The heterogeneous effects of student loans on college enrollment and degree attainment

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Abstract

We examine the impact of FIES, the primary funding program for postsecondary education in Brazil, on college access and completion. Our identification strategy relies on a reform that implemented regional quotas for annual loan allocations, enabling a comparison of regions that were differently affected. These quotas are determined discontinuously according to weights assigned for ranges of regional Human Development Index (HDI) values. We find that each additional loan leads to approximately 0.17 additional college graduates in six years. However, the effects are quite heterogeneous, and concentrated mostly in evening programs, in non-profit higher education institutions (HEIs), and students that graduated from public high schools. We also observe that for-profit HEIs respond to increased government funding by reducing their own grants. Such behavior is not observed for non-profit institutions. Thus, our analysis indicates that financial constraints and crowding out of other funding sources are important causes of heterogeneity in the effects of student loans programs.

Keywords: Student loans, higher education, FIES

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1 Introduction

Access to higher education has been a point of intense debate in many countries. Issues such as persistent social disparities in enrollment, tuition prices, student debt levels, among others, have been at the center of this discussion. Despite this, relatively little is known about the effects of loan policies on enrollment and, particularly, completion of higher education (Dynarski et al., 2022). Recent work has shed some light on these effects, but even so, little is known about how they vary according to characteristics of the students and higher education institutions (HEIs) attended.

In the case of grants, there is consistent evidence on their effect on entering and completing higher education. Recent experimental and quasi-experimental analyzes include Castleman and Long (2016), Bettinger et al. (2019), and Denning et al. (2019), which find positive results. Nguyen et al. (2019) presents a meta-analysis of the effect of grants on persistence and degree attainment, finding that, conditional on enrolling, grant aid increases the probability of degree completion by 2-3 percentage points. As pointed by Nguyen et al. (2019), even for grants, the number of studies estimating unconditional (on entry) effects is limited.

For student loans, studies are even scarcer (Dynarski et al., 2022; Card and Solis, 2022). Only a handfull of papers measure the effect of college loans on enrollment, persistence and completion in higher education. Notably, Gurgand et al. (2023), Solis (2017) e Melguizo et al. (2016) use a RD approach to estimate the effect of loans on enrollment for South Africa, Chile and Colombia, respectively. Card and Solis (2022) complement the results of Solis (2017) for Chile, being the only study, to the best of our knowledge, to present evidence on the effects on degree attainment, but only to a population of students that had already completed the first year of college. Chu and Cuffe (2021), for the New Zealand, as well as Black et al. (2020) and Denning and Jones (2021), for the United States, study the *intensive margin* effects of loans, by exploring the expansion of borrowing limits, with the first two papers finding positive results of loan availability, but not the latter. However, studies on other effects of loans, such as their effect on other funding sources, and on how the effects vary based on characteristics of the student and the institutions attended are practically non-existent – as can be noticed by the recent literature review carried out by Dynarski et al. (2022). The exception is the evidence of higher effects for poorer students (Solis, 2017; Melguizo et al., 2016). Existing estimates of unconditional (extensive margin) effects are relatively short term, so that estimates of effects on persisting more than two years or on degree attainment are also not currently available.

This paper contributes to this incipient literature in several ways, and is more closely related to Gurgand et al. (2023), Solis (2017), Melguizo et al. (2016) and Card and Solis (2022). We study the effect of FIES, a federal program that provides funding for postsecondary education in Brazil, on several outcomes, including most of the outcomes covered by the mentioned papers. More precisely, we estimate effects of loans on outcomes such as admission, enrollment and graduation, up to the sixth year since entry. Additionally, we explore the heterogeneity of these effects based on student and HEI characteristics.

FIES applies mostly to students entering private for-profit and non-profit HEIs, since public HEIs in Brazil do not charge tuition or fees – with very few exceptions. Our identification strategy relies on a natural experiment created by by the implementation of "loan quota" across different regions of the country. Prior to 2015, there were no restrictions on the number of loans granted per year, and all eligible applicants meeting the necessary criteria were provided with funding[1].

However, amidst a worsening fiscal and economic crisis, among other measures of fiscal tightening, the federal government also limited the amount of loans granted annually. Capping the number of loans would make FIES more selective, dispro-

[1] These requirements include restrictions such as a minimum score in the national high school exam (ENEM), maximum per capita household income, minimum score for the intended program in an assessment carried out by the Ministry of Education, among others.

portionately affecting less developed regions, since students in these regions had, on average, lower scores at the national exams. To mitigate this issue and promote a more equitable regional distribution of loans, the government introduced a system that allocated a predetermined number of loans for each region. Specifically, since the reform, the allocation of loans to regions follows a *weighted relative demand index*, with greater weights assigned to regions presenting lower Human Development Index (HDI) levels. These weights were assigned in an arbitrary and discontinuous manner, according to average municipal HDI ranges of Brazilian "microregions" (*microrregiões*), each microregion comprising approximately 10 municipalities. Exploiting this arbitrary weighting scheme, we leverage the resulting variations to estimate the causal effect of student loans on access to higher education in Brazil. We employ robustness tests similar to those used in Regression Discontinuity analysis, showing the consistency of our results across various bandwidths around the primary weight discontinuity.

Brazil's large and diversified higher education market provides a valuable context for examining the effects of student aid. This market comprises tuition-free public institutions, non-profit and for-profit private institutions, and an aid system that encompasses gratuity, government subsidies, means-tested and merit-based scholarships, as well as loans. With 8.7 million enrollments in 2018, Brazil ranks as the fourth largest tertiary education market globally, trailing only China (44.9 million), India (34.3 million), and the United States (18.9 million)[2].

Our findings indicate that each additional loan is associated with an increase of 0.43 enrolled students and 0.17 graduates up to the sixth year following admission[3]. Previous studies by Solis et al. (2017) and Melguizo et al. (2016) found that loans raise enrollment probabilities by 0.175 and 0.2, respectively. Such a high initial effect seems to be related to characteristics of the loan selection process in Brazil,

[2]According to World Bank data, available in https://databank.worldbank.org/indicator/SE.TER.ENRL?id=c755d342&report_name=EdStats_Indicators_Report.

which leads some students to enroll before their loan status is confirmed. On the other hand, we find a substantial negative effect of loans on enrollment in online higher education programs, which are not covered by FIES.

In addition, we observe that the program's impact is far from homogeneous, primarily affecting students that graduated from public high schools (typically lower-income), in programs with courses taught at the evening (which have work-compatible schedules), and in non-profit HEIs. Students that graduated from private high schools, which are generally higher income and not likely to be financially constrained, as they were able to pay for secondary education, are mostly unaffected by the program. The effect for evening programs probably follows similar reasoning, as financially constrained individuals would likely prefer to study in the evening, allowing them to work during the day.

We also measure the effect of loans on the take up of other funding sources, finding that greater availability of loans induce for-profit HEIs to reduce their own provision of loans and grants. However, non-profit institutions do not exhibit similar behavior. Consequently, government funding partially crowds out private funding, increasing effective tuition prices, in line with the Bennet hypothesis. Therefore, the concentration of loan effects on non-profit HEIs seems to result from the pricing behavior of for-profit institutions. In a related paper (Ávila and Terra, 2023), we study the effect of FIES on the behavior and finances of for-profit HEIs, finding that loans are very profitable for them.

In summary, our findings reveal a substantial impact of FIES on higher education enrollment and completion. However, these effects are very heterogeneous, depending on both student and HEI characteristics. Firstly, characteristics indicative of financial constraints, such as studying in the evening or being a public high school

[3]Figure 5 in the Appendix shows that, by the sixth year, the completion rate in Brazilian private universities is very close to its peak value. After this year, they increase only by approximately 3 (4) percentage points, in the case of for-profit (non-profit) HEIs, reaching around 38% (41%) in the tenth year since admission.

graduate, seem to amplify the effects of loans. On the other hand, the behavior of HEIs may also affect the effectiveness of funding programs.

The paper is organized as follows: Section 2 provides an overview of the institutional characteristics of the higher education sector in Brazil. Section 3 reviews the relevant literature. Section 4 outlines the empirical strategy and dataset construction. Section 5 presents the results of the empirical analysis. Finally, Section 6 concludes.

2 Institutional background

The Brazilian higher education system comprises a diverse mix of institutions, including federal, state, and municipal public universities, as well as for-profit and non-profit private institutions. Public universities are tuition-free and prohibited from charging any fees, including tuition, according to the Brazilian Federal Constitution[4]. The demand for student funding is therefore associated with private universities, which have experienced significant growth in recent decades (panels (a) and (b) of Figure 1).

In 1996, private institutions accounted for 67% of admissions and 45% of enrollments in Brazilian higher education. Two decades latter, in 2019, these figures reached 85% and 76%, respectively, still presenting an upward trend. The high participation of the private sector in the supply of higher education in Brazil is a characteristic rarely observed in other countries (Lovenheim and Smith, 2022). In recent decades, distance learning has also stimulated the growth of private institutions, especially the larger ones, despite the fact that such educational programs were not eligible for Fies loans until 2022.

Government action to promote access to higher education occurs in three main ways: public universities, scholarships at private universities and student loans, in

[4] A few public institutions that charged tuition fees prior to the enactment of the Constitution in 1988 were exempted from this rule.

descending order of attractiveness to the student. Public universities offer tuition-free college degrees, accounting for 24.6% of enrollments in 2018. This type of access is typically favored by students due to its cost-free nature, but also because the absence of costs increases selectivity and, consequently, the prestige of the programs offered. In addition, they are also research universities, which is usually associated to a more qualified academic staff.

Scholarships for economically disadvantaged students are granted through the University for All Program (*Programa Universidade para Todos* – Prouni), a federal program created in 2005. Participating institutions are required to offer scholarships at a ratio of 1 to 10.7 paying students and, in return, receive exemption from (some) federal taxes. Prouni is both merit-based and means-tested. To qualify, student must have a monthly per capita household income (PCHI) of up to 1.5 times the national minimum wage[5]. Selection among eligible candidates is based on scores achieved in the National High School Exam (ENEM), meaning that only high-scoring eligible candidates are granted access to the scholarships. Non-profit HEIs can also participate in a second federal scholarship program (CEBAS Educação) and receive additional tax exemptions in exchange for offering scholarships. These grants are also means-tested, as Prouni, but not necessarily based on merit, since HEIs have autonomy to allocate the grants.

The federal government also offers student loans through the FIES program, established in 1999. In Brazil, funding for private higher education dates back to 1975, with the creation of the Educational Credit Program (Creduc). This program was reformulated several times throughout its existence. In 1999, it was finally replaced by FIES, which experienced great fluctuations in the number of loans offered since its inception. It witnessed rapid growth in the first half of the 2010s, but has been

[5] Instead of a full scholarship for every 10.7 paying students, participating institutions can opt to offer a full scholarship for every 22 paying students, supplementing with a combination of 50% and 25% partial scholarships, until the total benefit reaches 8.5% of revenue (simillarly to the standard 1 to 10.7 rule) . In the case of partial grants, the eligibility limit is PCHI of up to 3 minimum wages.

declining since 2015 (Panels (c) and (d) of Figure 1). Figure 1 shows the evolution of loans granted, as well as the ratio of new loans to annually admitted students during 1976-2020. In 1976-1998, around 9% of those entering higher education had student loans granted by the federal government. However, this percentage decreased as admissions soared after 1996.

Prior to 2011, access to FIES required the presentation of a guarantor with sufficient income to repay the loans, which could be a hindrance, particularly to low-income individuals. This requirement could be seen as contradictory, since it targeted the program to individuals less likely to face financial restrictions. Therefore, intending to expand the program, the government instituted the FIES Guarantee Fund (Fgeduc), which acted as the guarantor of future FIES loans. As a result of the change, FIES became more targeted on low-income students, but default rates promptly increased (Brasil, 2020).

The plan to increase the program's attractiveness paid off. The number of loans granted experienced substantial growth, surging from 76,000 in 2010 to a peak of 733,000 in 2014, as can be seen in panel (c) of Figure 1. However, there was dissatisfaction with the apparent low impact of the program, since the number of enrollments in higher education, already booming, showed no perceptible acceleration in the period, despite the strong expansion of funding. This concern was expressed both within Government (Ministério da Fazenda, 2017) and in the general press[6]. As a result, the proportion of FIES-funded students among on-campus private higher education enrollments rose from 5% in 2009 to 39% in 2015 (Ministério da Fazenda, 2017), while the percentage of new loans granted among students admitted to higher education increased from 3% to 24% during the same period (panel (d) of Figure 1). However, the program's expansion incurred significant costs due to high default rates and subsidized interest rates. As a final blow, the Brazilian economy, already

[6]e.g., in https://oglobo.globo.com/sociedade/educacao/expans˜ao-de-fies-prouni-nao-do-matriculas-acelerarem-em-universidades-particulars-15452743 ("*Expansion of Fies and Prouni did not speed up enrollment at private universities*").

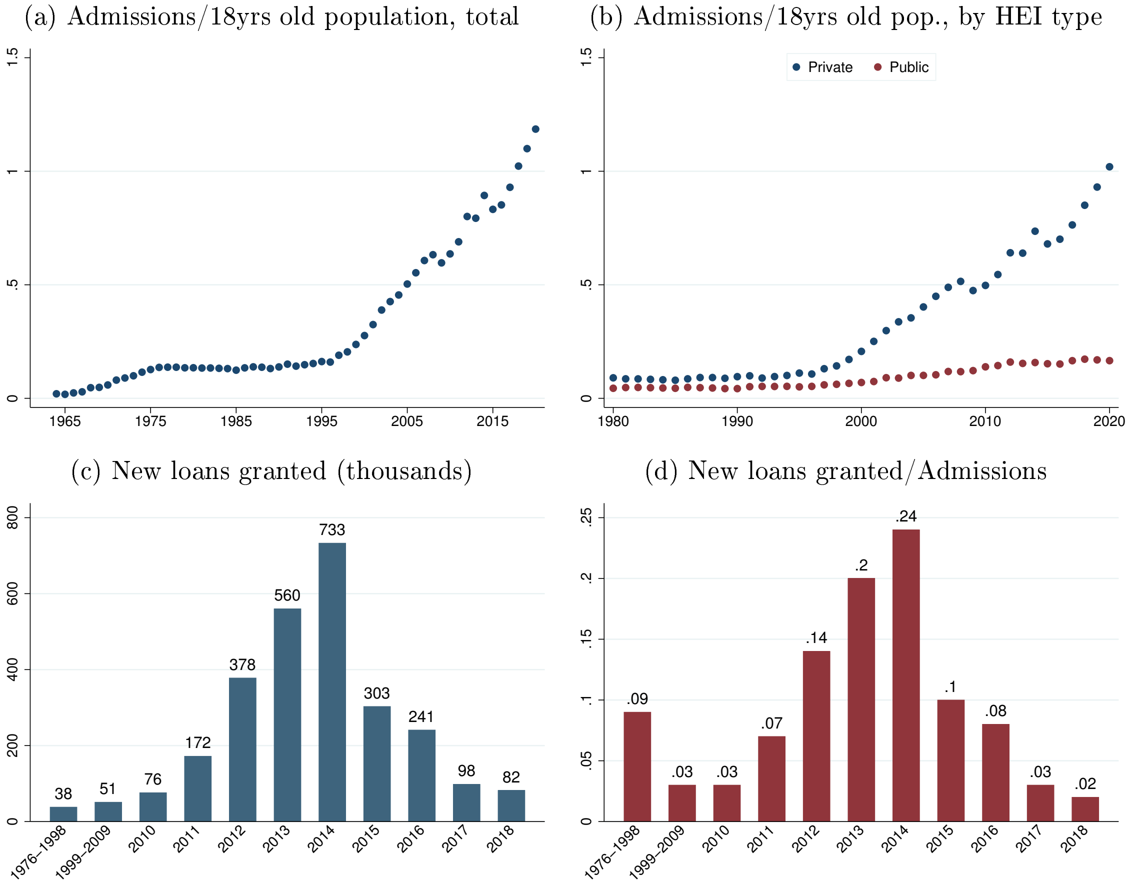
decelerating, entered its worse recession since the 1930s, culminating in the Federal Government's decision of limiting the number of loans offered and, ultimately, carry out a reformulation of FIES in 2017, with effect in the following year. Dearden and Nascimento (2019) discuss the main aspects of the program's reformulation, as well as institutional aspects of higher education in Brazil.

A simple comparative analysis could lead to the conclusion that the program expanded primarily by crowding out other forms of funding, rather than stimulating new enrollment. However, the observed expansion (2010-2014) and subsequent contraction (2015-2017), coincided with a period of economic growth (2.4% annual expansion in GDP per capita) and a subsequent sharp downturn (-2.7% per year), suggesting that the correlation between funding and enrollment could be actually reflecting the economic cycle. Generally, higher education enrollment has a countercyclical component due to opportunity costs. During economic downturns, enrollment rates can increase as individuals may decide to invest more in education in order to improve their employment prospects. Conversely, enrollment may decline during economic booms due to a more favorable labor market. Most studies testing this hypothesis, such as Dellas and Sakellaris (2003), Sakellaris and Spilimbergo (2000) and Hillman and Orians (2013), find countercyclical enrollment rates for developed countries, but Sakellaris and Spilimbergo (2000) finds procyclical enrollment for non-OECD countries, indicating that credit constraints may be more important in this case. Consequently, simple correlations between loans and enrollment can be biased in either direction, depending on the importance of credit constraints.

During 2012-2017, the majority of FIES loans covered full tuition. Other costs, such as books and living expenses, are not covered. Loans did not cover full tuition mostly when it was not required by the student or when the remaining cost was covered by partial scholarships. To apply for the loan, the student must first be accepted by a participating HEI, which cannot reject her later if she obtains funding. The average annual tuition fee for FIES participants was R$ 15,000.79 in 2018 (US$

7,407.92 PPP), approximately 45.8% of the Brazilian per capita GDP for the same year. This percentage, according to data presented by Solis (2017), is similar to that observed for countries such as Argentina, Chile and the United States, but substantially above the OECD average, likely due to the prevalence of subsidized public HEIs in many of the member countries.

Figure 1: Number of new loans granted (Creduc and Fies) and admittance to higher education, by year (1976-2020).



Notes: Panel (a) presents the ratio of higher education admissions to the population estimate of 18-year-old individuals in each year. Panel (b) presents the same statistic disaggregated for public and private higher education institutions. Panel (c) presents the number of *new* FIES loans granted in each year. Panel (d) presents the annual ratio of newly granted loans to the number of admissions in higher education. Admitted students refer to those who were enrolled at any point, excluding accepted students who never enrolled. The number of admissions exceeds the number of admitted students due to the possibility of students being admitted to multiple institutions within a given year. *Data sources*: Ministério da Fazenda (2017), INEP (2000), FNDE (2022), Higher Education Census (INEP) and population estimates from IBGE (Brazilian Geography and Statistics Bureau). For the 1976-1979 and 1983-1984 periods, admissions were estimated based on the number of vacancies and the average filling rate observed during 1980-1982.

3 Related literature

The literature on educational access generally points to three main access barriers: financial constraints, informational/behavioral constraints, and academic constraints (Long and Riley, 2007; Page and Scott-Clayton, 2016). These barriers not only affect the likelihood of entering higher education but also influence persistence and completion rates.

The literature concerning financial constraints to college access is the most relevant in the context of this paper, although assessing the existence of informational problems can be important, as these may also affect access to funding. Some studies, mainly in the context of the United States, indicate that informational complexities can lead individuals, especially those with less favored socioeconomic status, to overestimate the costs involved in higher education, specially when there is a great diversity of federal, state and private sources of financial aid and financing[7]. In the Brazilian case, however, it is observed that a high percentage of potential applicants (individuals who have taken the national high school exam) are aware of and show interest in the main funding programs (FIES and Prouni)[8].

The main economic justification for providing student loans is the presence of imperfections in the credit market, which lead individuals to invest suboptimally in education. Unlike investments in physical capital, the human capital to be acquired through education cannot be used as collateral for obtaining loans. As several authors have pointed out, such as Lochner and Monge-Naranjo (2012), in the absence of market imperfections, the decision to pursue higher education should be based on estimates of cost and return, which, in general, would be independent of family income. Such independence, however, as mentioned by Carneiro and Heckman (2002), is conditioned to several other factors, normally correlated with family income, such

[7]See Dynarski et al. (2022) for a recent literature review on the subject.

[8]In 2016, 52% and 75% of students taking ENEM declared applying for FIES and Prouni, respectively, as one of the most relevant factors (5 in a scale of 0 to 5) for their decision to take the exam.

as: greater investments in education at earlier life stages, which would result in greater ability; fewer informational constraints; greater appreciation of education, among others. Moreover, if education is also considered a consumption good, individuals with higher incomes could demand a greater amount of it. In light of these considerations, eliminating borrowing constraints alone would not suffice to eliminate the existing correlation between family income and the likelihood of entering higher education.

Lochner and Monge-Naranjo (2016) review the theoretical and empirical literature on credit restrictions and educational investments, while Dynarski et al. (2022) presents a recent review on the empirical literature regarding the effects of financial aid on student decisions. Dynarski et al. (2022) reviews the literature on non-financial barriers to college access. Yannelis and Tracey (2022) survey the empirical literature on student loans, focusing on defaults, credit outcomes, and earnings. Due to the difficulty in controlling for all the factors involved, the empirical identification of credit constraints has proved to be fairly complex, with evidence on this issue being, until recently, scarce and, in most cases, indirect.

Indirect evidence. Carneiro and Heckman (2002) argue that, after controlling for ability, parental income has a relatively small effect on enrollment in the US context. According to these authors, only 8% of individuals face credit constraints that would prevent them from entering higher education. Cameron and Taber (2004), also in the case of the US, uses return to schooling to test the existence of credit constraints but do not find evidence of inefficiencies in access to college. Attanasio and Kaufmann (2009), on the other hand, study the case of Mexico, where the availability of university loans is limited. They argue that expectations regarding wages after graduation should be positively correlated with college entry, so that a break in this correlation – which they find in data – would also be evidence of financial constraints.

However, other indirect methods, exploring the effects of financial deregulation (Sun and Yannelis, 2016) and income windfalls (Manoli and Turner, 2018) find evidence of financial constraints. For Brazil, Chein and Pinto (2018) find that enrollment probabilities are related to wealth among middle and higher income individuals, but not for low income ones. This pattern cannot be explained by means-tested funding policies, since, although FIES and Prouni restrict eligibility by income, in practice, they only restrict access to funding for relatively high income levels[9]. If higher-income individuals face financial constraints, the same would likely apply to low-income individuals. Hence, the lack of correlation between wealth and enrollment in higher education among low-income groups may be explained by other barriers, such as financial constraints that impede access to earlier levels of education.

Overall, the indirect evidence does not point to an important role for credit constraints in explaining access to higher education. It suggests that, instead, the correlation between family income and access to college are mostly a result from dynamic complementarities between early and late investments in education (Cunha and Heckman, 2007; Cunha et al., 2010). In other words, the returns to higher education are influenced by investments in earlier levels of education. Nevertheless, recent research applying quasi-experimental methods has provided direct evidence of financial constraints.

Direct evidence: extensive margin. One of the earliest studies to directly examine the effects of financial constraints, focusing on the drop-out decision, is Stinebrickner and Stinebrickner (2008). They find that credit constraints explain only a small portion of attrition among students from low-income families. More re-

[9]The lowest income threshold for eligibility pertains to full PROUNI scholarships, requiring applicants to have per capita family incomes below 1.5 times the national minimum wage. However, in 2017, a student at this income level would fall just within the top income quartile. FIES and partial PROUNI scholarships, on the other hand, require incomes below 3 times the minimum wage.

cently, four studies present direct empirical evidence on the effect of loans on college enrollment, persistence, and completion (Gurgand et al., 2023; Solis, 2017; Melguizo et al., 2016; Card and Solis, 2022). These studies employ regression discontinuity methods to analyze the effect of student loans in South Africa, Chile and Colombia. The first study focuses on a student loan program in South Africa, in which eligibility is based on meeting a pre-established credit score threshold. The remaining three studies are based on the requirement of a minimum score on exams to access funding. Gurgand et al. (2023) find that access to credit increases higher education enrollment by 42 percentage points among applicants, a substantially larger effect compared to other studies. Another notable aspect is that the entire effect is driven by female applicants, while the effects on men are essentially null. Similarly, Solis (2017) observe an increase of 18 percentage points in the probability of entering higher education in the year following high school (with a 16 p.p. increase in the subsequent three years), with a greater effect observed among students in the lowest quintile.

Solis (2017) also notes that access to loans significantly reduces the enrollment gap between the highest and lowest quintiles of the income distribution. For individuals just below the cutoff point, the richest quintile was twice as likely to access higher education as the lowest quintile, but for individuals just above the cutoff point, the difference becomes statistically insignificant. Card and Solis (2022) extend the work of Solis (2017), finding that access to loans increases persistence in the second year by 20 p.p., mostly through a reduction in transfers to vocational colleges, and graduation by 12 p.p., among students who had already completed the first year. Bucarey et al. (2020) and Montoya et al. (2018), however, in the context of the same Chilean program, find that loans induced transfers from vocational education to universities, but resulted in reduced degree completion and future earnings while increasing debt. Higher scoring students, on the other hand, seem to benefit from the policy.

Melguizo et al. (2016) use cutoff scores on high school exit exams, which determine eligibility for a Colombian student credit program. The study finds that access to the program increases the probability of entering higher education by between 0.16 and 0.34 percentage points, depending on the use of controls. They also observe higher effects for low-income individuals.

For Brazil, Duarte (2020) find that crossing the minimum eligibility score for federal aid (450 points in ENEM) increases the probability of students enrolling in higher education by 10 percentage points. Unlike other studies, this threshold refers to both loans and grants, and passing does not guarantees access to financial aid, giving only the right to apply for it[10]. The population considered refers to all participants in ENEM, and not just applicants to loans and grants. This broader population probably explains the smaller effect size found in this study, which can be seen as a lower bound for the true effect.

Direct evidence: intensive margin. In the context of New Zealand, Chu and Cuffe (2021) find that continued access to loans by students with low academic performance increases re-enrollment, completion, and future labor market returns. For the United States, Black et al. (2020) finds that increasing borrowing limits raised student debt, but improved degree completion, future earnings, with no discernible impact on homeownership or other forms of debt. However, Denning and Jones (2021) find that higher limits increased borrowing, but they find no effect on student GPA, credits, persistence, or graduation rates.

4 Problem definition and empirical strategy

Prior to 2015, FIES loans were not subject to a cap, and all qualified applicants receiving funding. However, due to worsening fiscal conditions and increasing default

[10]In fact, only a small fraction of students are able to access FIES with a score close to 450, as show in Figure 6, in the Appendix, since FIES cutoff scores are considerably higher than that value for most programs.

rates, the government introduced a nationwide limit on loan approvals. This new system also implemented loan quotas for each region of the country, defined at the level of microregions (*microrregiões*). To each microregion were assigned weights, defined according to a discontinuous scale, decreasing in average HDI values[11]. Figure 4 in the Appendix illustrates Brazilian microregions and their corresponding HDI values. Throughout the paper, the term "slots" refers specifically to loans allocated within the FIES program. For the sake of brevity, we also use the term "region" interchangeably with "microregion".

To allocate the loans, the total number of available slots for the FIES program is determined on an annual basis, taking in account the budget allocated to program[12]. Once total slots are established, they are distributed among different regions according to the following formula:

$$F_{mt} = \frac{SRC_{mt}\sigma_m}{\sum_{m' in \mathcal{M}} SRC_{m't}\sigma_{m'}} \mathcal{F}_t \tag{1}$$

where SRC is the Social Relevance Criteria, described in the Appendix, σ_m is the weight assigned to region m; and $\mathcal{F}_t = \sum_m F_{mt}$ is the total number of slots available in year t. The weights are presented in Table 1.

As shown in Table 1, the policy rule exhibits discontinuities at the arbitrary HDI levels of 0.5, 0.6, 0.7 and 0.8. Crossing these cutoff points, from a higher to a lower HDI, would generate increments of 8.3%, 9.1%, 22.2% and 28.6%, respectively, in the allocation of slots reserved for the region.

No other government policies employ a similar rule. The utilization of HDI weights began during the selection process for the first semester of 2016, with slots

[11]The municipal HDI is a version of the Human Development Index initially proposed by UNDP (1990). The index is calculated from four indicators: life expectancy at birth, average years of schooling, expected years of schooling and GDP per capita. These indexes are not directly comparable to country HDIs, since they are standardized based on the average indicator values of Brazilian municipalities.

[12]Although the selection processes occur twice a year, the number of slots is determined annually. Furthermore, any unfilled slots from the first semester are carried over and made available in the second semester. Therefore, the analysis is conducted on an annual basis.

Table 1: Microregion weights and HDI ranges

HDI Level	HDI Range	Weights
Very low	0 to 0.499	1.3
Low	0.500 to 0.599	1.2
Middle	0.600 to 0.699	1.1
High	0.700 to 0.799	0.9
Very high	0.800 to 1	0.7

Data sources: Ministry of Education *Portarias* of number 13/2015, 9/2016, 25/2016, and 12/2017. The table shows weight values assigned to each HDI range.

allocated at the microregion level. This approach was maintained until the first half of 2018. Subsequently, the system transitioned to an allocation by mesoregions, representing a more aggregated regional level. The remaining components of the rule remained unchanged and are still in effect[13]. The Brazilian territory is divided into 5,570 municipalities, which correspond to the lowest government level. It is important to note that microregions and mesoregions do not serve as administrative or political divisions, but rather represent regional classifications established by the Brazilian Institute of Geography and Statistics (IBGE). Among the 558 microregions, around 370 had higher education enrollment in 2012-2017.

After the regional allocation, the slots within each region are further divided based on areas of knowledge and HEI quality levels. The application of these criteria results in multiple distinct "boxes," each differing in at least one criterion. Ultimately, the slots within each box are filled in a descending order of ENEM scores.

Based on the preceding discussion, we detail the problem and the identification strategy employed. The total number of individuals entering higher education in region m at a specific time can be represented as the sum of individuals who would enter regardless of government funding (*always takers*) and individuals who would

[13] Brazil consists of 137 mesoregions, which are further subdivided into a total of 558 microregions. On average, each microregion comprises approximately 10 municipalities.

only enter with funding (*compliers*), representing those truly affected by the policy:

$$I_{mt} = A_{mt} + \tau F_{mt} \tag{2}$$

where I_{mt} represents the number of students entering higher education, A_{mt} are the *always takers* and F_{mt} represents the number of loans granted. The coefficient $\tau = \delta - \gamma$ represents the effect of loans on entry, where δ is the probability of the loan recipient entering higher education and γ is the probability that loan recipients would have entered higher education even without the loan[14].

Equation 1 can be rewritten as:

$$F_{mt} = \sigma(HDI_m) \left(\frac{D_{mt}}{\mathcal{D}_t} \right) \mathcal{F}_t \tag{3}$$

where $D_{mt} = SRC_{mt}$ and $\mathcal{D}_t = \sum_{m' in \mathcal{M}} SRC_{m't}\sigma_{m'}$. It should be noted that HDI_m values do not vary by t, as this index is based on variables measured at the municipal level and derived from the Demographic Census, which was last conducted in 2010. Therefore, in principle, HDI values cannot be manipulated and were not influenced by policy changes, as they were determined prior to the implementation of the rule.

The term $\left(\frac{D_{mt}}{\mathcal{D}_t} \right)$ can be regarded as a *proxy* of relative demand for loans in region m. This term is probably correlated with the unobservable variable A_{mt}, rendering F_{mt} endogenous in equation 4. We represent the variable A_{mt} as the sum of region fixed effects η_m, year fixed effects ι_t, and a term ξ_{mt}, with $E[\xi_{mt}] = 0$. Substituting into equation 2, we obtain:

$$I_{mt} = \tau F_{mt} + \eta_m + \iota_t + \xi_{mt} \tag{4}$$

[14]The τ effect is comparable to the effects (locally) estimated by Gurgand et al. (2023), Solis (2017) e Melguizo et al. (2016). In the case of these papers, given a variable z that determines funding based on a cutoff point $\bar{z}$, the probability of an individual entering higher education would be $p_i(T) = \delta(z)T_i + \gamma(z)(1 - T_i)$, where $T_i = \mathbf{1}[z_i \geq \bar{z}]$ and the treatment effect would be $p_i(1) - p_i(0) = \delta(\bar{z}) - \gamma(\bar{z})$ for individuals with a score of $\bar{z}$. In this case, $\tau = \delta - \gamma$ has a similar meaning to that presented in the equation 2, but would only refer to individuals with score in the neighborhood of $\bar{z}$.

On the other hand, $\sigma(HDI_m)$ is correlated with F_{mt}, but results from weights that vary arbitrarily and discontinuously by HDI, making it, arguably, uncorrelated with ξ_{mt}. Therefore, we define the instruments as follows:

$$Z_Y^j = \begin{cases} 1, & \text{if t} = Y \text{ and } HDI_m = j \\ 0, & \text{otherwise} \end{cases} \tag{5}$$

where $j \in \{[0.5, 0.6), [0.6, 0.7), [0.7, 0.8), [0.8, 0.9)\}$ are the HDI_m ranges and $Y \in \{2016, 2017\}$ refer to the treated years. We omit the mt subscript for simplicity. As previously discussed, these ranges are based on a previously determined municipal HDI. Consequently, any contemporary correlation between the instruments and shocks in the demand for higher education is excluded[15]. Therefore, the only possible violation of the condition $E[Z_{mt}^j \xi_{mt}] = 0$ would arise from spurious correlations due to differences in previous trends.

Based on the above, Equations 3 and 5 suggest an instrumental variables model incorporating region and year fixed effects, one endogenous explanatory variable F_{mt}, and instruments Z_{mt}^j.

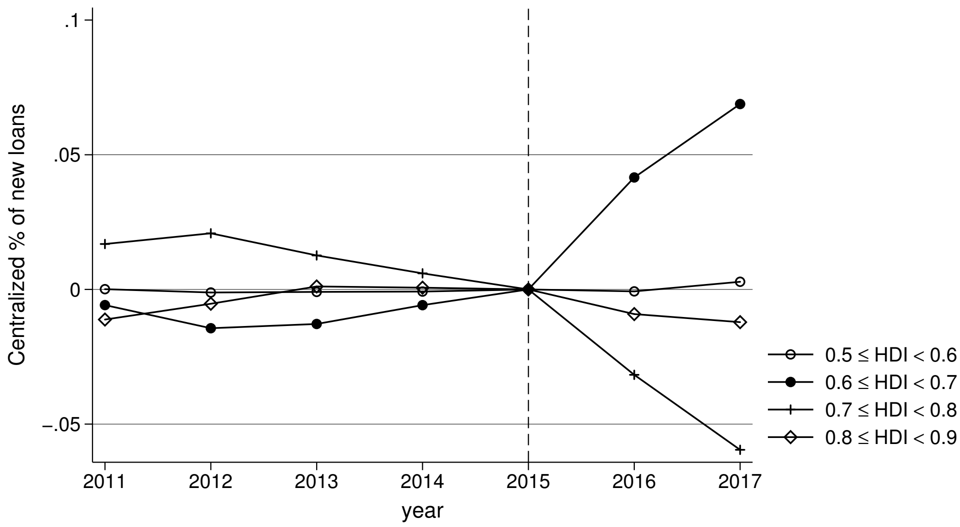
In the following, equation 4 can be generalized to other situations, where I_{mt} represents the number of individuals taking a particular action (admission, enrollment, conclusion, among others), A_{mt} represents the number of individuals who would have taken that action regardless, and τF_{mt} represents the number of individuals who take the action solely due to receiving funding. For instance, τ can also be negative, so that, in this case, τF_{mt} would refer to the number of individuals who refrain from taking the action due to funding (for example, choosing not to enroll in online programs).

[15]The only way of changing the HDI range of a region would be creating enrollment in a municipality that was previously not served by HEIs, since only municipalities with higher education enrollment are considered when calculating the regional average. In our study, we generate the instruments based on the mode of the regional HDI for each region. However, this choice is inconsequential as there are no instances of *crossing*, meaning that using any year would yield the same weights.

Figure 2 illustrates the evolution of FIES loans, categorizing regions based on their HDI levels: (1) $0.5 \leq HDI < 0.6$, (2) $0.6 \leq HDI < 0.7$, (3) $0.7 \leq HDI < 0.8$; and (4) $0.8 \leq HDI < 0.9$. We center the shares in 2015, the last year before the introduction of the allocation rule, by subtracting out of all values the percentage of loans received by each region in that year. Notably, the implementation of the rule significantly altered loan distribution across HDI groups. As expected, there was an increase in the participation of groups (1) and (2), which received higher weights, and a decrease for groups (3) and (4), which received lower weights.

In order to simplify the presentation, the number of loans granted was treated as a direct function of the parameters that determine the number of slots per region. In practice, however, the effect of the slots on the number of loans is not one to one due to the fact that not all slots are filled. Nonetheless, as shown in Figure 2, this did not prevent the weights from having the intended impact of changing the distribution of slots in favor of less developed regions.

Figure 2: Percentage of FIES loans granted by HDI group, centered around the 2015 values.



Note: The graph displays the percentage of FIES loans granted to each HDI group, centered around the 2015 values, denoted by a vertical line. This year represents the last year prior to the rule change. *Data source*: Fundo Nacional de Desenvolvimento da Educação (FNDE).

The most notable changes are observed in groups (2) and (3). This is due to the fact that, despite the distribution rules providing weights for five HDI intervals, in practice, over 90% of the regions with at least one functioning HEI fall within the HDI range of 0.6 to 0.8[16].

4.1 Data

Table 11 in the Appendix provides the sources of the data used in this paper. As the allocation rule is defined at the regional level, our empirical analysis is conducted at this level. The data allows us to estimate two types of effects, depending on how the dependent variables are constructed. Specifically, we can aggregate enrollment by the region of birth of the student or by the region of the program. In the former case, we estimate an *individual* effect, measuring the increase in the probability of enrollment in higher education for students exposed to the treatment (i.e., greater loan availability). In the latter case, we estimate a *regional* effect, capturing both the increase in enrollment by students in the region and a relocation effect, as the allocation of more loans to less developed regions not only increases enrollment among locals but also attracts students from other regions. While the literature, to the best of our knowledge, has only estimated the individual effect, this paper also provides the first estimation of the regional effect, which can be relevant in various policy contexts.

To estimate the individual effect, we use information on the students' region of birth. However, this information is missing for 23.9% to 34.9% of the data points, depending on the year, as presented in Table 17 in the Appendix. About 80% of the missing data points occur in programs/years with at least 80% missing individual data, and 72.3% of missing data points occur in programs/years with 90% or more missing individual data. Thus, the missing data appears to be primarily caused by

[16]This fact can be seen in Figure 4, in the Appendix A. In 2017, 361 regions had positive enrollment in higher education, and none had HDI below 0.5. Only 12 had HDI between 0.5 and 0.6 (not included) and only five had HDI of 0.8 or higher.

programs that did not adequately capture or report the information for a given year, rather than being directly determined by student characteristics.

Students receiving FIES loans are typically older and already employed. For instance, approximately half of the program's beneficiaries have formal jobs while pursuing their college education (Brasil, 2020). Consequently, a significant portion of these students are expected to be local residents. Calculations using the Census of Higher Education data indicate that the percentage of students enrolling in private higher education in their region of birth varies between 94.6% to 95.4% annually during 2012-2017[17].

In the absence of corrections, calculating enrollment by birth region ignoring the data points with missing information would result in a downward bias, as part of the benefited students would be omitted. On the other hand, imputing values for the missing data results in dependent variables with measurement error, which could also bias the estimated coefficients, depending on the type of measurement error and whether or not it is correlated with the instrument. In practice, the estimated model with measurement error ν_{mt} in the dependent variable becomes:

$$I_{mt} + \nu_{mt} = \tau F_{mt} + \eta_m + \iota_t + (\xi_{mt} + \nu_{mt}) \tag{6}$$

If the measurement error is correlated with the instrument, it would render the instrument invalid. However, as the measurement error, in this case, would be part of the model error, the Sargan test of overidentified constraints – possible when there is more than one instrument – would indicate the invalidity.

As for the type of measurement error, Hyslop and Imbens (2001) show that the estimated coefficient can be biased downwards if the measurement error is of the *Optimal Prediction Error* (OPE) type. In the Classic Measurement Error (CME)

[17]This high percentage does not seem to be caused by erroneous filling of the reports, since, for example, in 2017, only 13 of the 2,136 reporting HEIs had 100% of their students born in the municipality where the HEI is located.

case, the error is not correlated with the true values of the variable, but only with the variable measured with error. In the OPE, however, the measurement error is correlated with the real variable, which biases the estimated coefficients downwards even if the explanatory variable is not correlated with the error. This happens when the measurement error results not from random coding or reporting errors, for example, but from the fact that the variable in question is obtained from an estimate, constructed from the minimization of some loss function. Put differently, in this case, instead of an independent estimate for each observation, the reporting agent would estimate the values trying to minimize a joint function of the measurement errors[18].

Consequently, trying to find the "best" estimate for the number of students born in each region would likely generate an OPE error, causing a downward bias in the coefficients from the regression of Y on X. Thus, we take a "naive" approach and assume that the missing data has the same distribution of births as the non-missing for a "comparable" group of students, constructed by combining observations with similar values for year of admission, municipality of study, type of institution attended (for-profit or non-profit), study shift (daytime or evening) and type of secondary educational institution attended (public or private). In practice, this correction was made by weighting the non-missing observations. For example, if a group has $x\%$ of students missing the information on municipality of birth, each non-missing observation receives the weight $1/(1-x\%)$, in order to compensate for the missing data. The previous procedures were performed separately for admitted, enrolled and graduating students, at the municipality level, and latter aggregated to the regional level,

[18]Following Hyslop and Imbens (2001), consider the model $Y^* = \alpha + \beta X + e$, in which the real value Y^* is measured with error ν. Thus, we have:

$$\tilde{Y} = Y^* + \nu = \alpha + \beta X + e + \nu$$

where $\tilde{Y}$ is a signal of the true value Y^* observed by the reporting agent, which is subject to a classical error ν. The unconditional mean of Y^* is $\alpha + \beta\mu_X$, with variance $\beta^2\sigma_X^2 + \sigma_e^2$.

As argued by Hyslop and Imbens (2001), given a signal $\tilde{Y}$, reporting $Y = \tilde{Y}$ would likely generate a classic-type measurement error (ν), whereas reporting $Y = E[Y^*|\tilde{Y}]$ would result in an OPE-type measurement error, since, in the latter case, Y would become a weighted average of $\tilde{Y}$ and the unconditional mean of Y^*, with weights depending on the attempt to minimize the error.

resulting in a zero mean error, since the actual number of admissions, enrollments and graduations is known. Importantly, the estimated coefficients should not be affected by this procedure, since the measurement error is unlikely correlated with our instruments[19].

We restrict our empirical analysis to the period from 2012 to 2017, as the program underwent significant reforms in 2011, 2016, and 2018. Additionally, since the highest HDI group contains only five regions, our analysis includes only groups 0.5, 0.6, and 0.7. Our instrumental variables are constructed based on the reforms implemented in 2016, as discussed earlier. In Brazil, students are admitted into specific programs/degrees, selecting their majors at the time of college application, and any subsequent changes in majors (except in very specific transfer scenarios) are treated as new admissions. This allows us to track cohorts of students over time based on their year of admission.

In the subsequent analysis, the variable t represents the number of years since admission, with $t = 1$ denoting the year of admission into higher education. Consequently, the count of enrolled and graduating students for $t = \{1, 2, 3, 4, 5, 6\}$ is limited to individuals who entered in $t = 1$. For example, "enrolled in $t = 3$" refers to students who were admitted in $t = 1$ and remained enrolled by the end of year $t = 3$. Due to data limitations, we do not have enrollment and completion information in $t = 6$ for students admitted in 2017. Thus, the estimates for $t = 6$ are based on a smaller sample, comprising only those who entered between 2012 and 2016.

[19] Note that the number of missings does not determine the direction of the error, since its sign will depend on how the distribution of birth municipalities for missings differs from the non-missing. Additionally, the same place of birth may be underrepresented in a given region and over-represented in another. Thus, the aggregate (regional) error will be sum of errors for each municipality with higher education enrollment.

5 Results

5.1 Previous Trends

The validity of the IV method can be compromised if the instruments are correlated with region-specific trends. For instance, if regions with the lowest HDI values were already experiencing a catch-up process, the estimated effects would be biased upward. Figure 7, in the Appendix, presents event studies for admissions, enrollment and graduation spanning years 1 to 6. For outcomes in year 1, we observe that regions within the 0.5 and 0.6 HDI ranges exhibit similar trends to the baseline group (range 0.7) prior to the introduction of the rule. For outcomes in years 2 to 6, we also observe similar trends up to the 2014 cohort. However, for the 2015 cohort, we observe slightly higher values compared to the baseline year (which is set as 2014 in this case). This discrepancy arises because loans are not necessarily granted to first-year students. Therefore, students from the 2015 cohort, initially unaffected by the rule, could benefit from increased loan availability in the subsequent year if they are still enrolled. There are no signs, however, that older cohorts (2012 to 2014) are affected in a similar fashion. For instance, the effect of remaining in college in the third year only because of loans, conditional on having already completed two years, is likely negligible, since most students that depend on loans would not persist for that long, specially considering that many programs funded by FIES have a expected duration of 3 years or less. In light of the above, as a conservative measure, we drop the 2015 cohort from regressions when the outcomes refer to years $t \geq 2$. Nonetheless, this exclusion has minimal effects on the coefficients and does not qualitatively alter the interpretation of the results.

5.2 Effects on admission, enrollment, and graduation

The results of the first stage estimations are presented in Table 2, considering the various time windows employed in this study. The instruments are significant, with positive coefficients as anticipated.

We begin by estimating the impact of loans on admission and enrollment in the region where the program is taught, as shown in Table 3. The top panel presents the results for ordinary least squares (OLS) estimates, which exhibit a notable positive bias with coefficients generally exceeding 1. This upward bias is expected since F_{mt} is influenced by the regional demand for higher education (as indicated in Equation 3), which is correlated with A_{mt}. On the other hand, fixed effects (FE) coefficients tend to be small and apparently downward biased.

IV estimates are presented in the bottom panel of Table 3. Granting loans has a high effect on the number of local higher education admissions: 100 more loans would result in 46 additional entrants. The effect on enrollment remains significant throughout the duration of the program but diminishes over time. This decline is expected, as graduates exit the dataset in the year following their graduation, and some programs have a duration of less than four years. It is important to note that the regional effect should be an upper bound for the individual effect, as the relocation effect is positive.

Table 4 presents a similar estimation, but this time focusing on the individual effect. As expected, the estimated coefficients are smaller compared to the regional effect, but they remain substantial. Specifically, we observe an increase of 0.431 in admissions for each additional loan granted. This value is notably higher than most estimates found in previous studies, which typically range around 0.2. However, the effect decreases rapidly over time. By the end of the first year, the effect on enrollment diminishes to 0.332, a 0.1 drop, indicating that many of the induced admissions drop-out during the first year of college. This high initial effect, followed by a sharp decline, is likely associated with a known problem of the FIES selection process. At the end of each year, the Brazilian Ministry of Education faces a tight schedule, since they have to run, in sequence, the 1) National High School Exam (ENEM), 2) the unified selection process for public universities (SISu), 3) the selection process for Prouni grants, and, finally, 4) the selection process for FIES. Consequently, the FIES selection typically takes place around March of the following year, when most HEIs have already commenced their classes for the first semester. As a result, many students enroll before their loan status is confirmed (Brasil, 2020). If they receive the loans, their entire first semester will be covered, but students who do not meet the criteria to access the loans may be forced to drop out.

To the best of our knowledge, our study is the only one that presents estimates of loan effects throughout the duration of higher education. Therefore, we are unable to determine if and to what extent effects decline over time in other contexts, or if this phenomenon is unique to the Brazilian case.

The top panel of Table 5 presents the impact on graduation. The first six columns display the effect of loans on graduation in years 1 to 6, respectively. As expected, we find no effects on graduation in year 1, since higher education programs funded by FIES have a minimum duration of 2 years[20]. The last column of the top panel presents the cumulative effect of loans on graduation up to the sixth year, with the dependent variable being the sum of the dependent variables in the first six columns. We find that one additional FIES loan increases graduation by 0.171. This effect is substantial considering that graduation rates up to the sixth year are only around 35% in Brazil for private HEIs (see Figure 5 in the Appendix)[21].

Taking advantage of the linear setting, we take a similar approach in the bottom panel of Table 5. We estimate persistence effects by summing up, for a given year

[21]It should be noted that changing majors are usually computed as evasions in Brazil, which lowers the graduation rate.

[21]Higher education programs in Brazil normally last between 2 and 6 years, with 3 and 4 years being the most common, but 2 and 5 years also being adopted in many cases. The estimated effects match this pattern.

T, the number of graduates in years $t < T$ to the enrollment in year $t = T$. Hence, the dependent variables, in this case, are the number of students that did or still could graduate in year t. Persistence is higher by 0.217 in the sixth year, since some students are still enrolled. However, effects on graduation cease to be statistically significant in $t = 6$, indicating that our estimates capture most of the effect of the program[22].

[22]The duration of FIES loans depend on the *expected* duration of the program. If a student takes longer than expected to graduate, the final years would not be covered by the loans. This deadline can be extended for up to one year, since students can request the suspension of funding for 2 consecutive semesters. Hence, for most students, it is unlikely that the effects extend for more than 6 years.

Table 2: First stage - IV.

Year of admission:	2012-2017	2012-2017[a]	2012-2016[a]	2012-2016	2013-2017[a]	2013-2016[a]
$Z_{2016}^{[0.5,0.6)}$	1070.101***	1299.259***	1252.045***	1234.469***	1672.329***	1592.311**
	(390.298)	(448.970)	(474.764)	(459.973)	(585.504)	(619.866)
$Z_{2016}^{[0.6,0.7)}$	1108.037***	1331.196***	1391.355***	1197.232**	1552.218**	1604.142**
	(412.668)	(509.060)	(514.725)	(471.762)	(636.180)	(648.400)
$Z_{2017}^{[0.5,0.6)}$	1789.209***	2024.585***		1962.451***	2431.393***	
	(522.056)	(583.367)		(594.281)	(733.347)	
$Z_{2017}^{[0.6,0.7)}$	1470.781**	1694.390**		1554.812**	1910.285**	
	(597.474)	(695.398)		(656.483)	(822.435)	
N	1786	1484	1167	1498	1195	879
Clusters	314	313	306	312	310	303
F(4, Clusters-1)	13.0132	11.9533	4.0181	11.1425	7.4949	3.6464
Prob > F	0.0000	0.0000	0.0189	0.0000	0.0000	0.0272

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) 2015 not included.

Notes: The table presents the estimation results for the first stage of the panel IV model, for the diferent periods used throughout the paper, demonstrating that the instruments – dummies for each HDI range – affect the number of loans granted in a statistically significant way and with the expected signs. Robust standard errors, clustered by region, are presented in parentheses. All regressions include region and year fixed effects.

Table 3: Effect of the FIES loans on admission and enrollment in private universities – regional effect.

Dep. var.:	Admissions	Enrollment	Enrollment	Enrollment	Enrollment	Enrollment	Enrollment
Model	$t = 1$	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$	$t = 6$
OLS	2.633***	2.024***	1.411***	1.071***	0.799***	0.513***	0.235***
	(0.100)	(0.056)	(0.033)	(0.017)	(0.024)	(0.043)	(0.036)
N	1794	1794	1493	1493	1486	1480	1178
R2	0.71	0.72	0.72	0.72	0.72	0.72	0.79
FE	0.196***	0.166***	0.186***	0.143***	0.106***	0.081***	0.073***
	(0.060)	(0.060)	(0.043)	(0.041)	(0.036)	(0.029)	(0.008)
N	1794	1794	1493	1493	1486	1480	1178
R2	0.33	0.43	0.59	0.56	0.54	0.55	0.67
IV	0.462***	0.396***	0.361***	0.331***	0.274***	0.194***	0.118***
	(0.126)	(0.106)	(0.065)	(0.066)	(0.062)	(0.047)	(0.021)
N	1786	1786	1484	1484	1477	1471	1167

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The table shows estimates of the impact of loans on admissions and enrollment in years 1 to 6 in higher education programs taught in the region, using the methods of OLS (top panel), FE (middle panel) and IV (bottom panel). Cluster-robust standard errors presented in parentheses. FE and IV regressions include region and year fixed effects.

Table 4: Effect of the FIES loans on admission and enrollment in private universities – individual effect.

Dep. var.:	Admissions	Enrollment	Enrollment	Enrollment	Enrollment	Enrollment	Enrollment
Model	$t = 1$	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$	$t = 6$
OLS	2.459***	1.889***	1.316***	1.005***	0.721***	0.450***	0.205***
	(0.125)	(0.083)	(0.053)	(0.034)	(0.031)	(0.045)	(0.033)
N	1794	1794	1493	1493	1493	1493	1185
R2	0.75	0.75	0.76	0.76	0.73	0.71	0.77
FE	0.457***	0.357***	0.344***	0.253***	0.127***	0.071***	0.057***
	(0.041)	(0.020)	(0.024)	(0.011)	(0.016)	(0.021)	(0.005)
N	1794	1794	1493	1493	1493	1493	1185
R2	0.58	0.66	0.76	0.78	0.50	0.47	0.69
IV	0.431***	0.332***	0.288***	0.259***	0.176***	0.098***	0.063***
	(0.083)	(0.056)	(0.057)	(0.030)	(0.024)	(0.028)	(0.020)
N	1786	1786	1484	1484	1484	1484	1174

[*] $p < 0.10$, [**] $p < 0.05$, [***] $p < 0.01$.

Note: The table presents estimation results for the impact of FIES loans on the number of admissions and enrollments in higher education of individuals born in the region, using the methods of OLS (top panel), FE (middle panel) and IV (bottom panel). Cluster-robust sdandard errors presented in parentheses. FE and IV regressions include region and year fixed effects.

Table 5: Effect on graduation – individual effect.

Dep. Var:	Graduation $t=1$	Graduation $t=2$	Graduation $t=3$	Graduation $t=4$	Graduation $t=5$	Graduation $t=6$	Graduation $1 \leq t \leq 6$
IV							
loans	0.001	0.026***	0.041***	0.044***	0.021**	0.017	0.171***
	(0.002)	(0.007)	(0.008)	(0.008)	(0.010)	(0.013)	(0.021)
N	1786	1484	1484	1484	1484	1174	1174

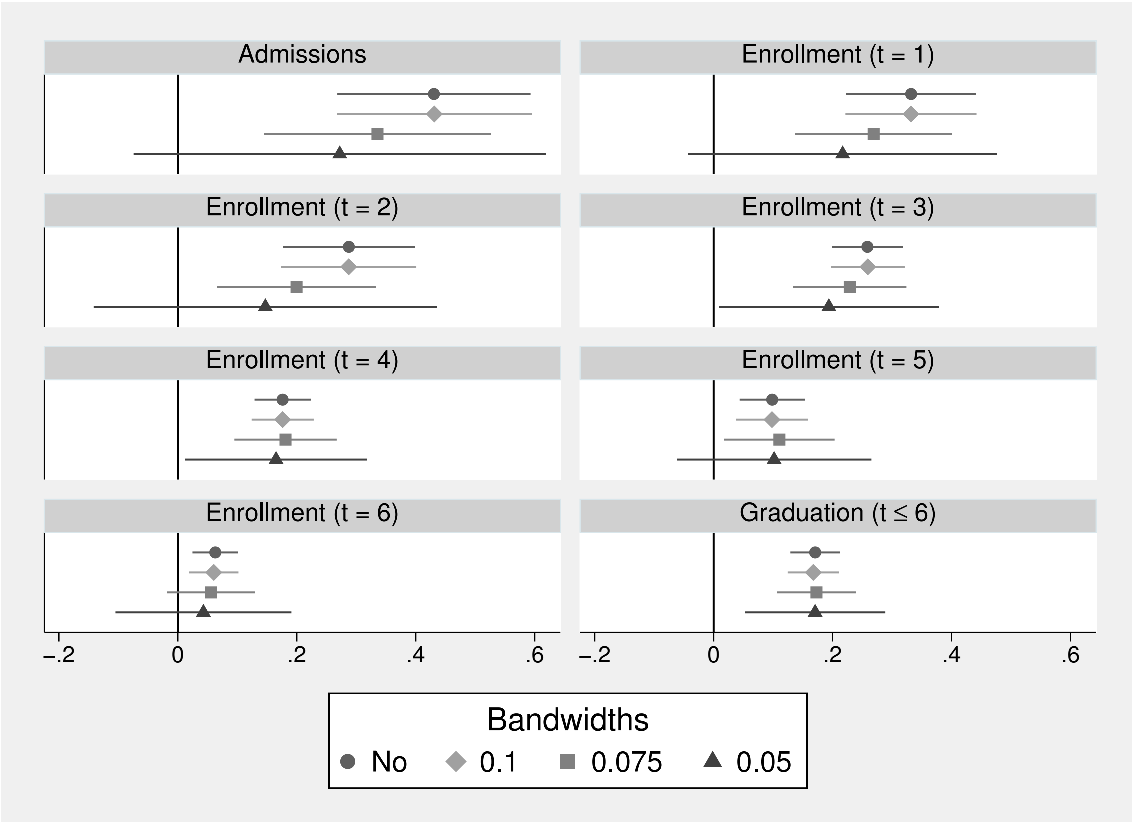
Dep. Var:	Persistence $t=1$	Persistence $1 \leq t \leq 2$	Persistence $1 \leq t \leq 3$	Persistence $1 \leq t \leq 4$	Persistence $1 \leq t \leq 5$	Persistence $1 \leq t \leq 6$
IV						
loans	0.332***	0.289***	0.286***	0.245***	0.211***	0.217***
	(0.056)	(0.057)	(0.034)	(0.023)	(0.021)	(0.023)
N	1786	1484	1484	1484	1484	1174

Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: The table presents estimates of the impact of loans on graduation and persistence. Graduation is cumulative. Persistence for year T represents the sum of students still enrolled in year T, plus graduation up to year $T-1$. Cluster-robust errors in parenthesis. All regressions include region and year fixed effects.

Robustness. To assess the robustness of the results, we run some of the previous regressions focusing on regions with HDI values closer to 0.7. Although the limited number of regions containing HEIs restricts the use of a Regression Discontinuity Design, we implement a similar robustness check by focusing on narrowing bandwidths around the main HDI threshold. Figure 3 illustrates the results of these tests, showing that even with narrower bandwidths, the estimated coefficients remain fairly similar, particularly for outcomes in years $t \geq 4$.

Figure 3: Estimates around $HDI = 0.7$ – estimated coefficients and 95% confidence intervals.



Notes: The graph presents estimates and 95% confidence intervals for the effect of the loans on admissions, enrollment and graduation, for different bandwidths around the HDI value of 0.7. The "No" bandwidth estimates correspond to the ones presented previously in the paper.

As mentioned, the discontinuities of the weights and the size of their changes were set arbitrarily. The only non-arbitrary feature of the weights is their negative correlation with regional HDIs. However, the presence of parallel previous trends, along with the stability of the coefficients across varying bandwidths, indicates that the results are not driven by this correlation.

Regarding the imputation procedure for the missing region of birth information, altering the definition of comparable group – using or not the criteria of type of institution, study shift, and type of secondary educational institution attended – does not qualitatively change the results presented. As mentioned, the estimates based on the region of the program should be viewed as an upper bound for the estimates based on the region of birth, and indeed, we find that all the coefficients are lower in the latter case. This pattern further confirms the robustness of the results to the imputation procedure, as the region of the program is known in all cases and does not depend on the imputation.

5.3 Heterogeneity

We now turn to the analysis of how the estimated effects are influenced by selected characteristics of students and HEIs they attend.

Type of high school attended. Table 6 presents estimates by type of high school attended by the student[23]. Graduating from a public high school can serve as an indicator of lower socioeconomic status, as students from these schools generally have lower income. The impact of loans is primarily observed among students who graduated from public high schools. The coefficients for private school graduates are consistently positive, but small and rarely significant. These results indicate a concentration of the effect on individuals most likely to face financial constraints, since graduating from a private school indicates a greater capacity to pay for education.

[23]Since we do not restrict the loans by type of high school attended, but only the outcome, higher values for public school graduates should be expected, since they represent approximately 69% of students entering higher education in 2013-2017. For this estimate, unlike the others, we use data for the period 2013-2017, since the 2012 data had a relatively high incidence of non-reporting for this variable.

Shift of study. Next, we test if the effects vary depending on whether courses are taught during the day or in the evening. Table 7 presents the results, revealing a robust positive effect of FIES on admissions, enrollment, and (cumulative) graduation in evening programs. One additional loan increases admissions in evening programs by 0.457. In the case of daytime programs, however, coefficients are not statistically significant, and even negative in many cases.

The fact that loans induce students to enroll only in evening programs is worth being explored in further detail. An important characteristic of the evening shift is that it allows the student to work during the day. Notably, there is a clear preference for the evening shift among FIES beneficiaries. For instance, in 2016, nearly two-thirds of the programs receiving FIES loans were evening programs.

Evidence suggests that students reduce their labor supply in response to increased financial aid in the form of grants (Denning, 2019; Broton et al., 2016; Park and Scott-Clayton, 2018; Carlson et al., 2022; Kofoed, 2022). In the context of loans, Black et al. (2020) find that increasing borrowing limits also lead to a reduction in labor supply among college students. These findings are not inconsistent with our results, as the former refers to all students receiving loans, while our findings indicate, albeit indirectly, that students induced to enroll because of loans are likely to work during their college years. Moreover, it is not clear whether intensive and extensive margin effects should be similar, given that changes in borrowing limits only affects those already enrolled. For instance, another strand of literature suggests that indebted students behave differently in the labor market, with a lower probability of choosing public interest jobs and a greater probability of choosing higher-paying positions (Rothstein and Rouse, 2011; Field, 2009). Hence, a related possibility is debt aversion, which could cause students to try to repay their debts as quickly as possible. Caetano et al. (2019) and Gopalan et al. (2021) find evidence of debt aversion in the case of student loans, while Di Maggio et al. (2019) find that debt influences risk-taking behavior, reducing the probability of job changes and geographical mobility. Moreover, Booij et al. (2012), for the Netherlands, find that, despite informational constraints, informing students about student loan conditions did not significantly increase take-up, even under favorable conditions, as students preferred to work part-time to avoid accumulating debt. Although debt aversion could also be a contributing factor, we believe that the preference for the evening shift is more likely associated with financial constraints. This is supported by the fact that debt-averse students have the option to borrow less than the maximum amount, but such behavior is rarely observed in the case of FIES.

Student responses should also depend on whether loans can fund living expenses or just tuition and college fees, such as in FIES. In this regard, one important concern is determining whether working while studying has a detrimental effect on students. Neyt et al. (2019) review the literature on the impact of student employment on educational outcomes, arguing that student employment tends to have a more detrimental effect on persistence than on academic performance. Conversely, work experience accumulated during college, when related to the field of study, can enhance job prospects, but does not lead to higher future wages (Weiss et al., 2014; Sanchez-Gelabert et al., 2017).

In summary, while our study does not determine whether students change labor supply in response to loans, our findings indicate that work compatibility plays a crucial role in shaping how students respond to loan policies.

Institution type. Table 8 presents the estimated effects separately by type of institution. Despite the comparable size of for-profit and non-profit HEIs in aggregate, our findings indicate that the effect is primarily driven by non-profit institutions.

For-profit HEIs exhibit a distinct pricing behavior in comparison to non-profit HEIs (Cellini, 2021; Baird et al., 2022; Cellini and Goldin, 2014; Dynarski et al., 2022)[24], allowing them to adjust prices or provide their own funding in response to reduced external funding sources. This practice has become increasingly prevalent in large HEIs in Brazil following the reduction of FIES. Thus, for-profit institutions may utilize various funding combinations as tools for price discrimination (Dynarski et al., 2022; Fillmore, 2023). Analyzing the impact of loans on alternative funding sources in subsequent analyses will enable us to better understand the mechanisms that contribute to the stronger effect of loans on non-profit institutions.

The primary determinant of loan effects is found to be the study shift. To further explore this relationship, we conducted an additional test (not included in the paper) by examining all pairwise combinations of the three categories: Shift/Type of High School, Type of High School/Type of HEI, and Shift/Type of HEI. Most combinations involving the day shift exhibit null effects, with two exceptions. Firstly, there is a small negative effect on admissions and enrollment in the day shift/for-profit institutions combination. Secondly, in the day shift/non-profit combination, the effects become positive and statistically significant for late enrollment ($t \geq 2$) and graduation. On the other hand, all pairs involving the evening shift present positive and statistically significant coefficients, indicating that this characteristic appears to be a stronger determinant of loan effectiveness.

The type of high school attended is the "weaker" determinant, as the effect of loans become statistically significant for graduates from private high schools when combined with the evening shift or non-profit Higher Education Institutions (HEIs).

[24] Dynarski et al. (2022), for example, argue that the existing evidence in favor of the Bennet Hypothesis rests primarily in for-profit institutions.

Table 6: Effects by type of secondary educational institution attended (public or private).

Outcomes:	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$	$t=6$
			Public secondary education			
Admissions	0.268***					
	(0.068)					
Enrollment	0.222***	0.224***	0.195***	0.132***	0.086***	0.050***
	(0.054)	(0.047)	(0.023)	(0.014)	(0.016)	(0.013)
Graduation	-0.002*	0.016**	0.048***	0.078***	0.098***	0.120***
	(0.001)	(0.006)	(0.013)	(0.017)	(0.015)	(0.015)
Persistence	0.222***	0.222***	0.211***	0.180***	0.164***	0.155***
	(0.054)	(0.047)	(0.028)	(0.016)	(0.015)	(0.016)
N	1498	1195	1195	1195	1195	886
			Private secondary education			
Admissions	0.097**					
	(0.049)					
Enrollment	0.067*	0.036	0.034	0.009	-0.001	0.002
	(0.036)	(0.035)	(0.027)	(0.023)	(0.022)	(0.011)
Graduation	0.004*	0.005	0.009	0.011	0.008	0.014
	(0.002)	(0.004)	(0.006)	(0.007)	(0.012)	(0.013)
Persistence	0.067*	0.040	0.038	0.018	0.009	0.016
	(0.036)	(0.035)	(0.029)	(0.027)	(0.026)	(0.016)
N	1498	1195	1195	1195	1195	886

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The table presents estimates of the impact of loans on admissions, enrollment, graduation and persistence effect, from year 1 to 6 since admission, by type of secondary education institution attended by the student (public or private). The data covers the period 2013-2017. Graduation is cumulative. Persistence in year T denotes the number of graduates in years $t < T$, along with those currently enrolled in year T. Robust standard errors, clustered by region of birth, are reported in parentheses. All regressions include region and year fixed effects.

Table 7: Study shift (daytime or evening).

Outcomes:	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$	$t = 6$
			Classes during the day			
Admissions	-0.026					
	(0.042)					
Enrollment	-0.032	-0.010	0.000	-0.016	-0.022	-0.009
	(0.037)	(0.031)	(0.021)	(0.021)	(0.021)	(0.013)
Graduation	-0.000	-0.000	0.003	0.002	-0.007	-0.014
	(0.001)	(0.002)	(0.003)	(0.005)	(0.006)	(0.009)
Persistence	-0.032	-0.011	0.000	-0.014	-0.020	-0.013
	(0.037)	(0.031)	(0.021)	(0.020)	(0.020)	(0.013)
N	1786	1484	1484	1484	1484	1174
			Classes in the evening			
Admissions	0.457***					
	(0.062)					
Enrollment	0.364***	0.298***	0.258***	0.193***	0.121***	0.072***
	(0.044)	(0.036)	(0.022)	(0.017)	(0.017)	(0.013)
Graduation	0.001	0.027***	0.066***	0.110***	0.141***	0.185***
	(0.002)	(0.006)	(0.012)	(0.018)	(0.018)	(0.020)
Persistence	0.364***	0.299***	0.286***	0.259***	0.231***	0.230***
	(0.044)	(0.037)	(0.026)	(0.020)	(0.019)	(0.022)
N	1786	1484	1484	1484	1484	1174

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The table presents estimates of the impact of loans on admissions, enrollment, graduation and persistence, from year 1 to 6 since admission, by the shift in which the courses are taught (evening or day). Graduation is cumulative. Persistence in year T denotes the number of graduates in years $t < T$, along with those currently enrolled in year T. Robust standard errors, clustered by region of birth, are reported in parentheses. All regressions include region and year fixed effects.

Table 8: Effects by type of HEI (for-profit or non-profit).

Outcomes:	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$	$t=6$
			For-profit institutions			
Admissions	0.079					
	(0.053)					
Enrollment	0.097***	0.020	0.008	0.011	0.019	0.027
	(0.036)	(0.047)	(0.052)	(0.043)	(0.033)	(0.020)
Graduation	0.002	0.009**	0.014**	0.019**	0.018	0.039*
	(0.002)	(0.004)	(0.006)	(0.009)	(0.018)	(0.024)
Persistence	0.097***	0.023	0.016	0.025	0.038	0.056*
	(0.036)	(0.047)	(0.051)	(0.045)	(0.038)	(0.030)
N	1786	1484	1484	1484	1484	1174
			Non-profit institutions			
Admissions	0.332***					
	(0.100)					
Enrollment	0.219***	0.267***	0.251***	0.165***	0.079***	0.036***
	(0.064)	(0.074)	(0.054)	(0.028)	(0.013)	(0.008)
Graduation	-0.001	0.018**	0.055***	0.093***	0.116***	0.131***
	(0.001)	(0.008)	(0.017)	(0.024)	(0.027)	(0.027)
Persistence	0.219***	0.266***	0.269***	0.220***	0.172***	0.161***
	(0.064)	(0.075)	(0.060)	(0.041)	(0.032)	(0.028)
N	1786	1484	1484	1484	1484	1174

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The table presents estimates of the impact of loans on admissions, enrollment, graduation and persistence, from year 1 to 6 since admission, by type of higher education institution (for-profit or non-profit). Graduation is cumulative. Persistence in year T refers to the number of graduates in years $t < T$, along with those currently enrolled in T. Robust standard errors, clustered by region of birth, are presented in parentheses. All regressions include region and year fixed effects.

Other sectors. Table 9 presents estimates of the effects of FIES on access to three other sectors of postsecondary education: public HEIs, fully online programs, and vocational education[25]. Public HEIs do not charge tuition and fully online programs were not covered by FIES before 2022. We observe that the effect on enrollment in distance higher education is negative, as expected, and the effect size is quite large. Remote degrees are typically more affordable than face-to-face education (Deming et al., 2015), but generally perceived as being of inferior quality, a notion supported by empirical evidence – see, for example, Xu and Jaggars (2013), Bettinger et al. (2017) and the literature reviewed in Dynarski et al. (2022)[26].

We also observe negative effects in public higher education, but these are not persistent over time. Duarte (2020) also finds that eligibility to financial aid decreases the probability of enrolling in public universities in Brazil. This is a more surprising effect, probably related to major/field choice, as public universities in Brazil do not charge tuition or any other fee, and are generally more prestigious and selective.

For vocational education, we find varying effects depending on the type of institution. For private institutions, the effect is positive, which is expected since FIES also funds technical and professional education, but these represent a small portion of total loans. For public vocational institutions we do not find a statistically significant effect.

The evidence on how students change educational paths as a response to loans is very sparse. Bucarey et al. (2020) find, for the Chilean case, that loans induce students to forgo vocational educational in favor of universities. However, this shift results in increased debt accumulation, despite similar labor market returns. In our study, we also find that loans influence students to change their educational paths. Nonetheless, we note that loans primarily lead students to forgo distance degrees, which are presumably of lower quality.

[25] We consider only postsecondary vocational educational enrollment.

[26] The evidence for "blended" learning (combining online and in-person instruction) tends to be more favorable (Dynarski et al., 2022).

One important consideration in interpreting the results is determining whether the effects come from relaxing financial constraints or through a subsidy effect, due both to interest rates lower than market rates and to higher default rates than conventional loans (Solis, 2017). A subsidy effect would impact higher-income students by reducing the present value of college costs[27], and it would also lead to an increase in enrollment in daytime programs. However, similarly to Solis (2017) and Card and Solis (2022), the patterns we observe do not support the presence of strong subsidy effects. On the contrary, our heterogeneity analysis strengthens the case for the importance of financial constraints in determining access to postsecondary education.

This is a policy-relevant observation, since, while loan programs often provide subsidized interest rates, this alone does not appear to have a substantial impact on enrollment. Consequently, although more research is needed on this subject, results indicate that, under a limited budget, charging market rates, but offering more loans, could be a more effective way of increasing college access than offering a smaller number of subsidized loans.

[27] This would not happen if enrollment rates were already too high for this group, but this is not the case, since enrollment rates were still increasing for the highest income quartile in 2012-2017 (Brasil, 2020).

Table 9: Effects on remote education and public universities.

Outcomes	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$	$t=6$
			Remote higher education			
Admissions	-0.405***					
	(0.070)					
Enrollment	-0.264***	-0.146***	-0.079***	-0.082***	-0.053***	-0.015**
	(0.049)	(0.033)	(0.022)	(0.025)	(0.021)	(0.007)
Graduation	-0.003***	-0.029***	-0.053***	-0.091***	-0.118***	-0.106***
	(0.001)	(0.006)	(0.011)	(0.020)	(0.029)	(0.028)
			Public higher education			
Admissions	-0.106***					
	(0.030)					
Enrollment	-0.093***	-0.063***	-0.049***	0.059**	0.061	0.015
	(0.023)	(0.020)	(0.016)	(0.024)	(0.039)	(0.038)
Graduation	0.001	0.001	-0.004	0.010	0.041***	0.027
	(0.001)	(0.002)	(0.003)	(0.006)	(0.010)	(0.026)
			Total enrollment in vocational education			
Public Inst.	0.015					
	(0.040)					
Private Inst.	0.297**					
	(0.126)					
N	1786					

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The table presents loan impact estimates on remote higher education (top panel), public higher education (middle panel), and vocational education (bottom panel). For the top and middle panels, the outcomes are admissions, enrollment, and graduation, for years 1 to 6 since admission. The bottom panel presents estimates for the effect of loans on total enrollment at year t for public and private vocational educational institutions. We present effects on total enrollment due to the unavailability of data for the year of admission in the case of vocational education. Graduation is cumulative. Persistence in year T refers to the number of graduates in years $t < T$, along with those currently enrolled in T. Robust standard errors, clustered by region of birth, are presented in parentheses. All regressions include region and year fixed effects.

Other funding sources. Table 10 presents the effects of loans on the take up of other funding sources, disaggregated by for-profit institutions (first panel) and non-profit institutions (second panel). In contrast to the previous analysis, the explanatory variable in this case is not granted loans but the number of admitted students who benefit from FIES loans. The dependent variables are the number of students admitted with other funding sources. Consequently, we examine whether an increase in the admission of FIES students influences the admission of students receiving other types of funding. It should be noted that the alternatives are not mutually exclusive, meaning that the same student may have multiple funding sources simultaneously.

In the case of for-profit HEIs, we find a substantial crowding out effect. Specifically, for each student admitted receiving FIES, there is a reduction of 0.696 students admitted who receive any other form of funding. This effect is primarily driven by a decrease in the number of students admitted with HEI grants. Unfortunately, data on the percentage of tuition covered by these grants is not available, but only whether a student receives that type of funding or not[28]. However, for an effect of this size, the change in actual prices would be meaningful even if grants covered only a relatively minor portion of tuition.

For non-profit HEIs, on the other hand, we do not observe a crowding out effect, with coefficients being actually positive but not statistically significant. HEI loans are negatively affected in both groups, but the effects are relatively small. The bottom panel of Table 10 presents the first stage estimates, with instrument coefficients being all positive, as expected, and statistically significant in all but one case.

Theoretical models and empirical evidence support the idea that HEIs engage in price discrimination (see Rothschild and White (1995), Epple et al. (2017), and Fillmore (2023), for example), particularly by charging higher prices to higher in-

[28]Without knowing the size of grants, it is difficult to assess whether the size of grant reduction is sufficient to explain the null effect of loans on enrollment in for-profit HEIs.

come students. Since financial aid increase willingness to pay, HEs may extend a similar treatment to students benefited by aid, particularly in the case of for-profit institutions. Therefore, our finding that FIES had no effect on enrollment in for-profit HEIs is consistent with the theoretical literature, considering the the distinct pricing behaviors observed between for-profit and non-profit institutions.

Table 10: Effects on the adoption of other funding sources.

Dep. var.:	Loans, except FIES			Grants			All,
	All	HEI	Other	All	HEI	Other	except FIES
For-profit HEIs							
FIES adm.	-0.069**	-0.051	-0.015	-0.648***	-0.660**	0.044	-0.696***
	(0.034)	(0.032)	(0.016)	(0.233)	(0.289)	(0.087)	(0.139)
N	1611	1611	1611	1611	1611	1611	1611
Non-profit HEIs							
FIES adm.	-0.042	-0.095*	0.003	0.232	0.077	0.096	0.186
	(0.046)	(0.049)	(0.031)	(0.289)	(0.220)	(0.147)	(0.272)
N	1613	1613	1613	1613	1613	1613	1613

First stage results

	For-profit	Non-profit
$Z_{2016}^{[0.5,0.6)}$	344.1*	203.6***
	(178.2)	(64.7)
$Z_{2016}^{[0.6,0.7)}$	371.4***	200.3***
	(121.3)	(62.9)
$Z_{2017}^{[0.5,0.6)}$	482.6***	238.8***
	(175.5)	(60.8)
$Z_{2017}^{[0.6,0.6)}$	451.6***	234.7***
	(163.1)	(62.0)
F(4, 282)	6.9937	5.6943
Prob > F	0.0000	0.0002

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The table presents estimates of crowding-out effects of FIES loans on other forms of aid. Negative coeffients indicate that more students admitted with FIES loans are associated with less students admitted with that form of aid. The first panel presents results for for-profit HEIs, while the second panel presents the results for non-profit HEIs. The third panel presents the first stage results. Robust standard errors, clustered by HEI, are presented in parentheses. All regressions include HEI and year fixed effects.

It should be noted that Regression Discontinuity (RD) estimates, such as those based on admission cutoffs, are unable to account for these price effects. To illustrate, consider an extreme example where higher education institutions (HEIs) face a maximum capacity of $\bar{q}$ students and set prices to operate at capacity. Additionally, suppose some students face financial constraints. In this scenario, providing more loans may enhance the recipient's capacity to pay, thereby increasing their probability of enrolling in higher education. A RD approach would find a positive effect in this case. However, the total effect of the policy is zero by design since HEIs would still admit $\bar{q}$ students, raising prices in response to excess demand. Consequently, in this extreme example, we would observe a discontinuity in enrolling probabilities caused by loans, despite loans having no overall effect on enrollment.

6 Final remarks

In this paper, we investigate the effects of loans on college access by leveraging a natural experiment created by the introduction of a regional allocation rule for FIES, the primary funding program for postsecondary education in Brazil. By tracking cohorts throughout their college years, we estimate the effects of loans on enrollment, persistence, and completion, up to the sixth year since entry. Previous related studies have primarily focused on the early years of higher education, reporting the effects on admission or persistence only up to the second year. However, our study provides a complete picture of how these effects evolve until graduation.

Despite some skepticism about the effectiveness of FIES in fostering higher education enrollment, our findings indicate higher overall impacts compared to similar programs in other countries. However, the scarcity of international evidence limits our ability to assess the potential for further improvements in this aspect.

Adding to the existing literature, our analysis uncovers notable heterogeneity in the effects of loans, with greater impacts observed among individuals more likely to face financial constraints. In particular, the effects of the program are more pronounced among students who attended public secondary schools, a population that typically originates from economically disadvantaged families, thus contributing to the reduction of inequality. Additionally, loans are particularly effective in increasing enrollment, persistence, and completion in programs offered during evening hours. We interpret this finding as also related to financial constraints, since study shift is closely related to work-study compatibility, and financially constrained individuals may be compelled to work while studying in order to cover their living expenses[29].

On the other hand, FIES does not effectively increase enrollment in daytime programs or among students coming from private secondary schools, which are less likely to be financially constrained. Our results suggest that, if one intends to maximize enrollments in higher education, prioritizing work-study compatibility can significantly enhance the extensive margin effects of higher education loan programs. It remains to be determined whether covering living expenses would change students choices of study shift.

Previous estimates of the effect of loans have primarily focused on the effect of aid on individual enrollment probabilities. However, one of the findings of this paper is that individual enrolling probabilities do not reflect the full impact of loans, as they cannot account for the externalities arising from the pricing behavior of higher education institutions. For instance, the level of government funding could impact the prices charged to other students. Therefore, in a scenario where funding results in sufficiently higher prices (reduced institutional aid) for other students, we would observe a positive effect of loans in RD designs without a corresponding increase in overall enrollment, as students with loans would simply be replacing other students. In this regard, we show that for-profit institutions respond to increased government funding by reducing their own grant programs, while non-profit institutions do not

[29] Working while in college, and consequently accumulating labor experience, may also serve as a means of risk diversification, considering the uncertainty in returns from higher education.

show a similar behavior. Unlike RD studies, our design captures these externalities, and accordingly, we observe that loans lead to higher overall enrollment in non-profit institutions, but not in for-profit institutions.

Gaining a better understanding of the behavior of non-profit, for-profit and public higher education institutions, how they compete, and how this competition is affected by funding, is an intriguing and policy-relevant area of research. Despite its position as the fourth largest educational market globally, the Brazilian context is significantly underrepresented in the international literature. Therefore, we believe that it provides a fruitful setting for future research in the economics of higher education. In particular, our empirical strategy can be extended to many other research questions, as shown by a related paper (Ávila and Terra, 2023), that relies on the same policy change to examine the impact of student loans on the behavior and finances of higher education institutions, focusing on the for-profit sector.

In the Brazilian case, further research could focus on the relationship between institutional quality and labor market returns of marginal students, since educational quality has been a point of contention in funding access to private higher education institutions. In addition, it is important to understand how these factors influence repayment, a key aspect for the fiscal sustainability of loan policies, and a major obstacle to the implementation of such policies in developing countries.

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A Loan allocation rules

1. The "Social Relevance Criteria" (SRC) is calculated through the formula:

$$SRC = 0,7 \times CDHE + 0,3 \times CDSF$$

   where:

   - CDHE is the Coefficient of Demand for Higher Education, given, in year t, by the share of the region in the country-wise total of individuals that scored at least 450 points in ENEM in year $t-2$ and/or registered to take ENEM in year $t-1$.
   - CDSF is the Coefficient of Demand for Student Financing, given by the share of the region in the total number of applicants for FIES in year $t-1$.

2. To prioritize less developed regions, the distribution implied by the SRC is recalculated based on weights depending on HDI ranges. Table 1 presents the weights for each of these ranges.

B Data sources and summary Statistics

Table 11 presents data sources and Tables 12 to 16 present descriptive statistics for the main variables used in the paper. Table 17 present the percentage of missings in the region of birth data, by year. The regional distribution of Brazilian microregions and respective average HDIs is shown in Figure 4, considering only the municipalities that had HEIs in 2017. Figure 5 presents the evolution of completion rates for students admitted in 2011, for each type of HEI. Figure 6 presents the distribution of cutoff scores, by degree/shift/institution, for FIES and Federal

Universities. Figure 7 presents event studies for the dependent variables used in the paper.

Table 11: Data sources.

Variables	Sources	Years
Municipal Human Development Index - HDI	UNDP - United Nations Development Programme[30]	2010
FIES loans	Dados abertos FIES – FNDE	2012 a 2019
National high school exam takers and applicants	Microdados do ENEM - INEP	2012 a 2019
Higher education admissions, enrollment and conclusion	Censo da Educação Superior - INEP[31]	2012 a 2019

Table 12: Summary statistics, all regions.

	Mean	SD	Min	Max	N
All regions					
HDI	0.722	0.051	0.503	0.824	2547
Loans granted	1577	6179	1	153627	2081
Admissions	4397	16745	83	303349	2547
Evening	3158	11777	69	218894	2547
Daytime	1239	5028	13	84456	2547
For-profit	2164	7595	25	118135	2547
Non-profit	2220	9628	20	185214	2547
Public High Sch.	2927	11093	60	226458	2547
Private High Sch.	1470	6067	23	122339	2547
Enrollment ($t = 1$)	3446	12810	75	227452	2547
Enrollment ($t = 2$)	2591	9265	56	169162	2547
Enrollment ($t = 3$)	2081	7012	40	125313	2547
Enrollment ($t = 4$)	1670	5190	36	90376	2547
Enrollment ($t = 5$)	1160	3322	16	52570	2547
Enrollment ($t = 6$)	561	1601	4	21204	2173
Graduation ($1 \leq t \leq 6$)	1655	5773	37	103387	2173

Table 13: Summary statistics, HDI in 0.5 range.

	Mean	SD	Min	Max	N
$HDI \in [0.5, 0, 6)$					
HDI	0.562	0.030	0.503	0.593	59
Loans granted	369	663	1	2898	35
Admissions	810	495	226	2994	59
Evening	525	318	105	1670	59
Daytime	285	196	81	1324	59
For-profit	575	404	182	2008	59
Non-profit	234	157	20	986	59
Public High Sch.	607	379	172	1941	59
Private High Sch.	203	151	26	1053	59
Enrollment ($t = 1$)	657	397	164	2367	59
Enrollment ($t = 2$)	522	306	116	1734	59
Enrollment ($t = 3$)	440	258	103	1439	59
Enrollment ($t = 4$)	359	205	76	1154	59
Enrollment ($t = 5$)	265	159	45	913	59
Enrollment ($t = 6$)	118	57	20	271	47
Graduation ($1 \leq t \leq 6$)	338	163	71	821	47

Table 14: Summary statistics, HDI in 0.6 range.

	Mean	SD	Min	Max	N
$HDI \in [0.6, 0, 7)$					
HDI	0.660	0.025	0.604	0.699	597
Loans granted	745	2777	1	35932	401
Admissions	1711	4000	112	45350	597
Evening	1167	2372	90	26724	597
Daytime	543	1664	23	18742	597
For-profit	1122	3001	48	35265	597
Non-profit	588	1072	31	10649	597
Public High Sch.	1112	2062	79	21608	597
Private High Sch.	598	1995	30	25407	597
Enrollment ($t = 1$)	1407	3299	95	37663	597
Enrollment ($t = 2$)	1092	2462	77	28751	597
Enrollment ($t = 3$)	906	1967	46	23057	597
Enrollment ($t = 4$)	745	1560	46	17959	597
Enrollment ($t = 5$)	556	1192	30	13918	597
Enrollment ($t = 6$)	288	727	12	8050	505
Graduation ($1 \leq t \leq 6$)	686	1312	37	14435	505

Table 15: Summary statistics, HDI in 0.7 range.

	Mean	SD	Min	Max	N
$HDI \in [0.7, 0, 8)$					
HDI	0.746	0.024	0.700	0.799	1870
Loans granted	1771	6768	1	153627	1624
Admissions	5254	19194	83	303349	1870
Evening	3809	13542	69	218894	1870
Daytime	1445	5704	13	84456	1870
For-profit	2495	8605	25	118135	1870
Non-profit	2742	11101	42	185214	1870
Public High Sch.	3507	12756	60	226458	1870
Private High Sch.	1747	6914	23	122339	1870
Enrollment ($t = 1$)	4096	14663	75	227452	1870
Enrollment ($t = 2$)	3069	10595	56	169162	1870
Enrollment ($t = 3$)	2456	8003	40	125313	1870
Enrollment ($t = 4$)	1967	5907	36	90376	1870
Enrollment ($t = 5$)	1355	3758	16	52570	1870
Enrollment ($t = 6$)	647	1791	4	21204	1603
Graduation ($1 \leq t \leq 6$)	1952	6592	39	103387	1603

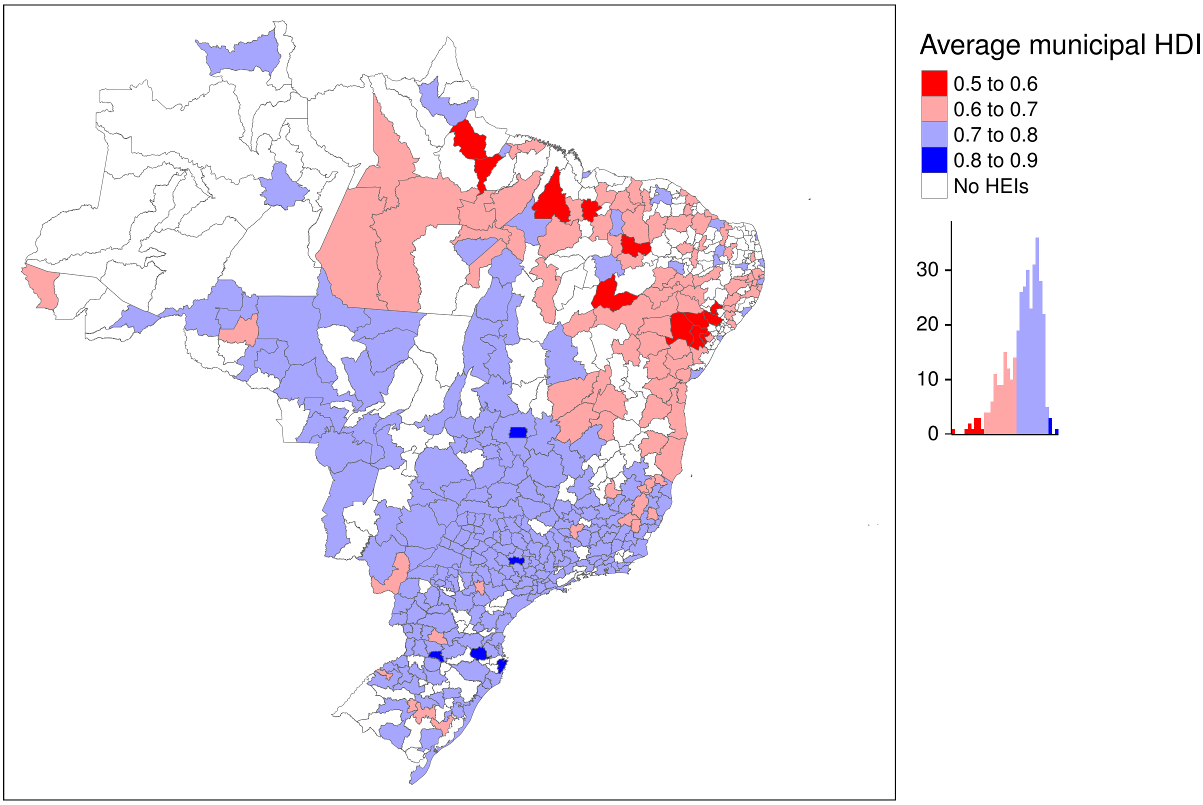
Table 16: Summary statistics, HDI in 0.8 range.

	Mean	SD	Min	Max	N
$HDI \in [0.8, 0, 9)$					
HDI	0.809	0.011	0.800	0.824	21
Loans granted	4540	8320	7	29732	21
Admissions	14521	18950	541	44217	21
Evening	9195	11571	446	28383	21
Daytime	5326	7464	95	17363	21
For-profit	6757	9075	247	21010	21
Non-profit	7719	10027	233	25461	21
Public High Sch.	9310	12001	415	31519	21
Private High Sch.	5211	7142	127	17119	21
Enrollment ($t = 1$)	11322	14772	427	35101	21
Enrollment ($t = 2$)	8450	10928	381	27633	21
Enrollment ($t = 3$)	6690	8637	309	21417	21
Enrollment ($t = 4$)	5192	6606	316	17123	21
Enrollment ($t = 5$)	3536	4452	329	11864	21
Enrollment ($t = 6$)	1694	2164	152	5889	18
Graduation ($1 \leq t \leq 6$)	5746	7351	486	17569	18

Table 17: Missings in the region of birth variable.

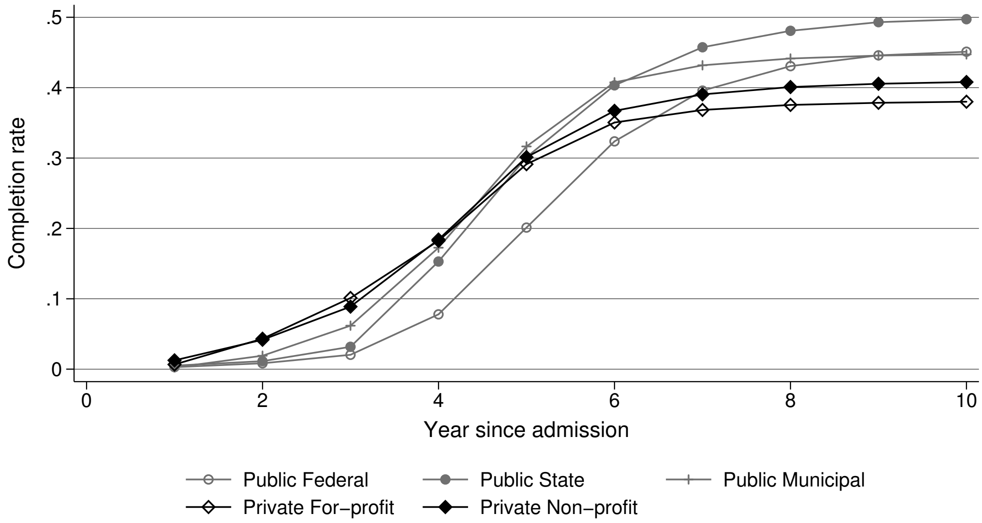
Year	Non missing	Missing	%
2012	1,324,932	415,621	0.239
2013	1,234,464	498,141	0.288
2014	1,335,618	543,373	0.289
2015	1,208,259	513,403	0.298
2016	1,125,780	511,681	0.312
2017	1,074,457	575,674	0.349

Figure 4: Average HDI of the Brazilian microregions with (operating) higher education institutions in 2017.



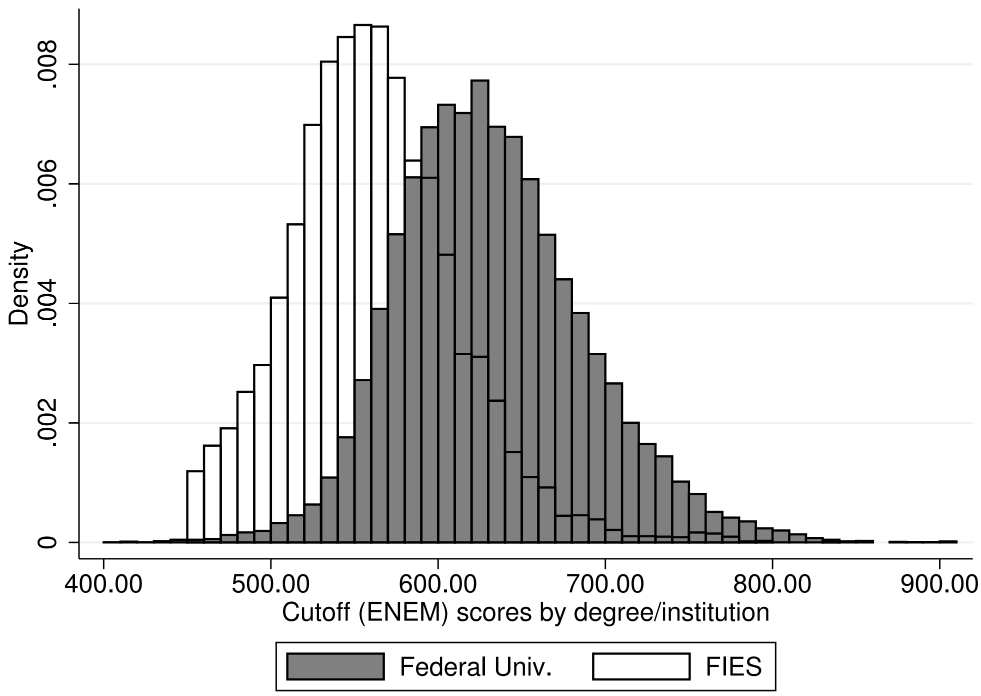
Data sources: Brazilian Institute of Geography and Statistics (IBGE) and United Nations Development Programme (UNDP).

Figure 5: Evolution of completion rates in higher education, by type of institution, for students admitted in 2011.



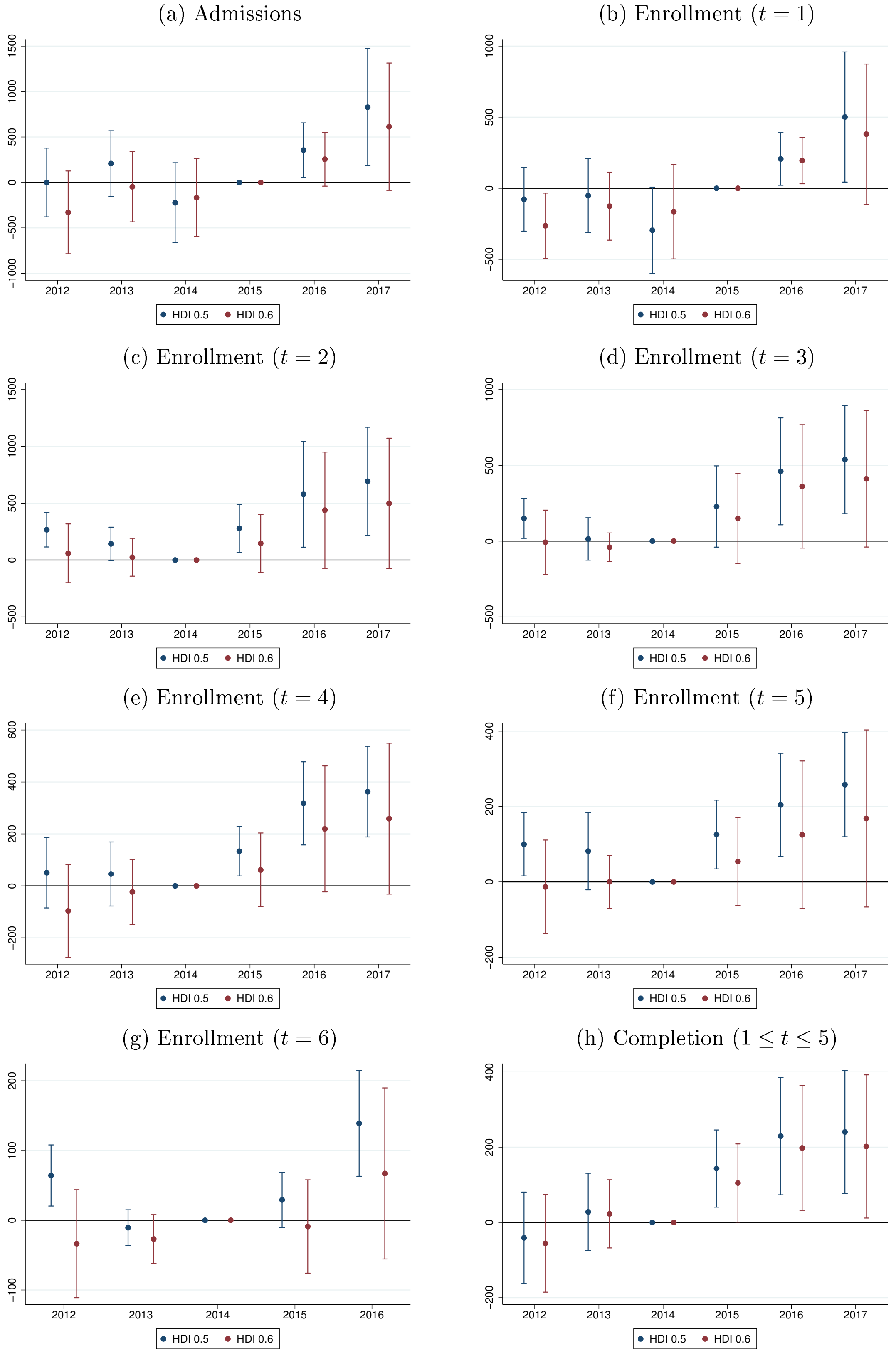
Data source: INEP.

Figure 6: Distribution of cutoff scores, by degree/shift/institution, for FIES and Federal Universities.



Data source: Ministry of Education.

Figure 7: Event study for admissions, enrollment and completion.



Note: Panel (a) to (h) present the event study estimation that measures the difference of admission, enrollment and completion levels of groups 0.5 and 0.6, relative to group 0.7. For $t = 1$ variables, 2015 is the baseline year. For $t > 1$, we adopt 2014 as the baseline.