the original group of subscribers $1,289$ had a valid identification number that allowed us to match them to the administrative data in RAIS.

Tables 2 and 3 suggest that characteristics are balanced across treatment offer arms,

both in the full sample and considering the sub-samples of women and men separately. Both tables present means across offer arms (columns (a)-(b)), p-values (column (c)) from a test of difference between means within groups computed using regressions with lottery strata dummies, the percent size of the difference between treatment offer arms normalized by the control group mean (column (d)) and the sample size (column (e)). Some of the differences between treatment and control are large in magnitude but they become statistically insignificant at reasonable confidence levels once we rely only in within-strata variation. Finally, p-values for the test of joint significance are not significant, suggesting that there is no systematic correlation between observables and the offer to participate in PRONATEC.

4 Results

Below, we provide a series of estimates of the impact of the program offer on educational investments in technical and general education, labor market outcomes, noncognitive skills and self-reported risky behaviors. All tables share a similar structure. The results for the full set of surveyed applicants are in Panel A and the results for the sub-groups of female and male surveyed applicants are in Panels B and C, respectively. Most of the variables used in the analysis are self-reported and were collected from June to August of 2016, an average of two years after the end of the program, and the rightmost columns of Table 5 use the administrative data discussed in Section 3. In each panel, estimates are from the intent-to-treat specification (1), i.e., an ordinary least squares regression with course-by-site strata dummies, and standard errors computed using heteroskedasticity-robust standard errors are shown in brackets. Control group offer means, conventional p-values and p-values from randomization inference tests are displayed in the bottom rows.

Educational Investments Individuals that received an offer to participate in the program were 45 p.p.s more likely to graduate at Pronatec at endline. These results are very similar for men and women and are shown in Column (a) of Table 4. Note that there were some leakages as some non-offer group individuals graduated too but with much lower incidence. Indeed, 5.3 percent of women and 10.2 percent of men from the groups that did not receive an offer claimed to have graduated in the program[15]. PRONATEC could have simply cannibalized other technical courses. In order to investigate this possibil-

[15]These are in general individuals that did not get the scholarship but did enroll nonetheless in a technical course paying the tuition themselves.

ity, we also asked the individuals if they have graduated from other secondary technical education and estimated the impact of having a scholarship offer on the probability of an individual to graduate in any (PRONATEC or non-PRONATEC) secondary technical education. The results are presented in Column (b) of Table 4. Of all individuals in the no offer group, 43% did graduate from any technical education (34.4 and 46.7% of women and men, respectively). Nevertheless, the offer increased the probability to graduate from any secondary technical education. The estimated coefficient using the full sample shows that individuals with PRONATEC offer are 23.3 p.p.s more likely to graduate from any technical course and it is statistically significant different from zero. The effects are 20.3 p.p.s and 23.8 p.p.s for women and men, respectively. Taken all together, these results suggest that the program increased the overall likelihood to obtain a secondary technical education degree in more than 50% and thus seemed to have relaxed some choice constraints faced by eligible individuals who expressed interest in participating.

We also investigate the impact of the scholarship program on two margins of human capital investments in general education. First, the Brazilian education system requires all individuals to have a general education high school degree. Even those pursuing secondary technical education — for instance, through the Student Training Scholarship in a PRONATEC course — must comply with this norm, at the cost of not receiving a formal secondary technical education degree. Thus, high school graduation acts as a conditionality for obtaining a secondary technical education degree and could reinforce the compliance with the compulsory education standard. We estimate the actual size of its effect. Column (c) in Table 4 presents intention-to-treat effects on the probability of completing regular secondary education or high school among individuals that were in high school when applied for the program — and thus were only offered a chance to participate in the concomitant modality. We find that those offered an opportunity to enroll were 15 p.p.s (st.err.= 0.068) more likely to have completed secondary, a 19% increase over what would be expected in the absence of the offer. This result is driven primarily by the sample of men, who are 21.5 p.p.s (st.err.= 0.091) more likely to have a high school diploma at endline. Among women, there is already a high incidence of regular high school completion (89%). Perhaps for this reason, the effect in this sub-sample is positive (2.5 p.p.s) but not statistically significant different from zero.

Second, there is another latent incentive associated with holding a secondary technical education degree, since PRONATEC could induce individuals not to seek post-secondary general education. Column (d) in Table 4 considers whether applicants that received the offer were more or less likely to be enrolled or have completed college at endline. The estimates are small in magnitude, but imprecisely estimated: taking the

full sample (Panel A) as an example, a 95% confidence interval covers values that are empirically relevant, from approximately −15 to 10 p.p. over a counterfactual probability of approximately 40%.

Labor Market Panel A in Table 5 presents evidence on the impact of the program on various measures of labor market attachment and job quality considering the full sample of applicants. Column (a) shows that individuals offered training were 4.3 p.p.s (st.err.=0.042) more likely to be employed at the time of the survey, but this difference is statistically insignificant (p-value= 0.303). Lottery winners were also 5.2 p.p.s more likely to have a formal employment (i.e., to have a signed work card [*carteira assinada*]), but these effects is also imprecisely estimated in the full sample (st.err.=0.051, p-value= 0.320, for formality). Columns (d) and (e) suggest that the impacts on work earnings were similar: self-reported work earnings increased by roughly 100 Reais or, considering the conditional distribution, 4.2 log points. In both cases, we are also unable to reject the null hypothesis of zero intent-to-treat parameters.

These results hide significant gender heterogeneity, as can be seen by comparing Panels B and C in Table 5. Panel B suggests that women offered training were 19.2 p.p.s (st. err.= 0.090) or 25% more likely to be employed at the time of the endline survey, as can be seen in column (a). Column (b) shows that women who won the lottery had also been for roughly 10 months longer in their current most important job, but this coefficient is imprecisely estimated. There is also a highly significant and large impact of the program on the probability of working in the formal sector, as opposed to either not working at all or working in the informal sector, suggesting that much of the gain in employment was into formal jobs. More precisely, column (c) shows that women offered training were 32.4 p.p.s (st. err. = 0.097) more likely to be formally employed, a large difference that amounts to approximately 70% of the mean of the group not offered training. Also, work earnings rose substantially because of the program, by roughly 420 Reais or almost 50%, or 18 log points or 20. These results are even more surprising if one notes that most of the coefficients for the sub-sample of men displayed in Panel C are imprecisely estimated, but often negative. In fact, at least with respect to formality, men that were randomly offered a chance to enroll seem fare worse and the result is marginally significant when considering randomization inference p-values.

The results based on the administrative data matched to the full sample of applicants displayed on the rightmost columns of Table 5 largely confirm these conclusions. The larger number of observations reflects the fact that some of the applicants could not be contacted for the in-person or telephone interviews, mostly because their contacts in the

provider data were outdated. Column (f) describes how the probability of being matched in the administrative data and having a positive wage varies with the offer assignment. We find that the likelihood of such a match is insignificant in the full sample. Once again, we find that the coefficient for women is positive and large (16.2 p.p.s, st. err. = 0.066) and insignificant and negative for men. Our confidence interval for the latter rules out an impact of more than 5%. Firm tenure was not significantly responsive to the offer, a fact that also confirms what was found in the survey data. Also, columns (i) and (j) qualitatively corroborate the fact that work earnings were highly responsive to the scholarships in the sub-sample of women. In particular, inputting zero earnings for those that were not successfully matched provides further evidence of the large magnitude of the impact on earnings. Different from Attanasio et al. (2017), we do not find that training was associated with individuals working in firms with larger size.

Noncognitive Skills We now consider estimates of the impact of the scholarship offer on the noncognitive skills indicators discussed in Section 3. All variables have been standardized with respect to values in the full sample of applicants, so the coefficients can be interpreted as changes in terms of the no offer group standard deviation. Thus, Panels B and C provide some preliminary evidence on how men and women in our sample differ in terms of noncognitive skills in the absence of the intervention. These differences are large in almost every measure we analyse. For instance, women have scores that are, on average, 0.5 and 0.2 standard deviations lower than the no offer group of men in extraversion and agreeableness. In the full sample, there is no strong evidence that the program impacted the measures of agreeableness, neuroticism, openness to experiences and locus of control. Those who were offered the opportunity to participate were nevertheless found to fare better on measures that are intuitively considered important for labor market success, as can be seen in columns (b) and (c): conscientiousness and extraversion. The estimate of effect for the former is only marginally significant at 0.23σ (st.err.= 0.15), and predicts a shift in approximately 10 positions from the median of a normal distribution. Extraversion in the offer group was also 0.18σ (st.err.0.16) higher, but the results are not significant at standard confidence levels (p-value= 0.254).

These results in the full sample hide, once again, substantial gender heterogeneity and were mainly driven by what happened with women following the intervention. As can be seen in column (a), women who won the scholarship lottery scored approximately 0.3 standard deviations higher in the agreeableness indicator, which captures traits such as the ability of being cooperative, flexibility and objectivity when dealing with people. The coefficient is not significant, but impressively large given that men who received an

offer were not affected at all. Columns (c), (d) and (e) show that the same can be said about the extraversion, neuroticism and openness to experiences. In all cases, there is no evidence at all that the program was associated with improvements in any of the indicators for men, while women fared at least 0.2 standard deviations better in all of then. In the case of the extraversion, which describes personality characteristics like sociability and self-confidence, the index rose sharply and significantly for women, by 0.63σ (st. err. $= 0.34$). All in all, this suggests that noncognitive skills in women may be malleable to the investments channeled through the program in dimensions that are intuitively important for labor market success.

Self-reported Criminal and Risky Behaviors We also investigate the effect of the scholarship offers on self-reported risky behavior, summed up by the likelihood of engagement in interpersonal conflicts, the use of drugs and the excessive drinking. There are no effects in any indicators of risky behavior in the full sample, as can be seen in Table 7. However, women have engaged in arguments or fights and reported with higher probability that they had been driving after drinking. More precisely, women in the PRONATEC offer group are 9.9 p.p.s (st.err.=0.049) more likely to have engaged an argument or fight. Compared to the no offer group mean of 2.7% , this represents a threefold increase in the likelihood of arguing or fighting. Additionally, treated women are more likely to binge drinking and drink and drive. Their point estimates are 0.132 (st.err.= 0.066) and 0.122 (st.err.= 0.061), respectively.

5 Conclusion

The ways through which VET programs could affect the trajectories of youth are multiple and are particularly important in low or middle income countries, where those unemployed out-of-school represent a large share of the society (approximately one in every five individuals from 15 to 23 years old in Latin America, cf., Bertrand et al. (2017)). Investing in occupational skills that are demanded in the labor market can improve formal employability and work earnings in a context in which these specific skills are seen by firms as requirements that are not fulfilled in general education schools. VET may also provide the setting where noncognitive skills such as organization, hard-working ability and self-confidence are prone to be developed. Finally, by widening youth's choices and opportunities and hence increasing the opportunity cost of dangerous behaviors, one would expect VET programs to trigger a more conservative behavior towards risk.

We find that these dimensions were differentially affected among men and women in

the setting of a scholarship program that targets current and former high school students of the public educational system and grants them the opportunity to attend VET courses free of charge. Women experienced large gains in labor market outcomes (employment, formality and earnings) and noncognitive skills (conscientiousness and, particularly, extraversion). We also found that treated women were more likely to engage in arguments and conflicts, to binge drink and reported more frequent drink and driving, which is surprising given our priors on how VET would interact with risky behaviors. The results for men suggest null effects on most of the dimensions we analyze. The heterogeneity we find on labor market outcomes has been pointed out in systematic reviews about technical and vocational education in developing countries (Blattman and Ralston, 2015) and other active labor market policies in general (Bergemann and Van den Berg, 2008; Card et al., 2010, 2015; Caliendo and Künn, 2015). We bring novel information of heterogeneous effects on risky behaviors and "soft" skills that are arguably valued in the labor market.

These results are subject to a number of interpretations that highlight the importance of future work in this area. First, it may be the case that VET impacts both labor market outcomes and noncognitive skills simultaneously and directly. Second, it may be that the program had an impact on noncognitive skills which, in turn, affected labor market prospects. Another hypothesis is that labor market outcomes were altered first, which led to the subsequent development of noncognitive skills. The rise in employment and income for women, if translated into greater woman's bargaining power for control over their lives, may have been the channel through which our measures of risky behavior were influenced. Another potential explanation for this result would be that enhanced self esteem for women impacted empowerment and hence triggered stronger positions in arguments and conflicts. The mechanisms behind the associations we find are unclear and additional research could shed light on the interactions among the outcome dimensions.

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Figures and Tables

Figure 1: Types of Courses Tracks Offered Through PRONATEC (Amorim et al., 2015)

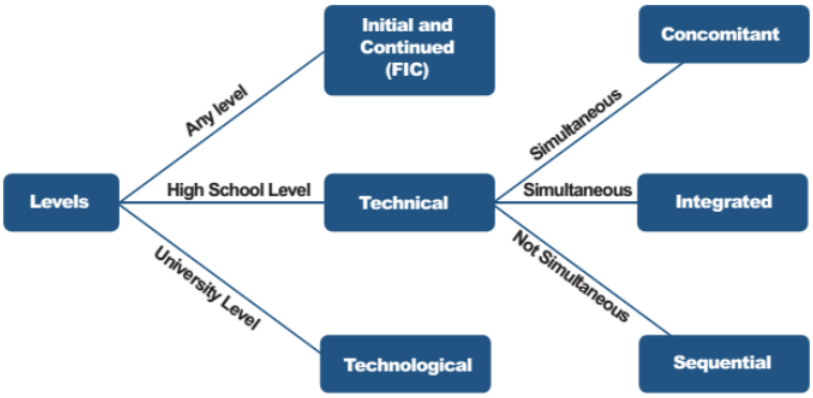

Table 1: Attrition

Dep. Var.:	All Subsc.	Women	Men
PRONATEC Offer	-0.041	-0.051	-0.036
	[0.038)	[0.073]	[0.045]
p-value	0.288	0.485	0.422
Number of observations	1,435	498	937

Notes: This table considers attrition in the full group of subscribers, and conditional on gender. Coefficients, standard errors and p-values are computed using specification (1), an ordinary least squares regression with course-by-site strata dummies and heteroskedasticity-robust standard errors [in brackets], using an indicator of not being in the endline sample as the dependent variable.

Table 2: Within-Strata Balance Across Treatment Offer Arms, All Subscribers

	(a) Mean (No offer)	(b) Mean (Offer)	(c) p-value (w. strata)	(d) % Size	(e) Number of obs.
Male	0.698	0.673	0.783	-3.6%	735
Black, "pardo" or indigenous	0.206	0.261	0.377	26.5%	735
Age	27.25 (7.60)	23.80 (6.46)	0.949	-12.7%	735
Mother completed high-school	0.190	0.258	0.645	35.6%	734
Mother completed tertiary	0.071	0.090	0.418	26.4%	735
Father completed high-school	0.230	0.247	0.311	7.2%	734
Father completed tertiary	0.048	0.061	0.944	27.6%	735
Concomitant modality	0.246	0.473	**0.072**	92.2%	735
N	126	609			
	F-stat	p-value			
	0.838	0.569			

Notes: This table considers balancedness in the group of applicants surveyed at endline. Column (a) (column (b)) shows means and standard deviations (in parenthesis) for a pre-determined variable, for those not offered (offered) the opportunity to participate in the program with the scholarships in the schools and time frame considered. p-values in column (c) are computed for each variable using specification (1), an ordinary least squares regression with course-by-site strata dummies and heteroskedasticity-robust standard errors. Column (d) displays the unadjusted percent size of the raw difference between treatment offer arm means with respect to the control group mean, and column (e) shows the sample size. The last row shows the F-statistic and p-value from a regression of the treatment indicator on the full set of pre-determined variables and course-by-site strata dummies. Additional details on how the outcomes were created can be found in Appendix A.

Table 3: Within-Strata Balance Across Treatment Offer Arms, by Gender Group

	Women					Men				
	(a) Mean (No offer)	(b) Mean (Offer)	(c) p-value (w. strata)	(d) % Size	(e) Number of obs.	(a) Mean (No offer)	(b) Mean (Offer)	(c) p-value (w. strata)	(d) % Size	(e) Number of obs.
Black, "pardo" or indigenous	0.184	0.256	0.189	39.1%	237	0.216	0.263	0.829	22.0%	498
Age	28.21 (8.24)	22.71 (5.40)	0.109	19.5%	237	26.84 (7.32)	24.33 (6.86)	0.228	-9.3%	498
Mother completed high-school	0.184	0.256	0.909	39.1%	237	0.193	0.259	0.774	34.2%	497
Mother completed tertiary	0.053	0.095	0.974	81.4%	237	0.080 (0.272)	0.088 (0.283)	0.304	10.4%	498
Father completed high-school	0.211	0.266	0.373	26.5%	237	0.239	0.237	0.506	-0.6%	497
Father completed tertiary	0.026	0.040	0.922	52.8%	237	0.057	0.071	0.917	24.5%	498
Concomitant modality	0.237	0.548	0.989	131.3%	237	0.250	0.437	**0.064**	74.6%	498
N	38	199				88	410			
		F-stat 1.062	p-value 0.390				F-stat 0.704	p-value 0.669		

Notes: This table considers balancedness in the group of female (leftmost columns) and male (rightmost columns) applicants surveyed at endline. Column (a) (column (b)) in each panel shows means and standard deviations (in parenthesis) for a pre-determined variable in a gender group, for those not offered (offered) the opportunity to participate in the program with the scholarships in the schools and time frame considered. p-values in column (c) are computed for each variable using specification (1), an ordinary least squares regression with course-by-site strata dummies and heteroskedasticity-robust standard errors, and p-values less than 0.15 are bolded. Column (d) displays the unadjusted percent size of the raw difference between treatment offer arm means with respect to the control group mean, and column (e) shows the sample size. The last row shows the F-statistic and p-value from a regression of the treatment indicator on the full set of pre-determined variables and course-by-site strata dummies. Additional details on how the outcomes were created can be found in Appendix A.

Table 4: Average Intent-to-treat Estimates on Technical and Non-Technical Education Graduation Rates

	Technical Education		General Education	
Dep. Var.:	(a) Graduated (PRONATEC)	(b) Graduated (any)	(c) Graduated High-school, concomitant	(d) Enrolled (Tertiary)
Panel A: All Subscribers				
Offer	0.451	0.233	0.150	-0.034
	[0.042]	[0.054]	[0.068]	[0.052]
No offer group mean	0.087	0.437	0.774	0.389
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	**<0.001**	**<0.001**	**0.028**	0.517
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	**<0.001**	**<0.001**	**0.050**	0.510
Number of observations	735	735	319	735
Panel B: Women				
Offer	0.451	0.203	0.027	0.01
	[0.067]	[0.099]	[0.075]	[0.104]
No offer group mean	0.053	0.368	0.889	0.447
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	**<0.001**	**0.042**	0.718	0.921
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	**<0.001**	**0.051**	0.904	0.999
Number of observations	237	237	118	237
Panel C: Men				
Offer	0.457	0.238	0.215	-0.036
	[0.053]	[0.066)	[0.091]	[0.060]
No offer group mean	0.102	0.466	0.727	0.364
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	**<0.001**	**<0.001**	**0.018**	0.556
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	**<0.001**	**0.001**	**0.053**	0.539
Number of observations	498	498	201	498

Notes: This table considers the impact of the offer on technical and general education investments in the full sample of surveyed applicants (Panel A) and the sub-sample of female and male surveyed applicants (Panels B and C, respectively). All variables are self-reported and were collected from June to August of 2016 (an average of 2 years after the end of the program in our sample). Estimates are from the intent-to-treat specification (1), i.e., an ordinary least squares regression with course-by-site strata dummies, and standard errors computed using heteroskedasticity-robust standard errors are shown [in brackets]. Two p-values are displayed in the bottom rows for each panel: *(i)* two-tailed p-values for the null hypothesis of a zero average parameter $\mathsf{H}_0 : \tau_{\text{ITT}} = 0$, using the reported standard errors; *(ii)* two-tailed randomization inference p-values for the sharp null hypothesis of no effect of the offer for any individual $\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$, using 1,000 randomly drawn assignment vectors with fixed within-strata probabilities taken from the realized assignment. Additional information about the samples in each panel (no offer arm mean and sample size) can be found in the bottom rows and details on how the outcomes were created can be found in Appendix A.

Table 5: Average Intent-to-treat Estimates on Labor Market Outcomes

	Survey Data					Administrative Data				
Dep. Var.:	(a) Employment	(b) Tenure	(c) Formal employment	(d) Work earnings	(e) Work earnings (in log)	(f) Matched (pos. wage)	(g) Tenure	(h) Firm Size	(i) Work earnings	(j) Work earnings (in log)
Panel A: All Subscribers										
Offer	0.043	3.95	0.051	101.07	0.042	0.015	3.12	-0.009	-17.51	0.010
	[0.042]	[4.62]	[0.051]	[112.75]	[0.055]	[0.030]	[2.98]	[0.317]	[97.78]	[0.041]
No offer group mean	0.802	33.01	0.690	1399.14	7.345	0.890	30.21	4.750	1391.00	7.52
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	0.303	0.393	0.320	0.370	0.438	0.615	0.295	0.976	0.858	0.815
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	0.270	0.408	0.354	0.346	0.397	0.601	0.255	0.860	0.809	0.820
Number of observations	735	726	735	731	567	1,289	1,289	1,289	1,289	829
Panel B: Women										
Offer	0.192	10.25	0.324	418.32	0.182	0.162	6.243	0.320	496.361	0.119
	[0.090]	[7.639]	[0.097]	[156.20]	[0.106]	[0.066]	[4.97]	[0.698]	[173.72]	[0.098]
No offer group mean	0.658	24.71	0.474	873.03	7.06	0.735	20.30	4.03	602.94	7.29
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	**0.032**	0.181	**0.001**	**0.008**	**0.089**	**0.015**	0.209	0.647	**0.004**	0.229
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	**0.025**	0.133	**0.003**	**0.009**	**0.061**	**0.030**	0.209	0.601	**0.002**	**0.140**
Number of observations	237	235	237	237	166	431	431	431	431	245
Panel C: Men										
Offer	-0.018	1.99	-0.066	-18.18	0.015	-0.030	1.518	-0.143	-144.779	0.016
	[0.046]	[5.89]	[0.058]	[143.55]	[0.061]	[0.033]	[3.75]	[0.356]	[119.39]	[0.046]
No offer group mean	0.864	36.63	0.784	1626.32	7.45	0.928	32.66	4.93	1585.16	7.55
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	0.701	0.736	0.252	0.899	0.810	0.364	0.685	0.687	0.226	0.733
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	0.865	0.760	0.229	0.893	0.811	0.313	0.705	0.627	0.196	0.756
Number of observations	498	491	498	494	401	858	858	858	858	584

Notes: This table considers the impact of the offer on labor market outcomes in the full sample of surveyed applicants (Panel A) and the sub-sample of female and male surveyed applicants (Panels B and C, respectively). All variables are self-reported and were collected from June to August of 2016 (an average of 2 years after the end of the program in our sample). Estimates are from the intent-to-treat specification (1), i.e., an ordinary least squares regression with course-by-site strata dummies, and standard errors computed using heteroskedasticity-robust standard errors are shown [in brackets]. Two p-values are displayed in the bottom rows for each panel: *(i)* two-tailed p-values for the null hypothesis of a zero average parameter $\mathsf{H}_0 : \tau_{\text{ITT}} = 0$, using the reported standard errors; *(ii)* two-tailed randomization inference p-values for the sharp null hypothesis of no effect of the offer for any individual $\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$, using 1,000 randomly drawn assignment vectors with fixed within-strata probabilities taken from the realized assignment. Additional information about the samples in each panel (no offer arm mean and sample size) can be found in the bottom rows and details on how the outcomes in columns (a)-(e) were created can be found in Appendix A.

Table 6: Average intent-to-treat Estimates on Standardized Indicators of Noncognitive Skills

Dep. Var.:	(a) Agreeableness	(b) Conscientiousness	(c) Extraversion	(d) Neuroticism	(e) Openness to experiences	(f) Locus of control
Panel A: All Subscribers						
Offer	0.081	0.228	0.184	0.010	-0.032	-0.072
	[0.147]	[0.145]	[0.160]	[0.156]	[0.157]	[0.160]
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	0.582	**0.116**	0.254	0.95	0.837	0.656
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\, i$)	0.287	**0.057**	**0.102**	0.460	0.401	0.333
Number of observations	364	364	363	364	364	364
Panel B: Women						
Offer	0.295	0.362	0.627	0.270	0.243	-0.257
	[0.253]	[0.278]	[0.336]	[0.291]	[0.320]	[0.307]
No offer group mean	-0.171	-0.038	-0.378	-0.513	-0.368	0.235
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	0.246	0.197	**0.065**	0.357	0.450	0.405
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\, i$)	0.146	**0.085**	**0.014**	0.205	0.222	0.190
Number of observations	106	106	105	106	106	106
Panel C: Men						
Offer	-0.002	0.203	0.045	-0.017	-0.072	-0.025
	[0.185]	[0.181]	[0.187]	[0.188]	[0.184]	[0.194]
No offer group mean	0.056	0.012	0.123	0.167	0.12	-0.077
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	0.992	0.263	0.809	0.929	0.694	0.899
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\, i$)	0.506	0.121	0.399	0.478	0.356	0.458
Number of observations	258	258	258	258	258	258

Notes: This table considers the impact of the offer on noncognitive skills in the full sample of surveyed applicants (Panel A) and the sub-sample of female and male surveyed applicants (Panels B and C, respectively). All variables have been standardized with respect to the no offer group mean and standard deviation and were collected from June to August of 2016 (an average of 2 years after the end of the program in our sample). Estimates are from the intent-to-treat specification (1), i.e., an ordinary least squares regression with course-by-site strata dummies, and standard errors computed using heteroskedasticity-robust standard errors are shown in brackets. Heteroskedasticity-robust standard errors are in brackets, and two p-values are displayed in the bottom rows for each panel: (i) two-tailed p-values for the null hypothesis of a zero average parameter $\mathsf{H}_0 : \tau_{\text{ITT}} = 0$, using the reported standard errors; (ii) two-tailed randomization inference p-values for the sharp null hypothesis of no effect of the offer for any individual $\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\, i$, using 1,000 randomly drawn assignment vectors with fixed within-strata probabilities taken from the realized assignment. Additional information about the samples in each panel (no offer arm mean and sample size) can be found in the bottom rows.

Table 7: Average Intent-to-treat Estimates on Self-reported Criminal and Risky Behaviors

Dep. Var.:	(a) Argument or fight	(b) Marijuana use	(c) Other drugs use	(d) Binge drinking	(e) Drink and drive
Panel A: All Subscribers					
Offer	0.012	-0.021	-0.01	0.052	0.045
	[0.034]	[0.025]	[0.017]	[0.046]	[0.052]
No offer group mean	0.098	0.066	0.033	0.189	0.328
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	0.730	0.389	0.545	0.260	0.392
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	0.333	0.168	0.307	0.145	0.204
Number of observations	702	702	702	702	702
Panel B: Women					
Offer	0.099	-0.011	0.008	0.132	0.122
	[0.049]	[0.029]	[0.008]	[0.066]	[0.061]
No offer group mean	0.027	0.027	0	0.054	0.054
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	**0.043**	0.718	0.364	**0.045**	**0.048**
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	**0.096**	0.387	0.999	**0.053**	**0.057**
Number of observations	226	226	226	226	226
Panel C: Men					
Offer	-0.029	-0.023	-0.018	0.008	-0.003
	[0.044]	[0.035]	[0.024]	[0.061]	[0.069]
No offer group mean	0.129	0.082	0.047	0.247	0.447
(i) p-value ($\mathsf{H}_0 : \tau_{\text{ITT}} = 0$)	0.515	0.517	0.465	0.894	0.960
(ii) p-value ($\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$)	0.217	0.248	0.356	0.488	0.487
Number of observations	476	476	476	476	476

Notes: This table considers the impact of the offer on self-reported criminal and risky behaviors in the full sample of surveyed applicants (Panel A) and the sub-sample of female and male surveyed applicants (Panels B and C, respectively). Estimates are from the intent-to-treat specification (1), i.e., an ordinary least squares regression with course-by-site strata dummies, and standard errors computed using heteroskedasticity-robust standard errors are shown in brackets. Heteroskedasticity-robust standard errors are in brackets, and two p-values are displayed in the bottom rows for each panel: (i) two-tailed p-values for the null hypothesis of a zero average parameter $\mathsf{H}_0 : \tau_{\text{ITT}} = 0$, using the reported standard errors; (ii) two-tailed randomization inference p-values for the sharp null hypothesis of no effect of the offer for any individual $\mathsf{H}_0 : \tau_{\text{ITT},i} = 0, \forall\ i$, using 1,000 randomly drawn assignment vectors with fixed within-strata probabilities taken from the realized assignment. Additional information about the samples in each panel (no offer arm mean and sample size) can be found in the bottom rows and details on how the outcomes in columns (a)-(f) were created can be found in Appendix A.

Appendix A Definition of Main Variables

All information used in this analysis was originally collected for the purpose of evaluating the PRONATEC program in Chapecó, Itapiranga, São Miguel do Oeste and Xanxerê. In what follows we consider how the main variables used in the analysis were defined after data collection.

A.1 Education, Technical and General

- *Graduated (PRONATEC)*: Indicator variable that takes the value of one if the person reported having graduated in a PRONATEC course;
- *Graduated (any)*: Indicator variable that takes the value of one if the person reported having graduated in a PRONATEC course or any other technical education program;
- *Graduated (High-school, concomitant)*: Indicator variable that takes the value of one if the person reported having finished high-school — it is only defined for the concomitant modality, which is the only one allowed for individuals enrolled in school;
- *Entered (Tertiary)*: Indicator variable that takes the value of one if the person reported having started tertiary at some point.

A.2 Labor Market

- *Employment*: Indicator variable that takes the value of one if the person reported to have a job in the month before the survey or zero if the person reports being unemployed or out of the labor force;
- *Formal employment*: Indicator variable that takes the value of one if the individual claims to have a signed work card (carteira assinada) in the current most important employment we imputed zeros for all individuals who report being either unemployed or out of the lavor force in the month before the survey;
- *Days per month*: Censored continuous variable defined as the number of days worked in an usual week in the current most important job, multiplied by four — we imputed zeros for all individuals who report being unemployed or out of the labor force for more than one month at the survey date;
- *Hours per week*: Censored continuous variable defined as the number of hours worked in an usual day in the current most important job, multiplied by five — we imputed zeros for all individuals who report being unemployed or out of the labor force for more than one month at the survey date;
- *Tenure*: Censored continuous variable defined as the length of duration of employment in months in the current most important job — we imputed zeros for all individuals who report being unemployed or out of the labor force for more than one month at the survey date;
- *Work earnings*: Monthly wage or earnings in the in the current most important employment we imputed zeros for all individuals who report being either unemployed or out of the labor force in the month before the survey. The variable in logs is computed as the natural logarithm of the latter variable.
- *Worked in course area*: Indicator variable that takes the value of one if the person reported to have a job in the course area for which she applied in the month before the survey or zero if the person reports being unemployed or out of the labor force.

A.3 Self-reported Criminal and Risky Behaviors

- *Argument or fight*: Indicator variable that takes the value of one if the person reported being involved in an argument or fight in the month before the survey;
- *Marijuana use*: Indicator variable that takes the value of one if the person reported having used marijuana in the month before the survey;
- *Other drugs use*: Indicator variable that takes the value of one if the person reported having used other drugs in the month before the survey;

- *Binge drinking*: Indicator variable that takes the value of one if the person reported an event binge drinking in the month before the survey;
- *Drink and drive*: Indicator variable that takes the value of one if the person reported drinking and driving in the month before the survey;