Economics of Education Review 95 (2023) 102433

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Liquidity constraints in free post-secondary education: Evidence from Colombia☆

Luis Fernando Gamboa [a], Jaime Millán-Quijano [b,c,*]

[a] Pontificia Universidad Javeriana, Colombia
[b] Navarra Center for International Development at Universidad de Navarra, Edificio de Bibliotecas Entrada Este, 2da Planta, Pamplona, 31009, Spain
[c] Centro de Estudios Manuel Ramírez, Colombia

ARTICLE INFO

JEL classification:
I26
C36
J21

Keywords:
Liquidity constraints
Tertiary education
Regression discontinuity

ABSTRACT

This paper provides evidence of the importance of liquidity constraints in a tuition-free post-secondary education setting in Colombia. We exploit exogenous variation in the relative cost of tuition-free vocational education from a nationwide cash transfer program. We show that eligibility for a USD 136 grant every other month increases enrollment by up to 12 percentage points. We also show that men with larger returns to education are more affected by the availability of grants, which is consistent with the presence of liquidity constraints. However, we do not find the same for women. This paper highlights the importance of non-tuition costs and discusses whether individuals under-invest in their human capital when free education is available.

1. Introduction

The income gap in tertiary education enrollment is well documented in both developed (Carneiro & Heckman, 2002; Lochner & Monge-Naranjo, 2011) and developing economies (González-Velosa, Rucci, Sarzosa, & Urzúa, 2015; Murakami & Blom, 2008). Nonetheless, the debate on the importance of liquidity constraints in explaining this gap is still unresolved.[1] The majority of the literature associates the problem of liquidity constraints with a household's ability to pay tuition fees. However, little is known about whether individuals under-invest in their own human capital when free education is available. Understanding the barriers to tertiary education beyond analyzing tuition fees is of growing importance in middle and low-income countries where governments are making efforts to expand tuition-free tertiary education (see the discussion in de Gayardon, 2019; Molina & Rivadeneyra, 2021).

Carneiro and Heckman (2002) describes short-run liquidity constraints as a lack of family resources or access to credit to invest in education; in other words, individuals have difficulty in covering the cost of consumption while studying.[2] Thus, in the absence of tuition fees, personal expenses (transportation, cost of living, etc.) and foregone income become more relevant when deciding on investment in schooling. Cameron and Taber (2004), Fuller, Manski, and Wise (1982), and Flannery and O'Donoghue (2013) use variations in local labor

☆ We thank the *Jóvenes en Acción* impact evaluation team at Econometría S.A. and SEI led by Marta Isabel Gutierrez. We are also thankful to the teams at DPS, SENA, Ministerio de Salud, and Jóvenes en Acción for their collaboration and for providing data and insight into the institutional context. In addition, we appreciate the valuable comments provided by Matilde Machado, William Fuchs, Julio Cáceres, Luigi Minale, Jorge Rodriguez, Luis Carlos Reyes, Richard Murphy, Paul Rodriguez, Miguel Angel Borrella, and all the participants at several related seminars and invited talks. Any errors are solely the responsibility of the authors and do not represent the views of any public or private institution. This paper was previously circulated as *"Reducing credit constraints to post-secondary technical education in developing countries: Evidence from Colombia"*.

* Corresponding author at: Navarra Center for International Development at Universidad de Navarra, Edificio de Bibliotecas Entrada Este, 2da Planta, Pamplona, 31009, Spain.
E-mail address: jmillanq@unav.es (J. Millán-Quijano).

[1] Some studies show that parental income or access to credit are strong predictors of access to tertiary education (Belzil, Maurel, & Sidibé, 2021; Brown, Karl Scholz, & Seshadri, 2011; Lochner & Monge-Naranjo, 2011). Meanwhile, other authors have found small or null effects when examining these indicators (Cameron & Taber, 2004; Carneiro & Heckman, 2002; Keane & Wolpin, 2001). In Latin America, there is more robust evidence supporting the existence of short-run liquidity constraints when it comes to enrollment in tertiary education (for example Cáceres-Delpiano, Giolito, & Castillo, 2018; Didriksson, 2018; Londoño-Vélez, Rodríguez, & Sánchez, 2020; Melguizo, Sanchez, & Velasco, 2016; Rau, Rojas, & Urzúa, 2013; Solis, 2017).

[2] In contrast, long-run constraints to education, associated with income, reflect differences in cognitive and non-cognitive skills when investing in tertiary education. Skills strongly correlate with family income because high-income households can invest in better quality education throughout their children's formative years.

https://doi.org/10.1016/j.econedurev.2023.102433
Received 25 August 2022; Received in revised form 20 June 2023; Accepted 22 June 2023
Available online 13 July 2023

markets to quantify how opportunity cost and living expenses affect schooling decisions. Other authors explore the effect of financial aid on students' outcomes after enrollment, meaning additional income only affects students' outcomes after tuition fees have been paid (Murphy & Wyness, 2023). However, of these studies which quantify costs not related to tuition, all are of situations where students also pay tuition fees to enter tertiary education. As such, little is known about the effect of financial aid when tertiary education is tuition-free.

This paper aims to fill this gap by directly testing the importance of liquidity constraints when individuals do not have to pay any tuition fees at any time. We analyze a large-scale program in Colombia, called Youth in Action (YIA - Jóvenes en Acción, in Spanish). The program offers a cash transfer of USD 136 every other month to young, low-income individuals whilst they are enrolled in free vocational education (one- or two-year programs) at a nationwide public institution called Servicio Nacional de Aprendizaje (SENA) (Prosperidad-Social, 2017). YIA uses a proxy-means test, called SISBEN, and a set of predetermined cutoffs as one of the program's eligibility conditions. This targeting strategy allows us to compare at the margin eligible and non-eligible individuals for YIA's by means of a Regression Discontinuity Design (RD), in order to estimate changes in enrollment in tuition-free tertiary education driven by the availability of financial aid.

Furthermore, we follow Card (2001) in comparing Ordinary Least Squares (OLS) and Instrumental Variables (IV) estimations to test for the presence of short-run liquidity constraints. By considering the analogy between Fuzzy Regression Discontinuity (FRD) and IV estimations (Lee & Lemieux, 2010), we use eligibility for YIA as an instrument (FRD-IV) for enrolling in SENA in an estimation of the returns to SENA education. In this case, instrumenting enrollment in SENA with the obtention of a SISBEN score below the YIA cutoff, captures variations in enrollment resulting from changes in the relative cost of education. Thus, the resulting RD-IV estimates regarding the effect of enrollment in SENA on labor market participation can be interpreted as the Local Average Treatment Effects (LATE) for the compliers to changes in the cost of SENA's education. Hence, if IV estimates are larger than OLS, one can argue that compliers are individuals who before the opportunity of financial aid would have wanted to enroll, as they expected large returns to education, but could not because they were not able to cover the cost of studying. We indeed show that compliers to YIA are individuals with larger returns to education than the average.

Summarizing our results, first stage estimations show that YIA eligibility is a strong predictor of enrollment in two-year programs at SENA. For both genders the estimated change in enrollment is about 10 percentage points (pp) from a baseline of 16%. However, key heterogeneity arises in the second-stage estimations. For men, we find that FRD-IV estimates are consistently larger than OLS estimates. Meanwhile, for women the FRD-IV estimates are not significantly different from the OLS estimates; if anything, FRD-IV estimates are smaller than the OLS ones. Following previous literature, the results support the hypothesis that men who applied to two-year programs at SENA may have abstained from enrollment in the absence of financial aid because they face liquidity constraints. We do not find that women at just below the YIA eligibility cutoff face liquidity constraints, and nor we do find that SENA has positive returns.

This paper contributes to different streams of the literature. First, we provide new evidence by testing the existence of liquidity constraints when tertiary education is tuition-free, expanding knowledge on the role played by income when explaining investments in education. As such, the paper documents how non-tuition costs are still a constraint for low-income individuals who are interested in attending tertiary education. These costs are usually overlooked in the literature, but, as we point out in this paper, they are relevant in the public policy debate. In this sense, our results also enrich the literature on the limitations of tuition-free education when it comes to reducing human capital accumulation differences in developing countries (as Molina & Rivadeneyra, 2021).

Second, our framework allows us to overcome the limitations of testing for liquidity constraints by comparing OLS and IV estimates, as highlighted by Carneiro and Heckman (2002) and Griliches (1977). We provide evidence supporting the idea that, in our FRD framework, the availability of and preferences for tertiary education are continuous around different cutoffs. We also show that individuals who did not enter SENA did not systematically enroll in other educational institutions. This may result from the fact that, in most regions, SENA is the only tuition-free educational option. To address the possible concern that YIA grants may have an income effect that could impact future labor market participation, we use additional survey data from YIA beneficiaries to show that beneficiaries principally used the cash transfer to cover transportation costs and supporting household expenses. However, the grant was not used to increase savings or investments, which could impact beneficiaries' labor market decisions after finishing their SENA education.

Third, we estimate the returns to SENA education and provide new evidence on the importance of vocational education as an alternative to tertiary education for low-income individuals (for example Carroll & Ihnen, 1967; Chakravarty, Lundberg, Nikolov, & Zenker, 2019). In developing economies, vocational education is regularly used to increase human capital among poor individuals. These programs usually provide specific skills at less cost, or even without tuition fees, and require a smaller time investment than professional studies or college degrees. Therefore, any attempt to shed light on the importance of the monetary and non-monetary costs associated with the demand for these programs is crucial for developing economies. What is more, we find stark heterogeneous effects by gender, and explore possible reasons that could explain why, at the margin of YIA eligibility, women have no returns to vocational education at SENA.

Finally, this paper differs from previous analyses of YIA in Attanasio, Guarín, Medina, and Meghir (2017), Attanasio, Kugler, and Meghir (2011) and Kugler, Kugler, Saavedra, and Herrera-Prada (2020). The first version of YIA was implemented between 2001 and 2005. The program then offered six months of in-work training, providing a setup to study the short, medium and long-term effects of professional training for low-income individuals, as the papers listed before did. A new version of YIA was launched in 2012 and focused on increasing the demand of post-secondary education by providing financial aid to study at SENA. We examine these new features of YIA to answer questions related to the income gap in tertiary education, instead of focusing on the effects of training on labor market outcomes and welfare.

As countries in Latin-America and other regions turn to tuition-free tertiary education (Bernasconi & Celis, 2017; Espinoza & Urzúa, 2015; Molina & Rivadeneyra, 2021), we provide new evidence to help policy makers design better policies aiming to increase access to tertiary education for individuals from low-income households.

2. Institutional context: Youth in action, sena, and liquidity constraints for the Colombian youth

YIA is a nationwide program in Colombia that started in 2006. The program initially offered cash transfers to young individuals from low-income households while they received in-job training.[3] However, the program subsequently changed its goals, target population, and intervention. In 2012 YIA started piloting a new version of the program which was nationally launched from the second semester of 2014. The new version of YIA has the following characteristics. The program offers a bimonthly grant of USD 136 (COP 400,000) to high school graduates aged 16 to 24 years old with the condition of remaining enrolled in vocational education provided by SENA. The YIA grant is equivalent to

[3] There are several studies of the first version of YIA (Attanasio et al., 2017, 2011; Kugler et al., 2020)

34% of the average monthly income for people with similar profiles.[4] In addition to the grant, YIA reserves 30% of available program spaces for its beneficiaries in the case of excess demand, regardless of their score on the entrance exam. Beneficiaries can participate in an optional component, called *Habilidades para la Vida (HPV)*, to strengthen their non-cognitive abilities.

YIA selects its beneficiaries using two broad criteria. First, YIA chooses beneficiaries according to a proxy means test known as the SISBEN score. Second, YIA offers benefits for minorities and specific vulnerable groups.[5] Between 2014 and 2015, 87% of the 192,896 YIA beneficiaries entered the program because their SISBEN scores were below the predefined cutoff.

In order to receive benefits, YIA beneficiaries need to enroll in SENA, a national public institution which provides tuition-free vocational education with educational centers along 99.5% of Colombian municipalities (urban and rural areas). Indeed, in many places, SENA is the only source of tertiary education available. SENA offers one- and two-year programs in an ample spectrum of fields, including sector-specific skills. In order to select its students, an entry exam is used to allocate available spaces in each program when there is an excess of demand. Additionally, SENA provides individuals with an internship in the last six months of their program.[6]

Despite offering tuition-free education, studying at SENA over one or two years implies other costs. First, individuals may have to exit the labor market, or at least work only part time in order to attend their classes. This forgone income represents their opportunity cost. Additionally, travel, accommodation, and other living expenses may be important, as young individuals may not have personal savings or family support to cover them.

If credit markets were perfect, an applicant accepted into SENA should be able to obtain a loan to cover their expenses while studying, provided that the returns to their educational investment are higher than the market interest rate. Therefore, it is worth asking whether vocational education is a profitable investment for young individuals. To address this question, we analyze data from the 2015 National Household Survey (GEIH, for its acronym in Spanish), focusing on the two lowest socioeconomic strata.[7] Our goal is to estimate the changes in income resulting from access to vocational education. According to our calculations, acquiring vocational education instead of having only secondary education increases lifetime income by USD 6,323 and USD 7,947 for men and women, respectively.[8] The income lost from the two years invested in SENA is around USD 1,254 for men and USD 1,078 for women. Given that SENA is tuition-free, both men and women have large and positive returns.

However, as in many developing economies, Colombian credit markets are highly segregated and disadvantaged youth are usually excluded (Murcia, 2007; Rodríguez-Raga & Rodríguez, 2016). The supply of educational loans comes from private banks and governmental institutions such as ICETEX, a public institution for higher education financing. This lending market has many issues. First, creditors give preference to professional/college studies (4–5 years) and do not provide loans for vocational studies (Melguizo et al., 2016). Second, private banks and ICETEX require collateral from their customers, limiting access to a population that does not have physical assets to leverage the payment of their debts against their expected incomes.

3. Data and sample selection

We combine information from different sources. First, we use administrative data from SENA on all individuals who applied for one- or two-year programs from the second semester of 2014 to the first semester of 2015. In addition to basic demographics (age and gender), we have information about the program each individual applied to, their entry exam score and their enrollment status. Second, we merge SENA data with the administrative registries of SISBEN scores at the household level. By using this combined database, it is possible to determine which individuals were eligible for YIA. Third, we use registry records from YIA to identify which individuals were beneficiaries of the program because of their SISBEN scores, and exclude vulnerable/minority YIA beneficiaries. Unfortunately, matching SISBEN and SENA data was not complete. For individuals who applied to YIA using their SISBEN score we were able to match all registries as they had to show their SISBEN when applying to the program. For all other individuals we match using national identity numbers, but we were only able to match 68%. This created some imbalances in the SISBEN distribution which we discuss later in this document.

In order to assess the medium- and long-term effects of financial aid for SENA applicants, we combine our data with information on the social security contributions from the Colombian Ministry of Labor from August 2014 until December 2017. This data is available from PILA, a database which contains information on compulsory contributions to social security. In addition, we also have information about the type of contract (temporary or permanent), firm size, sector (public or private) and also an identifier for self-employment. Attanasio et al. (2017) defined an individual as a formal worker if they make compulsory contributions to social security. Therefore, if in a given month we find an individual in PILA's database, it is because that individual had a formal job that month. Individuals with no reports in PILA are either not working (unemployed or inactive) or working in the informal market. For our analysis, we do not consider individuals younger than 18 years old when they applied to SENA because they are not listed in the PILA registry, as national identification numbers are issued at the age of 18.[9]

We restricted the sample of analysis to those programs and individuals where YIA could have had affected enrollment decisions. Hence, out of a total of 12,298 different programs, we only include individuals who applied to programs with an entry exam (9016 programs), because programs without an entry exam are typically the result of alliances between a firm and SENA. We only included programs with an excess of demand (6912 programs) and without a second entry exam (3461 programs), because the entry exam selects which individuals receive the first offer to enroll and YIA's affirmative action component could affect enrollment decisions in these programs.

[4] We compared this financial aid with the average monthly wage of young individuals (18 to 24 years old) who have completed secondary education.

[5] The groups are: the adoption list of the ICBF, indigenous and afro-descendant communities, individuals from Red Unidos, and registered victims of the Colombian armed conflict.

[6] We define a SENA degree as vocational education in a given field of either one or two years; for example, a two-year accounting degree. Furthermore, we define a SENA program as vocational education in a given field of either one or two years, offered in a given SENA center, in a given cohort; for example, a two-year accounting degree in the SENA center of a small municipality, starting in the second semester of 2014.

[7] Socioeconomic strata is an administrative categorization used to allocate subsidies in public utilities, and has been traditionally used as a proxy for the distribution of household wealth in other studies such as Kugler et al. (2020) and Londoño-Vélez et al. (2020)

[8] We estimate the probability of having a formal or an informal job, and the respective income for different cells by age, gender, and education. We were then able to calculate the present value for men and women in two scenarios of the maximum educational level attained, namely high school and vocational studies. We use a 10% interest rate and an exchange rate of COP 4,000 per USD.

[9] We excluded 143,585 individuals and 202,573 applications due to the age restriction, which constitutes approximately 28% of all applicants to SENA. Younger individuals have larger rates of enrollment, but do not differ from older individuals regarding SISBEN score, entry exam score, or their participation in YIA.

Table 1
Descriptive statistics. SENA applicants to one- and two-year.

	Program length			
	One-year		Two-year	
Variable	mean (1)	s.d. (2)	mean (3)	s.d. (4)
Applications	64102	.	75294	.
Individuals	45431	.	47212	.
Individual level variables				
Enrolled	0.16	0.37	0.22	0.42
Enrollment period				
Second semester 2014	0.36	0.48	0.57	0.49
Targeting area				
Area 1	0.44	0.50	0.47	0.50
Area 2	0.50	0.50	0.45	0.50
Area 3	0.06	0.23	0.08	0.27
Migrant[a]	0.20	0.40	0.26	0.44
Male	0.45	0.50	0.48	0.50
Age	20.70	1.92	20.66	1.93
Entry exam score	37.14	17.22	41.66	16.31
Eligible for FEA	0.52	0.50	0.41	0.49
Other support	0	0.07	0.01	0.09
Program level variables				
Seats per program	35.13	15.24	42.46	34.31
Applicants per program	139.57	188.77	125.95	174.16
Take-up proportion	0.54	0.29	0.52	0.33
Simulated cutoff	40.33	12.05	42.55	11.38
Labor market variables				
Working in the last semester	0.36	0.48	0.52	0.50

Notes: Authors' calculations using data from SENA and SISBEN. Only includes SENA centers where YIA beneficiaries applied. For detailed statistics by position with respect to the cutoff see Table C.1 in Appendix C.
[a]Migrant is defined as a person who studied in a municipality different from the one where they were interviewed for the SISBEN score.

As a result (see Table 1), we obtained information from 139,396 applications by 92,833 individuals, as individuals can apply to more than one program. For both one- and two-year programs, the proportion of men is marginally below 50%. Enrollment levels are low, only 16% and 22% for one- and two-year programs, respectively. The average number of seats is lower in shorter programs, but their demand is higher than in two-year programs. Despite the fact that we only select programs with excess of demand, the average take-up rate is just above 50%. Additionally, a higher proportion of one-year program applicants are eligible for Familias en Acción (FEA), a nationwide conditional cash transfer program allocated to low-income households.[10]

We measure the economic returns to SENA education, only taking into account information after the expected completion of studies. For example, for those individuals who applied in August 2014 to one-year programs, we only use information about their participation in the labor market beginning in September 2015, regardless of whether or not they were enrolled in SENA. Without information about wages to estimate returns to education (as Mincer, 1974), we choose as our indicator of individual performance whether the individual had a formal job in the last semester we observed (second semester 2017).[11] The lower panel of Table 1 shows that 36% and 52% of applicants to one- and two-year programs respectively had a job for at least one month in the last semester of 2017.

There are three main limitations to our data. First, as explained above, we cannot match SENA data to PILA data for individuals who applied to SENA when they were 16 or 17 years old. These individuals may have a greater desire to enroll in tertiary education because they apply shortly after finishing high school. Figure C.1 in Appendix C shows that, especially for one-year programs, the enrollment rate of individuals younger than 18 is significantly higher than the enrollment rate of the individuals in our analysis.

Second, PILA only reports on formal labor market participation. This is a challenge for our analysis, because Colombia has a large informal economy. By 2018, the informal employment rate in Colombia (informal employment as proportion of total employment) was 62.4%, greater than the informality rate in other Latin-American OECD countries such as Chile (29.3%), or Costa Rica (37.9%).[12] Therefore, our estimates do not consider the returns to education through informal employment.

Third, in an ideal scenario, we would estimate returns to education in SENA using information on earnings, in this case earnings in the formal labor market. However, we only have information about labor market participation and certain characteristics of the job, but we do not have access to earnings. This may hinder the scope of our results. Nevertheless, in the Colombian context, with high levels of informality and unemployment, having a formal job is a good indicator of high-quality and high-earning jobs.

4. Estimation strategy

To estimate the returns to vocational education and the importance of liquidity constraints, we take advantage of a discontinuity in the probability of receiving YIA benefits around the program's SISBEN cutoffs. Eligibility for YIA should increase the likelihood of enrolling in SENA. Following Lee and Lemieux (2010), the changes in enrollment in SENA around the cutoff are exogenous if potential outcomes are continuous around the cutoff. We discuss the validity of our strategy later in this section.

Then, we define v_i as the difference between the SISBEN score of the individual i and the cutoff set by YIA for their area. Therefore, $D_i = 1\left[v_i \leq 0\right]$, indicates which individuals are YIA eligible. Following

[10] FEA also uses the SISBEN score to allocate benefits with cutoffs smaller than the ones used by YIA (Attanasio, Battistin, Fitzsimons, Mesnard, & Vera-Hernández, 2005).

[11] We also use indicators such as the proportion of months working, working at least one month, and longest periods of employment and unemployment (maximum number of continuous months employed/unemployed during the period of analysis).

[12] Data from Elgin, Kose, Ohnsorge, and Yu (2021).

a standard RD (Lee & Lemieux, 2010), to quantify the effect of the discontinuity on enrollment in SENA, for $\upsilon \in [-h, h]$, we estimate the following equation:[13]

$$S_i = \beta_1 D_i + g_1\left(\upsilon_i, D_i\right) + X_i\mathcal{B} + \epsilon_i \quad (1)$$

Where, β_1 represents the effect of YIA eligibility on enrollment in SENA for individuals in a neighborhood of size h around the cutoff, $g(\upsilon, D)$ is a function of the running variable below and above the cutoff and X is a vector of applicant's characteristics including SENA center and SENA degree fixed effects. When $\beta_1 > 0$ one can conclude that YIA had an impact on enrollment in SENA. However, it does not necessarily mean that YIA reduced the liquidity constraints of eligible individuals. It is important to point out that changes in enrollment driven by YIA reflect the effect of differences in the relative cost of education between individuals just above and just below the cutoff. This point is crucial for the next step of our analysis.

We follow Card (2001) discussion about the comparison between OLS and IV estimated coefficients. In the standard estimation of returns to schooling (Mincer, 1974), OLS estimates are upper biased due to factors such as unobserved ability and/or desire for further education. Furthermore, IV estimates represent a LATE of the compliers, instead of the Average Treatment Effect (ATE) (Imbens & Angrist, 1994). By considering the heterogeneity of returns to education, for a given instrument, the LATE of the compliers may be larger (smaller) than the ATE, if the compliers' returns to schooling are larger (smaller) than the average returns to schooling of the population. Therefore, instrumenting schooling with a variable related to changes in the cost of education will result in IV estimates larger than the OLS, if the compliers to that instrument are individuals whose expected returns to education are larger than the average expected returns to education. However, the individuals who reacted to the change in the cost of education did not acquire more education in the past, due to the existence of liquidity constraints.

After showing the direct effect of YIA eligibility on enrollment in SENA, we focus on estimating the returns to SENA education. First, a biased OLS estimator of the returns to vocational education will result from estimating:

$$Y_i = \alpha_Y S_i + g_2\left(\upsilon_i, D_i\right) + X_i\mathcal{A} + \epsilon_i \quad (2)$$

Second, we can use a FRD-IV design (Lee & Lemieux, 2010). We estimate the effect of SENA enrollment on labor market participation using YIA eligibility cutoffs over SISBEN score as an instrument. Thus, we use a Two-Stage Least Square estimation (TSLS), where Eq. (1) represents the first stage and the following equation represents the second stage.

$$Y_i = \tau_Y \hat{S}_i + g_3\left(\upsilon_i, D_i\right) + X_i\mathcal{T} + \epsilon_i \quad (3)$$

Where τ_Y is an unbiased estimate of the effect of enrolling in SENA on the labor market indicator Y for individuals in the neighborhood of size h around the cutoff. τ_Y quantifies the effect of enrollment in SENA on the labor market for individuals who enrolled because they were eligible to receive financial aid through YIA, but would not have enrolled without financial aid. Thus, we expect that in the presence of liquidity constraints $\tau_Y > \alpha_Y$.

Given that we focus on understanding the effect of non-tuition costs (opportunity cost and living expenses), the length of the program gains importance. In addition, young men and women face different conditions in the labor market, therefore, all our estimation will be divided by program length (one- or two-year programs), and gender.

4.1. Validity of the estimates

Our estimations of the returns to education will be informative of the existence of liquidity constraints if τ_Y is an unbiased estimator of the effect of enrollment in SENA on formal labor market participation.

Following Imbens and Lemieux (2008) and Lee and Lemieux (2010), τ_Y is an unbiased estimator of the returns to education when potential outcomes are continuous around the cutoff. One can assume that potential outcomes are continuous if: (1) the probability of enrolling in SENA changes around the cutoff, (2) individuals are not able to manipulate their SISBEN score, and (3) all other confounding factors are continuous around the cutoff. In the next section, we show that being eligible for YIA increases enrollment in SENA. In addition, we provide detailed evidence that individuals are not manipulating their SISBEN score and that relevant confounding variables are continuous around the cutoff in Appendix A.

Summarizing, we first check whether the distribution of the SISBEN score is continuous around the cutoff. The graphical evidence and the discontinuity test (McCrary, 2008) are unfortunately not satisfactory, and we find more SENA applicants below the cutoff than above the cutoff, especially for two-year programs (see Fig. A.1 in Appendix A). This discontinuity in SISBEN distribution can be the result of three factors. (1) SISBEN manipulation, (2) changes in demand of SENA due to YIA, and (3) problems when matching the SENA and SISBEN data.

Using data from a larger sample of youth individuals, we find that the SISBEN distribution is continuous around the cutoff and we can reject the hypothesis that households manipulate their SISBEN scores (see Fig. A.2 in Appendix A).[14] In addition, we provide evidence suggesting that YIA did not increase demand on SENA. The latest may have been driven by the lack of information about the program. For instance, about 42% of SENA enrollees who were eligible for YIA did not sign up for it. Furthermore, among SENA applicants less than 30% knew about the program or conditions for participation.

However, as explained before, we were able to match all YIA participants with their SISBEN score, but for non-YIA applicants the matching rate between SENA and SISBEN was about 68%. Then, if conditional on non-YIA status the probability of data matching was independent to the SISBEN score of those individuals that did not match, we can observe more individuals below the cutoff because we add only YIA applicants with scores below the cutoff. In the appendix we show that after excluding YIA applicants and beneficiaries, the SISBEN score is continuous around the cutoff. Therefore, the discontinuity in the SISBEN distribution comes from imperfect matching between SENA and SISBEN information, and does not compromise identification as it does not depend on applicants' actions.

Regarding continuity of confounding factors around the cutoff, we first show that most observable characteristics do not jump at the cutoff (see Fig. A.5 in Appendix A). However, SISBEN score and YIA cutoffs are also used to select beneficiaries of two other programs: Ser Pilo Paga (SPP), which gives scholarships to college for low-income students at the top of the Saber-11 distribution, and Régimen Subsidiado en Salud (RSS), which offers subsidized health to low-income families.

We first show that SPP and YIA targeted different populations, because SPP focuses on the best high school graduates in the country while SENA's education is usually for high school graduates with lower grades. Also, we provide evidence that RSS had no effect on labor market participation when the individuals applied to SENA. Thus, changes in labor market participation after SENA could be attributed to better labor market opportunities thanks to access to vocational education.

[13] We do not have data of completion from all cohorts and programs. Thus, all our estimates are related to the effect of having at least some SENA education.

[14] Our evidence aligns with Londoño-Vélez et al. (2020) who also shows evidence of non-manipulation for the individuals that took the end of high school exam.

Table 2
Effect of discontinuity in the SISBEN score on enrollment in SENA.

	One-year programs				Two-year programs			
	Female		Male		Female		Male	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SISBEN score $\leq cutoff$	0.050**	0.113	0.046**	0.048	0.105**	0.187*	0.119**	0.190**
	(0.019)	(0.070)	(0.016)	(0.069)	(0.026)	(0.072)	(0.028)	(0.033)
Passed the exam		Yes		Yes		Yes		Yes
Control group mean	0.11	0.43	0.15	0.46	0.13	0.39	0.17	0.40
F test	7.00	2.59	7.97	0.47	15.99	6.81	17.75	33.11
R^2	0.20	0.26	0.20	0.21	0.20	0.19	0.17	0.15
Bandwidth	13.80	12.57	12.07	12.26	16.01	12.46	15.00	17.23
N	10267	1889	9411	2446	17436	4186	16933	6522

Notes: Reported coefficients correspond to β_1 from Eq. (1) where the outcome S_i is the probability that the applicant enrolled in SENA. Standard errors clustered at municipality level, $+p < 0.1$, $* p < 0.05$, $** p < 0.01$. Estimation bandwidths computed following Calonico, Cattaneo, and Titiunik (2014). We use a two-degree polynomial for the distance to cutoff. Control variables include age, entry exam score, FEA eligibility, application year, SISBEN area, program available seats, SENA center fixed effects and SENA degree fixed effects. The F-test is the Cragg–Donald Wald F-statistic of $\beta_1 = 0$ from Eq. (1).

In addition, it is worth noting that τ_Y includes the aggregated effect of monetary aid, preferential entry, and soft skills training offered by YIA. In Appendix B, we show evidence to support that financial aid, more than other benefits, drives YIA's impacts. In brief, we show that the 30% quota reserved for YIA beneficiaries did not systematically bind (see Fig. B.1 in Appendix B). Furthermore, only 6.5% of the total YIA beneficiaries gained a place due to preferential entry. Therefore, all others would have had obtained a seat in SENA even if there was no affirmative action component. Finally, we show that the distribution of exam scores of YIA applicants and non-applicants (eligible and non-eligible) overlapped. Thus, it can be said that preferential entry did not attract individuals with lower abilities than the average SENA applicant. We also show that our main results do not change when we add programs where affirmative action plays no role.

Regarding the soft-skills training component - HPV, we found that it was only taken by less than 3% and less than 30% of YIA beneficiaries in one- and two-year programs respectively. Also, we show that the only positive correlation with labor market participation is for women in two-year programs taking HPV. Hence, as we will show in the following section, if anything, our estimates will underestimate the effect of SENA enrollment on labor market outcomes for women (see Table B.3 in Appendix B).

Finally, comparing OLS and IV estimates to study the existence of liquidity constraints is not exempt from critique. Angrist and Krueger (1991) and Griliches (1977) argue that IV estimates may be larger than OLS coefficients because of measurement error. Additionally, Carneiro and Heckman (2002) discusses the limitations of this type of analysis. First, instruments are usually weak or invalid. Second, even if the instruments are valid, IV estimates can be larger than OLS estimates due to some unobserved comparative advantages in labor markets. Third, instruments do not usually take into account the quality of education at different schools. After showing our results, in Section 5.5 we show evidence of how our strategy overcomes the limitations listed in this literature.

5. Results

We begin by showing evidence that YIA eligibility creates exogenous changes in SENA enrollment. Following Eq. (1), Table 2 shows that being eligible for YIA significantly increases the probability of enrolling in SENA around the cutoff.[15] The effect of being eligible for financial aid is around 5 pp and 11 pp for one-year and two-year programs respectively. In both cases, YIA eligibility represents an important increase in enrollment, as the rate for individuals just above the cutoff were about 15%. However, when we limit the estimation for those applicants who passed SENA's entry exam (even numbered columns), the estimated impact is only statistically significant for individuals who applied to two-year programs.

This is our first piece of evidence for the existence of liquidity constraints. It is important to remember that our analysis only includes individuals who applied to SENA. Therefore, given that the effect of eligibility for financial aid increases with the length of the program, we can infer that financial aid gains importance as the opportunity cost and living expenses increase.[16]

The estimates in Table 2 represent the first stage of the system of equations described in the previous section. Thus, we also report the Cragg–Donald Wald F-statistic for β_1. In the case of one-year programs, where we find smaller impacts, the F-test is smaller than 10. Hence, we exclude one-year programs from our analysis because a weak instrument jeopardizes the comparison between OLS and IV estimates.

5.1. FRD-IV estimations

Comparing OLS and IV estimates helps to determine cases where the average return of schooling for compliers is larger (or smaller) than the average return of the population. In order to make OLS and IV estimations comparable, we fixed the estimation bandwidth to the one computed for our first stage estimations as in Table 2.

Given that our focus is the complier population, we first characterize population following Dahl et al. (2014). Table 3 shows that the fraction of compliers is about 11% of men and women. When we compare their characteristics with the eligible population, the most striking difference is with regard to eligibility for FEA. While only 15% of eligible applicants were eligible for FEA, this proportion rises to 29.8% and 27.7% for women and men, respectively. FEA is the largest CCT program in Colombia and could be the source of information about the supply of other social services. We already explained that the low take-up of YIA was in part given by the lack of information about the program. Hence, FEA eligible applicants may have had access to information about YIA that other SENA applicants did not have.

Compliers (women and men) are also more prevalent among the younger group, and less likely to come from urban areas or apply to SENA center in a Department capital city. Additionally, complier women were more likely to apply to large programs. Complier women and men differ in their position with respect the eligible population in only one characteristic. While complier women are marginally less

[15] Figure C.2 in Appendix C shows the graphical representation of the estimations.

[16] An alternative explanation could be that the differences in the effect of the SISBEN discontinuity come from individuals choosing different program lengths. Nonetheless, we do not find evidence of sorting for one- or two-year programs around the cutoff (Fig. A.5 in Appendix C)

Table 3
Characterization of compliers for men and women who applied to two-year programs.

	Women					Men				
	Full sample	Within optimal bandwidth				Full sample	Within optimal bandwidth			
	All	All	SENA enrollees	Eligible	Compliers	All	All	SENA enrollees	Eligible	Compliers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age (18 to 20)	0.509	0.498	0.559	0.542	0.657	0.502	0.492	0.533	0.531	0.591
Entry exam (more than 55 points)	0.122	0.123	0.242	0.121	0.104	0.125	0.129	0.242	0.128	0.153
Eligible for FEA	0.437	0.147	0.202	0.167	0.298	0.372	0.113	0.159	0.142	0.277
Area 1 (urban)	0.469	0.505	0.408	0.456	0.369	0.475	0.533	0.450	0.465	0.333
Lives in capital city	0.741	0.751	0.643	0.732	0.700	0.773	0.792	0.706	0.759	0.707
Course size (more than 35 seats)	0.428	0.438	0.537	0.434	0.598	0.365	0.367	0.415	0.359	0.382
Population shares	.	.	0.196	0.630	0.106	.	.	0.222	0.567	0.120

Notes: This characterization follows Dahl, Kostøl, and Mogstad (2014), Londoño-Vélez et al. (2020), and Gross and Baron (2022), using estimations by group of Eq. (1). Estimation bandwidths computed following Calonico, Cattaneo, and Titiunik (2014) using enrollment in SENA as outcome variable (as in Table 2). We use a two-degree polynomial for the distance to cutoff. Control variables include age, entry exam score, FEA eligibility, application year, SISBEN area, program available seats, excluding the variable we are testing. All estimations include SENA center fixed effects and SENA degree fixed effects.

Table 4
Effect of enrollment in SENA for applicants to two-year degree programs on formal labor market participation. OLS vs IV estimates.

	Outcome variable: Formal work in the last semester					
	Female			Male		
	OLS	Reduced Form	IV	OLS	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)
SISBEN score $\leq cutoff$		−0.039+			0.084**	
		(0.021)			(0.023)	
Enrolled	0.019		−0.374	0.030**		0.703*
	(0.015)		(0.240)	(0.006)		(0.301)
IV - OLS			−0.394+			0.674**
			(0.202)			(0.198)
Control group mean	0.581	0.581	0.581	0.620	0.620	0.620
F test			15.995			17.749
R^2	0.13	0.13	−0.04	0.14	0.14	−0.21
Bandwidth	16.01	16.01	16.01	15.00	15.00	15.00
N	17436	17436	17436	16933	16933	16933

Notes: Reported OLS coefficients correspond to α_Y from Eq. (2). The IV coefficients correspond to τ_Y from Eq. (3). The outcome Y_i is the probability of working in the formal market for at least one month in the last semester of 2017. Standard errors clustered at municipality level, $+p < 0.1$, $* p < 0.05$, $** p < 0.01$. Estimation bandwidths computed following Calonico, Cattaneo, and Titiunik (2014) using enrollment in SENA as outcome variable (as Table 2) . We use a two-degree polynomial for the distance to cutoff. Control variables include age, entry exam score, FEA eligibility, application year, SISBEN area, program available seats, SENA center fixed effects and SENA degree fixed effects. The F-test is the Cragg–Donald Wald F-statistic of $\beta_1 = 0$ from Eq. (1).

likely to be at the top of the exam distribution, complier men are more likely to get top results in exams. However, the differences are very small and may not account for any differences in our estimates of the returns to SENA education by gender.

Table 4 shows the resulting OLS (α_Y from Eq. (2) in columns 1 and 4) and IV (τ_Y from Eq. (3) in columns 3 and 6) for applicants to two-year programs by gender. The table also shows the difference between IV and OLS estimates for each case.[17] We also show the reduced form estimates (columns 2 and 5) as they help to understand the dimension of the IV estimates. As mentioned before, our outcome variable is the probability of having a formal job for at least one month in the last six months of 2017. The criteria behind this choice is that it avoids capturing the effect of SENA's internship program.[18]

Summarizing, in all cases, OLS estimates are positive and statistically significant (columns 1 and 4). For male applicants, column 6 shows that the FRD-IV estimate is also positive and significantly larger than the OLS estimate. However, this is not the case for women, as the FRD-IV estimate is negative and not statistically significant (column 3). This pattern, FRD-IV larger than OLS only for men but not for women, is consistent in many scenarios. For instance, Table C.3 in Appendix C shows similar results for other labor market outcomes. We find the same pattern also when we estimate the direct effect of YIA on labor market participation (Table C.4 Appendix C) and when we make estimations at the individual level instead of the application level (Table C.5 Appendix C).[19]

Our results for men comply with the idea of liquidity constraints proposed by Card (2001). The observed differences in labor market participation below and above the cutoff (8.4 pp according to column 5), are driven by SENA applicants with large unobserved returns to education who were able to study in SENA because of the availability of YIA. However, the estimations for women show a different picture. We do not find evidence of women facing liquidity constraints for SENA's

[17] To test the difference between IV and OLS we use a control function approach as in the case of linear models Wooldridge (2015) shows how the coefficient of introducing the residual of the first stage estimation into the second stage represents the difference between the OLS estimate and the IV estimate.

[18] Table C.2 in Appendix C shows that our resulting estimates are not statistically different when we constrain the analysis to the probability of having a formal job in the last six months for individuals who applied in 2014, which completely avoids including the internship period. However, the estimates become less precise as the sample size reduces.

[19] In addition, Table C.6 shows smaller effects of SENA on labor market participation when we constraint the sample to applicants whose exam score was above cutoff of the program they apply to. However, the estimates then do not significantly differ from the estimates in Table 4.

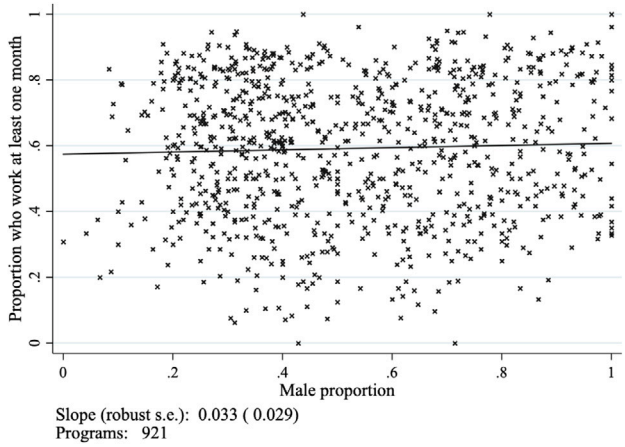


Fig. 1. Average labor market participation and male proportion by SENA program (two-year programs only). Notes: Authors' calculations using SENA and PILA information. *X*-axis shows the proportion of males over total applicants by SENA program. *Y*-axis shows the proportion of applicants who work for at least one month over total applicants by SENA program, only taking into account the period 24 months after they apply to SENA.

tuition-free education. What is more, our estimates show that SENA does not improve women's labor market participation and, if anything, reduces their chances of getting a formal job.

5.2. Gender differences

From the estimates in Table 4 the following questions arise: Are the formal labor market returns to vocational education exclusive to men? Are liquidity constraints unimportant for women? Our previous results suggest that the answer to both questions is yes; however, it is important to point out that all our estimations come from FRD-style estimations. Hence, our results are only relevant for women around SISBEN cutoff and should not be directly expanded. We already know that differences between men and women are more likely to come from unobservable differences than observable characteristics, as the first stage estimates (Table 2) and complier characteristics (Table 3) do not show differences by gender. Hence, multiple hypotheses can explain this pattern.

First, it could be that the expected returns to SENA education are smaller for women than for men. A naive assessment of the estimated returns to vocational education seen in Section 2 does not support this hypothesis. According to our calculations, SENA has an *Internal Rate of Return* above 30% for both men and women.

Second, women may choose to enroll in programs with lower average returns than the programs where men usually enroll. To provide evidence in support or against this hypothesis, we plot the average employment rate and the proportion of males by program in Fig. 1, focusing on two-year programs. If this hypothesis is true, the slope in Fig. 1 should be positive because programs with a larger expected employment rate should have a higher male proportion. However, as shown in Fig. 1, the slope is close to zero and non-significant.

Third, the positive returns to education that we found for men are driven by unobserved differences in ability that exist for men and not for women. However, we can argue that this is not the case. We use SENA's entry exam score as a proxy of ability and show in Fig. A.5 in Appendix A that it is continuous around the cutoff for both genders. Furthermore, Figure C.3 in Appendix C shows that enrollees obtained higher exam scores than individuals who did not enroll, but such a difference is not larger for men than for women. If anything, the difference is larger for women than men. Hence, we cannot argue that applicant or enrolled women have lower ability than applicant or enrolled men.

Fourth, it may be the case that women are more likely to enroll in other types of post-secondary education than men. We do not observe direct enrollment in other sources of tertiary education; however, we do observe participation at the end of tertiary education national exams (Saber-TyT and Saber-Pro). Sitting for these exams is a good proxy of termination of other types of post-secondary education different than SENA. Fig. 2 shows that for men and women that applied to two-year degree programs at SENA, the probability of sitting Saber-TyT Saber-Pro is about 10% and continuous around the cutoffs. Hence, we cannot argue that women who enrolled in SENA are more likely to seek out more tertiary education than men.

The aforementioned evidence rejects the hypotheses of program selection, the quality of enrolled women, and access to further tertiary education as driving forces to explain gender differences in our estimates. Thus, the fact that we do not find positive returns for women may be related to frictions in the labor market.

One final possible explanation is related to the effects of fertility and informality. Our sample includes individuals from 18 to 24 years old (around 20 to 26 years old when we analyze labor market participation). According to our data, labor market participation is larger for men (63% and 56% for men and women, respectively). In addition, in Colombia, by age 25, 51% of women have already had their first child. The percentage increases to 73.5% by age 29 (Profamilia, 2016). Given that we do not observe unemployment, informal labor market participation or the likelihood of being outside of the labor market, the question remains regarding whether women prefer informality or to leave the labor market in order to take care of their children.

5.3. Job quality

Thus far, we have shown strong evidence regarding the effect of a SENA education on men's formal labor market participation. What is more, we have shown evidence that increases in SENA enrollment were due to a reduction in the liquidity constraints for individuals with large expected returns to SENA education. However, do these individuals have access to better jobs? As explained before, we have no access to income data; however, we have information regarding the type of contract and firm the individual is working for.

In this section, we focus on the types of jobs that best explain this effect. Table 5 Panel A shows FRD-IV estimates of the effect of enrollment in SENA on working in the last semester by the size of the firm the person works for. According to our results, individuals who enrolled in SENA because of financial aid eligibility are more likely to be working in very small firms (1 to 10 employees) or large firms (between 200 and 500). Following Attanasio et al. (2017), one can assume that working in larger firms implies higher quality jobs. In addition, Table 5 also shows FRD-IV estimates by different types of contracts. Panel B compares having a job in the private or public sector. Panel C differentiates individuals working as employees from those who are self-employed. Finally, Panel D distinguishes between full-time and part-time contracts. According to our results, SENA education increases the chances of working in the private sector, as an employee, and with a full-time job. All these results head to high quality formal jobs.

5.4. Other possible heterogeneous effects

Lastly, we analyze how our results may change for different types of individuals. Our focus is to further understand the role of liquidity constraints and determine if our estimates change when the economic context is more or less likely to constrain post-secondary education. We separate the analysis by age (18 to 20 and 21 to 24 years old), by SISBEN area (urban and rural), city type (capital city and others), and migration. We focus on men who applied to two-year programs, where, according to our previous results, liquidity constraints are more important.

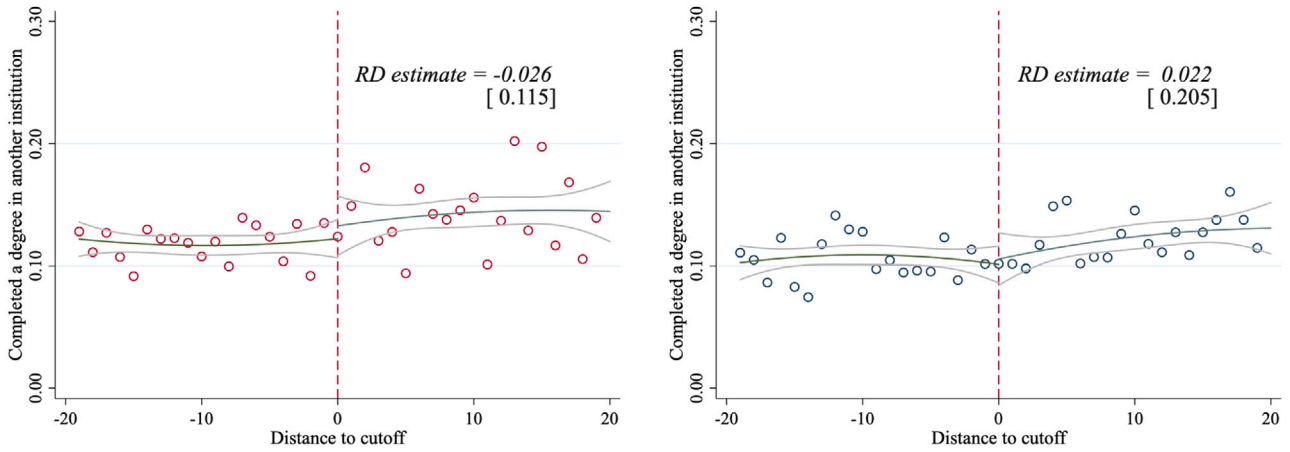


Fig. 2. Completion of other post-secondary education options by distance to the SISBEN cutoff. Applicants to two-year programs at SENA. Notes: Authors' calculations using SENA, SISBEN, and ICFES data. Bin size of 1 point. Adjusted function form using second-degree polynomial including 95% confidence interval. Outcome variable is probability of sitting either Saber-TyT for a non-SENA vocational program, or Saber-Pro exam after applying to SENA. Adjusted function form using two-degree polynomial including 95% confidence interval. Reported RD coefficient and p-value in brackets following Calonico, Cattaneo, and Titiunik (2014).

Table 5
FRD-IV estimates of the effect of SENA enrollment on men's formal labor market participation by firm and contract characteristics.

	Outcome variable: Formal work in the last semester										
	A. Firm size					B. Sector of employment		C. Worker type		D. Job type	
	1–10 (1)	11–50 (2)	51–200 (3)	201–500 (4)	500 or more (5)	Public (6)	Private (7)	Employee (8)	Self-employed (9)	Full-time (10)	Part-time (11)
Enrolled	0.344+	0.051	0.099	0.261**	0.195	0.091	0.620*	0.607**	0.067	0.590*	0.172
	(0.185)	(0.122)	(0.205)	(0.088)	(0.182)	(0.067)	(0.307)	(0.203)	(0.121)	(0.224)	(0.209)
Control group mean	0.10	0.14	0.14	0.10	0.30	0.02	0.60	0.59	0.03	0.59	0.18
F test	17.75	17.75	17.75	17.75	17.75	17.75	17.75	17.75	17.75	17.75	17.75
R^2	−0.16	0.01	−0.00	−0.10	−0.01	−0.05	−0.15	−0.14	−0.01	−0.13	−0.01
Bandwidth	15.00	15.00	15.00	15.00	15.00	15.00	15.00	15.00	15.00	15.00	15.00
N	16933	16933	16933	16933	16933	16933	16933	16933	16933	16933	16933

Notes: Reported IV coefficients correspond to τ_Y from Eq. (3). The outcome Y_i is the probability to work in the formal market for at least one month in the last semester of 2017, in a job with the characteristic named in each column. Full time jobs are defined of jobs with more than 20 days of work in a given month. Standard errors clustered at municipality level, $+p < 0.1$, $* p < 0.05$, $** p < 0.01$. Estimation bandwidths computed following Calonico, Cattaneo, and Titiunik (2014) using enrollment in SENA as outcome variable (as Table 2). We use a two-degree polynomial for the distance to cutoff. Control variables include age, entry exam score, FEA eligibility, application year, SISBEN area, program available seats, SENA center fixed effects and SENA degree fixed effects. The F-test is the Cragg–Donald Wald F-statistic of $\beta_1 = 0$ from Eq. (1).

Figure C.4 Appendix C shows the estimated coefficients of SISBEN discontinuity on enrollment (β_1 from Eq. (1)), and the FRD-IV estimates (τ_Y from Eq. (3)) by population groups for women and men who applied to two-year programs. We did not find strong differences by group that could help us to further understand differences in liquidity constraints.

5.5. Possible flaws and caveats

According to Carneiro and Heckman (2002), comparing IV and OLS estimates has some significant weaknesses. First, instruments in the literature are usually invalid or weak. We have already shown that YIA eligibility is a strong instrument of enrollment in two-year programs at SENA. Furthermore, in Appendix A, we show evidence supporting the validity of our instruments. Hence, our instrument is strong and valid.

Second, the difference between IV and OLS can be explained by differences in an individual's comparative advantage in the labor market. This drawback is common when instruments come from regional variations in the cost of education. In our estimations we control for each SENA location and SENA degree fixed effects. Therefore, applicants on both sides of each cutoff are likely to be facing the same labor markets. In addition, following the assumption of continuity of observable and unobservable factors around the cutoff, one can argue that an individual's comparative advantage in the labor market does not radically change around the cutoffs.

Finally, the OLS and IV comparison ignores choice of school quality. In our analysis all individuals applied to the same institution (SENA), and the use of fixed effects allows us to compare individuals who applied to the same degree at the same location at the same time. Furthermore, we show that eligibility for financial aid did not increase the number of two-year degree applications. Therefore, the quality argument does not apply in this setup.

In addition, according to Angrist and Krueger (1991) and Griliches (1977), another reason why IV estimates are larger than OLS estimates could be due to measurement error, which could overcome the effect of the unobserved ability bias. In our case, we can argue that the possibility of measurement error comes from alternatives to post-secondary education aside from SENA. Individuals above the SISBEN score cutoff may be more likely to look for other types of post-secondary education, for instance a college degree. As explained above, we do not observe enrollment in other tertiary education institutions, but we do observe whether each individual took a Saber-TyT or Saber-Pro test after applying to SENA. We use this as a proxy of completion of other types of tertiary education. Fig. 2 shows that regardless of the gender, for two-year program applicants, there is no significant change in this probability around the cutoff.

6. Discussion and final remarks

Reducing tuition fees is an important strategy for policy-makers who want to make post-secondary education available to individuals from low-income households. Therefore, it is worth asking why some people do not enroll in free vocational education even when there are monetary incentives available. High school graduates may not attain more education because they perceive low returns to post-secondary education, because they do not want more education (low preferences), or because they may still face liquidity constraints. Our results provide new evidence that even when education is tuition-free, low-income individuals do indeed still face liquidity constraints.

Our results align with the evidence found by Londoño-Vélez et al. (2020) in Colombia. For example, by the time YIA offered a grant to study at SENA, the program SPP also offered a substantial grant, including complete coverage of tuition fees, to support high-achieving individuals to go to college. Londoño-Vélez et al. (2020) show that SPP increased enrollment in post-secondary education by 27.5 percentage points (a 48% increase). We show that in the case of free education, eligibility for financial aid increased enrollment in SENA by 11 percentage points (a 64% increase). Even though the compliers in both cases may not be comparable, policies that support students with non-direct costs have impacts that are comparable with policies that cover tuition fees.

It is also important to understand who the applicants are in the comparison group for our estimations of returns to enrollment in SENA, meaning those who did not enroll in SENA because they were not eligible for YIA. The following question arises: if they do not enroll in SENA, what do they do afterwards? First, we already show in Fig. 2 that applicants above the cutoff are not less or more likely to enroll in other types of tertiary education. In addition, Figure C.5 in Appendix C shows the probability of having a formal job by month for applicants to two-year programs in the first semester of 2015. We show that for both genders, before applying the probability of having a job is the same for individuals who enrolled or who did not enroll. However, for the year after applying, SENA enrollees are less likely to keep working than those who did not enroll. Hence, one can expect that the comparison we make when we estimate the returns to SENA education is between individuals who study in SENA for two-years, and individuals who keep working instead of studying.

We do know that liquidity constraints matter in the case of free education. However, when tuition fees are removed, which costs drive investments in education? Is it the opportunity cost or the maintenance cost? In Section 5.4 we tested our hypothesis under different scenarios, aiming to disentangle the opportunity cost from the living expenses. Unfortunately, our results were inconclusive. For this reason, we complement our analysis using survey data collected from a sample of YIA beneficiaries in order to understand how they spend the program's bimonthly cash transfer (Econometría-S.A. & SEI, 2017). According to the data, 74% spend a fraction of the transfer on transportation, which is clearly part of the costs of attending SENA. Furthermore, 65% use part of the transfer for household cleaning expenses, which could be understood as helping at home (opportunity cost). According to the survey, YIA beneficiaries use about 30% of the transfer on direct living expenses (transport, food outside the home and educational expenses) and about 70% of the expenses for home support that can be likened to an opportunity cost. Once again, the evidence is inconclusive so we cannot argue that only one cost explains our results.

Finally, our paper borrows from previous papers such as Cameron and Taber (2004) using a comparison between IV and OLS estimates to test the existence of liquidity constraints. However, Cameron and Taber (2004) based the identification of liquidity constraints on the claim that direct costs (tuition fees) and indirect costs (opportunity cost) affect constrained and unconstrained individuals differently. Hence, IV estimates are larger than OLS estimates when the instrument affects direct costs alone. In that sense, our strategy does not work because our analysis does not include tuition fees. According to Cameron and Taber (2004), credit-constrained individuals are those who borrow with an interest rate above the market interest rate. Thus, if individuals only borrow when they need to pay tuition, the differences in educational choices between constrained and unconstrained individuals will only appear with the presence of tuition fees. As such, without tuition fees they should obtain the same level of education. This may be the case for college applicants in developed economies. However, this may not be the case for low-income youth in developing countries. In an impoverished environment, if an individual wants to stay out of the labor market while studying for a period of, for example, two years, they may need to borrow in order to cover a minimum level of living costs. Therefore, the difference between constrained and unconstrained individuals is found in the interest rate at which they are willing to borrow. Following this idea, constrained individuals will be more sensitive to changes in the cost of education (for example when a grant is offered) than unconstrained individuals, and the logic behind Cameron and Taber (2004) applies to this analysis as well.

To conclude, as free education gains importance in the public policy agenda in many developing and low-income countries, this paper provides evidence of the existence of liquidity constraints, even in the absence of tuition fees. Our results shed light on the limitations of free education policies by quantifying the importance of other costs in explaining enrollment in tuition-free post-secondary education.

Declaration of competing interest

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

Data availability

The authors do not have permission to share data.

Appendix A. Validity of the RD design

Following the RD literature (Imbens & Lemieux, 2008; Lee & Lemieux, 2010), even though we cannot observe whether potential outcomes are continuous around the cutoff, it is nonetheless possible to assume continuity of these outcomes if observable variables and the distribution of the forcing variable are continuous around the cutoff.

First, we check whether the distribution of the SISBEN score is continuous around the cutoff. Fig. A.1 shows the SISBEN distribution by gender and program length, and the *p-value* of the resulting continuity test by McCrary (2008). The results are not satisfactory, especially for two-year programs, as we reject the hypothesis that the distribution is continuous. This discontinuity can be the result of three factors.

First, applicants could have manipulated their SISBEN in order to be eligible for YIA, which would temper identification. We only observe SISBEN score for SENA applicants. Fig. A.2 shows SISBEN distribution using information from a representative sample of households from 2011 to 2014 for individuals 18 to 28. The figure also includes the McCrary (2008) test. Both the distribution and the test show that households do not manipulate their scores. In addition, Londoño-Vélez et al. (2020) uses data from a larger sample of young individuals finishing high school and shows evidence of no manipulation of SISBEN score.

Second, YIA may have had an effect on the demand for SENA, and for that reason we observe more applicants below than above the cutoff. Even though we cannot directly test this, we provide suggesting evidence that demand for SENA did not increase since YIA started. Fig. A.3 shows that since 2007, tertiary education in Colombia has increased at a constant rate. SENA enrollment shows the same trend and the proportion of SENA enrollees did not radically change when

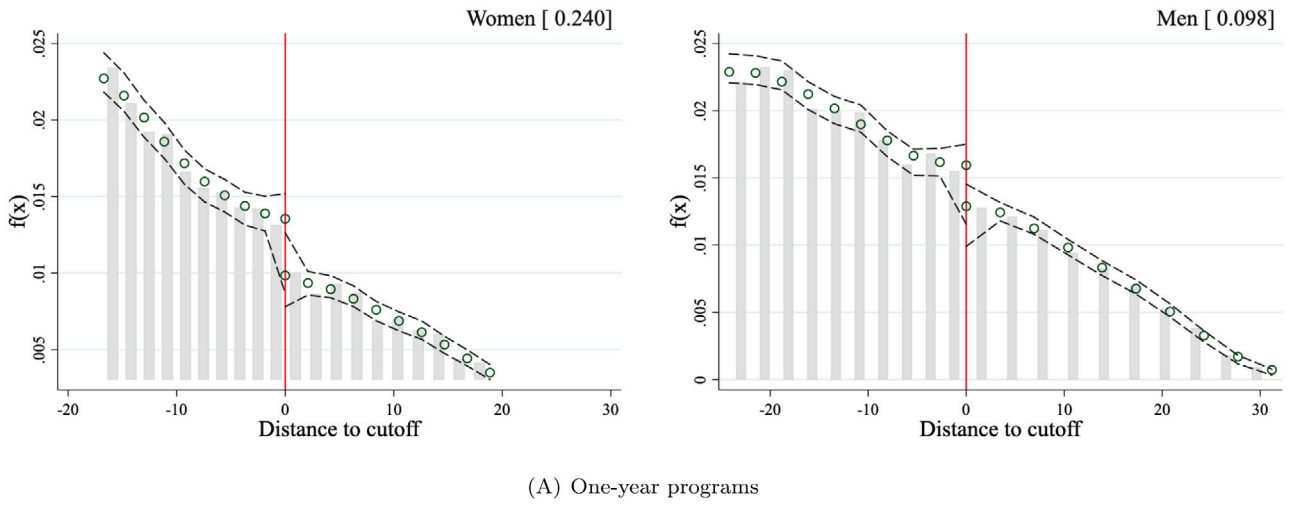


Fig. A.1. Manipulation test - SISBEN distribution around the cutoff by program length and gender. Notes: Density estimations following Cattaneo, Jansson, and Ma (2020). Adjusted 95% confidence intervals in dashed lines. Squared brackets [ ] contain the *p*-value of the difference at the cutoff following McCrary (2008).

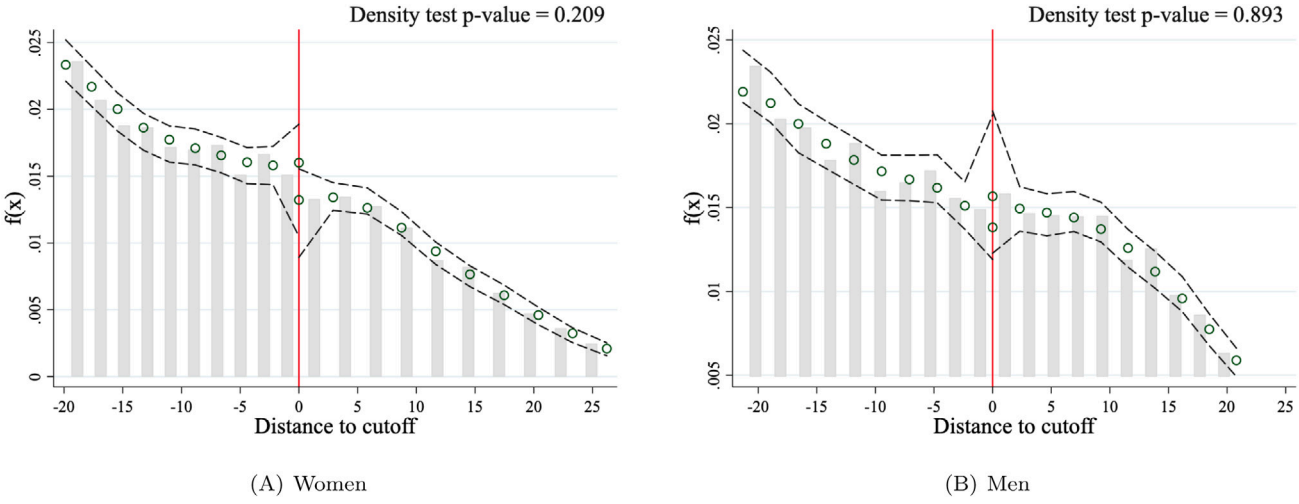
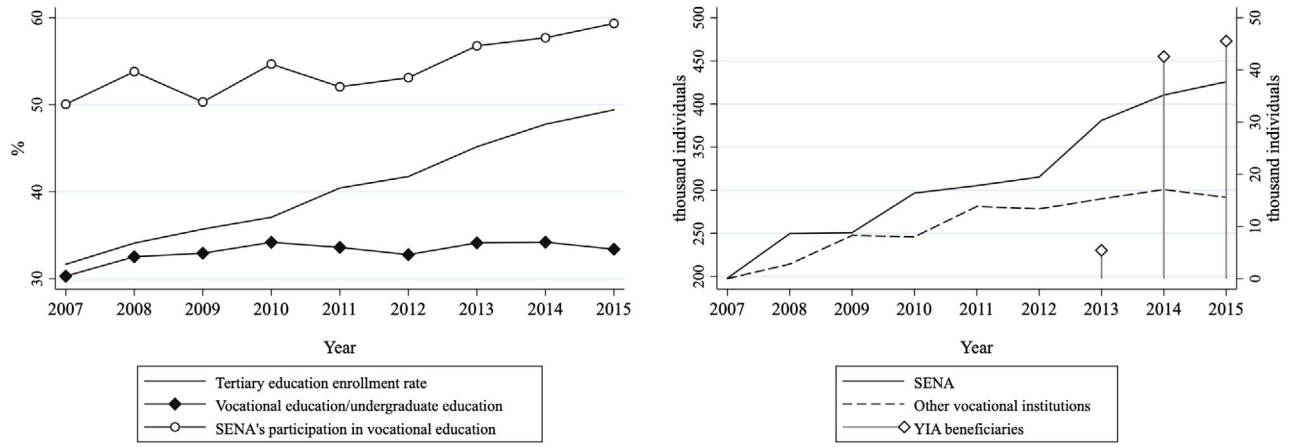


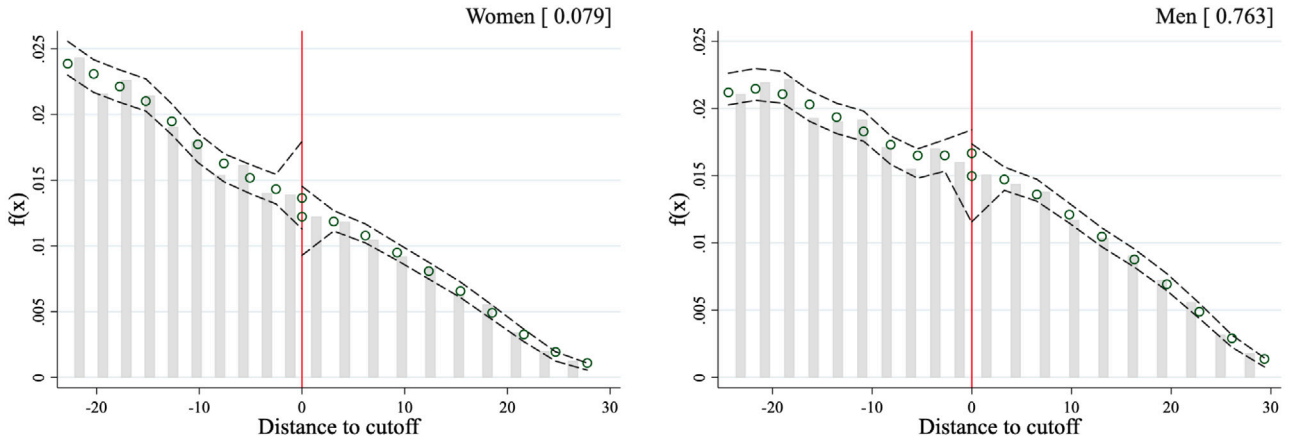
Fig. A.2. Manipulation test - SISBEN distribution around the cutoff by gender. Individuals with secondary education age 18 to 28 years old. Notes: Density estimations following Cattaneo et al. (2020). Adjusted 95% confidence intervals in dashed lines. Squared brackets [ ] contain the *p*-value of the difference at the cutoff following McCrary (2008). Using information from a representative sample of all households interviewed for SISBEN in the 14 main cities, from 2011 to 2014 (ANDA-DNP).

YIA passed from its pilot period to enrollment of beneficiaries nationwide (see the change from 2013 to 2014 in panel B). Also, survey data from Econometría-S.A. and SEI (2017) shows evidence suggesting that the program was not widely advertised and most SENA applicants did

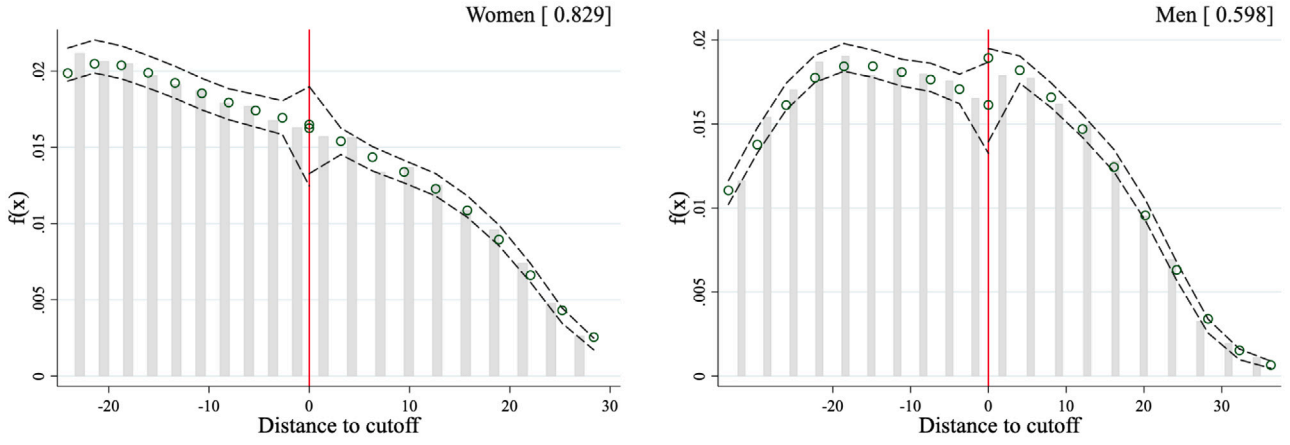


(A) Enrollment rate in tertiary and vocational education (B) Enrollees in vocational education by institution

Fig. A.3. Enrollees in tertiary education, vocational education and SENA. Total and participation rates. Notes: Data from SNIES (Sistema Nacional de Información de la Educación Superior, in Spanish), SENA and YIA. Tertiary education enrollment rate measured as the proportion of individuals enrolled in any type of tertiary education over total number of individuals from 17 to 21 years old. Vocational education includes SENA and vocational education in private institutions and universities. Panel B left *y*-axis for enrollees in SENA and Other vocational institutions, right *y*-axis for YIA enrollees.



(A) One-year programs



(B) Two-year programs

Fig. A.4. Manipulation test - SISBEN distribution around the cutoff by program length and gender excluding YIA applicants and beneficiaries. Notes: Density estimations following Cattaneo et al. (2020). Adjusted 95% confidence intervals in dashed lines. Squared brackets [ ] contain the *p*-value of the difference at the cutoff following McCrary (2008).

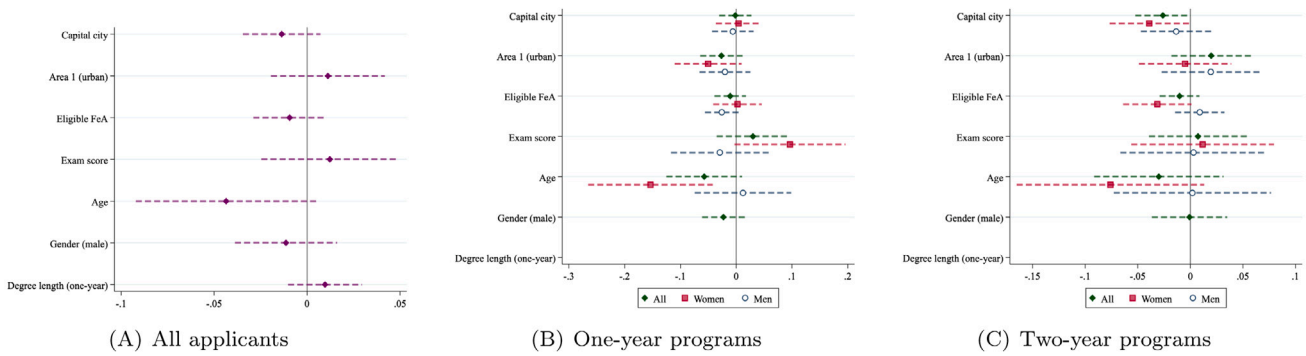


Fig. A.5. Continuity in observables around the SISBEN cutoff. Notes: The Figure shows the resulting coefficients and its 95% confidence interval (dashes lines) for the estimation of an RD model Calonico, Cattaneo, Titiunik, et al. (as in 2014), where the SISBEN score is the forcing variable and each indicator is the outcome variable. All the estimations use a two-degree polynomial for the distance to cutoff.

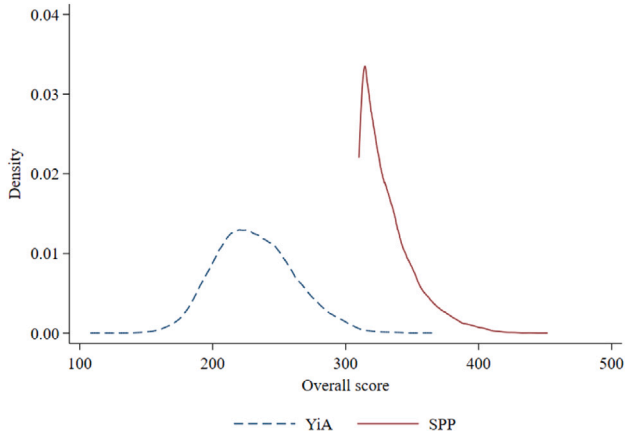


Fig. A.6. Saber-11 exam score distribution for participants in YIA and SPP (2014–2015) *Source:* ICFES. The Figure shows the distribution of the Saber-11 overall score for beneficiaries of YIA and SPP between 2014 and 2015..

not know about YIA. Among SENA applicants, 77% did not know if they met conditions to apply to YIA. As a result, among eligible individuals who enrolled in SENA only 36% were YIA beneficiaries.

Finally, the match between SENA and SISBEN data sets was not perfect. For YIA beneficiaries, matching was easy as they need to show their SISBEN score to be part of the program. For non-YIA beneficiaries, we were only able to match 68% of the applicants. Hence, matching was better below the cutoff than above the cutoff. Fig. A.4 shows how the discontinuities around the cutoff disappear when we exclude YIA applicants and beneficiaries, which was the population with whom we did not have problems matching SENA and SISBEN data.

Regarding continuity of confounding factors, we start by showing how a set of observable characteristics are continuous over the cutoffs. Fig. A.5 shows the resulting estimates of RD estimates using each control variable as an outcome variable following Calonico, Cattaneo, and Titiunik (2014). Apart from the age of women applying to one-year programs, no other variables have significant differences around the cutoffs. However, policy makers in Colombia at the national and regional level use SISBEN score to select beneficiaries for many other social programs. We reviewed all these programs and found that two other programs, Ser Pilo Paga (SPP) and Régimen Subsidiado en Salud (RSS) use the same cutoffs and target similar individuals than YIA.

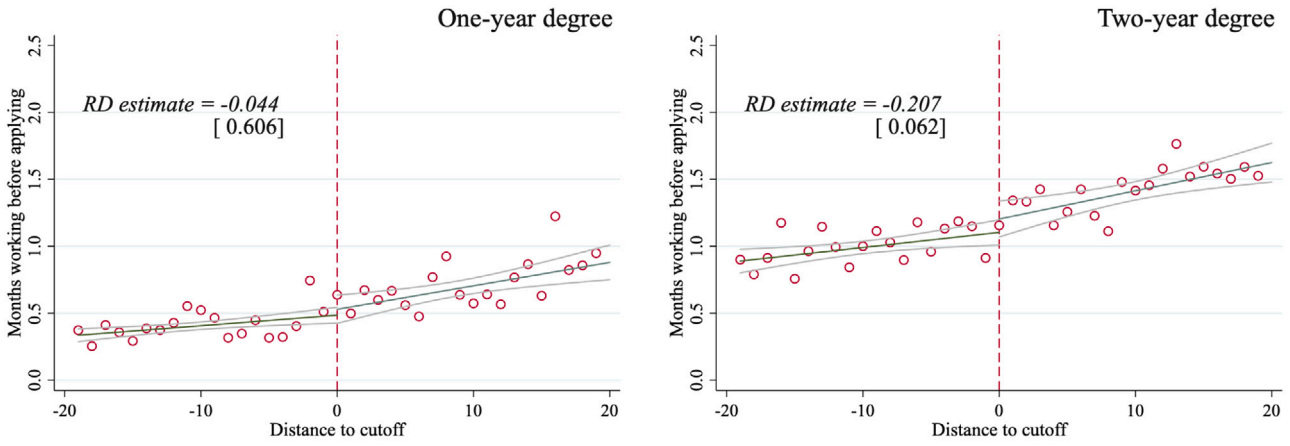
SPP offered a scholarship for college to low-income high school graduates who are at the top of the distribution of the national end of high-school exam Saber-11 (Londoño-Vélez et al., 2020). The program uses the same SISBEN cutoffs as YIA. Hence, it could be expected that changes in SENA enrollment around the cutoff are the joint result of both YIA and SPP. However, the target population of both programs are not the same. First, SPP applicants are individuals who are in their last year of high school, and our sample of analysis excludes applicants younger than 18 due to data constraints (see Section 3). As a result, we are very likely to exclude those individuals who applied to SENA when they were in their last year of high school. In addition, Fig. A.6 shows the distribution of Saber-11 scores for both YIA and SPP beneficiaries, for our period of analysis. It can be seen that SPP targets only the top of the distribution, as it was designed to help elite students, while YIA beneficiaries include individuals over the rest of the Saber-11 distribution.

The second program that uses YIA's SISBEN cutoffs for targeting is RSS, which provides access to medical services without paying periodic contributions and reduced co-payments to households (Prosperidad-Social, 2014). In order to be part of RSS, households should be below the cutoff and not have any members working in the formal sector. Hence, identification could be compromised because individuals just below the cutoff are not only eligible for YIA but also for RSS. Possible bias comes from correlation between the decision to demand education at SENA, and access to RSS benefits. On the one hand, this correlation may be negative because seeking tertiary education in SENA is the sign of a desire to obtain a job in the formal labor market; however, once an individual signs a contract, she and her dependents lose their RSS benefits. On the other hand, RSS means lower expenditure on healthcare, and as a result, individuals may face less pressure to work after finishing high school and therefore have more income to cover the costs of studying at SENA.

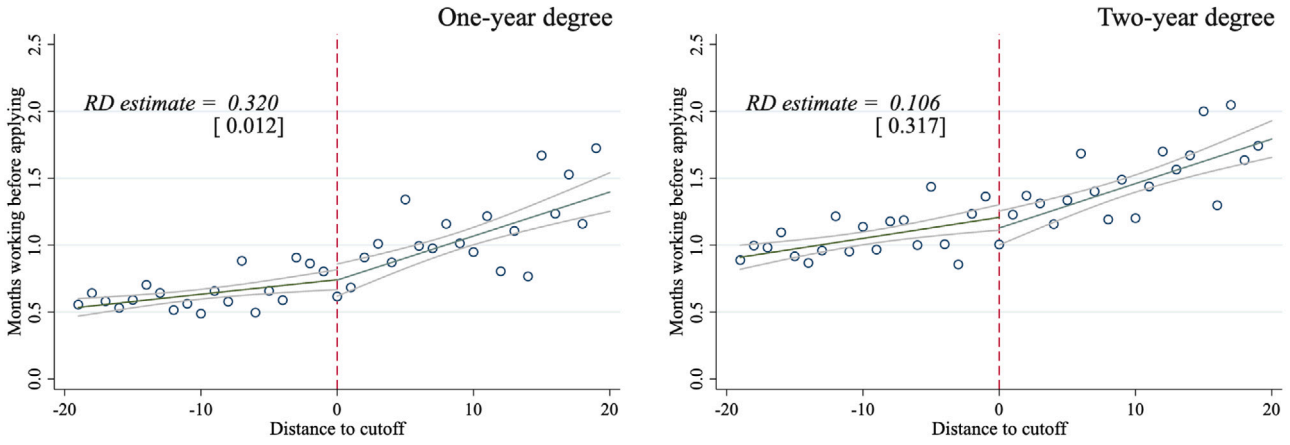
In order to address the effect of RSS on our sample of analysis, we estimate changes in labor market participation for a subset of applicants before they apply to SENA around the cutoffs. We observe five months of formal labor market outcomes for applicants to SENA in first semester of 2015. Fig. A.7 shows the correlation between SISBEN score and how many months an applicant worked between August and December 2014, for the subset of applicants we just described. We do not observe individuals participating less in the labor market just below the cutoff. Therefore, we argue that even if YIA and RSS share the same cutoff, for our population of interest RSS seems to have a null effect. Thus, RSS does not affect our estimations of the returns to SENA education.

Appendix B. Affirmative action, soft-skills training or financial aid

As mentioned in Section 4, an estimate of τ_Y from Eq. (3) represents the effects of all YIA components on outcome Y. In this section we show that, despite preferential entry helping some individuals to access SENA, monetary aid explains most of our results. Table B.1 shows the effect of affirmative action on the composition of SENA applicants and enrollees. We also show that HPV, a soft-skills training offered by YIA, had limited reach and that its participants did not do better in formal job-market after studying in SENA.



(A) Women



(B) Men

Fig. A.7. Effect of SISBEN discontinuity on formal labor market participation before applying to SENA. Notes: Authors' calculations using SENA applications, SISBEN and PILA data. Bin size of 1 point. Outcome variable is the number of months worked from August to December 2014 for individuals who applied to SENA in 2015. Adjusted function form using second-degree polynomial including 95% confidence interval. Reported RD coefficient and *p*-value in brackets following Calonico, Cattaneo, and Titiunik (2014).

Table B.1
Preferential entry effect on composition of SENA.

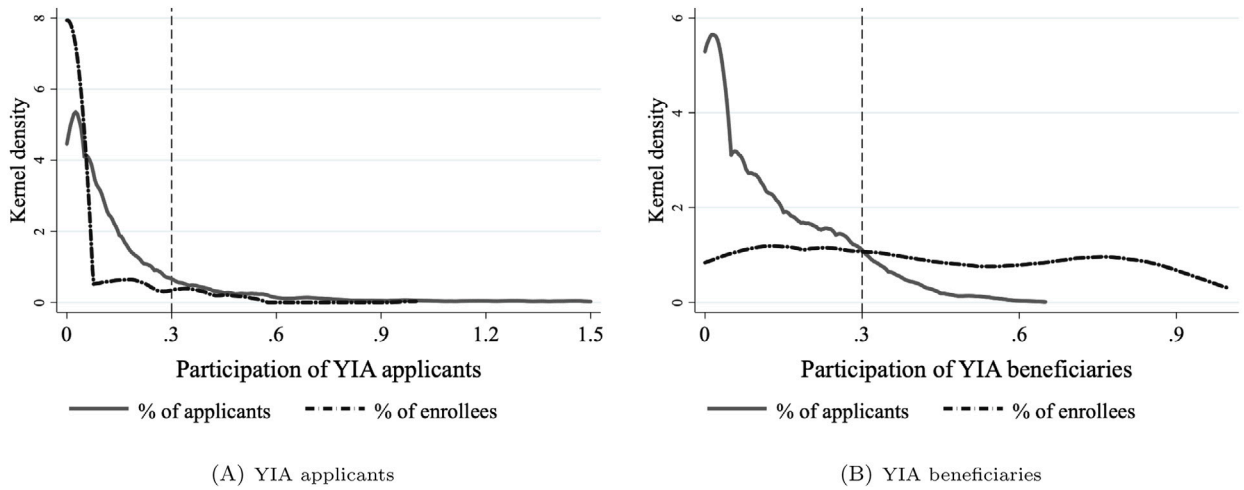
	Never offered first	First offer due to AA	Lost first offer due to AA	Always offered first
Entry exam's score	41.980	43.423	49.293	51.431
	(0.470)	(0.526)	(0.790)	(0.474)
% enrolled	0.071	0.467	0.289	0.503
	(0.008)	(0.026)	(0.025)	(0.017)
Individuals	78124	4793	3022	33631

Notes: Authors' calculations using data from SENA. Standard errors clustered at municipality level in parentheses. Simulations based on entry exam results using only programs with excess demand and at least one YIA applicant. We calculated the entry exam cutoff of each program in the following way. For each program we organize all applicants according their entry exam for the largest to the lowest. The unconditional cutoff c_U is the score of the N individual where N is the number of available seats. Then we organize all non-YIA applicants and c_{NoYIA} is the score of the N_{NoYIA} non-YIA applicant where $N_{YIA} = 0.7N$. Finally, we ordered YIA applicants by their entry exam and define c_{YIA} is the score of the N_{YIA} YIA applicant where $N_{YIA} = 0.3N$. A YIA applicant with score E_i gets an offer thanks to the AA if $c_{NoYIA} \leq E_i < c_U$. A non-YIA applicant with score E_i losses the first offer due to the AA if $c_U <= E_i < c_{NoYIA}$.

As we can see, 4,793 individuals received an offer due to the preferential admission of the YIA. This means that without being part of YIA, their score would not have been enough to receive a first offer to enroll in a program. Among these individuals, 47% enrolled in SENA. Furthermore, 3,022 individuals who were not part of YIA did not get a first offer because their exam cutoff increased due to YIA's preferential admission. Nevertheless, 29% did enroll in SENA. This means the preferential admission policy affected 6.5% of total applicants.

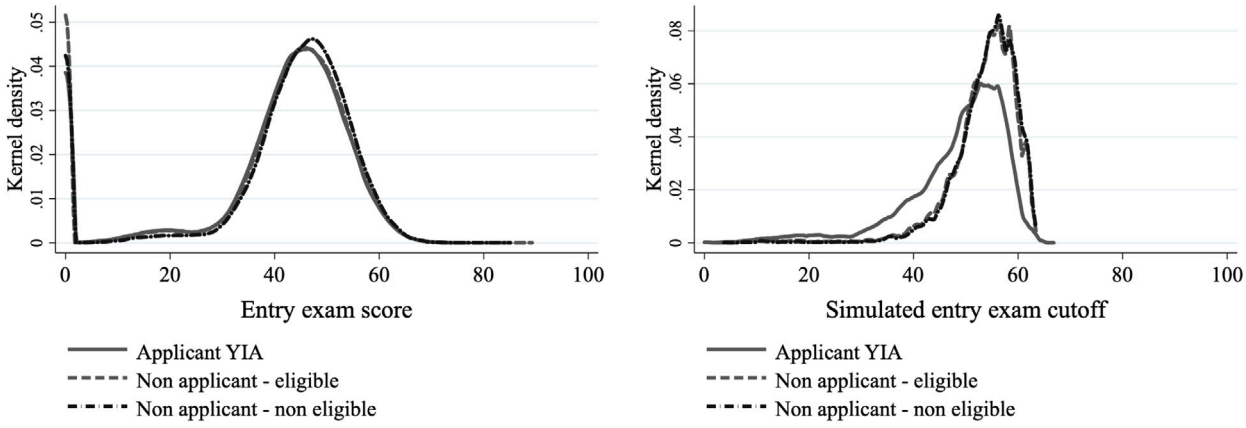
In addition, Fig. B.1 shows both the proportion of YIA applicants and beneficiaries among the total number of applicants and enrollees per program. None of these distributions jump around the preferential entry quota. The continuity of these distributions around 30% shows that preferential admission is not a binding constraint in most degrees, which is evidence of its low importance.

We can also test if the affirmative action program changed the ability distribution among SENA applicants and enrollees. Panel a in Fig. B.2 shows that there is no difference in exam distribution among YIA applicants and non-applicants (neither eligible nor non-eligible applicants). Therefore, one can say that YIA does not attract better or worse applicants. However, panel b shows how the simulated exam entry cutoff distribution for possible YIA beneficiaries moves left with respect to the distribution of those who did not request YIA benefits. Nevertheless, when examining the distribution of individuals who finally enroll in SENA, there are no differences in the exam score of YIA applicants and non-applicants. Thus, the distribution of abilities of individuals enrolled in SENA was not dependent on the effect of the affirmative action program on enrollment.

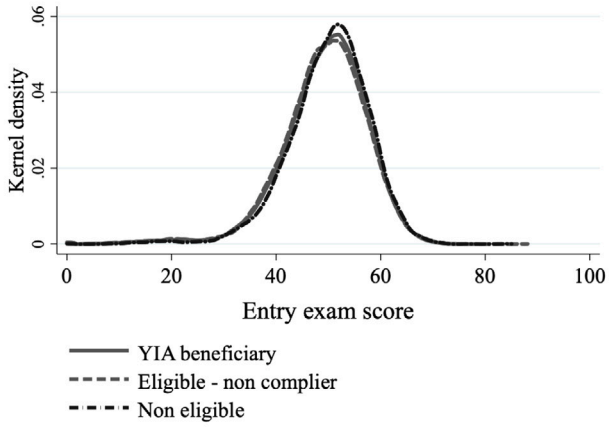


(A) YIA applicants (B) YIA beneficiaries

Fig. B.1. Importance of preferential entry: Distribution of the proportion of YIA applicants and YIA enrollees in a given program. Notes: Authors' calculations using data from SENA applicants. Panel A uses all applications to YIA at the moment of application to SENA and the sitting of the entrance exam. Panel B uses YIA beneficiaries after enrolling in SENA. Proportion measured at the program level.



(A) Applicants (B) Entry cutoff



(C) Enrolled

Fig. B.2. SENA entry exam and distribution of entry cutoffs for YIA applicants and non-applicants and enrollees. Notes: Authors' calculations using data from SENA applicants. We calculated the exam cutoff for each program in the following way. For each program we organize all applicants according their entry exam from the highest to the lowest. The unconditional cutoff c_U is the score of the N individual where N is the number of available seats. Then we organize all non-YIA applicants and c_{NoYIA} is the score of the N_{NoYIA} non-YIA applicant where $N_{YIA} = 0.7N$. Finally, we ordered YIA applicants by their entry exam and define c_{YIA} is the score of the N_{YIA} YIA applicant where $N_{YIA} = 0.3N$. For YIA applicants the cutoff will be the minimum between c_U and c_{YIA}. For non-YIA applicants the cutoff is the minimum between c_U and c_{NoYIA}.

Table B.2
Effect of SISBEN score discontinuity on enrollment in SENA by demand of the program.

	One-year programs				Two-year programs			
	Female		Male		Female		Male	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SISBEN score $\leq cutoff$	0.050*	0.054**	0.046**	0.053**	0.105**	0.102**	0.119**	0.124**
	(0.019)	(0.015)	(0.016)	(0.017)	(0.026)	(0.027)	(0.028)	(0.025)
Includes under-demanded courses		Yes		Yes		Yes		Yes
Control group mean	0.11	0.11	0.15	0.15	0.13	0.14	0.17	0.18
F test	6.96	12.11	8.02	9.63	16.00	14.45	17.73	23.83
R^2	0.20	0.19	0.20	0.19	0.20	0.20	0.17	0.17
Bandwidth	13.80	16.38	12.07	12.07	16.01	16.35	15.00	14.07
N	10267	12984	9411	9888	17436	19290	16933	17279

Notes: Reported coefficients correspond to β_1 from Eq. (1) where the outcome S_i is the probability that the applicant enrolled in SENA. Standard errors clustered at municipality level, $+p < 0.1$, $* p < 0.05$, $** p < 0.01$. Estimation bandwidths computed following Calonico, Cattaneo, and Titiunik (2014). We use a two-degree polynomial for the distance to cutoff. Control variables include age, entry exam score, FEA eligibility, application year, SISBEN area, program available seats, SENA center fixed effects and SENA degree fixed effects. The F-test is the Cragg–Donald Wald F-statistic of $\beta_1 = 0$ from Eq. (1).

Table B.3
Effect of HPV on formal labor market participation for YIA beneficiaries by program-length and gender.

	Outcome variable: Formal work in the last semester			
	One-year programs		Two-year programs	
	Female	Male	Female	Male
	(1)	(2)	(3)	(4)
HPV taker	0.078	−0.042	0.086**	0.035
	(0.074)	(0.086)	(0.026)	(0.024)
Proportion with HPV	0.028	0.024	0.282	0.221
Control group mean	0.336	0.411	0.472	0.554
R^2	0.187	0.265	0.218	0.217
N	2484	1724	3767	3345

Notes: Reported coefficients correspond to π_1 for estimations of $Y_i = \pi_1 HPV + X_i \Pi + \psi_i$ where the outcome Y_i is the probability that the applicant has a formal job at least one month in the last semester of 2017. Standard errors clustered at municipality level, $+p < 0.1$, $* p < 0.05$, $** p < 0.01$. Control variables include SISBEN score, age, entry exam score, FEA eligibility, application year, SISBEN area, program available seats, SENA center fixed effects and SENA degree fixed effects.

Finally, we test the effect of the YIA eligibility also including programs without excess of demand. If the effect of YIA was driven by the AA, including these programs should reduce our estimates, because in these programs the AA does not affect applicants' decisions. Table B.2 shows that, if anything, adding programs without excess of demand marginally increases the effect of YIA, supporting our claim that monetary support was the main driver of the impact of YIA on enrollment.

Regarding soft-skills training, HPV had a limited reach within YIA beneficiaries (see Table B.3). For one-year programs less than 3% took HPV, although this proportion rises to 28% and 22% for women and men in two-year programs. However, despite the low take-up, participating in HPV could have affected labor market participation as it aimed, among other objectives, to prepare individuals for interviews and helped them to write a better CV. Table B.3 shows conditional differences in having a formal job in the last semester of our study for YIA beneficiaries with and without HPV. These are not causal estimates, but they help to understand how HPV could affect our main estimations. According to the table, only women in two-year programs who took HPV had a larger probability to get a job after SENA. The increase is about 20% with respect to women in YIA who did not take HPV.

However, our main estimates show a null or negative impact of YIA eligibility on labor market participation. As such, if HPV does indeed improve the situation of some women, in Section 5 we are underestimating the final effect of financial on labor market participation for women.

Appendix C. Supplementary tables and figures

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.econedurev.2023.102433.

References

Angrist, J. D., & Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *Quarterly Journal of Economics*, *106*(4), 979–1014.

Attanasio, O., Battistin, E., Fitzsimons, E., Mesnard, A., & Vera-Hernández, M. (2005). How effective are conditional cash transfers? evidence from Colombia. *IFS Working paper series*, (p. 1). Institute for Fiscal Studies.

Attanasio, O., Guarín, A., Medina, C., & Meghir, C. (2017). Vocational training for disadvantaged Youth in Colombia: A long-term follow-up. *American Economic Journal: Applied Economics*, *9*(2), 131–143.

Attanasio, O., Kugler, A., & Meghir, C. (2011). Subsidizing vocational training for disadvantaged youth in Colombia: Evidence from a randomized trial. *American Economic Journal: Applied Economics*, *3*(3), 188–220.

Belzil, C., Maurel, A., & Sidibé, M. (2021). Estimating the value of higher education financial aid: Evidence from a field experiment. *Journal of Labor Economics*, *39*(2), 361–395.

Bernasconi, A., & Celis, S. (2017). Higher education reforms: Latin America in comparative perspective. *Education Policy Analysis Archives*, *25*, 67.

Brown, M., Karl Scholz, J., & Seshadri, A. (2011). A new test of borrowing constraints for education. *Review of Economic Studies*, *79*(2), 511–538.

Cáceres-Delpiano, J., Giolito, E., & Castillo, S. (2018). Early impacts of college aid. *Economics of Education Review*, *63*, 154–166.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, *82*(6), 2295–2326.

Calonico, S., Cattaneo, M. D., Titiunik, R., et al. (2014). Robust data-driven inference in the regression-discontinuity design. *Stata Journal*, *14*(4), 909–946.

Cameron, S. V., & Taber, C. (2004). Estimation of educational borrowing constraints using returns to schooling. *Journal of Political Economy*, *112*(1), 132–182.

Card, D. (2001). Estimating the return to schooling: Progress on some persistent econometric problems. *Econometrica*, *69*(5), 1127–1160.

Carneiro, P., & Heckman, J. J. (2002). The evidence on credit constraints in post-secondary schooling. *The Economic Journal*, *112*(482), 705–734.

Carroll, A. B., & Ihnen, L. A. (1967). Costs and returns for two years of postsecondary technical schooling: A pilot study. *Journal of Political Economy*, *75*(6), 862–873.

Cattaneo, M. D., Jansson, M., & Ma, X. (2020). Simple local polynomial density estimators. *Journal of the American Statistical Association*, *115*(531), 1449–1455.

Chakravarty, S., Lundberg, M., Nikolov, P., & Zenker, J. (2019). Vocational training programs and youth labor market outcomes: Evidence from Nepal. *Journal of Development Economics*, *136*, 71–110.

Dahl, G. B., Kostøl, A. R., & Mogstad, M. (2014). Family welfare cultures. *Quarterly Journal of Economics*, *129*(4), 1711–1752.

de Gayardon, A. (2019). There is no such thing as free higher education: A global perspective on the (many) realities of free systems. *Higher Education Policy*, *32*(3), 485–505.

Didriksson, A. (2018). *The new agenda for the transformation of higher education in Latin America*: *Technical report*, Global University Network for Innovation.

Econometría-S. A., & SEI (2017). *Informe de evaluación de impacto del programa Jóvenes en Acción*: *Technical report*, Departamento de Prosperidad Social - Colombia.

Elgin, C., Kose, M. A., Ohnsorge, F., & Yu, S. (2021). *Understanding informality*: *CERP discussion paper (16497)*, Centre for Economic Policy Research, London.

Espinoza, R., & Urzúa, S. (2015). Las consecuencias económicas de un sistema de educación superior gratuito en Chile. *Revista de Educación Nº 370. Octubre-Diciembre 2015*, Article 2015010.

Flannery, D., & O'Donoghue, C. (2013). The demand for higher education: A static structural approach accounting for individual heterogeneity and nesting patterns. *Economics of Education Review*, *34*, 243–257.

Fuller, W. C., Manski, C. F., & Wise, D. A. (1982). New evidence on the economic determinants of postsecondary schooling choices. *Journal of Human Resources*, 477–498.

González-Velosa, C., Rucci, G., Sarzosa, M., & Urzúa, S. (2015). *Returns to higher education in Chile and Colombia*: *Technical report*, Inter-American Development Bank.

Griliches, Z. (1977). Estimating the returns to schooling: Some econometric problems. *Econometrica*, 1–22.

Gross, M., & Baron, E. J. (2022). Temporary stays and persistent gains: The causal effects of foster care. *American Economic Journal: Applied Economics*, *14*(2), 170–199.

Imbens, G. W., & Angrist, J. D. (1994). Identification and estimation of local average treatment effects. *Econometrica*, *62*(2), 467–475.

Imbens, G. W., & Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, *142*(2), 615–635.

Keane, M. P., & Wolpin, K. I. (2001). The effect of parental transfers and borrowing constraints on educational attainment. *International Economic Review*, *42*(4), 1051–1103.

Kugler, A., Kugler, M., Saavedra, J. E., & Herrera-Prada, L. O. (2020). Long-term educational consequences of vocational training in Colombia: Impacts on Young trainees and their relatives. *Journal of Human Resources*.

Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, *48*(2), 281–355.

Lochner, L. J., & Monge-Naranjo, A. (2011). The nature of credit constraints and human capital. *The American Economic Review*, 2487–2529.

Londoño-Vélez, J., Rodríguez, C., & Sánchez, F. (2020). Upstream and downstream impacts of college merit-based financial aid for low-income students: Ser Pilo Paga in Colombia. *American Economic Journal: Economic Policy*, *12*(2), 193–227.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, *142*(2), 698–714.

Melguizo, T., Sanchez, F., & Velasco, T. (2016). Credit for low-income students and access to and academic performance in higher education in Colombia: A regression discontinuity approach. *World Development*, *80*, 61–77.

Mincer, J. A. (1974). Schooling and earnings. In *Schooling, experience, and earnings* (pp. 41–63). NBER.

Molina, T., & Rivadeneyra, I. (2021). The schooling and labor market effects of eliminating university tuition in Ecuador. *Journal of Public Economics*, *196*, Article 104383.

Murakami, Y., & Blom, A. (2008). *Accessibility and affordability of tertiary education in Brazil, Colombia, Mexico and Peru within a global context*: *Policy Research Working Paper (4517)*, The World Bank.

Murcia, A. (2007). Determinantes del acceso al crédito De Los Hogares colombianos. *Ensayos Sobre Política Económica*, *25*(55), 40–83.

Murphy, R., & Wyness, G. (2023). Testing means-tested aid. *Journal of Labor Economics*, *41*(3).

Profamilia (2016). *Encuesta Nacional de Demografía y Salud 2015*: *Technical report*, Profamilia.

Prosperidad-Social (2014). *Aseguramiento al Sistema General de Seguridad Social en Salud*: *Technical report*, Departamento Administrativo para la Prosperidad Social, Bogotá - Colombia.

Prosperidad-Social (2017). *Manual operativo programa Jóvenes en Acción*: *Technical report*, Departamento Administrativo para la Prosperidad Social, Bogotá - Colombia.

Rau, T., Rojas, E., & Urzúa, S. (2013). *Loans for higher education: Does the dream come true? Working paper (19138)*, National Bureau of Economic Research.

Rodríguez-Raga, S., & Rodríguez, F. F. R. (2016). Determinantes del acceso a los productos financieros en Los Hogares Colombianos. *Estudios Gerenciales*, *32*(138), 14–24.

Solis, A. (2017). Credit access and college enrollment. *Journal of Political Economy*, *125*(2), 562–622.

Wooldridge, J. M. (2015). Control function methods in applied econometrics. *Journal of Human Resources*, *50*(2), 420–445.