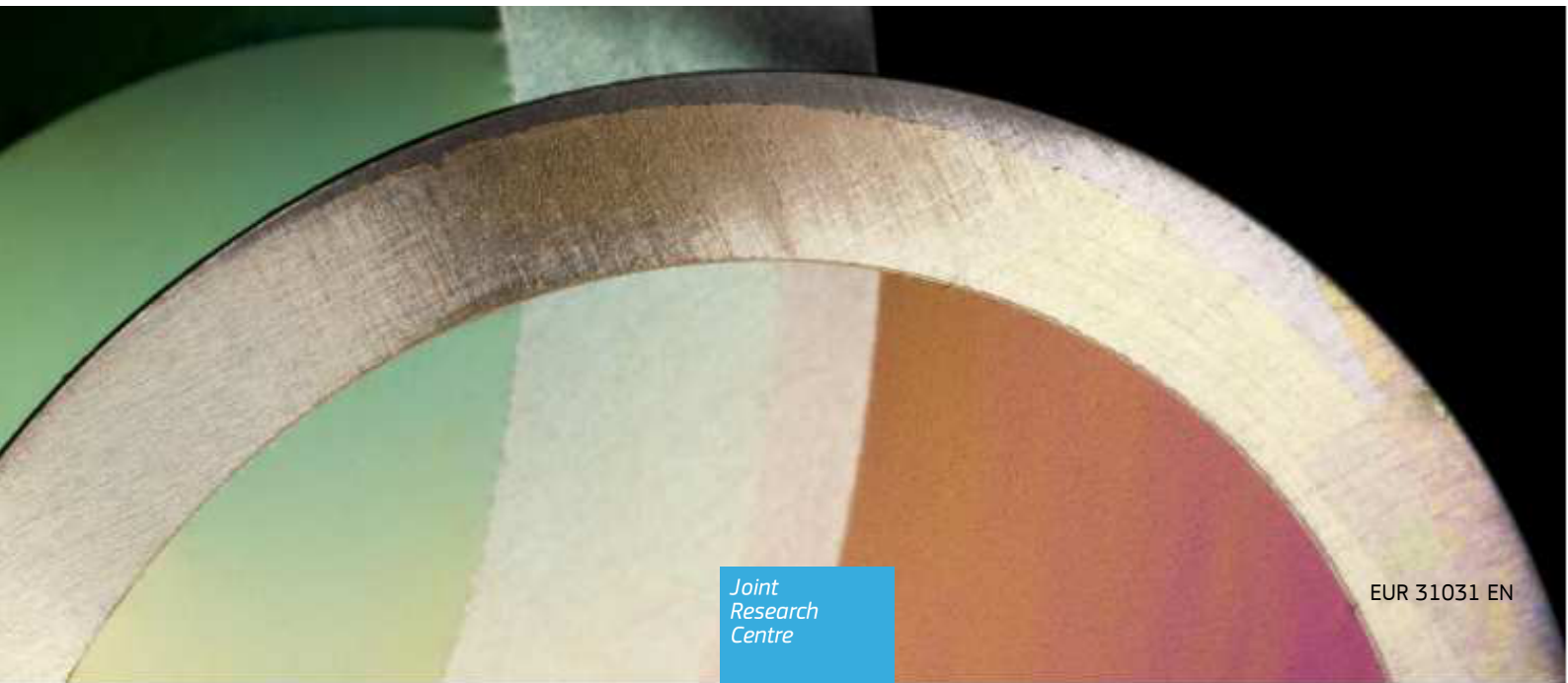
European Commission

JRC TECHNICAL REPORT

Evaluation of the higher education grant system for less privileged students in Portugal

Sophie Guthmuller
Elena Claudia Meroni



Joint Research Centre

EUR 31031 EN

Contact Information
Name: Elena Meroni
Address: Joint Research Centre, Via Enrico Fermi 2749, 21027 Ispra (VA), Italy
Email: elena.meroni@ec.europa.eu

EU Science Hub
https://ec.europa.eu/jrc

JRC128577

EUR 31031 EN

PDF ISBN 978-92-76-49649-6 ISSN 1831-9424 doi:10.2760/95533

Luxembourg: Publications Office of the European Union, 2022

How to cite this report: Guthmuller, S. and Meroni, E., Evaluation of the higher education grant system for less privileged students in Portugal, EUR 31031 EN, Publications Office of the European Union, Luxembourg, 2022, ISBN 978-92-76-49649-6, doi:10.2760/95533, JRC128577.

Contents

Foreword

The Portuguese Human Capital Operational Programme (HCOP) has commissioned the Joint Research Centre, Competence Centre on Microeconomic Evaluation (CC-ME CRIE) within the Data Fitness Initiative, support in the evaluation of the higher education grant system for less privileged students in Portugal, partly funded by EU Funds. This evaluation is carried out in collaboration with the Directorate-General of Higher Education in Portugal (DGES), the Directorate-General for Statistics on Education and Science (DGEEC) and the Social Inclusion and Employment Operational Program (SICOP).

Acknowledgements

The realization of this report would not have been possible without the precious help and active collaboration of colleagues from the Human Capital Operational Programme (HCOP):Joaquim Bernardo, Miguel Feio, Liliana Pereira de Melo, Marta Rocha, and Ana Salvado; from the Directorate-General for Statistics on Education and Science (DGEEC): Patricia Engracia, Joao Baptista, and Nuno Rodrigues; and from the Directorate-General of Higher Education in Portugal (DGES): Priscila Couto and Filipe Leite. The authors also acknowledge the support received by JRC legal department, in particular by Cesar Alonso Iriarte and Laura Nunez Barez

Authors
Sophie Guthmuller, Elena Claudia Meroni

Executive summary

This report focuses on the evaluation of "Higher education grant system for less privileged students" in Portugal, a grant program for students (coming from families) with low income whose purpose is to respond to the real needs of students, ensuring equity in the allocation of social benefits and, as a consequence, promoting social action that favors access to higher education and increases the attendance success.

This evaluation is justified by the fact that Portugal 2020 provides a broad set of support in this area, aiming to increase the proportion of the population with higher education to the threshold defined in the Portuguese Reforms Plan: 40% of the population aged 30–34 with a higher education diploma in 2020 (in 2013, the rate of graduates from higher education was 29.2%, around 10 percentage points from that target and below the EU average).

Since 2011, the grant has supported around 70,000 students every year, and it is supported by the state and by the European Social Fund (ESF) in the less privileged Norte, Centro, and Alentejo regions. This evaluation has a national scope as the grant rules are the same all over the country, being 100% supported by the state in the regions not supported by the ESF.[1]

The intervention is a yearly grant intended for students from low-income households. Eligibility for the grant is conditioned on two main criteria: (1) having resources (per capita income) below a certain threshold and (2) having completed a minimum number of credits the previous year of study (for students in curricular years above the first one). The grant covers tuition fees and provides additional cash to very poor students.

Thanks to two sources of administrative data, we are able to precisely identify eligible students, and we adopt a regression discontinuity approach to identify and measure the causal impact of the grant on academic success.

The first part of this report analyses the impact of the grant for students applying for the first time and enrolled in the first year of a degree. Access to the grant for this sub-sample of students is solely determined by family income. We use a regression discontinuity design, which compares students whose per capita income is just above and just below the threshold, to identify the causal effect of receiving the grant. We measure academic success through several variables: dropout at the beginning and end of the first year, type of course enrolled in, and credits obtained at the end of the first year. Finally, longer-term academic success is also investigated using the probability of graduating, the final mark obtained at graduation, and the probability of graduating on time.

The main results for the sample of first-year students are the following:

— The analyses on the full sample of first-year students show a negative effect of the grant on the probability of dropout at the beginning of the first year, a positive effect on the probability of completing at least 36 credits, which is the requirement to get the grant also in the following academic year, and of completing all credits in which a student is enrolled in at the end of the first year, and a positive effect on the probability of graduating on time.

— We investigate whether these main results are driven by particular types of students or university programs, and the results of this heterogeneity analysis show that the impact of the grant is different according to students' characteristics: the effect is stronger for males in terms of dropout and graduation on time, for females in terms of credits, as well as for students coming from less developed regions (Alentejo, the Azores, Centro, and Norte) and for students enrolled in a bachelors degree and attending a public university.

— We investigate the effects of receiving the grant for more than one year, and we find that the positive effect on the probability of graduating on time is greater for students who received the grant for two or three years.

— For bachelor students, the grant also contributes to progressing to a master's degree.

The second part of the analysis focuses on second-year students. We use two approaches to identify the causal effect of the grant on a set of similar outcomes: i) a regression discontinuity design on the sample of students who obtained at least 36 credits the previous year; ii) difference-in-differences on the full sample of second-year students. The main results for the sample of second-year students are the following:

— Being eligible for the grant at the beginning of the second year has a negative effect on dropout at the beginning of that year for students enrolled in a bachelor, but no effect on dropout at the end of that year

— There are differential effects of the grant in the second year according to whether the students also received it during the first year: students who were eligible in both years show lower dropout rates and a higher probability of graduating and of obtaining the credits they enrolled in.

[1] The available data go back to 2012, so the first year used in the analysis is the academic year 2012/2013

— Heterogeneity analysis also show that the impact of the grant is different according to students' characteristics: the effect is stronger for females (for the results regarding credits), for students from less developed regions (for dropout, graduation, and graduation on time), and for students attending a public university.

Overall, the grant has a positive and significant impact and is contributing to the Portuguese government's aim of increasing enrollment and the number of higher education graduates.

1 Introduction

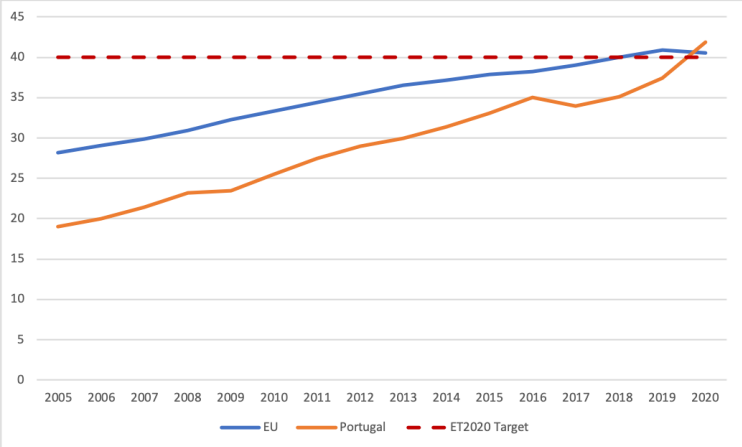
One of the EU "Education and Training" targets for 2020 states that at least 40% of people aged 30–34 should have completed some form of higher education. In recent years, the rate of higher education attainment in Portugal (in groups aged 30–34) increased significantly (41% in 2020) for the first time being above the EU average and the 2020 target of 40% (Figure 1). This was not the case in the past decade, as in 2010 for example, the rate in Portugal was 25%: almost 10 percentage points below the EU average. As a consequence, in the past years, the Portuguese government has been taking many actions to make higher education more attractive and increase the completion rate, including bolstering social support mechanisms for students from disadvantaged backgrounds through a significant increase in grants, the creation of specific grants for students with special educational needs corresponding to the amount of the fee effectively paid, a social scheme for paying tuition fees in multiple installments, and the implementation of a redefined +Superior program to promote and support enrollment in less densely populated regions and in regions where demand for higher education is lower, among other initiatives.

This report focuses on the evaluation of the "Higher education grant system for less privileged students", a grant program for students (coming from families) with low income. The purpose of this grant program is to respond to the real needs of students, ensuring equity in the allocation of social benefits and, as a consequence, promoting a social action that favors access to higher education and increases attendance success.

Since 2011, the grant has supported between 55,000 and 70,000 students every year, and it is supported by the state and by the European Social Fund (ESF) in the less privileged Norte, Centro, and Alentejo regions (Figure 2). All students can apply for the grant, but only those with an income below the predetermined threshold will receive it. (more details about the selection process can be found in Section 1.3).

The purpose of this evaluation is to assess whether the grant contributed to the 2020 European strategy for higher education using counterfactual evaluation methodologies. These methodologies allow answering the following evaluation question: Do students who receive the grant have better academic outcomes than students who applied but did not receive it?[2]. Therefore, in this report we focus on the evaluation criteria related to effectiveness only.[3]

Figure 1: Tertiary educational attainment among the population aged 25-34 in the EU and Portugal (%)



Note: The indicator measures the share of the population aged 25-34 who have successfully completed tertiary studies (e.g. university, higher technical institution, etc.). This educational attainment refers to ISCED (International Standard Classification of Education) 2011 level 5-8 for data from 2014 onwards and to ISCED 1997 level 5-6 for data up to 2013. The indicator is based on the EU Labour Force Survey. *Source*: Eurostat

[2] A variety of outcomes are employed; details are in Section 4.3.1

[3] Effectiveness analysis as defined by the EU Better Regulation guidelines considers how successful an EU action has been in achieving or progressing toward its objective. See `https://ec.europa.eu/info/sites/info/files/better-regulation-guidelines-evaluation-fitness-checks.pdf`

Figure 2: Number of grants submitted, awarded, and rejected since the academic year 2011-2012 (data from 2018-2019 are not final yet)

Academic year	Nr of requested grants	Nr. of grants awarded	Nr of grants rejected
2011-2012	96870	55999	40335
2012-2013	88600	58818	29640
2013-2014	85558	62312	23083
2014-2015	86936	63611	23173
2015-2016	90613	69353	21073
2016-2017	94538	71947	22383
2017-2018	**97003**	**74187**	**22326**
2018-2019	**96001**	**46870**	**10319**

Source: Directorate-General of Higher Education in Portugal (DGES), 2019.

1.1 The Portuguese education system

The Portuguese educational system is regulated by the Basic Law of the Educational System and is divided into three levels: basic (with three cycles), secondary, and higher education. More specifically, it begins with pre-school education, a cycle for children from three to six years of age, followed by basic education, which comprises three sequential cycles: a first cycle of four years (expected attendance from 6 to 10 years of age); a second cycle of two years (expected attendance from 10 to 12 years of age) corresponding to ISCED 1; and a third cycle of three years (expected attendance from 12 to 15 years of age) corresponding to ISCED 2 (lower secondary education). Upper secondary education is a three-year cycle (expected attendance from 15 to 18 years of age; corresponding to ISCED 3) and includes five types of courses: science and humanities courses, vocational courses, specialized artistic courses, own-school-curriculum courses (science-technology courses), and education and training courses. ISCED 4 corresponds to post-secondary non-higher education, while ISCED 5 corresponds to short-cycle higher education programs. The Portuguese education system also includes homeschooling and individual tuition.[4]

Portuguese higher education is organized in a binary system that integrates university education and polytechnic education and is taught in public and private institutions. The private higher education institutions must obtain prior recognition from the Ministry with the authority of Higher Education. University education includes universities, university institutes, and other university teaching institutions. Polytechnic education comprises polytechnic institutes and other polytechnic teaching institutions. Higher education is structured according to the principles of the Bologna Process (since 2005) and is designed for students who have successfully completed an upper secondary education course or obtained a legally equivalent qualification ISCED 6 comprises *licenciatura* (or equivalent) programs, and ISCED 7 corresponds to master's programs (or equivalent). ISCED 8 corresponds to doctoral programs (or equivalent). In 2014, a non-academic higher education cycle was created, called a professional higher technical course, which corresponds to the short cycle of studies linked to the 1st cycle provided for in the Qualifications Framework of the European Higher Education Area.

The Ministry of Education defines educational policies, coordinates their implementation, and as its main financer, ensures the running of the educational system. Other ministries, the autonomous regions, and municipalities also contribute to funding. The Ministry of Science, Technology and Higher Education is responsible for higher education policies and funding. The Ministry of Education finances its central and regional departments, as well as public education institutions, from the state budget. It also subsidizes private and cooperative basic and upper secondary education, according to the terms stipulated in the Private and Cooperative Education Statute. The Agency for the Evaluation and Accreditation of Higher Education is the authority charged with evaluating and accrediting higher education institutions and their study cycles.

The European Commission also funds education in Portugal through the European Regional Development Fund (ERDF) and the European Social Fund (ESF). [5] The Human Capital Operational Program aims to increase the proportion of the population with higher education or equivalent degrees through training with particular general and scientific components, a technical training component, and an on-the-job workplace apprenticeship. This objective is operationalized through the funding of grants in higher education, doctorate and post-doctoral grants, support to professional technical higher education (TeSP), and through the financing of a credit line for higher education students.

[4] By Ordinance no. 69/2019, February 26; Ordinance no. 69/2019, February 26; Decree-Law no. 152/2013, November 4; Decree-Law no. 55/2018, July 6; Ordinance no. 223-A/2018, August 3; Ordinance no. 226-A/201, August 7.

[5] Madeira and the Azores receive transfers from the government's budget to finance education there, except for higher education.

1.2 The higher education grant system as part of the Human Capital Operational Programme (HCOP) within Portugal 2020

Financial support to the higher education grant system in Portugal is co-funded by the European Social Fund (ESF) in the regions Norte, Centro, and Alentejo, through the following operational programs:

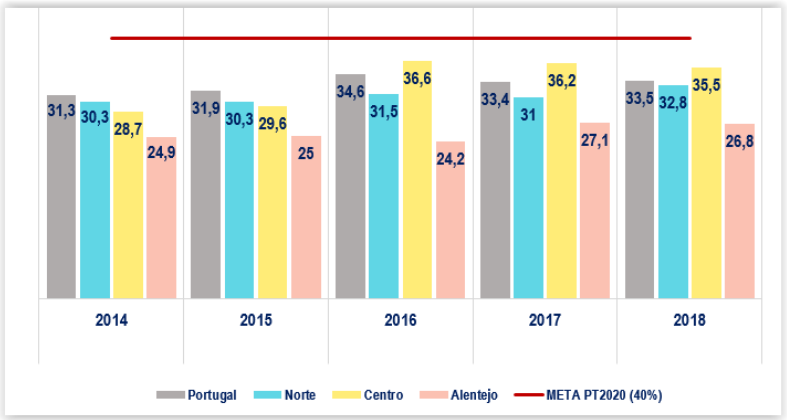
— In programming period (2007–2013), the higher education grant system was supported by the Human Potential Operational Programme (HPOP).

— In the programming period (2014–2020), it was supported by:

- The Human Capital Operational Programme (HCOP), from 2014 to 2017 within
  - Priority axis 10.2 - Improve the quality, efficiency, and access to higher and equivalent education, with a view to increasing levels of participation and skills, particularly for disadvantaged people.
  - Specific objective 2.2.2 - Increase the number of higher education graduates, improve the quality of offers, and strengthen their orientation towards the needs of the labor market.
- The Social Inclusion and Employment Operational Programme (since the reprogramming period in 2018) within:
  - Priority axis 9.1 - Active inclusion, including with a view to promoting equal opportunities and active participation and improving employability.
  - Specific objective 3.1 - Promote the development of socio-professional, personal, social, and basic skills of potentially more vulnerable groups, enhancing their employability and strengthening opportunities for their socio-professional integration.

From January 2014 to September 2019, HCOP supported 116,526 less privileged students in participating in the higher education system in the Portuguese regions of Norte, Centro, and Alentejo (Figure 3). Twelve operations were approved, with a total approved investment of €427M (€363 M ESF). Higher education grants have the highest realization and payment rates within the whole typology of programs. They represent a realization rate of 91% (€330M) of the approved fund, and 92% was paid to the beneficiaries (€334M ESF).

Figure 3: Population between 30 and 34 years of age with higher education (or equivalent) in the Portuguese regions eligible for the ESF



Source: HCOP adapted from EUROSTAT data.

1.3 The higher education grant system characteristics and eligibility conditions

The grant is an annual monetary benefit that allows students whose household financial resources are below a minimum threshold to attend a higher education course or to complete a compulsory internship (maximum per capita income reference values are provided in Annex 1).

The grant is awarded for a full school year (except in some cases), and the application can be renewed annually. All students attending professional higher technical courses, bachelor's degrees, integrated master's and master's courses in Portuguese higher education institutions (public, private, university, and polytechnic) are eligible.

In addition to having resources (income and movable assets) below a certain threshold, students need to have successfully completed the previous academic year and have completed the course within its normal duration. See Annex 2 for the full list of eligibility conditions. If one of these conditions is not met, the grant

is not awarded (reasons for not awarding a grant are provided in Annex 3). These grant award conditions are common in both public and private higher education (see Annex 4).

The amount of the grant depends on the per capita income of the household. The tuition fee is always covered but can never be higher than the maximum amount fixed annually for the 1st cycle of studies of public higher education. Supplements for accommodation and transport can also be granted (see Annex 5 for details regarding the grant amount calculation.)

2 Literature review

Higher education can have a great impact on many dimensions of students' lives, such as earnings and personal development. It has been demonstrated that economic returns related to human capital investment constitute both private and social benefits. For this reason, many countries have in recent years designed interventions aimed at increasing tertiary education. Some examples of interventions include merit-based and needs-based scholarships, loans and tuition subsidies, as well as programs to help students earn some money through a part-time occupation. The Portuguese government has been taking many actions to make higher education more attractive and increase the completion rate. In this context, the aim of the grant program for students (coming from families) with low income is to respond to the real needs of students, ensuring equity in the allocation of social benefits and, as a consequence, promoting a social action that favors access to higher education and increases attendance success. Since from the second year onward, in order to obtain the grant students need to have completed at least 36 credits in the previous academic year, this intervention can be classified as both needs- and merit-based.

Evidence from the literature shows that the majority of programs in place in Europe and the US are based on monetary incentives for low-income students (needs-based grants). Such policies are justified by the fact that, in theory, monetary aids should increase enrollment, performance, and effort. In the US, the largest of these grants (both in terms of the total number of awards and total dollars awarded) is the federal Pell Grant, a needs-based grant awarded to low and moderate income students pursuing a college education at an accredited institution. Concerning enrollment and attendance, the effects of monetary incentives are quite clear: the relationship between the cost of college and enrollment is negative, implying that if costs are reduced enrollment and attendance rates should increase. Dynarski (2003) aimed to evaluate the effect of aid on college attendance using a reform from 1981 that eliminated the Social Security Student Benefit Program, thanks to which students received generous monthly payments while enrolled full-time in college. The elimination of the program reduced the probability of going to college by half. The findings suggest that a grant payment of 1,000 dollars increases ultimate educational attainment by about 0.16 years and the probability of attending college by about 4 percentage points. Indeed, the elasticities of attendance and completed years of college with respect to schooling costs are 0.7 to 0.8. David and Dynarski (2009) and Dynarski and Scott-Clayton (2013) review the findings regarding US financial aid programs and show that there is robust evidence that needs-based grant eligibility can have a strong positive effect on whether students enroll in college, with the average estimated probability of enrollment increasing by 3 to 4 percentage points for each additional $1,000 in grant aid eligibility.

Mixed evidence is provided by European studies. In Germany, an increase in fees corresponding to $1,000 dollars decreased enrollment by between 2.5 and 4 percentage points (Hübner, 2012). However, in Catalonia no effects on enrollment were found in response to increased tuition fees (Montalvo et al., 2018). Similarly, older studies trying to quantify the effect of tuition fees on enrollment rates using European data have, in general, found only a very small or insignificant effect (for example, Canton and De Jong (2005) in the Netherlands and Fredriksson (1997) in Sweden).

The effects on performance or other long-term outcomes are more ambiguous. On the one hand, by reducing college costs, higher benefits may convince students to spend more time studying rather than working. Furthermore, economic incentives may encourage exerting more effort. Both of these factors should improve performance.

Focusing on students in Florida, Castleman and Long (2016) show that Florida Student Access Grant eligibility had a positive impact on short, medium, and long-term college outcomes. The additional $1,300 in grant aid eligibility (in 2000 dollars) increased the probability of immediate enrollment at a public 4-year university by 3.2 percentage points, while it also increased the probability of staying continuously enrolled through the spring semester of freshman year by 4.3 percentage points; no effect was found in terms of enrollment at a private 4-year college. Finally, an additional $1,300 in aid increased the probability of earning a bachelor's degree within 6 years by 4.6 percentage points, or 22%.

Finally, the Pell Grant program, one of the largest financial programs in the US, has been found to cause a small reduction in dropout (after the first year) of between 1.4 and 4 p.p. at Ohio University, depending on the specifications, but this was not always statistically significant (Bettinger, 2004).

As for Europe, some papers such as Agasisti and Murtinu (2016) and Sneyers et al. (2016) confirm these assumptions, finding positive effects on performance. Agasisti and Murtinu (2016) use a matching technique

to estimate the effect of financial aid on some performance indicators at an Italian University (Politecnico di Milano). The findings suggest that obtaining a grant has a positive effect on academic performance: students who receive financial aid obtain more formative credits and are more likely to graduate on time. Evaluating the impact of needs-based grants at five Italian universities, Sneyers et al. (2016) discovered a positive effect on credit accumulation in the first year, fewer dropouts, and more on-time graduations.[6] Garibaldi et al. (2012) find that a 1,000 euro increase in tuition fees at a private Italian university increased the probability of *not* graduating on time by 5.2 percentage points. Also in Italy, Mealli and Rampichini (2012) study the effect of grants on dropout rates during the first year of university, using a regression discontinuity approach applied to 5 universities. In 4 out of the 5, the grant had no effect on dropout during the first year. Only at the University of Padova did the grant contribute to reducing dropout, but this effect was not observed for the poorest students. In Denmark, these positive effects are confirmed even if the impact of aid on both the dropout rate and time of degree completion is smaller (Arendt, 2013).

In the literature, we also find examples of merit-based scholarships, given to students who achieve some specific merit requirements. Generally, programs that link a grant to certain merit requirements work either by reducing the cost of college or by increasing student effort (Scott-Clayton, 2011). In West Virginia, the PROMISE program is entirely based on academic performance and not financial need. The PROMISE scholarship fully covers tuition and other required fees for up to four years. In order to be eligible for the program, freshmen must have a 3.0 high school GPA and obtain a certain score on the ACT or SAT test.[7] In addition, after the first year students need to maintain a 3.0 GPA and complete 30 credits per year, which corresponds to the full number of credits foreseen for 1 year of full-time attendance at college. Those who fail to meet the renewal requirements once cannot later regain the scholarship. It has been demonstrated that that the program has a positive impact on both GPA and credit accumulation, both at the end of the first year (0.15 higher GPA and 2 additional credits) and for the overall college degree (4.6 additional credits and a 0.099 higher GPA over 4 years). It also has large effects on the share of students meeting key achievement thresholds: PROMISE recipients were nearly 25 percentage points more likely to have earned 30 or more credits, the threshold for PROMISE renewal. Finally, recipient students had a probability of graduating within the 4 years between 7 and 9.5 percentage points higher than other students (according to the two different specifications used).[8] Scott-Clayton and Zafar (2019) find that these positive effects on grant recipients still appear after 10 years: grant recipients are more likely to earn a graduate degree, more likely to own a home and live in high-income neighborhoods, less likely to have adverse credit outcomes, and more likely to be in better financial health than similar students who did not receive grant.

Another large merit-based scholarship is Georgia's HOPE program (Henry and Rubenstein, 2002). The two programs (PROMISE and HOPE) are of similar monetary value (both cover tuition and other fees), and both require students to maintain a 3.0 GPA while in college (although PROMISE allows a 2.75 GPA in the first year). But in Georgia, there are no minimum course load requirements for renewal—students need not even attend full-time. Henry and Rubenstein (2002) test the hypothesis that merit-based financial aid increases the quality of education. Their results show that students responded to HOPE. Grades in high school improved and the percentages of both males and females eligible for the scholarship increased. Contrary to the PROMISE program, HOPE does not have strong credit requirements for renewal. As a consequence, Cornwell et al. (2005) find that HOPE recipients at Georgia's flagship university were 9.3 percentage points *less* likely to complete a full-time course load in their freshman year. The difference suggests that students respond strategically to each program's incentives. HOPE's rules encourage students to reduce their course load in order to raise their GPA. Nevertheless, while HOPE may have slowed the time-to-degree, Dynarski (2008) estimates that it increased the college completion rate by 3 to 5 percentage points, with a particularly strong effect for female students.

Leuven et al. (2010) performed a randomized field experiment in Amsterdam where first-year students earned a financial reward if they passed all requirements within one year. The results concerning achievement are ambiguous because for low-ability students the grant seems to have had negative effects whereas for high-ability students performance improved.

The same evidence was found by Solis (2017), who aimed to uncover the effects of financial aid on enrollment in a country where programs are less expensive and policies could have a greater impact (Chile). He studies the impact of receiving a loan that is granted to students whose incomes are in the lowest quantile and whose college admission test scores are above a given threshold. Using a regression discontinuity design based on the admission test score, he finds that the loan programs lead to a large increase in enrollment rates in college, both in the year after high school and in subsequent years.

Solely merit-based aid programs are often criticized as they provide financial assistance to individuals who need it the least and who would almost certainly attend college anyway. Therefore, there is growing consent regarding the importance of packaging financial aid as both merit- and needs-based grants, loans, and work-

[6] It should be noted that these two studies use matching techniques on very small samples, and the parameter retrieve is quite different from what one can estimate using a regression discontinuity design.

[7] They must have scored at least a 21 overall on the ACT or 1,000 on the SAT.

[8] The authors also study possible heterogeneous effects between genders but find no differences between female and male students.

study. However, findings on the effect of combining a merit requirement with needs-based requirements are controversial. Dynarski and Scott-Clayton (2013) provide an extensive review of the effectiveness of financial aid programs in the US and suggest that merit-based incentives within the grant/aid systems are helpful for stimulating better performance by eligible students. If students do not have the resources or do not know how to convert effort into achievement, these are likely to obtain opposite or ambiguous results. For example, Scott-Clayton and Schudde (2016) show that higher financial incentives combined with weak requirements for renewal can cause moral hazard and convince underperforming students to persist in college. In the US, Scott-Clayton and Schudde (2020) examine the consequences of federal Satisfactory Academic Progress (SAP) requirements, which students receiving Pell Grants, student loans, and other need-based federal aid must meet to maintain eligibility: If they fail to reach the SAP requirements, they risk losing their financial aid. They find that among Pell recipients, students who fail to reach the SAP requirements at the end of the first year and are thus more at risk of losing their grant have worse outcomes than those who achieved the minimum SAP requirements. In particular, they find that discouragement effects appear larger and encouragement effects smaller for students further below the GPA threshold: the SAP policy is thus only partly doing its job. It does appear to reduce some unproductive re-enrollments while providing some encouragement for students to perform better, but for many students, by the time they receive their first warning it may be too late for them to improve their GPAs sufficiently to maintain their aid eligibility.

However, findings for Europe seem to point in the other direction. The papers by Agasisti et al. (2021) and Montalbán (2019) investigate the effect of increasing merit requirements for grants targeting low-income students in Italy and Spain, respectively. Agasisti et al. (2021) use a reform in an Italian region that increased by 40% (i.e. from 25 to 35 out of a maximum of 60) the number of credits to be earned in the first academic year in order to maintain aid eligibility. They find that tightening merit requirements had a statistically significant positive effect on various dimensions of performance for the "average" aid recipient. More specifically, positive effects were found on the probability of graduating within the degree's legal duration (three years), which increased by about 8.9 percentage points, and on the probability of graduating in three or four years, which went up by 7 p.p. The reform increased the number of credits earned in the first year. No effects on students' GPAs, final marks, or probability of enrolling in second or third year were found. However, the positive effects were concentrated among high- and medium-ability students, while low-ability students receiving financial assistance were discouraged from continuing in their studies. Montalbán (2019) study the effect of a similar reform in Spain that raised the minimum academic requirement for scholarship renewal. He finds strong positive effects of being eligible for a grant on student performance when combined with demanding academic requirements, while there are no effects on student dropout. Students improved their final exam attendance rate, their average GPA in final exams, and their probability of completing the degree. The results show that being eligible for an average grant of 825 euros (relative to being eligible only for a fee waiver) with strong academic requirements increases the average GPA of students and the fraction of credits earned by 0.45 points (on a 0 to 10 scale) and 6 percentage points, respectively, which corresponds to an increase of approximately 7.3 and 7.6 percent with respect to the baseline mean. These effects correspond to about 25 percent of the standard deviation of the dependent variable.

Few of the studies mentioned above report differential findings according to gender. Angrist et al. (2009), for example, find that a program that offered a tuition fee waiver conditional on reaching a given GPA at the end of the first year was effective exclusively for women, while no effect was found for men.

In conclusion, there is a consensus that needs-based grants have a positive (although small) effect on higher education enrollment. Mixed evidence is found for performance. Merit-based grants work especially if there are tight requirements on credits or GPA, and combining the two (merit and need) can work to increase enrollment and also performance (fewer dropouts, higher grades, lower completion time).

3 Methods

3.1 Counterfactual impact evaluation methods

Our aim is to assess the effectiveness of Portugal's "Higher education grant system for less privileged students". To do so, we compare the outcome of a student who receives the grant to the outcome of the same student had s/he not received the grant. Clearly, the latter outcome is not observable: If a student is treated, i.e., s/he received the grant, we only observe her or his outcome conditional on the fact of having received the grant. Similarly, if a student is not treated, i.e., s/he did not receive the grant, we can only observe his or her outcome conditional on the fact of not having received the grant. This is defined as the "fundamental problem of causal inference" in the economic literature: One cannot observe the status of a treated individual in the scenario where s/he did not receive the treatment (the counterfactual).

One possible way out of this problem is to use the outcomes of students who did not receive the grant as the counterfactual. However, since the students selected to receive the grant may be different from those not selected, we cannot simply retrieve the impact of the intervention by comparing the outcomes of the two groups

because the results would suffer from a bias related to the mechanism of selection into the treatment (selection bias). Based on the eligibility conditions, we know that students who do not receive the grant have a higher per capita income than those receiving the grant, and this may be associated with belonging to a family with a higher socioeconomic background, which can have a direct effect on the academic performance of students. Thus, just a simple comparison of the outcomes of the two groups of treated and non-treated students could lead to observing an effect that may not be the direct causal effect of receiving the grant. Counterfactual evaluation methodologies aim to identify the most suitable control group of students, so as to be sure that any difference in the outcomes between the group of students receiving the grant and the selected group of students not receiving the grant is due to having received the grant.

Among the several econometric techniques that can help us account for selection bias and perform a valid evaluation, the final choice of the most suitable counterfactual method is strongly related to institutional background and to data availability. In this particular situation, we exploit the fact that assignment to the treatment (i.e., the decision to award the grant or not) is based on a clear rule: Only students whose per capita income is below a predefined threshold (also called the "cutoff") are treated, and those whose income is above the threshold are not treated. Thus, the assignment is based on a well-identifiable variable—called a "running variable" or "score"—here, per capita income. This setting is perfect to apply the regression discontinuity design (RDD) methodology. RDD has emerged to be the most credible non-experimental method for the analysis of causal effects in observational studies. Within an RDD, all units have a score and treatment is assigned to those units whose value of the score is below a known cutoff and not assigned to units whose value of the score exceeds the cutoff. When all units are perfectly sorted around the cutoff i.e., all units eligible for the treatment receive the treatment, a "sharp RDD" is applied. When units are not perfectly sorted around the cutoff, a "fuzzy RDD" is applied.

The key feature of the approach is that the probability of receiving the treatment changes abruptly at the known threshold. The discontinuous change in this probability can be used to identify the local causal effect of the treatment on an outcome of interest because units with scores just below the cutoff can be used as the counterfactual for units with scores just above it. Indeed, RDD assumes that students with an income just above or just below the predefined threshold are very similar and thus comparable. More details are provided in the following section.

We use RDD methods to study the impact of receiving the grant for first-year students, as reported in Section 4, and for a sub-sample of second-year students. We complement the analysis on the second-year students using a difference-in-differences approach.

3.2 Data

The analysis is based on two sources of administrative data. The Directorate-General of Higher Education in Portugal (DGES) provides access to the universe of students applying for the grant from 2012 to 2018. Using a unique student identifier, this information is merged with another dataset containing information about their academic career and progression, provided by the Directorate-General for Statistics on Education and Science (DGEEC).

Data were granted to the JRC through a personal data transfer agreement, preceded by the approval of a "data protection record".[9] In order to comply with personal data regulations, in the data transfer agreement a number of data-related conditions were agreed upon between the various parties. The most relevant of these are the following:

- Students with extreme values of per capita income (both very high and very low) would not be included in the shared dataset, and only the data on students with an income level between –5,000 and 5,000 euros around the threshold would be provided. This is precisely the sample needed for the analysis, as the methodology used involves a comparison of students close to the threshold. Students with a per capita income further away from the thresholds are therefore not necessary.[10]

- Students with a disability and students with a foreign background would not be included in the sample.

The DGES provided information about all applicants, and the variables made available were the following: socio-demographic characteristics (gender, year of birth, region of residence), information on the university chosen by the student (type of university (public or private), type of degree (bachelor, master, or unique cycle), field of study, region, current academic year, current curricular year), per capita income rounded at the unit

[9] DPR-EC-04126.1

[10] The original per capita income distribution ranged from –7,924.87 to +2,080,000,000 around the threshold. We run the command developed by Calonico et al. (2017) on the original sample to choose the bandwidth. As the optimal bandwidth for all available specifications was below the income range [–2,500; + 2,500] and to have enough observations in each income bin, the parties involved agreed that extreme values of per capita income would be excluded and only the data on students with an income between –5,000 and 5,000 around the threshold would be provided.

digit (which is used to determine whether the student is eligible for the grant), the result of the application, and in cases of rejection the reason why the grant was not awarded. The DGEEC provided information about the academic progression of each applicant in the academic years from 2012/2013 to 2017/2018. For each academic year, students could be classified into eight possible categories (see Annex 7 for details about the categories).

In all analyses, a further restriction to the sample was made: old students (above 50 yers of age) and students enrolled in part-time education were excluded from the analysis. Students who were enrolled in two specific types of course were also excluded, and we focus solely on students enrolled in a bachelor, master, or integrated master.[11] Finally, we drop students who are enrolled in fewer than 30 credits, as they are not eligible for the grant.

4 Analysis on first-year students

In this section, we analyze the effect of the grant on students in their first year of study and who are thus applying for the grant for the first time, having not been enrolled in the previous academic year (i.e., enrolled in zero credits in the previous academic year). Therefore, we do not include in our sample students who are requesting the grant to enroll in course-years other than the first one.

4.1 Sample selection

Students can apply for the grant at any point in their university career and can apply multiple times (as they progress to the following course-year). Here, we focus on first-time applicants (as recorded by the system) applying at the beginning of their first year (independently of whether it is the first year of a bachelor, master, or integrated master). For this particular subset of students, the assignment variable is solely per capita income, as no merit requirement is in place for first-year students.

The per capita income used to assess eligibility is normalized around the cutoff value for the grant, for each academic year (as reported in Annex 1).[12]

The total sample comprises 94,964 students. Based on this sample, we checked whether the running variable—the per capita income used to assess eligibility for the grant—is a real predictor of receiving the grant. We built two variables: (1) the "treatment" variable, which is built using information on the current status of the application as registered in the system, after all the eligibility checks have been conducted (i.e., the variable reflects the actual number of students who receive the grant); (2) the "intention to treat" variable, which measures students' eligibility for the grant according to their per capita income, is a binary indicator indicating whether the per capita income is below the predefined threshold.[13]

The cross-tabulation of these two variables reveals that there are 21 students who received the grant even though their income was above the threshold and 1,545 students who did not receive the grant even though their income was below the threshold. For the rest of the sample, "intention to treat"= "treatment"; 14,818 students did not receive the grant and have an income above the threshold, and 78,580 received the grant and had an income below the threshold. This is summarized in Figure 4, which plots the probability of being treated according to the running variable.

4.2 Empirical strategy

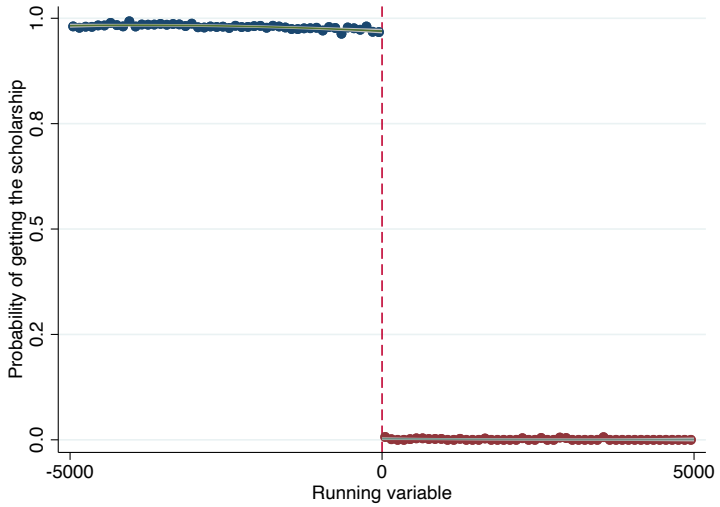
As reported in Figure 4, students are not perfectly sorted around the income threshold. Therefore, we follow a fuzzy regression discontinuity design (RDD) to estimate the effect of receiving the grant on future academic outcomes. We exploit the fact that only students with a per capita income below the predefined threshold are eligible for the grant, which results in discontinuity in the probability of receiving the grant for students with different per capita incomes, along with a smoothly increasing running variable (as shown in Figure 4). The idea behind this methodology is that students who are very close to the cut-off point are very similar in all respects

[11] The other two excluded courses are: 1) Curso de especializacão tecnológica (CET) is a post-secondary technological course that provides students with ISCED level 5. Since 2016, CET moved from higher education institutions to professional schools, and it now provides level 4. It is a one-year course. In the data, there are only 5,700 students in this type of course, with applications only in 2012, 2013, and 2014. 2) Curso técnico superior professional (TESP) is a post-secondary higher education course that also provides students with level 5. It confers a pre-university diploma that allows students to continue to higher education. In the data, there are around 9,000 applicants in these courses, applying in the academic years 2015, 2016, and 2017. It was decided that these two types of degrees would not be considered together with bachelors, masters, and integrated masters as they are substantially different in nature. However, given the interest in the effect of the grant for TESP students, a separate analysis focusing only on this type of degree is reported in Annex 9.

[12] For each students' per capita income, we subtract the threshold value and use this new variable as a running variable: All students with a value below 0 are eligible for the grant, and all students with a value above 0 are not eligible.

[13] From this sample, we exclude students who were rejected due to not having completed the application process: these students were asked to provide more documentation related to their income and never did; the income variable is therefore not valid for this sample of students and they are not included in the analysis.

Figure 4: Probability of receiving the grants based on the running variable



but some have access to the grant and some do not, so those students with an income just above or just below the threshold are comparable (Lee and Lemieux, 2010).

Formally, Y_i is academic success, the outcome variable of interest for each student i, and X_i represents the income of the student's household, which determines treatment assignment for each student. T_i indicates whether a student is eligible for the grant, and D_i whether the student receives the grant. In our case, $D_i \neq T_i$, i.e., there is imperfect compliance between being eligible for the grant and receiving the grant. $T_i = 1(X_i < c)$, meaning a student is eligible for the grant when their per capita income X_i is smaller than the eligibility threshold c.

Within the potential outcomes framework, D_i can be witten as $D_i = D_i(0) * (1 - T_i) + D_i(1) * T_i$, where $D_i(1)$ is the treatment taken when the student i is assigned to the treatment condition (i.e, when T_i = 1) and $D_i(0)$ is the treatment when student i is assigned to the control condition (i.e, when T_i = 0). Y_i is defined as $Y_i = Y_i(0) * (1 - D_i) + Y_i(1) * D_i$, where $Y_i(1)$ and $Y_i(0)$ are the potential outcomes of interest with and without the grant.

In a fuzzy RDD, the average treatment effect, i.e., the average effect of the grant can be written as (Hahn et al. (2001)):

$$E[Y_i(1) - Y_i(0)|X_i = c] = \frac{\lim_{x \downarrow c} E[Y_i|X_i = x] - \lim_{x \uparrow c} E[Y_i|X_i = x]}{\lim_{x \downarrow c} E[D_i|X_i = x] - \lim_{x \uparrow c} E[D_i|X_i = x]}. \quad (1)$$

We estimate regression discontinuities non-parametrically.[14].Non-parametric local polynomial estimators involve approximating the regression functions above and below the cutoff by means of weighted polynomial regressions, with weights computed with a kernel function on the distance between each observation's score and the cutoff. These kernel-based estimators require a choice of bandwidth for implementation, and several bandwidth selectors are now available in the literature. We apply the one proposed by Calonico et al. (2014) and Calonico et al. (2020), which selects the optimal bandwidth based on one common MSE-optimal bandwidth selector and a triangular kernel (see Cattaneo et al. (2019)). In this baseline model, we include a control for the academic year. In addition, we check for the presence of mass points in the running variable and account for them accordingly as in Calonico et al. (2014). We estimate the confidence intervals relying on the bias-corrected RD estimates with a robust variance estimator, which provides valid inference when the MSE-optimal bandwidth is used. We estimate the model first without covariates and then including covariates, as developed in Calonico et al. (2019). The covariates included should not affect the point estimates but are included to help improve efficiency.

Following Lee and Lemieux (2010), we perform several tests to validate the underlying RDD assumptions. (1) We first check the absence of manipulation of the running variable around the income eligibility threshold graphically and by running density tests proposed by Cattaneo et al. (2020b). (2) We verify that there is no

[14]The alternative to non-parametric estimation is parametric estimation. Parametric estimations basically use all observations to find an effect, while non-parametric methods provide estimates based on data closer to the cut-off, reducing bias that may otherwise result from using data further away from the cutoff to estimate local treatment effects. Non-parametric methods are by now the standard framework for empirical regression discontinuity (RD) analyses because they offer a good compromise between flexibility and simplicity

discontinuity at the income threshold in the distribution of relevant covariates. We do this both graphically and by estimating Eq. (1) using the covariates as outcomes. (3) We verify the absence of any other discontinuity in the likelihood of receiving the grant on each side of the income eligibility threshold. (4) Finally, we replicate the analysis selecting the bandwidth manually: the first one includes the complete sample, including all observations with running variables in the range (–5000, +5000) of the threshold, then we restrict the range to (–3000, +3000), (–1000, +1000), and (–500, + 500). Finally, we also present the results using different polynomials.

4.3 Data and descriptive statistics

4.3.1 Outcomes

From the information provided by the DGEEC, we retrieve each student's situation in the academic years from 2012/2013 to 2017/2018. For each academic year, each student could be classified into eight possible categories (see Annex 7 for details on these categories). When a student graduates from the same course for which they originally apply we have information on the final grade of the student at graduation. If the student did not graduate but is still enrolled in the university course associated with the grant application, we know whether they are in their the first year of enrollment and the number of credits completed at the end of the first year. The main outcomes of interest can be grouped into five categories 1) whether the students actually enroll and start the higher education degree (outcome measured in December of the first year); 2) whether the students are enrolled in any course at the beginning of the second year (outcome measured in December of the second year); 3) whether the students obtained at least 36 credits at the end of the first year;[15] 4) whether the students obtained all the credits they were enrolled in; 5) whether they graduated; 6) whether graduation was on time; 7) their final grades. We also estimate the effect on the probability of applying for the grant again in the following academic year. From the information reported in the dataset containing the students' outcomes (see Annex 7 for details), we define the following outcomes of interest:

1. **Immediate dropout**: a dummy variable equal to 1 when a student is not found in the database in December of the first year;
2. **Immediate dropout, version B**: a dummy variable equal to 1 when an applying student is not found in the database recording students' academic outcomes (DGEEC) in December of the first year, not including students who are never found in the DGEEC database;
3. **Never found**: a dummy variable equal to 1 when an applicant is never found in the DGEEC database. These students could either be students who applied for the grant but never actually started higher education and never enrolled again, or students who are not found due to matching issues between the two datasets;
4. **Enrolled in the same course of application**: a dummy variable equal to 1 when the student is enrolled in the same course as that indicated on their grant application in December of the first year;
5. **Enrolled in another course**: a dummy variable equal to 1 when the student is enrolled in a different course of the same level as the course of application in December of the first year;
6. **Dropout end of first year**: a dummy variable equal to 1 when a student is not found in the database in December of the second year, not including students who are never found in the database. This also includes students who did not enroll in the year of the application but enrolled the following year (we cannot distinguish whether students are in their first or second year);
7. **Obtained at least 36 credits**: a dummy variable equal to 1 when the student obtained at least 36 credits (or all credits if enrolled in fewer than 36) the end of the first year;
8. **Obtained all credits in which they were enrolled**: a dummy variable equal to 1 if the student obtained all credits in which they were enrolled at the end of the first year;
9. **Ever graduated**: a dummy variable equal to 1 when the student graduated from a course of the same ISCED level as the one they originally indicated on the grant application;
10. **Graduated on time**: a dummy variable that equals 1 if the student graduated from a master within 2 years, a bachelor within 3 years, and an integrated master within 5 years;[16]

[15] Thirty-six credits is the minimum number of credits students need to obtain in order to be awarded the grant in the following academic year. See Annex 8 for more details on the strategy used to build this variable, as information on the number of credits is not available for the whole sample.

[16] within 4 years for a bachelor in a health-related field

11. **Final mark** at graduation: This information is only available when students graduated from the same course they originally apply for;

12. **Apply again**: a dummy variable equal to 1 when the students apply for the grant again the following academic year.

The average values of these outcomes are reported in Table 1 for the two groups of students in the working sample. The outcomes of interest are different between students receiving the grant (i.e., the treatment group) and those not receiving it (i.e., the control group).

Table 1: Descriptive statistics: outcome variables

	Non-treated	Treated	Difference	se
Immediate dropout	0.0392	0.0082	0.0311***	0.002
Immediate dropout- B	0.0221	0.0048	0.0173***	0.001
Never found	0.0175	0.0034	0.0142***	0.001
Enrolled in the same course of application	0.9396	0.9672	-0.0276***	0.002
Enrolled in another course	0.0271	0.0184	0.0086***	0.001
Dropout end of first year	0.0740	0.0527	0.0213***	0.002
Obtained at least 36 credits	0.8175	0.8735	-0.0560***	0.004
Obtained the enrolled credits	0.5121	0.5132	-0.0011	0.005
Graduated	0.5100	0.5478	-0.0378***	0.006
Graduated on time	0.3770	0.4206	-0.0436***	0.006
Final mark	14.41	14.33	0.09***	0.027
Apply again	0.3268	0.8242	-0.4974***	0.004
Observations	16,363	78,601		

Note: The table reports the mean values of the outcomes of interest in the non-treated group (column 1) and the treated group (column 2), their difference, and the relative standard error. *** p<0.01, ** p<0.05, * p<0.1.

4.3.2 Students' characteristics

The descriptive statistics for these variables are reported in Table 2. This information is also used in Section 4.5.2 to test the validity of the regression discontinuity design.

Table 2 reports the characteristics of students who received the grant and of those who did not. Students receiving the grant are more often women, younger, enrolled in a bachelor, and attending a public university. The three most common fields of study are social sciences, business and law; health and social protection; and engineering, transforming industries and construction, regardless of whether students received the grant or not. The proportion of students in the treatment and control groups also varies by region of residence.

Table 2: Descriptive statistics: control variables

	Non-treated	Treated	Difference	se
Running variable	1280.61	-2800.23	4080.84***	12.329
Age	20.55	19.76	0.80***	0.035
Student is female	0.6121	0.6428	-0.0307***	0.004
Region: A.M.L.	0.1894	0.1506	0.0387***	0.003
Region: Alentejo	0.0839	0.0777	0.0062***	0.002
Region: Algarve	0.0271	0.0217	0.0054***	0.001
Region: Azores	0.0265	0.0212	0.0053***	0.001
Region: Centro	0.2520	0.2517	0.0003	0.004
Region: Madeira	0.0339	0.0359	-0.0020	0.002
Region: Norte	0.3872	0.4411	-0.0539	0.004
Degree: Bachelor	0.7540	0.8183	-0.0643	0.004
Degree: Master	0.1076	0.0754	0.0321	0.003
Degree: Integrated master	0.1385	0.1063	0.0322	0.003
Field: Education	0.0438	0.0466	-0.0029	0.002
Field: Social Sciences, Business and Law	0.1272	0.1380	-0.0107***	0.003
Field: Arts and Humanities	0.3125	0.3328	-0.0202***	0.004
Field: Sciences, Mathematics and Computing	0.0983	0.0869	0.0114***	0.003
Field: Engineering, Transforming Industries and construction	0.1580	0.1347	0.0234***	0.003
Field: Agriculture	0.0159	0.0129	0.0030***	0.001
Field: Health and social protection	0.1704	0.1715	-0.0011	0.003
Field: Services	0.0721	0.0757	-0.0036	0.002
Field: Unknown	0.0017	0.0009	0.0008***	0.000
Public university	0.9000	0.9140	-0.0140***	0.003

Note:The table reports the mean values of the outcomes of interest in the non-treated group (column 1) and treated group (column 2), their difference, and the relative standard error. *** p<0.01, ** p<0.05, * p<0.1.

4.4 Results: full sample

4.4.1 The impact of the grant: RDD estimates

We first start with a graphical inspection of discontinuities in outcomes at the threshold. Figures 5 and 6 show that students eligible for the grant, i.e., with an income below the cutoff, seem to have a lower rate of immediate dropout (both definitions) and of never being found in the databases. They also have higher enrollment rates when considering the degree for which they applied, but lower rates when considering other degrees at the same ISCED level. Treated students show higher rates of completing 36 credits and of graduating and graduating on time, and much higher rates of applying for the scholarship again in the following academic year.

This graphical analysis gives only a hint of the true effect of receiving the grant, however, as we only descriptively compare the outcomes between eligible and non-eligible students. The estimates of the causal impact of the treatment are presented in the next two subsections. Eq. (1) presented in Section 4.2 is estimated first without student characteristics other than income and academic year fixed effects (without covariates), and then including students' characteristics (with covariates).

Figure 5: Discontinuity in outcomes

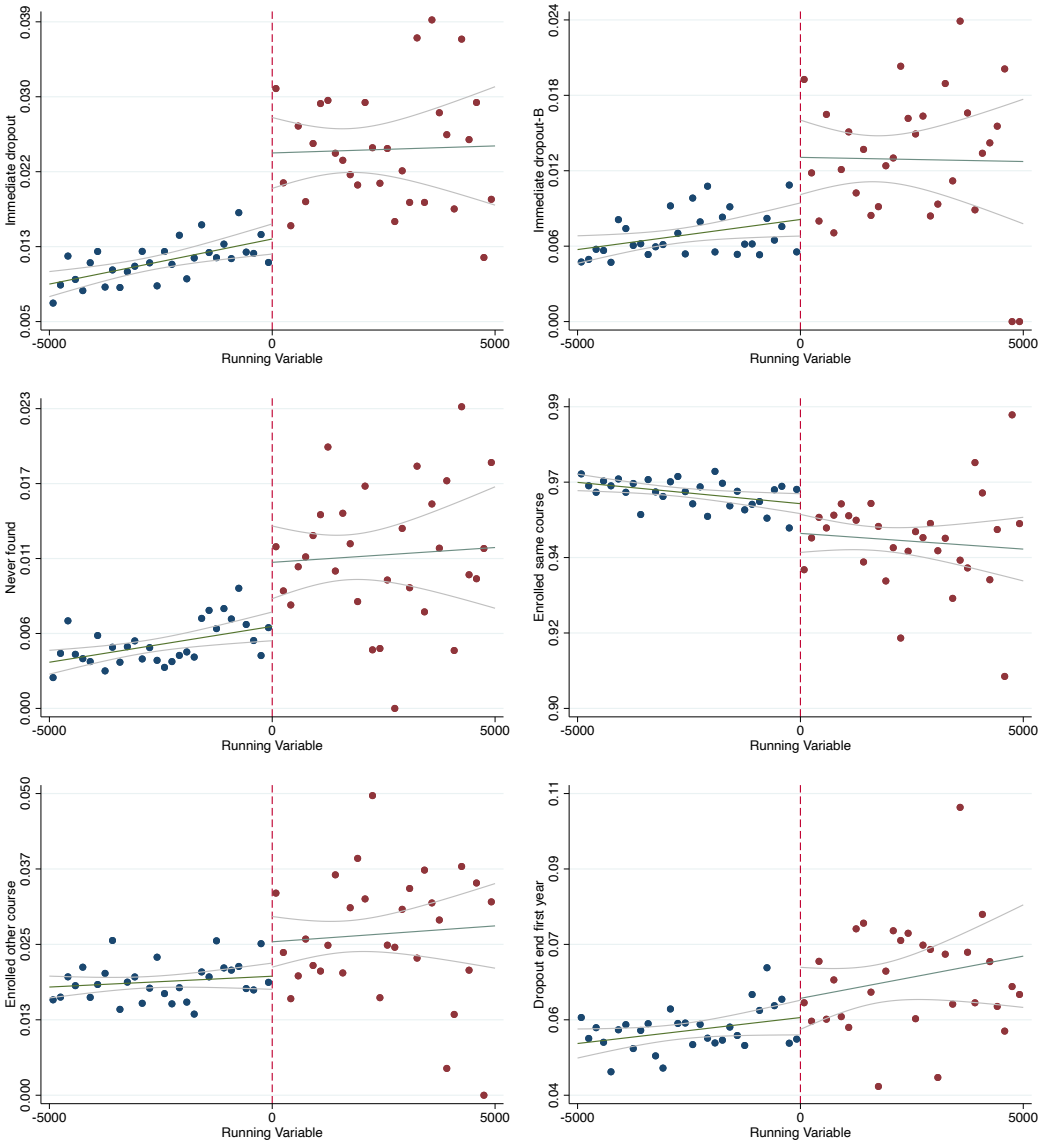
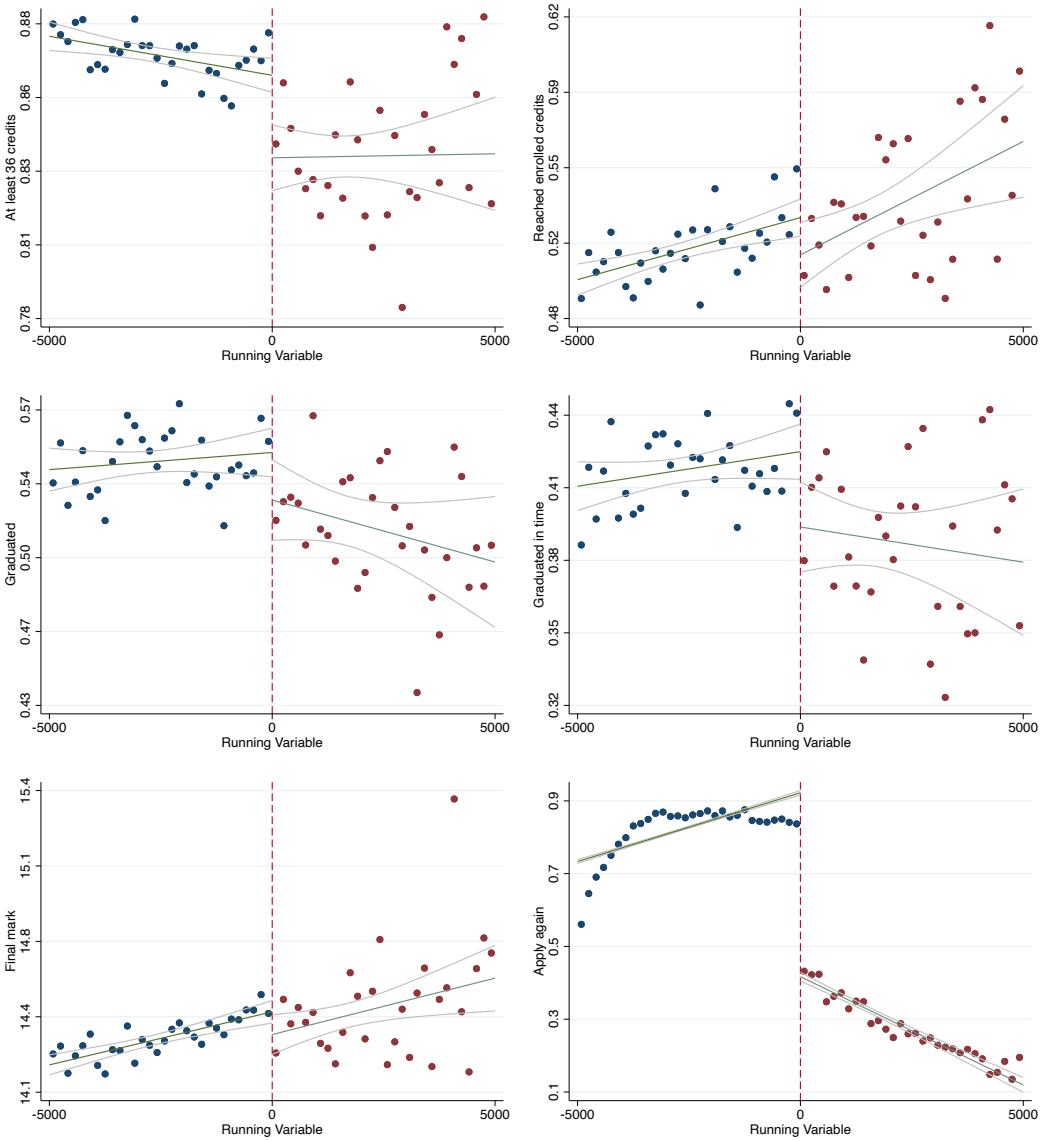


Figure 6: Discontinuity in outcomes



4.4.2 RDD estimates: without covariates

The main results are reported in Table 3. We estimate the effect of receiving the grant on the outcomes of interest (summarized in Section 4.3.1). As explained in Section 4.2, we use the bandwidth selected by one common MSE optimal bandwidth bias-corrected RD estimate with a robust variance estimator. The order of the local polynomial used to construct the point estimator is 1 (local linear regression), and the order of the local polynomial used to construct the bias correction is 2. For each of the outcomes, we also report the first stage: the effect of the running variable on the probability of being treated.

The main results point to a negative effect of the grant on the probability of immediate dropout. This is true both if we include the students who are never found in the data (column 1) and if we do not include them (column 2). Receiving the grant indeed has a direct effect on the probability of not being found in the DGEEC dataset, which could mean that getting the grant has an effect on the quality of the matching or that students who do not get the grant do not enroll that year and never do (column 3). We find that receiving the grant reduces immediate dropout by between 1 and 1.7 percentage points (p.p.). We then look at whether the grant has any impact on the course the students actually enroll in, and we see interesting differences between the two groups. In particular, receiving the grant increases the probability of still being enrolled in the course for which the student applied for the grant by around 2.6 percentage points (p.p.) but reduces by 1.5 p.p. the probability of being enrolled in another course of the same ISCED level (columns 4 and 5). So pupils who get the grant are more likely to actually start the course for which they apply for the grant, while those who do not receive the grant are more likely to enroll in a different course. We then look at results at the end of the first year, and we see that getting the grant increases the probability of obtaining at least 36 credits by 2.6 p.p., and of obtaining all credits the student enrolled in by 3.8 p.p. (columns 7 and 8). There is also a negative but not statistically significant effect on the probability of dropout at the end of the first year (column 6). [17]

We also look at longer-run effects, estimating the impact of receiving the grant on the probability of graduating, of graduating on time, and on the final mark obtained. We find a positive and significant effect on the probability of graduating on time, with an increase of 5.6 p.p. (column 9).[18] The sign of the coefficients for the probability of graduating and for the final mark are positive but not statistically significant (columns 10 and 11). Finally, we also estimate the effect of receiving the grant on the probability of applying again in the following academic year, and we find a large positive effect of 40 p.p.

In total, based on this analysis we find that receiving the grant has an immediate effect on the probability of starting higher education, of obtaining the credits needed to receive the grant the following year, and of obtaining all credits in which the students enrolled. Receiving the grant also leads to a higher probability of graduating on time.

[17] This particular estimate excludes students who are not found, so this variable reflects the probability of dropout conditional on being observed in the data at least once, and thus most likely conditional on having at least enrolled in the first year. However, this variable also takes a value of 0 for students who are currently enrolled, even if they were not in December of the first year. These are the students who immediately drop out, as of December of the year of the application, t, but who in December of year $t+1$ are enrolled in any course. As a robustness check, we exclude from the sample those students who were not found in the databases in December of year t, but we still do not find a significant effect of receiving the grant. It is worth mentioning that in the sample of males, using this definition we find a negative effect on dropout of 3.8 percentage points, similar to what we find using the main definition.

[18] We also use an alternative definition, defining as graduating on time all students who graduate up to 1 year after the normal time: 3 years for a master and 4 years for a bachelor. We find a slightly smaller coefficient but pointing to the same result (beta= 0.050, se=0.027).

Table 3: Main results

	(1)	(2)	(3)	(4)
	Immediate dropout	**Immediate dropout-B**	**Never found**	**Enrolled same course**
First stage	0.966***	0.967***	0.966***	0.966***
	(0.005)	(0.004)	(0.005)	(0.005)
Robust	-0.017***	-0.010**	-0.007**	0.026***
	(0.006)	(0.004)	(0.004)	(0.009)
Observations	[80125:14839]	[79744:14667]	[80125:14839]	[79744:14667]
Bandwidth	[1222:1222]	[1402:1402]	[1177:1177]	[1120:1120]
Effect. observations	[12775:6832]	[14956:7486]	[12194:6639]	[11456:6336]

	(5)	(6)	(7)	(8)
	Enrolled other course	**Dropout end year 1**	**At least 36 credits**	**Obtained enrolled credits**
First stage	0.966***	0.964***	0.971***	0.971***
	(0.005)	(0.005)	(0.003)	(0.003)
Robust	-0.015**	-0.011	0.026**	0.038**
	(0.007)	(0.009)	(0.012)	(0.017)
Observations	[79744:14667]	[65371:12020]	[69950:10915]	[68914:10819]
Bandwidth	[1147:1147]	[1434:1434]	[1976:1976]	[2077:2077]
Effect. observations	[11768:6458]	[12717:6176]	[20597:7136]	[21620:7263]

	(9)	(10)	(11)	(12)
	Graduated on time	**Graduated**	**Final mark**	**Apply again**
First stage	0.960***	0.960***	0.956***	0.967***
	(0.007)	(0.007)	(0.009)	(0.004)
Robust	0.056**	0.042	0.177	0.404***
	(0.027)	(0.027)	(0.110)	(0.017)
Observations	[36857:7007]	[36857:7007]	[19746:3328]	[80125:14839]
Bandwidth	[1364:1364]	[1329:1329]	[1377:1377]	[1253:1253]
Effect. observations	[7242:3421]	[7028:3348]	[3926:1660]	[13152:6970]

Note: The table reports RDD estimates of Eq. (1). Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

4.4.3 RDD estimates: with covariates

In Table 4, we report the same results with the inclusion of control variables, i.e., student characteristics, following the methodology proposed by Calonico et al. (2019). The covariates included are age, gender, region, type of degree (bachelor, master, or integrated master), and whether the university is public or private. Calonico et al. (2019) suggest that the inclusion of covariates that are truly predetermined should not change the point estimates obtained in the specification without covariates but should increase efficiency by reducing the standard errors.

The inclusion of controls confirms all of the previous results: We see that the magnitude of the coefficient is similar to the estimates obtained in the model without covariates, but the standard errors are smaller, as expected.

Table 4: Results including control variables

	(1)	(2)	(3)	(4)
	Immediate dropout	**Immediate dropout-B**	**Never found**	**Enrolled same course**
Robust	-0.016***	-0.009**	-0.008**	0.024***
	(0.005)	(0.004)	(0.004)	(0.008)
Observations	[79320:14680]	[78945:14510]	[79320:14680]	[78945:14510]
Bandwidth	[1340:1340]	[1396:1396]	[1167:1167]	[1197:1197]
Effect. observations	[14132:7235]	[14735:7377]	[11944:6532]	[12284:6579]
	(5)	(6)	(7)	(8)
	Enrolled other course	**Dropout end year 1**	**At least 36 credits**	**Obtained enrolled credits**
Robust	-0.013**	-0.011	0.027**	0.044**
	(0.006)	(0.008)	(0.012)	(0.018)
Observations	[78945:14510]	[64679:11885]	[69279:10803]	[68252:10705]
Bandwidth	[1224:1224]	[1732:1732]	[1817:1817]	[1709:1709]
Effect. observations	[12596:6685]	[16083:6955]	[18294:6717]	[16596:6374]
	(9)	(10)	(11)	(12)
	Graduated on time	**Graduated**	**Final mark**	**Apply again**
Robust	0.057**	0.043	0.152	0.405***
	(0.026)	(0.026)	(0.100)	(0.017)
Observations	[36388:6910]	[36388:6910]	[19534:3286]	[79320:14680]
Bandwidth	[1384:1384]	[1356:1356]	[1397:1397]	[1235:1235]
Effect. observations	[7275:3409]	[7116:3356]	[3932:1655]	[12813:6813]

Note: The table reports RDD estimates of Eq. (1). Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. The following controls are included: age, gender, region, type of degree, and public university. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

4.5 Validation and falsification

To test the validity of the assumptions underlying the RDD, we perform the following checks. We verify the absence of manipulation of the running variable, and that the grant has no impact on predetermine covaraties. We run some placebo faking the threshold, we run the estimates for manually selected bandwidths, and finally using different polynomials.

4.5.1 Manipulation of the running variable

We first investigate whether there is any indication of potential manipulation of the running variable around the threshold. Some students with an income just above the threshold might declare less income, someone in their family could choose to work fewer hours in order for them to become eligible for the grant. We do not think that the former is plausible, as the data used to assess eligibility is the income declared for tax purposes, and this is verified by the Portuguese administration. The latter might be possible only if the student knows one year beforehand what the eligibility threshold will be for the next academic year and if those in the family who are working have the flexibility to adjust the number of hours worked, which is quite unlikely. For these reasons, we do not think that a substantial number of students are able to manipulate their income to become or remain eligible for the grant. However, the main issue in this setting is that we have access only to applicant students and not to the whole population of Portuguese students. This might be an issue if "wealthier" students do not even apply for the grant, knowing that they will probably not get the grant. If all non-eligible students do not apply, we would not have the necessary information to carry out the regression discontinuity analysis.

We test this assumption empirically, first by plotting the distribution of income around the eligibility threshold and checking whether there is an accumulation of observations just below the threshold. Graph (a) of Figure A.1 displays no jump at the threshold. In addition, the presence of discontinuity in the density function at the cutoff point is tested and rejected using tests proposed by Cattaneo et al. (2020b). Graph (b) of Figure A.1 reports the result of the local polynomial density estimation test (unrestricted model) by Cattaneo et al. (2020b). We can see that there is no significant discontinuity in the distribution of income, i.e., the running variable around the eligibility threshold. The corresponding coefficient is the following: the robust estimate equals 1.5241 with a p-value of 0.1275 (optimal bandwidth selection; default settings: $(p) = 2$ and $(q) = 3$). Therefore, we conclude that there is no sign of manipulation around the threshold (including both real manipulation and a differential application pattern above or below the threshold). This is probably true for two main reasons. While it is true that the thresholds are known, the calculation of "per capita" income is not so straightforward, and it could be that some students who are around the threshold will apply anyway, hoping to be eligible. This is especially true in our working sample of students: students who apply for the first time do not know (yet) if their income will be considered eligible or not, meaning that they have no prior experience since this is their first application. We can expect different patterns from the second application onward, when students who were rejected the first time may be less inclined to apply again if their income was considered to be too high in the first application.

4.5.2 Effect on predetermined covariates

The other main assumptions of the RDD is that around the cutoff there are no differences in the distribution of covariates across the two samples. The idea is simply that, if students lack the ability to precisely manipulate their per-capita income, there should be no systematic differences between students with similar values of the per-capita income. Thus, except for their treatment status, units just above and just below the cutoffnshould be similar in all variables that could not have been affected by the treatment. To check whether this is true, we first plot the distribution of the covariates around the threshold and then run the model defined in Equation 1 using the covariates as the outcome. Finding zero "effect" means that the predetermined covariates are truly predetermined. A graphical inspection of the distribution of student characteristics around the threshold show that students who received the grant are similar to those who did not receive it in terms of age, gender, field of study, and region of origin (see Figures A.2, A.3, and A.4). This graphical inspection is confirmed by the estimates of Eq. (1) using student characteristics as outcomes. The results are reported in Table 5. The two samples of treated and control students have a similar distribution of covariates around the threshold and are therefore comparable. Graphically, students who received the grant seem more likely to be enrolled in master. The RDD estimates reveal that this difference is not significant around the optimal bandwidth. This confirms that around the threshold, the two groups of students are comparable. In the heterogeneity analysis in Section 4.6 we take a closer look at these differences, estimating the impact of the grant by gender, university sector, type of degree, and region of origin.

Table 5: Discontinuity in covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	**A.M.L**	**Alentejo**	**Algarve**	**Azores**	**Centro**	**Madeira**	**Norte**
Robust	0.010	-0.000	0.001	-0.004	-0.009	-0.003	0.007
	(0.014)	(0.010)	(0.006)	(0.005)	(0.015)	(0.007)	(0.017)
Observations	[79320:14680]	[79320:14680]	[79320:14680]	[79320:14680]	[79320:14680]	[79320:14680]	[79320:14680]
Bandwidth	[1491:1491]	[1632:1632]	[1273:1273]	[1441:1441]	[1635:1635]	[1538:1538]	[1612:1612]
Effect. obs	[16163:7835]	[18104:8375]	[13287:6966]	[15459:7659]	[18152:8380]	[16793:8039]	[17815:8300]

	(8)	(9)	(10)	(11)	(21)	(22)	(12)
VARIABLES	**Female**	**Bachelor**	**Master**	**Integrated m.**	**Age**	**Public**	**Education**
Robust	-0.017	-0.001	-0.001	-0.001	0.031	0.009	0.000
	(0.017)	(0.015)	(0.011)	(0.011)	(0.187)	(0.010)	(0.007)
Observations	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]
Bandwidth	[1538:1538]	[1429:1429]	[1323:1323]	[1643:1643]	[1064:1064]	[1574:1574]	[1715:1715]
Effect. obs	[16953:8130]	[15440:7695]	[14049:7258]	[18429:8490]	[10858:6181]	[17465:8273]	[19503:8718]

	(13)	(14)	(15)	(16)	(17)	(18)	(19)
VARIABLES	**Social sciences**	**Arts**	**Sciences**	**Engineering**	**Agriculture**	**Health**	**Services**
Robust	0.015	0.000	-0.008	-0.001	0.000	-0.015	0.014
	(0.011)	(0.019)	(0.010)	(0.011)	(0.004)	(0.015)	(0.009)
Observations	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]	[80125:14839]
Bandwidth	[1688:1688]	[1154:1154]	[1637:1637]	[1848:1848]	[1666:1666]	[1219:1219]	[1619:1619]
Effect. obs	[19067:8637]	[11916:6561]	[18356:8474]	[21499:9162]	[18759:8563]	[12740:6814]	[18095:8424]

Note: The table reports RDD estimates of Eq. (1),using as outcome variables the covariates reported in Table2. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

4.5.3 Placebo cutoffs

To test the robustness of our results, we run the RDD estimates using placebo thresholds to the right and to the left of the income eligibility threshold. Finding no effect at these placebo thresholds would ensure that our results are truly driven by the treatment, i.e., receiving the grant. Table A.1 reports the RDD estimates of Eq. (1) using the following placebo thresholds to the right and to the left of the income eligibility cutoff value: –1,000, –2,000, –3000, and –4000 on the left and +1,000, +2,000,+3,000, and +4,000 on the right. For each of these placebo thresholds, Eq. (1) is estimated to the right and to the left, with one common MSE-optimal bandwidth (Calonico et al., 2017). We do no not find robust evidence of discontinuity to the left and to the right of the variables of interest, in particular, for these on which we find a significant impact of the grant (see Section 4.4.1).

4.5.4 Manually selected bandwidth

The approach we follow to select the bandwidth is optimal in terms of picking the right sample so that all assumptions needed to run the RDD are respected. However, we also try to manually select the bandwidth around the threshold and test how the results change according to the selection of different intervals. The results are reported in Figures A.5 and A.6. For each of the outcomes, we report estimates based on ten different samples, manually selecting the bandwidth and including all observations with the running variable in the range of (–500,500), (–1000, 1000), (–1500,1500), and so on until (–5000, +5000). The main conclusion is that our main findings are robust to the manually selected bandwidth in most cases. Only for the "never found" variable do we not find significant results. The "obtaining at least 36 credits" variable is significant in the larger bandwidths (between 2500 and 4000), and the variable "graduated on time" is significant in the shorter bandwidths (between 1000 and 2000).

4.5.5 Different polynomial choice

Table A.2 reports the RDD estimates for different polynomial specifications and confirms that our results are robust to different specifications of the running variable (quadratic and cubic).

4.6 Results: heterogeneity analysis

In this section, we investigate whether the results that we found for the full sample are driven by particular types of students or university programs. We divide the sample into groups according to the following characteristics: gender, higher education institution type (public or private institution), type of degree (bachelor, master, or integrated master, which is a five-year degree including both a bachelor and master), and type of region of origin (less developed regions: Norte, Centro, Alentejo, and the Azores; in-transition regions: Algarve and Madeira; and developed regions: Lisbon Metropolitan Area(A.M.L.). Results are reported in Tables A.3 to A.4.

4.6.1 Results: RDD estimates on dropout

As seen in the main analysis section, the grant significantly decreases the probability of immediate dropout. This result is mainly driven by public universities and bachelor's degrees (see the RDD estimates for both immediate dropout variables in Table A.3, columns 5, 16, and 27, and 6 and 16, respectively). Male students also have a reduced probability of dropping out by December of the first year (columns 3 and 14). Indeed, the decreasing effect on immediate dropout among females who received the grant is explained by their lower probability of never being found (column 24). As for regional differences, the effects are found for students from the less developed regions only (columns 9, 20, and 31). An effect is found in the developed regions (A.M.L) only for the immediate dropout variable, excluding the students who are never found (column 22). The effect of the grant on dropout at the end of the first year (measured in December of the second year) is not significant in the full sample but is negative and significant for male students (see Table A.3, column 58).

4.6.2 Results: RDD estimates on enrollment

The main results show that the impact of the grant on the probability of being enrolled in the degree indicated on the application is positive, but it is negative for the probability of enrolling in a different degree at the beginning of the first year (measured in December of the first year). The heterogeneity analysis reveals interesting patterns.

Regarding the probability of being enrolled in the same degree, we see that the effect for males is 2 percentage points higher than for females (see Table A.3, column 36). Regarding the type of degree, the positive effect of the grant on enrollment in the same degree is stronger for master's degree students than for bachelor's degree students (columns 39 and 40). In addition, this positive effect is mainly driven by students in public universities (column 38) and students from less developed regions (column 42). As for the probability of being enrolled in another degree, the picture differs. The negative effect of the grant for the full sample is only driven by male students, public universities, master students, and students from less developed regions (see Table A.3, columns 47, 49, 51, and 53).

4.6.3 Results: RDD estimates on credits

The positive effect of the grant on having achieved the minimum number of credits required at the end of the first year is explained by females (see Table A.4, column 68) and master's degree students (column 73). Similarly, the increased probability of completing the total number of credits enrolled in with the grant is driven by females, (column 79) and students enrolled in master programs (column 84). However, we see that the positive effect of the grant on obtaining the number of credits enrolled in is higher for students in private universities than for those in public universities (columns 81 and 82). The effect on obtaining the number of credits enrolled in also seems to be driven by students coming from the regions in transition (column 87).

4.6.4 Results: RDD estimates on graduation, final mark, and applying the following year

Similar to the analysis on the full sample, we do not find significant effects of the grant on the probability of graduating according to sub-characteristics. However, the positive effect of the grant on graduating on time is mainly driven by females, bachelor's degree students, and private universities (see Table A.4, columns 90, 94, and 92). Interestingly, in the heterogeneity analysis we find that for students at public universities, students from less developed regions and students in integrated master programs, the grant has a positive and significant effect on the final mark (columns 115, 118, and 119).[19] Finally, the strong effect we find in the full-sample analysis for applying again when already being a recipient of the grant the year before is found again, and with comparable magnitude, for all sub-characteristics (see Table A.4, columns 122 to 132).

4.6.5 Results: differences between displaced and non-displaced students

In the previous paragraphs, we investigate heterogeneity based on somewhat predetermined covariates. The covariates that we saw were continuous at the thresholds (see Section 4.5.2). Here, we explore a final source of heterogeneity based on an indicator of whether the students is displaced or not, that is, whether the student is studying in a region other than his/her region of origin. This variable is itself affected by the fact that a student receives the grant. Indeed, when we run the same tests as in Section 4.5.2, we find evidence that this variable is not continuous at the threshold: students who are eligible for the grant are more likely to be displaced (by 3 percentage points). Therefore, this variable is affected by being awarded the grant. We still run the heterogeneity analysis in the two sub-samples of students, just to have some information regarding other potential channels for the grant's effect. These results are reported in Table A.5. Results regarding credits

[19] The estimate for the effect of the grant on the final mark for the sub-sample of students from "in-transition" regions could not be run, due to insufficient variability.

and on-time graduation time are totally driven by students who are not displaced: no effects are found among the sub-sample of displaced students, and displaced students who get the grant have a lower probability of obtaining 36 credits than those who do not get the grant. This controversial effect could be due to the fact that displaced students are, on average, more motivated than non-displaced students. Thus, overall, students who are displaced may be a group of highly motivated students, and that is why being granted the grant is not very effective.

The effects on dropout are mixed: displaced students show a significant effect for the second definition of dropout and non-displaced students show stronger effects on the probability of dropout (according to the first definition including "never found" students) and on the never found students.

4.7 Extended analysis on first-year students

So far, we have documented the effect of being awarded the grant in the first year on subsequent outcomes. We also want to assess the effect of receiving the grant while being enrolled in later years and the effect of being a grant recipient for more than one year. The identification strategy to analyze these aspects is more complicated because from the second year onward the second eligibility condition for receiving the grant applies: completing at least 36 credits in the previous academic year. Moreover, we saw in the main results that the number of credits completed is also affected by receiving the grant, as shown in Table 3.

4.7.1 The effect of receiving the grant for more than one year

To solve this issue, here we adopt a different strategy. We focus on the sample used in the analysis so far (first year, first applicant), and we estimate the impact of receiving the grant for two years, and for three years (only for students enrolled in a bachelor or integrated master) on short- and long-term outcomes, namely immediate dropout at the beginning of the second year (third year), enrollment in the same or another course, dropout at the end of the second year (third year), graduation, graduation time, and final mark. We use per capita income measured in the first year as the running variable in a fuzzy design. Per capita income in the first year clearly affects the probability of getting the grant in the first year, and very likely affects the probability of getting the grant again in following years, as we expect that per capita income does not change that much from one year to the other. This is shown in Figure 7, which plots the probability of receiving the grant two years in a row on the left side and the probability of receiving the grant three years in a row on the right side. While there no longer is perfect compliance, as for the first year (see Figure 4), per capita income measured in the first year of application is a good predictor of receiving the grant in subsequent years as well. This is also confirmed by the first-stage estimation presented in Table 6. The treatment being "receiving the grant for two (three) years", implies that students used as controls are both those who are never treated and those who received the grant only in the first (two) year(s). The results in the first panel (receiving the grant, i.e., being treated for 2 years) refer to all three types of degrees (bachelor, integrated master, and master), while the results in the second panel (treated for 3 years) refer only to students enrolled in a bachelor or integrated master. The results suggest that receiving the grant for two years in a row increases the probability of graduating on time by 7.4 p.p., and it also has a small positive effect on the final mark obtained. Receiving the grant for three years in a row increases the probability of graduating on time by 11.5 p.p. and increases the final mark at graduation.[20]

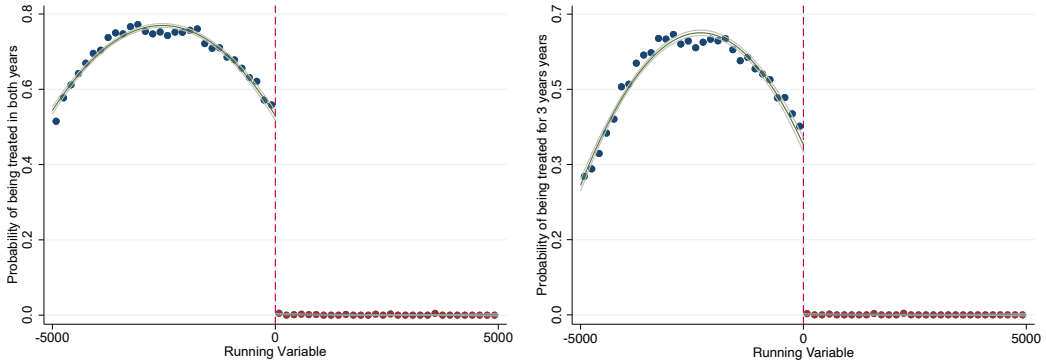
We further investigate whether there are differences by type of degree or other student characteristics. Results are reported in Tables A.6 and A.7 for the effect of receiving the grant in both the first and second year, and in Table A.8 for the effect of receiving the grant for three years. While in the full sample, receiving the grant for two years has no impact on the probability of immediate dropout, there is a negative effect of the grant on immediate dropout for male students, those in public universities, and for students from less developed regions: receiving the grant twice prevents these sub-samples of students from not even starting their second year (columns 3, 4, and 6). The grant also prevents dropout at the end of the second year for students from less developed regions (column 39). The positive effect on graduating on time, instead, is driven by female students, students coming from the A.M.L. region, those enrolled in bachelor's degrees, and those studying at private universities (columns 57, 60, 63, and 64). The effect of receiving the grant for 2 years has a positive effect on final marks in public universities, for students from less developed regions, and in particular among students enrolled in an integrated master (columns 70, 72, and 77).[21]

The heterogeneity analysis on the effect of receiving the grant for three consecutive years reveals that being treated for 3 years increases the probability of graduating on time, and this effect is driven by male students, those studying for a bachelor's degree, those at private universities, and students coming from the A.M.L. region (Table A.8, columns 33, 35,38, and 39). Receiving the grant for three years also has a positive effect on one's final mark; this effect is particularly large for students in an integrated master program, female students, and those at public universities (Table A.8, columns 42, 44, and 50).

[20] Results are robust to the inclusion of covariates.

[21] Although this last estimate is based on a very small sample.

Figure 7: Probability of receiving the grant for two and three years



Note: The figure plots the probability of being treated for two years (left panel) and three years (right panel) on the y-axis and first-year per capita income on the x-axis.

Table 6: Effect of receiving the grant for two and three years.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated 2 years	**Dropout start year 2**	**Enrolled same course, start year 2**	**Enrolled other course, start year 2**	**Dropout end year 2**	**Graduated**	**Graduated on time**	**Final mark**
First stage	0.531**	0.533**	0.524**	0.548**	0.565**	0.566**	0.640**
	(0.014)	(0.013)	(0.016)	(0.012)	(0.014)	(0.015)	(0.024)
Robust	-0.027	0.101***	-0.077***	-0.028	0.059	0.074*	0.336*
	(0.018)	(0.029)	(0.029)	(0.023)	(0.039)	(0.041)	(0.186)
Observations	[65371:12020]	[65371:12020]	[65371:12020]	[50648:9428]	[36857:7007]	[36857:7007]	[19746:3328]
Bandwidth	[1158:1158]	[1242:1242]	[919:919]	[1745:1745]	[1814:1814]	[1616:1616]	[1006:1006]
Effect. observations	[9829:5272]	[10716:5563]	[7526:4417]	[12959:5550]	[10365:4178]	[8834:3881]	[2750:1332]

	(8)	(9)	(10)	(11)	(12)
Treated 3 years	**Dropout start year 3**	**Dropout end year 3**	**Graduated**	**Graduated on time**	**Final mark**
First stage	0.414**	0.337**	0.438**	0.439**	0.548**
	(0.015)	(0.013)	(0.018)	(0.018)	(0.019)
Robust	-0.038	-0.020	0.074	0.115*	0.268*
	(0.029)	(0.036)	(0.066)	(0.065)	(0.162)
Observations	[46992:8472]	[22847:4433]	[31702:5720]	[31702:5720]	[17361:2810]
Bandwidth	[1250:1250]	[5000:5000]	[1242:1242]	[1282:1282]	[1782:1782]
Effect. observations	[7744:3959]	[22846:4433]	[5406:2641]	[5625:2705]	[4724:1705]

Note: The table reports RDD estimates of Eq. (1) for the two treatment definitions. In columns (1) to (7), the treatment is receiving the grant for two years, while in columns (8) to (12) the treatment is receiving the grant for three years. The sample used to estimate the effect of receiving the grant for three years is composed of bachelor and integrated master students only, as a master only lasts two years. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

4.7.2 Progression to higher degree

For a limited sample of students (i.e. students starting their Bachelor's program in 2012 and 2013), we study the effect of the grant on enrollment in a master's program after graduating with a bachelor's degree. This information is not easy to recover because of the structure of the dataset: For non-applicants, we do not know which type of course they are enrolled in—we only know if and when they graduated and if after graduation they are enrolled in another degree of the same or higher level than the one they originally applied for. We build a new variable that takes a value of 1 if the student:

— has the status "Graduated from the course (either the one their application was for or another one of the same level)" at the end of the third (fourth) year since their first application year,

— has the status "Enrolled in any course of the same level or higher" at the end of the fourth (fifth) year since their first application.

We therefore assume that if a student graduated from a bachelor's program in year t and is enrolled in a course after graduation, it means that the student has progressed with his/her studies and is now enrolled in a master's program. This can unfortunately only be tested for students observed for at least 5 years, and so the analysis is limited to the sample of students who started a bachelor in the 2012/2013 or 2013/2014 academic years. The results are reported in Table 7 for students who received the grant in their first year, in their first and second years, and in all three years.

We find that receiving the grant increases the probability of progressing to a master's program, and the effect increases if the grant is received for more than one year (7 p.p. for the first year, 13 p.p. for the first and second, 18 p.p. for the first, second, and third years).

Table 7: Effect on progression to a master's program

	(1)	(2)	(3)
	Treated year 1	**Treated year 1 and 2**	**Treated year 1,2, and 3**
First stage	0.951***	0.524**	0.376**
	(0.0107)	(0.0223)	(0.0218)
Robust	0.071*	0.133**	0.186**
	(0.038)	(0.061)	(0.084)
Observations	[16873:2903]	[16873:2903]	[16873:2903]
Bandwidth	[1351:1351]	[1594:1594]	[1594:1594]
Effect. observations	[3199:1404]	[3948:1593]	[3948:1593]

Note: The table reports RDD estimates of Eq. (1)., for the three treatment definitions: in columns (1) the treatment is receiving the grant in the first year, in column (2) the treatment is receiving the grant for two years, while in column (3) the treatment is receiving the grant for three years. The sample used are students who started a Bachelor in academic year 2012 and 2013. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

4.8 Discussion regarding first year students

The analyses on the sample of first-year students—first-time applicants—use the eligibility condition related to having resources below a threshold to access the grant and employ a regression discontinuity design approach to identify the causal effect of the grant on various academic outcomes, namely immediate dropout, still being enrolled at the end of the academic year, the number of credits completed, and longer-term outcomes including graduation, on-time graduation, and final mark. The first set of analyses focused on measuring the effect of receiving the grant in the first year on these outcomes. The second set of analyses extended the first set of results by investigating the effect of receiving the grant more than one time (i.e., 2 times) on the sub-sample of students enrolled in a bachelor, an integrated master, or a master, and the effect of receiving the grant 3 times on the sub-sample of students enrolled in a bachelor or an integrated master only.

The main results of these analyses are the following:

— The analyses on the full sample of first-year students show that receiving the grant has an immediate effect on the probability of actually starting higher education, on the likelihood of obtaining the credits needed to receive the grant the following year, and on the probability of completing the total number of credits in which a student is enrolled. Receiving the grant also leads to a higher probability of graduating on time.

— The extended analysis on first-year students shows that receiving the grant for two years in a row increases the probability of graduating on time by 7.5 p.p., and it also has a small positive effect on the final mark

obtained. Receiving the grant for three years in a row increases the probability of graduating on time by 11 p.p. and increases the final mark at graduation. Thus, we see that the effect on graduating on time increases with the number of times a student receives the grant (the effect of receiving the grant in the first year on graduating on time is 5.6 p.p.).

— The grant also contributes to the progression to a master's program for bachelor students.

— The results of a heterogeneity analysis show that the impact of the grant is different according to students' characteristics:

  - The grant significantly reduces immediate dropout for bachelor students. The grant thus seems to have a lock-in effect particularly for those who have not yet started their studies, when the decision to start studying or not is indeterminate. In the absence of the grant, these students would make another choice and would probably look for a job instead of studying. In contrast, this effect is less relevant for master students, i.e., more experienced students with clearer objectives and a shorter timescale until graduation.
  - The lock-in effect of the grant is most relevant for male students and students coming from less developed regions. We find that the probability of dropout at the end of the year decreases with the number of years these students are granted the grant. This result could be explained by alternative opportunities or, in particular, by the effect of the environment or social networks of these students. Indeed, the proportion of individuals enrolled in higher education is lower in less developed regions compared to more developed ones. Individuals in more developed regions are more likely to study with or without the grant, being influenced by their peers or an encouraging environment, while the grant has a positive effect on enrollment and maintaining individuals in higher education in less developed regions. The incentive created by the grant seems to be particularly effective for male students. This result is also reflected in the probability of staying enrolled in the same course indicated upon applying for the grant. Male students and students coming from less developed regions are less likely to change courses when they are awarded the grant.
  - The grant has a significant and positive effect on the probability of graduating on time. This effect is found among females, bachelor students, and students in more developed regions.
  - The grant also has a positive effect on performance, in particular for students coming from less developed regions and students enrolled in an integrated master. For these students, we find that their final mark increases with the grant, and the magnitude of this effect increases with the number of years it is awarded to them.

We try to relate the magnitude of our findings to what has been previously found in the literature. For students close to the threshold—students most likely to contribute to the regression discontinuity estimates—the grant only provides a tuition fee waiver (up to the maximum tuition fee for a public university).[22] The tuition fee in these universities and observed in the data is around 1,000 euro, as the maximum is set to 1060 euro in the years considered. Thus, we can claim that for most students in public universities,whose per capita income is close to the threshold, the grant corresponds to the tuition fee amount, which is 1,000 euros. Apparently, we find smaller effects on enrollment than in the previous literature (which we measure by "immediate dropout"): While on average, a $1,000 increase in financial support increased enrollment by around 3–4 percentage points, in our case financial support of 1,000 euro leads to an increase in enrollment of between 1 and 2 percentage points (depending on the sub-sample considered). However, the reference population in our case is not the full population of secondary education graduates, as in most studies, but rather the already selected sample that decides to at least apply for the grant and so is already somewhat motivated to pursue higher education. This represents a much smaller sample than the true population used in previous studies.

Finally, as documented by Hübner (2012) for Germany, effects in Europe tends to be smaller or even null (as found by Montalvo et al. (2018), Canton and De Jong (2005) in the Netherlands, and Fredriksson (1997) in Sweden).

The results on dropout in the literature are not as conclusive as those regarding enrollment, so it is hard to compare our findings with others. If we look at performance, measured by the credits completed, the probability of graduating, and the probability of graduating on time, we also find some common patterns with previous studies. Both European and American studies looking at graduation time find effects similar to ours: Garibaldi et al. (2012) find that an increase in tuition of 1,000 euro increases the probability of not graduating on time by 5.2 percentage points, which parallels our finding that a 1,000 euro decrease in fees increases the probability of graduating on time by 5.6 percentage points. PROMISE recipients in the US had a probability of graduating on time between 7 and 9 percentage points higher than other students (Scott-Clayton, 2011).

[22] See Annex 2.

We cannot estimate the effect of the grant on the actual credits obtained at the end of the first year, but only on the probability of obtaining the credits required to get the grant the following year. The only other paper that does this is Scott-Clayton (2011), and they find effects that are about 10 times larger than those found here. However, the requirements in the two programs are quite different: For them, the requirement represents almost 100% of the credits in which students are enrolled, while in our case it represents 60%, on average, and it is more likely that students reach this latter level, regardless of having been awarded a grant. In addition, the PROMISE scholarship cannot be renewed if a student fails to fulfill the requirement in one year, whereas the grant under consideration here can be renewed in the following year if the requirements (income and credits) are met. Thus, the PROMISE scholarship requirements are much stricter than those of the grant offered to Portuguese students, which probably works as better motivation for students.

As for the differential gender effects, we find that males generally benefit most in terms of dropout and that females benefit most from the grant in terms of performance (more credits, higher probability of graduating on time), at least when we look to the analysis focusing on receiving the grant in the first year and in the first and second years. We find the opposite for the effect of receiving the grant for three consecutive years, where the results regarding time to graduation are driven by males. Very few studies can be used for a comparison as most papers do not distinguish between males and females; however, the few that do find that the promise of financial rewards works better for female students, as observed here—females benefit the most in terms of performance (more credits, higher probability of graduating on time). Others, instead, have obtained mixed results, such as Agasisti et al. (2021), who find that "*the reform increased the probability of graduating on-time by 11 pp (significant at 10%) for women and 7.1 pp for men (statistically nonsignificant). However, the results are reversed when we consider the probability of graduating in 3 or 4 years, for which we observe an about 11 pp increase for men but a nil effect for women.*"[23] So, like others in the literature, we do not find a clear pattern for females and males. They both seem to benefit from the grant in certain dimensions.

[23] Page 17, footnote 15.

5 Analysis of students who apply for the grant at the beginning of their second curricular year

Section 4.7 already provided some hints regarding the effect of receiving the grant for more than one year, but it is limited to the sample of students who first receive the grant for their first year, as first-time applicants. In this particular sample, we could apply regression discontinuity design using per capita income as the sole running variable. The focus of this section is on students who apply for the grant at the beginning of their second curricular year and have thus already (successfully) completed their first year.

In extending the analysis to years after the first one, there are a few main issues to deal with: i) Assignment to the grant is based on two requirements (in terms of per capita income and credits obtained in the previous academic year), so a standard RDD similar to that applied to the sample of first-year students as first-time applicants cannot be applied as both selection rules need to be taken into account; ii) the credit variable is also an outcome of interest, so the second eligibility rule based on the minimum number of credits that students need to complete to be eligible for the grant is endogenous and cannot be used as running variable to identify the causal effect of the grant.

In addition, we cannot apply the method using the two running variables, as described in Annex 6, as after several checks, the variable "credits obtained in the previous academic year" is not continuous at the threshold, one of the main assumptions of this method.

Therefore, in order to assess the effect for students who get the grant in their second year, we rely on the paper by Scott-Clayton and Schudde (2020), which suggests two approaches:

— RDD on per capita income, conditional on having completed enough credits in the previous academic year;

— DiD, which compares students above and below the income and credit thresholds.

Our setting has one main limitation with respect to the data used by Scott-Clayton and Schudde (2020): Our sample does not include the universe of Portuguese students but, instead, includes applicants only. Therefore, we expect the sample of students whose credits are below the threshold or whose per capita income is above the threshold not to be fully represented in our sample. Still, even keeping this caveat in mind it is possible to retrieve some information on the grant's effect on students beyond the first year.

The next sections describe the approaches in detail and provide the respective results

5.1 RD design on the per capita income, conditional on having obtained enough credits in the previous academic year

This approach is the same as in the previous section, with the only difference that we restrict our sample to second-year students who obtained at least 36 credits in the previous academic year. Conditioning on success in the previous academic year implies that the only running variable that matters for eligibility is per capita income. Therefore, we apply the exact same methodology as explained in Section 4.2, that is, we use a regression discontinuity approach and compare students whose income is just above and just below the eligibility threshold. The two main assumptions needed to apply the method to this sample are the same as before: 1) absence of manipulation in the running variables around the income eligibility threshold; 2) no discontinuity at the income threshold in the distribution of relevant covariates.

Therefore, as a first step we test whether these two assumptions are met. We focus only on students enrolled in a bachelor's degree who are in their second curricular year, who apply for the grant for the first time, and who obtained at least 36 credits in the previous academic year. This is the only sample in which the two conditions are met.[24] Applying these restrictions, our working sample now includes 23,055 students, and in this is particular sub-sample the RDD assumptions are satisfied.[25] As for the first-year students, not all students who should have been treated according to their per capita income actually received the grant; the 23,055 students are divided as reported in Table 8.

As the treatment group is not fully compliant with the per capita income requirement (1,037 eligible students do not get the grant and 5 non-eligible students do get it), we again rely on a fuzzy design, and we replicate the analysis conducted in the previous section.

[24] When we focus on the sample of second-year students who successfully completed their first year and we test for the continuity of the running variable, we note that per capita income does not pass the test described in Section 4.5.3. (T=2.49 with p-value=0.013.) This implies that there is evidence of "manipulation" in the density of the running variable at the threshold. As explained in Section 4.5.1, we do not believe that students are able to manipulate their per capita income, but this absence of continuity probably stems from the fact that students above the threshold are less likely to apply for the grant, and even more so if their application was previously rejected. We therefore restrict the sample to second-year students who complete at least 36 credits and who apply for the first time. Thus, these are students who in their first year, for whatever reason, did not apply for the grant. If we restrict the sample to these students, the income distribution is continuous at the threshold (T=1.551 with p-value=0.121.). However, when we check for the continuity of predetermined covariates at the threshold (as in Section 4.5.2), we find that the variables "type of degree" corresponding to "master" or "integrated master" are not equally distributed on both sides of the income threshold. That's why we only consider bachelor students

[25] T=0.517 with p-value=0.605, and none of the covariates are different on the two sides of the threshold.

Table 8: Actual treatment and per capita income

	Income below the threshold		
Treated student	0	1	Total
0	4,191	1,037	5,228
1	5	17,822	17,827
Total	4,196	18,859	23,055

We focus on the same outcomes explained in the previous section but referring to the second year for what regards immediate dropout, dropout, and credits obtained at the end of the year. [26]

The results are provided in Table 9. As we can see, no effect of the grant is observed in this sample, with the exception of a higher probability of completing at least 36 credits at the end of the second year, the amount needed to receive the grant again in the following year. Of course, a positive effect on the probability of applying again is also found. The lack of any other effect, especially for enrollment and dropout, should not be surprising: We are dealing with a particular sample of students who managed to reach year 2 of higher education and who, in the previous academic year, already completed 36 credits. Dropping out at this stage is more costly than at the beginning of the first academic year as these pupils had already made it through the previous year without the grant, which means that this sub-sample is composed of motivated students who already passed one year of their bachelor's degree: Having the grant or not is "less important" in their decision to continue studying. These results are stable to the inclusion of the usual covariates.[27]

We investigated the heterogeneity of the results according to gender, private vs. public university, and region. The results are reported in Table A.9. We report only the outcomes for which we find some interesting results (outcomes for which the effect of the grant is not significant are not reported). The results show that for females, receiving the grant at the beginning of the second year decreases the probability of dropout at the end of that year (column 18). We also see that the result regarding achieving at least 36 credits is driven by males, by students at private universities, and by students from less developed regions (columns 35, 36, and 38). Treated students in this last group (coming from less developed regions) also show a higher probability of completing all of the credits they were enrolled in (column 46). However, we also find a negative probability of graduating (column 62). As for the probability of applying again the following academic year, the positive effect is valid for all sub-samples and the coefficients are very similar in magnitude. Thus, for second-year bachelor students who apply for the grant for the first time and who obtained at least 36 credits in the previous academic year, the grant seems to be less effective compared to what we found for first-year students. However, for some sub-samples, we still see some interesting effects.

[26] However, the definition of immediate dropout does not include students who are never found in the dataset: Being in their second year, they must have appeared before. If they are not found in the dataset, this means that the matching was not working between the two different data sources. To test whether the probability of not being found differs between the treated and control groups, we run Equation 1 on the variable "never found", and we do not find significant differences (coefficient -0.004, with standard error 0.005.)

[27] Tables not reported here.

Table 9: Main results, second year - RDD

	(1) Immediate dropout -B beginning of year 2	(2) Enrolled same course beginning of year 2	(3) Enrolled other course beginning of year 2	(4) Dropout end year 2	(5) At least 36 credits end of year 2
First stage	0.936***	0.936***	0.936***	0.936***	0.931***
	(0.010)	(0.010)	(0.009)	(0.011)	(0.011)
Robust	0.008	-0.007	-0.003	-0.023	0.054**
	(0.007)	(0.012)	(0.010)	(0.014)	(0.027)
Observations	[18817:4179]	[18817:4179]	[18817:4179]	[17200:3710]	[14812:3255]
Bandwidth	[1441:1441]	[1671:1671]	[1691:1691]	[1375:1375]	[1494:1494]
Effct. Observations	[3993:2047]	[4806:2283]	[4859:2300]	[3560:1775]	[3320:1626]

	(6) Obtained enrolled credit end of year 2	(7) Graduated on time	(8) Graduated	(9) FInal mark	(10) Apply next year
First stage	0.931***	0.937***	0.937***	0.935***	0.937***
	(0.010)	(0.013)	(0.012)	(0.013)	(0.010)
Robust	0.040	-0.058	-0.037	0.087	0.446***
	(0.035)	(0.041)	(0.030)	(0.152)	(0.032)
Observations	[15331:3269]	[15675:3269]	[15675:3269]	[12411:2348]	[18859:4196]
Bandwidth	[1869:1869]	[1195:1195]	[1355:1355]	[1381:1381]	[1452:1452]
Effct. Observations	[4513:1918]	[2821:1442]	[3301:1579]	[2669:1185]	[4045:2069]

Note: The table reports RDD estimates of Eq. (1) on the sample of students who apply for the grant for the first time at the beginning of their second curricular year, and have obtained at least 36 credits at the end of the previous academic year, enrolled in a "Bachelor", which is equivalent to a bachelor degree. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Difference in difference approach

In this setting, difference-in-differences is used to compare students whose per capita income is above and below the threshold and who obtained more or fewer than 36 credits in the previous academic year. Using this approach does not require continuity of the running variable at the threshold; therefore, we can extend the analysis to the whole sample of students who apply for the grant at the beginning of their second curricular year, including also pupils who are not first-time applicants and who are enrolled in a master or integrated master. Therefore, we can run the analysis on the full sample of second-year students, in all type of degree. This sample includes 123,497 students, of which 99,254 are treated. Following Scott-Clayton and Schudde (2020), we allow for a very flexible relationship between per capita income and outcomes by including a set of per capita income bins (with a width of 200 euro)., and we estimate the following equation:

$$\begin{aligned} Y_i =& \alpha_0 + \alpha_1(I\_below_i * C\_above_i) + \alpha_2(IncomeBin_i) + \alpha_3(C\_above_i) \\ &+ academic\_yearFE + \alpha_n Z_i + \epsilon_i \end{aligned} \tag{2}$$

Where Y_i is one of the outcomes for student i, I_below_i is a dummy variable taking value 1 if the per-capita income is below the threshold; C_above_i is a dummy variable taking value 1 if the credits obtained at the end of the first year are above 36.; $IncomeBin_i$ are the set of income bins. The coefficient α_1 is the DiD estimate. In the analysis, we include academic year fixed effects, and the usual set of control variables (Z_i) (age, gender,region, public or private university, and field of study).

We estimate the model for different bandwidths of the running variable:While this is not necessary for a DiD model, we do so in order to allow for a better comparison with the results shown in Section 5.1 and in the first year students' analysis. Equation 2 is estimated on 3 different bandwidths: the main one: (-1500;+1500), which is similar to the optimal bandwidth identified in the set of analyses on the first-year students, its half (-750;+750) and its double (-3000;+3000).

Note that the estimated effect is similar to an "intention to treat" as there is non-compliance with the eligibility criteria, so there are treated students among the non-eligible and vice versa.

DiD requires stronger assumptions about the relationship between first-year income and subsequent outcomes, namely that whatever differences in potential outcomes exist between students who completed fewer than 36 credits in the first year and those whose completed more than 36 credits are fixed as we move across the range of per capita income (after controlling for any differences in observable characteristics). We cannot use the usual common-trend assumption as we do not have pre-treatment outcomes, so we provide a covariate balance check, which tests whether the treatment variable has any significant impact on background characteristics (Pei et al., 2019). This test is reported in Table A.10 for the sample used: second-year bachelor students (column 1).[28] However, we see that in the full sample there are some differences, mainly between types of degree. Therefore, we run the covariate checks by type of degree, and indeed, the picture looks better (columns 2,3, and 4). Therefore, we run the model divided by type of degree and not on the full sample.

[28] In these estimations, we only use the 1,500 income bandwidth

Tables 10, 11, and 12 refer to the full sample of bachelor, master, and integrated master students, respectively, including also non-first-time applicants.

Interestingly, we see that in the full sample of bachelor students (Table 10), being eligible for the grant has a negative effect on dropout at the beginning of the second year. This effect is valid only in the main bandwidth, however. Similarly, we also see a negative effect on dropout at the end of the second year, which is robust to all bandwidth choices. There is also an effect on the probability of being enrolled in the same course indicated on one's application, as was found in analysis of first-year students. As usual, being eligible for the grant also influences the likelihood of applying again. In the samples of master and integrated master students, we do not see any effect.[29] We do see a negative effect on the probability of reaching 36 credits for students studying an integrated master.

We run a heterogeneity analysis focusing only on the sample of bachelor students, where we do see some similar effects to those observed for the full sample. Table A.11 reports these results. The effect on immediate dropout is driven by females, public universities, and students from less developed regions. While the effect for dropout at the end of the second year is driven by males and by students from less developed regions, the students from these regions who are eligible for the grant are also more likely to graduate. For students from regions in transition, we see a positive effect of the grant on the probability of completing 36 credits, but also a positive effect on the probability of dropping out at the end of the second year. We also estimate differential effects for displaced and non-displaced students. As was observed for first-year students, the effects are most evident for non-displaced students.

As a further heterogeneity analysis, we include in the regression an interaction for students who received the grant in their first academic year. This interaction is useful to understand whether there are differential effects for students who were awarded the grant in the first year. Equation 2 becomes

$$\begin{aligned} Y_i = &\alpha_0 + \alpha_1(I\_below_i * C\_above_i) + \alpha_2(IncomeBin_i) + \alpha_3(C\_above_i) \\ &\alpha_4(I\_below_i * C\_above_i * T1_i) + \alpha_5 T1_i + academic\_yearFE + \alpha_n Z_i + \epsilon_i, \end{aligned} \tag{3}$$

where $T1_i$ is a dummy taking a value of 1 if a student was treated in the previous year. Thus, α_1 captures the effect of being eligible for the grant in year 2 for those who did not get it in the previous year, while $\alpha_1 + \alpha_4$ captures the effect of being eligible in year 2 for those who also got the grant in the previous year. α_5 captures the effect of having received the grant in the first year but not being eligible in the second year (the reference group being those who never got the grant, either in year 1 or in year 2). The results are reported in Table 13.[30] We see that being eligible for the grant at the beginning of the second year has a differential effect for students according to whether they received the grant the year before or not.

In the first row of the table, we report the effect of being eligible for the grant in the second year for students who also received the grant the year before ($\alpha_1 + \alpha_4$). The second row reports the effect for those who did not receive the grant the previous year (α_1). We see that the negative effect on immediate dropout is driven by students who also benefitted from the grant the year before, while the effect is 0 for students who received the grant for the first time in their second academic year. This result suggests that for the set of students who received the grant in their first year, it is very important to also receive it in the subsequent one in order to continue studying: Losing the grant implies dropping out immediately, as shown by the coefficient associated with the variable $T1_i only$ indicating having received the grant in the first year only. Compared to the students who never got the grant, students who received the grant during their first year but were no longer eligible in the second year are more likely to drop out immediately.

On the other hand, the effect on dropout at the end of the second year is negative for both sets of students and is somewhat larger for those who got the grant for the first time during their second year.

Another interesting result is the effect on credits: being eligible for the grant the second year increases the probability of completing at least 36 credits for students who were not offered it the previous year and increases the probability of completing all enrolled credits for students who also benefited from it the previous year. Finally, being eligible for the grant in the second year increases the probability of graduating only for students who also had the grant the year before. The effect on the probability of applying again is positive for both groups.

In Table A.12, we report the usual heterogeneity analysis. Overall, students from less developed regions show greater benefits from being awarded the grant, namely with a lower dropout probability and a higher probability of graduating.

[29] Some outcomes are not estimated for the master students since at the end of the second year they should finish their degree; thus, for example, we do not have information about the credits obtained, and it does not make sense to estimate the probability of applying for the grant again in the following academic year.

[30] We report the results only for the sample of bachelor students, as no effects are identified for the other two types of degree.

Table 10: DID estimates- sample of bachelor students, second year

	750	1500	3000	750	1500	3000
	(1)	(2)	(3)	(4)	(5)	(6)
	Immediate dropout start of year 2			**Enrolled same course start of year 2**		
Income below* credits above 36	-0.000	-0.010**	-0.005	0.004	0.020**	0.012*
	(0.006)	(0.004)	(0.003)	(0.012)	(0.009)	(0.007)
Credits above 36	-0.002	0.001	-0.004	0.010	-0.003	0.002
	(0.004)	(0.004)	(0.003)	(0.010)	(0.008)	(0.006)
Observations	9,618	20,297	46,898	9,618	20,297	46,898
	(7)	(8)	(9)	(10)	(11)	(12)
	Enrolled other course start of year 2			**Dropout end of year 2**		
Income below* credits above 36	-0.004	-0.010	-0.007	-0.043***	-0.020**	-0.031***
	(0.011)	(0.008)	(0.006)	(0.014)	(0.010)	(0.008)
Credits above 36	-0.008	0.002	0.001	-0.083***	-0.104***	-0.091***
	(0.009)	(0.007)	(0.005)	(0.011)	(0.009)	(0.007)
Observations	9,618	20,297	46,898	7,974	16,882	39,049
	(13)	(14)	(15)	(16)	(17)	(18)
	At least 36 credits end of year 2			**Obtained enrolled credits end of year**		
Income below* credits above 36	0.005	0.024	0.017	0.008	0.025	0.028
	(0.024)	(0.018)	(0.014)	(0.038)	(0.028)	(0.022)
Credits above 36	0.341***	0.338***	0.343***	0.296***	0.291***	0.287***
	(0.020)	(0.016)	(0.013)	(0.030)	(0.024)	(0.020)
Observations	8,098	17,185	40,080	7,926	16,822	39,151
	(19)	(20)	(21)	(22)	(23)	(24)
	Graduated on time			**Graduated**		
Income below* credits above 36	-0.027	0.001	0.006	-0.008	0.023	0.025
	(0.038)	(0.028)	(0.022)	(0.032)	(0.024)	(0.019)
Credits above 36	0.452***	0.433***	0.425***	0.396***	0.382***	0.376***
	(0.031)	(0.024)	(0.020)	(0.026)	(0.021)	(0.017)
Observations	6,470	13,691	31,707	6,470	13,691	31,707
	(25)	(26)	(27)	(28)	(29)	(30)
	Final mark			**Apply next year**		
Income below* credits above 36	0.151	0.023	-0.008	0.310***	0.322***	0.334***
	(0.183)	(0.135)	(0.106)	(0.027)	(0.019)	(0.014)
Credits above 36	1.195***	1.229***	1.262***	0.138***	0.106***	0.081***
	(0.146)	(0.114)	(0.095)	(0.022)	(0.017)	(0.013)
Observations	4,612	9,681	22,350	9,637	20,336	46,979

Note: The table reports DiD estimates of Eq. (2) on the full sample of students who apply for the grant at the beginning of their second curricular year, enrolled in a Bachelor degree. Each column is a different regression, on the 10 outcomes, using 3 different bandwidths (750, 1500, 3000). Control variables included in the regressions are: academic year FE,region, age, gender, public university and field of study and income bins. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: DID estimates- sample of master students, second year

	750	**1500**	**3000**	**750**	**1500**	**3000**
	(1)	(2)	(3)	(4)	(5)	(6)
	Immediate dropout start of year 2			**Enrolled same course start of year 2**		
Income below* credits above 36	-0.019	0.001	-0.011	-0.009	-0.028	0.007
	(0.012)	(0.010)	(0.008)	(0.030)	(0.023)	(0.017)
Credits above 36	0.008	-0.006	0.002	-0.002	0.022	0.005
	(0.010)	(0.008)	(0.007)	(0.024)	(0.019)	(0.015)
Observations	2,783	5,994	13,597	2,783	5,994	13,597
	(7)	(8)	(9)	(10)	(11)	(12)
	Enrolled other course start of year 2			**Dropout end of year 2**		
Income below* credits above 36	0.026	0.037**	0.019	-0.018	0.042	0.003
	(0.023)	(0.017)	(0.013)	(0.071)	(0.050)	(0.038)
Credits above 36	-0.018	-0.030**	-0.018	-0.036	-0.033	-0.076**
	(0.018)	(0.015)	(0.012)	(0.057)	(0.042)	(0.034)
Observations	2,783	5,994	13,597	2,270	4,926	11,198
	(13)	(14)	(15)	(16)	(17)	(18)
	Graduated on time			**Graduated**		
Income below* credits above 36	-0.022	-0.047	-0.060	-0.043	-0.069	-0.046
	(0.080)	(0.055)	(0.042)	(0.077)	(0.053)	(0.040)
Credits above 36	0.041	0.064	0.111***	0.097	0.105**	0.145***
	(0.063)	(0.046)	(0.037)	(0.061)	(0.045)	(0.036)
Observations	2,274	4,935	11,221	2,274	4,935	11,221
	(19)	(20)	(21)			
	Final mark					
Income below* credits above 36	-0.028	-0.174	-0.273			
	(0.298)	(0.212)	(0.172)			
Credits above 36	0.383	0.534***	0.703***			
	(0.239)	(0.180)	(0.154)			
Observations	1,378	2,923	6,565			

Note: The table reports DiD estimates of Eq. (2) on the full sample of students who apply for the grant at the beginning of their second curricular year, enrolled in a Master' Each column is a different regression, on the 7 outcomes, using 3 different bandwidths (750, 1500, 3000). Control variables included in the regressions are: academic year FE,region, age, gender, public university and field of study and income bins. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 12: DID estimates- sample of Integrated master students, second year

	750	1500	3000	750	1500	3000
	(1)	(2)	(3)	(4)	(5)	(6)
	Immediate dropout start of year 2			**Enrolled same course start of year 2**		
Income below* credits above 36	-0.010	-0.009	-0.009	0.007	-0.009	-0.002
	(0.010)	(0.006)	(0.006)	(0.022)	(0.016)	(0.012)
Credits above 36	0.004	0.001	0.000	-0.020	-0.002	0.001
	(0.007)	(0.005)	(0.005)	(0.017)	(0.014)	(0.010)
Observations	1,373	2,878	6,301	1,373	2,878	6,301
	(7)	(8)	(9)	(10)	(11)	(12)
	Enrolled other course start of year 2			**Dropout end of year 2**		
Income below* credits above 36	0.003	0.018	0.011	0.020	0.026	0.001
	(0.020)	(0.015)	(0.010)	(0.029)	(0.020)	(0.015)
Credits above 36	0.017	0.001	-0.001	-0.067***	-0.110***	-0.086***
	(0.016)	(0.012)	(0.009)	(0.022)	(0.016)	(0.013)
Observations	1,373	2,878	6,301	1,145	2,400	5,296
	(13)	(14)	(15)	(16)	(17)	(18)
	At least 36 credits end of year 2			**Obtained enrolled credits end of year**		
Income below* credits above 36	-0.094	-0.115***	-0.055*	-0.102	-0.099	-0.031
	(0.059)	(0.044)	(0.032)	(0.098)	(0.070)	(0.052)
Credits above 36	0.454***	0.448***	0.430***	0.445***	0.432***	0.415***
	(0.045)	(0.036)	(0.029)	(0.074)	(0.058)	(0.047)
Observations	1,210	2,570	5,643	1,167	2,478	5,431
	(19)	(20)	(21)	(22)	(23)	(24)
	Graduated on time			**Graduated**		
Income below* credits above 36	0.051	-0.077	-0.008	0.044	-0.122	-0.074
	(0.141)	(0.096)	(0.070)	(0.137)	(0.094)	(0.068)
Credits above 36	0.309***	0.377***	0.322***	0.356***	0.446***	0.383***
	(0.101)	(0.078)	(0.062)	(0.099)	(0.076)	(0.061)
Observations	498	1,020	2,225	498	1,020	2,225
	(25)	(26)	(27)	(28)	(29)	(30)
	Final mark			**Apply next year**		
Income below* credits above 36	1.136	0.769	-0.399	0.221***	0.150***	0.202***
	(0.994)	(0.780)	(0.455)	(0.069)	(0.047)	(0.033)
Credits above 36	0.002	0.253	1.527***	0.189***	0.177***	0.154***
	(0.725)	(0.702)	(0.413)	(0.053)	(0.039)	(0.030)
Observations	277	548	1,176	1,377	2,883	6,310

Note: The table reports DiD estimates of Eq. (2) on the full sample of students who apply for the grant at the beginning of their second curricular year, enrolled in a Integrated master. Each column is a different regression, on the 10 outcomes, using 3 different bandwidths (750, 1500, 3000). Control variables included in the regressions are: academic year FE, region, age, gender, public university and field of study, and income bins. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 13: DID estimates- sample of Bachelor students, heterogeneity by first year grant

	750	1500	3000	750	1500	3000
	(1)	(2)	(3)	(4)	(5)	(6)
	Immediate dropout start of year 2			**Enrolled same course start of year 2**		
Income below* credits above 36	-0.002	-0.013***	-0.008**	-0.004	0.017*	0.012*
for grant in year 1	(0.006)	(0.004)	(0.003)	(0.013)	(0.009)	(0.007)
Income below* credits above 36	0.001	-0.007	-0.001	0.008	0.021**	0.010
for No grant in year 1	(0.006)	(0.004)	(0.003)	(0.013)	(0.009)	(0.007)
Grant in year 1	0.004	0.004*	0.005***	0.012*	0.010**	0.005
	(0.003)	(0.002)	(0.002)	(0.006)	(0.005)	(0.004)
Observations	9,618	20,297	46,898	9,618	20,297	46,898
	(7)	(8)	(9)	(10)	(11)	(12)
	Enrolled other course start of year 2			**Dropout end of year 2**		
Income below* credits above 36	0.006	-0.004	-0.004	-0.039***	-0.015	-0.028***
for grant in year 1	(0.012)	(0.008)	(0.006)	(0.015)	(0.011)	(0.008)
Income below* credits above 36	-0.009	-0.014	-0.009	-0.047***	-0.023**	-0.033***
for No grant in year 1	(0.011)	(0.008)	(0.006)	(0.014)	(0.011)	(0.008)
Grant in year 1	-0.016***	-0.014***	-0.009***	-0.002	-0.012**	-0.010**
	(0.006)	(0.004)	(0.003)	(0.008)	(0.006)	(0.005)
Observations	9,618	20,297	46,898	7,974	16,882	39,049
	(13)	(14)	(15)	(16)	(17)	(18)
	At least 36 credits end of year 2			**Obtained enrolled credits end of year**		
Income below* credits above 36	-0.007	0.006	0.000	0.027	0.049*	0.059***
for grant in year 1	(0.026)	(0.019)	(0.014)	(0.040)	(0.029)	(0.022)
Income below* credits above 36	0.013	0.040**	0.032**	-0.006	0.002	-0.013
for No grant in year 1	(0.025)	(0.019)	(0.014)	(0.039)	(0.029)	(0.023)
Grant in year 1	0.018	0.032***	0.043***	-0.025	-0.038**	-0.032***
	(0.012)	(0.009)	(0.007)	(0.019)	(0.015)	(0.012)
Observations	8,098	17,185	40,080	7,926	16,822	39,151
	(19)	(20)	(21)	(22)	(23)	(24)
	Graduated on time			**Graduated**		
Income below* credits above 36	0.008	0.026	0.032	0.018	0.048*	0.062***
for grant in year 1	(0.042)	(0.031)	(0.023)	(0.035)	(0.026)	(0.020)
Income below* credits above 36	-0.046	-0.014	-0.015	-0.024	0.007	-0.004
for No grant in year 1	(0.039)	(0.029)	(0.023)	(0.033)	(0.025)	(0.019)
Grant in year 1	-0.039*	-0.032*	-0.030**	-0.022	-0.033**	-0.044***
	(0.023)	(0.018)	(0.013)	(0.019)	(0.015)	(0.012)
Observations	6,470	13,691	31,707	6,470	13,691	31,707
	(25)	(26)	(27)	(28)	(29)	(30)
		Final mark			**Apply next year**	
Income below* credits above 36	0.213	0.037	0.006	0.234***	0.259***	0.278***
for grant in year 1	(0.193)	(0.143)	(0.112)	(0.029)	(0.020)	(0.015)
Income below* credits above 36	0.110	0.000	-0.046	0.342***	0.356***	0.365***
for No grant in year 1	(0.187)	(0.139)	(0.108)	(0.028)	(0.020)	(0.015)
Grant in year 1	-0.047	0.033	0.058	0.172***	0.168***	0.160***
	(0.089)	(0.071)	(0.058)	(0.014)	(0.011)	(0.008)
Observations	4,612	9,681	22,350	9,637	20,336	46,979

Note: The table reports DiD estimates of Eq. (3) on the full sample of students who apply for the grant at the beginning of their second curricular year, enrolled in a Bachelor degree. Each column is a different regression, on the 10 outcomes, using 3 different bandwidths (750, 1500, 3000). Control variables included in the regressions are: academic year FE,region, age, gender, public university and field of study and income bins. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5.3 Discussion regarding second-year students

The analysis of students beyond their first curricular year is more complicated as there are two eligibility conditions and one of the conditions (having completed 36 credits) is an outcome of the first-year grant. Here, we use 2 main identification strategies based on different assumptions and different samples.

The most interesting results come from the second method used, namely difference-in-differences, which allows us to use the full sample and not only that composed of students who applied for the first time at their second year and who obtained at least 36 credits the year before (RDD method; Section 5.1).[31] However, we must stress that the sample used in the DiD analysis is most likely not representative of the real population of students as it is composed of those who, despite not being eligible (due to income or credits), apply anyway.

In particular, we find that being eligible for the grant at the beginning of the second year has a negative effect on dropout probabilities for bachelor students. No effects are found for master or integrated master students. The magnitude of the estimates is quite small for immediate dropout (1 p.p.), whereas, for example, Scott-Clayton and Schudde (2020) find a 6 p.p. effect on the probability of enrolling in the second year for students eligible for the Pell grant who met the merit requirement. However, the effect they find is null for students whose GPA is very low. They also find a small effect on the probability of completing the degree (2 p.p.) similar in magnitude to our point estimate, which is not statistically significant, however, and smaller than what we find for the sample of students who are eligible for the grant in both their first and second years (4.8 p.p.).

If we take into account differential effects for those who also received the grant the previous academic year, we find that for those who received the grant the year before, being eligible the second year has negative effects on immediate drop out probabilities and positive effects on the credits obtained at the end of the year and the probability of graduation. These effects are larger for female students (credits), for students from less developed regions, and for those enrolled in public universities. Students who are eligible for the grant in the second year despite not having received it their first year are still less likely to drop out immediately (students from less developed regions only), to drop out at the end of the second year (especially male students, those from less developed regions, and those enrolled in public universities), and to complete 36 credits (females and students from private universities).

[31] In this very selected sample, we basically find an effect only on the probability or obtaining 36 credits, which is the minimum necessary to renew the grant in the following year.

6 Conclusions

In this report, we study the effectiveness of a higher education grant awarded to Portuguese students with a per capita income below a predefined threshold. From the second year onward, to receive the grant students also need to have completed at least 36 credits in the previous academic year. The grant covers the tuition fee of the university and provides additional financial support according to per capita income (poorer students receive more funding). The report first analyzes the impact of the grant for first-year students, who apply for the first time. Then the effects on second-year students is investigated. While for the first part of the analysis it is possible to use a regression discontinuity design, the analysis of second-year students relies on a difference-in-differences methodology.

Overall, the grant has the following small but significant effects:

- Reduces dropout (immediate, at the end of the first year, and at the end of the second year);
- Increases the probability of completing at least 36 credits (needed to get the grant again in the following year) and of completing all credits a student is enrolled in;
- Increases the probability of graduating on time;
- Increases the probability of graduating (in some cases).

The effects are different according to student characteristics: male students show stronger effects in terms of dropout, while females show stronger effects on the credits obtained; students from less developed regions show larger effects for dropout and graduation and on-time graduation rates; and most of the effects on dropout are concentrated on bachelor students.

Receiving the grant for more than one year also has incremental effects: The more years a student can benefit from the grant, the better are the outcomes. We also observe that the merit requirement pushes students to complete the credits needed to get the grant again the following year, which also generates other results.

The methodologies used have both positive and negative features:

- Regression discontinuity is very credible but very local: What we found is limited to the students who are close to the threshold, and we cannot know what the effect is for "very" poor students.
- Regression discontinuity relies on the "continuity" of the running variable (per capita income): Having access only to data on applicants is a limitation in this respect, as the method could not be applied beyond first-time applicants. Many students are excluded from the analysis (e.g., master's degree students who applied for the grant during their bachelor).
- The difference-in-differences analysis used to study the effects for the second year is based on a very selected sample: Students who apply despite having too few credits and a high income.

The results we found could be expanded with more updated data and data that provides information on the credits and final marks obtained by everyone, as well as information about the current curricular year in which students are enrolled. In particular, the following points are suggested for future work:

- Having access to outcome data for more recent years (2018, 2019, 2020, and now also 2021) would help check the robustness of the results found for longer-terms outcomes (graduation, etc.).
- Additional data such as the current year enrolled and credits and final marks for all students (not only those enrolled in the same course as that indicated on their application) would help paint clearer picture of the results regarding credits, as well as regarding progression and the probability of repeating a year.

  Both of these types in information would allow for a more comprehensive investigation of the progression of students from bachelor's to master's programs, which here is based on a limited sample (2012 and 2013), and give us a better understanding of the effects of the grant on for students enrolled in integrated master's programs.

References

Agasisti, T., Bratti, M. and Minaya, V., 'When need meets merit: The effect of increasing merit requirements in need-based student aid', 2021.

Agasisti, T. and Murtinu, S., 'Grants in italian university: a look at the heterogeneity of their impact on students' performances', *Studies in Higher Education*, Vol. 41, No 6, 2016, pp. 1106–1132.

Angrist, J., Lang, D. and Oreopoulos, P., 'Incentives and services for college achievement: Evidence from a randomized trial', *American Economic Journal: Applied Economics*, Vol. 1, No 1, 2009, pp. 136–63.

Arendt, J. N., 'The effect of public financial aid on dropout from and completion of university education: evidence from a student grant reform', *Empirical Economics*, Vol. 44, No 3, 2013, pp. 1545–1562.

Barrow, L. and Rouse, C. E., 'Financial incentives and educational investment: The impact of performance-based scholarships on student time use', Tech. rep., National Bureau of Economic Research, 2013.

Belley, P. and Lochner, L., 'The changing role of family income and ability in determining educational achievement', *Journal of Human capital*, Vol. 1, No 1, 2007, pp. 37–89.

Bettinger, E., 'How financial aid affects persistence'. In 'College Choices: The Economics of Where to Go, When to Go, and How to Pay For It', University of Chicago Press, 2004. pp. 207–238.

Bettinger, E., 'Need-based aid and college persistence: The effects of the ohio college opportunity grant', *Educational Evaluation and Policy Analysis*, Vol. 37, No 1_suppl, 2015, pp. 102S–119S.

Calonico, S., Cattaneo, M. D. and Farrell, M. H., 'Optimal bandwidth choice for robust bias-corrected inference in regression discontinuity designs', *The Econometrics Journal*, Vol. 23, No 2, 2020, pp. 192–210.

Calonico, S., Cattaneo, M. D., Farrell, M. H. and Titiunik, R., 'rdrobust: Software for regression-discontinuity designs', *The Stata Journal*, Vol. 17, No 2, 2017, pp. 372–404.

Calonico, S., Cattaneo, M. D., Farrell, M. H. and Titiunik, R., 'Regression discontinuity designs using covariates', *Review of Economics and Statistics*, Vol. 101, No 3, 2019, pp. 442–451.

Calonico, S., Cattaneo, M. D. and Titiunik, R., 'Robust nonparametric confidence intervals for regression-discontinuity designs', *Econometrica*, Vol. 82, No 6, 2014, pp. 2295–2326.

Canton, E. and De Jong, F., 'The demand for higher education in the netherlands, 1950–1999', *Economics of Education Review*, Vol. 24, No 6, 2005, pp. 651–663.

Castleman, B. L. and Long, B. T., 'Looking beyond enrollment: The causal effect of need-based grants on college access, persistence, and graduation', *Journal of Labor Economics*, Vol. 34, No 4, 2016, pp. 1023–1073.

Cattaneo, M. D., Idrobo, N. and Titiunik, R., 'A practical introduction to regression discontinuity designs: Foundations', Cambridge University Press, 2019.

Cattaneo, M. D., Jansson, M. and Ma, X., 'Local regression distribution estimators', *Working Paper*, 2020a. URL `https://cattaneo.princeton.edu/papers/Cattaneo-Jansson-Ma_2020_JoE.pdf`.

Cattaneo, M. D., Jansson, M. and Ma, X., 'Simple local polynomial density estimators', *Journal of the American Statistical Association*, Vol. 115, No 531, 2020b, pp. 1449–1455. . URL `https://doi.org/10.1080/01621459.2019.1635480`.

Cattaneo, M. D., Titiunik, R. and Vazquez-Bare, G., 'Analysis of regression discontinuity designs with multiple cutoffs or multiple scores', *The Stata Journal*, 2020c, pp. 1–24.

Choi, J.-y. and Lee, M.-j., 'Regression discontinuity with multiple running variables allowing partial effects', *Political Analysis*, Vol. 26, No 3, 2018, pp. 258–274. .

Cornwell, C., Mustard, D. B. and Sridhar, D. J., 'The enrollment effects of merit-based financial aid: Evidence from georgiaâĂŹs hope program', *Journal of Labor Economics*, Vol. 24, No 4, 2006, pp. 761–786.

Cornwell, C. M., Lee, K. H. and Mustard, D. B., 'Student responses to merit scholarship retention rules', *Journal of Human Resources*, Vol. 40, No 4, 2005, pp. 895–917.

David, D. and Dynarski, S., 'Into college, out of poverty? policies to increase the postsecondary attainment of the poor', *NBER Working Paper*, Vol. 15387, 2009.

Denning, J. T., 'Born under a lucky star financial aid, college completion, labor supply, and credit constraints', *Journal of Human Resources*, Vol. 54, No 3, 2019, pp. 760–784.

Dynarski, S., 'Building the stock of college-educated labor', *Journal of human resources*, Vol. 43, No 3, 2008, pp. 576–610.

Dynarski, S. and Scott-Clayton, J., 'Financial aid policy: Lessons from research', 2013.

Dynarski, S. M., 'Does aid matter? measuring the effect of student aid on college attendance and completion', *American Economic Review*, Vol. 93, No 1, 2003, pp. 279–288.

Fosnacht, K. and Dong, Y., 'Financial stress and its impact on first-year students' college experiences', .

Fredriksson, P., 'Economic incentives and the demand for higher education', *Scandinavian Journal of Economics*, Vol. 99, No 1, 1997, pp. 129–142.

Garibaldi, P., Giavazzi, F., Ichino, A. and Rettore, E., 'College cost and time to obtain a degree: Evidence from tuition discontinuities', *NBER Working Paper*, Vol. 12863, 2007.

Garibaldi, P., Giavazzi, F., Ichino, A. and Rettore, E., 'College cost and time to complete a degree: Evidence from tuition discontinuities', *Review of Economics and Statistics*, Vol. 94, No 3, 2012, pp. 699–711.

Goldrick-Rab, S., Kelchen, R., Harris, D. N. and Benson, J., 'Reducing income inequality in educational attainment: Experimental evidence on the impact of financial aid on college completion', *American Journal of Sociology*, Vol. 121, No 6, 2016, pp. 1762–1817.

Hahn, J., Todd, P. and der Klaauw, W. V., 'Identification and estimation of treatment effects with a regression-discontinuity design', *Econometrica*, Vol. 69, No 1, 2001, pp. 201–209. ISSN 00129682, 14680262. URL `http://www.jstor.org/stable/2692190`.

Häkkinen, I. and Uusitalo, R., 'The effect of a student aid reform on graduation: a duration analysis', Tech. rep., Working paper, 2003.

Henry, G. T. and Rubenstein, R., 'Paying for grades: Impact of merit-based financial aid on educational quality', *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management*, Vol. 21, No 1, 2002, pp. 93–109.

Hübner, M., 'Do tuition fees affect enrollment behavior? evidence from a âĂŸnatural experimentâĂŹin germany', *Economics of Education Review*, Vol. 31, No 6, 2012, pp. 949–960.

Lee, D. S. and Lemieux, T., 'Regression discontinuity designs in economics', *Journal of Economic Literature*, Vol. 48, No 2, June 2010, pp. 281–355. . URL `https://www.aeaweb.org/articles?id=10.1257/jel.48.2.281`.

Leuven, E., Oosterbeek, H. and Van der Klaauw, B., 'The effect of financial rewards on students' achievement: Evidence from a randomized experiment', *Journal of the European Economic Association*, Vol. 8, No 6, 2010, pp. 1243–1265.

McCrary, J., 'Manipulation of the running variable in the regression discontinuity design: A density test', *Journal of Econometrics*, Vol. 142, No 2, 2008, pp. 698 – 714. ISSN 0304-4076. . URL `http://www.sciencedirect.com/science/article/pii/S0304407607001133`. The regression discontinuity design: Theory and applications.

Mealli, F. and Rampichini, C., 'Impact evaluation of university grants', *2002 Proceedings of the American Statistical Association, Statistical Computing Section [CD-ROM], Alexandria, VA: American Statistical Association*, 2002.

Mealli, F. and Rampichini, C., 'Evaluating the effects of university grants by using regression discontinuity designs', *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, Vol. 175, No 3, 2012, pp. 775–798.

Montalbán, J., 'Countering moral hazard in higher education: The role of performance incentives in need-based grants', 2019.

Montalvo, J. G. et al., 'The impact of progressive tuition fees on dropping out of higher education: a regression discontinuity design', Tech. rep., 2018.

Nielsen, H. S., Sørensen, T. and Taber, C., 'Estimating the effect of student aid on college enrollment: Evidence from a government grant policy reform', *American Economic Journal: Economic Policy*, Vol. 2, No 2, 2010, pp. 185–215.

Pei, Z., Pischke, J.-S. and Schwandt, H., 'Poorly measured confounders are more useful on the left than on the right', *Journal of Business & Economic Statistics*, Vol. 37, No 2, 2019, pp. 205–216.

Perna, L. W., 'The contribution of financial aid to undergraduate persistence', *Journal of Student Financial Aid*, Vol. 28, No 3, 1998, p. 25.

Rattini, V., 'The causal effect of scholarships targeted at low income students on performance: Evidence from italy', *Quaderni-Working Paper DSE*, 2014.

Scott-Clayton, J., 'On money and motivation a quasi-experimental analysis of financial incentives for college achievement', *Journal of Human resources*, Vol. 46, No 3, 2011, pp. 614–646.

Scott-Clayton, J. and Schudde, L., 'Performance standards in need-based student aid', Tech. rep., National Bureau of Economic Research, 2016.

Scott-Clayton, J. and Schudde, L., 'The consequences of performance standards in need-based aid evidence from community colleges', *Journal of Human Resources*, Vol. 55, No 4, 2020, pp. 1105–1136.

Scott-Clayton, J. and Zafar, B., 'Financial aid, debt management, and socioeconomic outcomes: Post-college effects of merit-based aid', *Journal of Public Economics*, Vol. 170, 2019, pp. 68–82.

Seftor, N. S. and Turner, S. E., 'Back to school: Federal student aid policy and adult college enrollment', *Journal of Human resources*, 2002, pp. 336–352.

Sneyers, E., Agasisti, T., De Witte, K. and Graziosi, G., 'The impact of scholarships on studentsâĂŹ performance: a study on five italian universities', 2016.

Solis, A., 'Credit access and college enrollment', *Journal of Political Economy*, Vol. 125, No 2, 2017, pp. 562–622.

List of tables

List of figures

Additional Tables

Table A.1: Placebo fake thresholds

	(4) -4000	(3) -3000	(2) -2000	(1) -1000	(5) 1000	(6) 2000	(7) 3000	(8) 4000
Immediate dropout	0.003	-0.001	0.006	0.002	-0.001	-0.018	0.015	0.016
	(0.006)	(0.003)	(0.004)	(0.007)	(0.016)	(0.014)	(0.013)	(0.028)
Observations	[18473:61652]	[38796:41329]	[56287:23838]	[70020:10105]	[5860:8979]	[9605:5234]	[12089:2750]	[13680:1159]
Bandwidth	[251:251]	[747:747]	[609:609]	[318:318]	[315:315]	[631:631]	[452:452]	[394:394]
Effect. observations	[5120:5144]	[15164:13380]	[10259:8919]	[3878:3593]	[1513:1372]	[2158:1660]	[1019:791]	[561:505]
Immediate dropout-B	0.004	-0.003	0.009**	-0.003	-0.005	-0.004	-0.002	-0.001
	(0.004)	(0.003)	(0.004)	(0.005)	(0.010)	(0.010)	(0.008)	(0.017)
Observations	[18396:61348]	[38629:41115]	[56053:23691]	[69704:10040]	[5797:8870]	[9490:5177]	[11953:2714]	[13523:1144]
Bandwidth	[292:292]	[612:612]	[441:441]	[299:299]	[345:345]	[670:670]	[522:522]	[496:496]
Effect. observations	[5939:5958]	[12311:11033]	[7283:6569]	[3629:3366]	[1645:1454]	[2294:1745]	[1185:887]	[714:625]
Never found	-0.002	0.000	-0.001	0.005	0.002	-0.014	0.014	0.016
	(0.004)	(0.002)	(0.003)	(0.005)	(0.011)	(0.010)	(0.010)	(0.021)
Observations	[18473:61652]	[38796:41329]	[56287:23838]	[70020:10105]	[5860:8979]	[9605:5234]	[12089:2750]	[13680:1159]
Bandwidth	[292:292]	[600:600]	[598:598]	[325:325]	[364:364]	[588:588]	[504:504]	[364:364]
Effect. observations	[5959:5985]	[12152:10854]	[10070:8781]	[3973:3661]	[1779:1564]	[1983:1548]	[1146:867]	[516:468]
Enrolled same course	0.004	-0.008	-0.015**	0.006	0.015	0.007	0.002	0.049
	(0.007)	(0.006)	(0.006)	(0.010)	(0.017)	(0.016)	(0.024)	(0.042)
Observations	[18396:61348]	[38629:41115]	[56053:23691]	[69704:10040]	[5797:8870]	[9490:5177]	[11953:2714]	[13523:1144]
Bandwidth	[422:422]	[669:669]	[717:717]	[393:393]	[313:313]	[960:960]	[557:557]	[257:257]
Effect. observations	[8571:8652]	[13448:12012]	[12213:10221]	[4810:4348]	[1469:1334]	[3496:2371]	[1267:929]	[351:339]
Enrolled other course	-0.008	0.002	0.004	-0.013*	0.002	-0.004	0.000	-0.028
	(0.006)	(0.004)	(0.005)	(0.008)	(0.012)	(0.013)	(0.016)	(0.023)
Observations	[18396:61348]	[38629:41115]	[56053:23691]	[69704:10040]	[5797:8870]	[9490:5177]	[11953:2714]	[13523:1144]
Bandwidth	[334:334]	[807:807]	[501:501]	[303:303]	[344:344]	[929:929]	[846:846]	[285:285]
Effect. observations	[6739:6854]	[16291:14272]	[8333:7367]	[3683:3402]	[1639:1447]	[3341:2298]	[2039:1356]	[394:373]
Dropout end year 1	0.023*	-0.016**	0.005	0.004	0.001	-0.012	0.040	-0.021
	(0.013)	(0.008)	(0.009)	(0.015)	(0.023)	(0.022)	(0.025)	(0.046)
Observations	[14735:50636]	[31388:33983]	[45747:19624]	[57039:8332]	[4705:7315]	[7726:4294]	[9748:2272]	[11059:961]
Bandwidth	[239:239]	[649:649]	[688:688]	[348:348]	[345:345]	[754:754]	[764:764]	[434:434]
Effect. observations	[3879:3972]	[10777:9610]	[9585:8181]	[3480:3233]	[1352:1174]	[2149:1595]	[1468:1050]	[514:445]
At least 36 credits	0.009	0.008	-0.008	-0.009	0.014	0.024	-0.015	0.098
	(0.018)	(0.010)	(0.013)	(0.023)	(0.037)	(0.037)	(0.050)	(0.075)
Observations	[15767:54183]	[33662:36288]	[49028:20922]	[61088:8862]	[4433:6482]	[7188:3727]	[8985:1930]	[10124:791]
Bandwidth	[254:254]	[782:782]	[651:651]	[300:300]	[422:422]	[653:653]	[628:628]	[273:273]
Effect. observations	[4530:4551]	[14020:12229]	[9695:8289]	[3225:2984]	[1579:1330]	[1665:1243]	[1052:766]	[268:248]
Obtained enrolled credits	0.014	-0.019	-0.031*	-0.031	0.018	0.020	0.004	0.083
	(0.021)	(0.019)	(0.018)	(0.034)	(0.058)	(0.044)	(0.052)	(0.091)
Observations	[15585:53329]	[33223:35691]	[48358:20556]	[60231:8683]	[4380:6439]	[7102:3717]	[8894:1925]	[10028:791]
Bandwidth	[445:445]	[550:550]	[778:778]	[276:276]	[295:295]	[842:842]	[885:885]	[400:400]
Effect. observations	[7769:7910]	[9656:8638]	[11528:9544]	[2902:2708]	[1041:946]	[2194:1546]	[1553:1029]	[398:339]
Graduated	-0.016	0.014	0.041	-0.038	0.050	-0.008	0.002	-0.018
	(0.032)	(0.024)	(0.026)	(0.044)	(0.060)	(0.064)	(0.063)	(0.123)
Observations	[7378:29479]	[16694:20163]	[25117:11740]	[31816:5041]	[2695:4312]	[4451:2556]	[5630:1377]	[6425:582]
Bandwidth	[354:354]	[624:624]	[617:617]	[292:292]	[455:455]	[609:609]	[772:772]	[346:346]
Effect. observations	[3024:3221]	[5883:5402]	[5006:4380]	[1770:1668]	[1071:892]	[983:767]	[872:652]	[236:217]
Graduated on time	-0.053	0.009	0.028	-0.015	0.055	0.045	0.007	-0.171
	(0.034)	(0.025)	(0.023)	(0.042)	(0.061)	(0.064)	(0.069)	(0.113)
Observations	[7378:29479]	[16694:20163]	[25117:11740]	[31816:5041]	[2695:4312]	[4451:2556]	[5630:1377]	[6425:582]
Bandwidth	[289:289]	[564:564]	[802:802]	[324:324]	[395:395]	[569:569]	[623:623]	[346:346]
Effect. observations	[2482:2621]	[5307:4906]	[6620:5508]	[1963:1846]	[910:784]	[906:720]	[667:534]	[236:217]
Apply next year	-0.017	0.014	0.022	0.001	0.031	0.017	0.022	-0.022
	(0.021)	(0.011)	(0.014)	(0.019)	(0.046)	(0.039)	(0.038)	(0.063)
Observations	[18473:61652]	[38796:41329]	[56287:23838]	[70020:10105]	[5860:8979]	[9605:5234]	[12089:2750]	[13680:1159]
Bandwidth	[253:253]	[586:586]	[531:531]	[415:415]	[331:331]	[621:621]	[844:844]	[371:371]
Effect. observations	[5158:5175]	[11836:10628]	[8862:7827]	[5136:4609]	[1592:1424]	[2118:1639]	[2045:1372]	[524:477]
Final mark	-0.048	-0.053	0.093	-0.043	0.151	0.406	0.794**	-1.302*
	(0.178)	(0.111)	(0.097)	(0.181)	(0.273)	(0.294)	(0.370)	(0.734)
Observations	[3799:15947]	[8796:10950]	[13440:6306]	[17014:2732]	[1313:2015]	[2150:1178]	[2714:614]	[3055:273]
Bandwidth	[225:225]	[522:522]	[899:899]	[341:341]	[434:434]	[553:553]	[399:399]	[322:322]
Effect. observations	[970:1042]	[2723:2474]	[4132:3291]	[1064:1032]	[494:395]	[425:335]	[205:159]	[86:98]

Note: The table reports the effect of having a per capita income above a set of "fake" thresholds: 1000, 2000, 3000, and 4000 euro above or below the real threshold,. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Difference polynomial choice

	(1)	(2)	(3)	(4)	(5)	(6)
	Immediate dropout		**Immediate dropout -B**		**Never Found**	
Robust	-0.020***	-0.023***	-0.010**	-0.012**	-0.008**	-0.010**
	(0.007)	(0.007)	(0.005)	(0.005)	(0.004)	(0.005)
Observations	[80125:14839]	[80125:14839]	[79744:14667]	[79744:14667]	[80125:14839]	[80125:14839]
Bandwidth	[1787:1787]	[2425:2425]	[2189:2189]	[2658:2658]	[2120:2120]	[2516:2516]
Effect. observations	[20569:8963]	[30882:10764]	[26777:10016]	[34823:11207]	[25767:9965]	[32438:10986]
Order Loc. Poly. (p)	2	3	2	3	2	3
Order Bias (q)	3	4	3	4	3	4
	(7)	(8)	(9)	(10)	(11)	(12)
	Enrolled same course		**Enrolled other course**		**Dropout end year1**	
Robust	0.029***	0.029***	-0.019***	-0.022***	-0.019	-0.021
	(0.009)	(0.011)	(0.007)	(0.007)	(0.012)	(0.014)
Observations	[79744:14667]	[79744:14667]	[79744:14667]	[79744:14667]	[65371:12020]	[65371:12020]
Bandwidth	[2198:2198]	[2400:2400]	[2040:2040]	[2813:2813]	[1741:1741]	[2269:2269]
Effect. observations	[26915:10034]	[30320:10579]	[24318:9613]	[37575:11562]	[16340:7068]	[23224:8336]
Order Loc. Poly. (p)	2	3	2	3	2	3
Order Bias (q)	3	4	3	4	3	4
	(13)	(14)	(15)	(16)	(17)	(18)
	At leaast 36 credits		**Obtained enrolled credits**		**Graduated on time**	
Robust	0.025	0.027	0.046**	0.065**	0.081**	0.084*
	(0.016)	(0.018)	(0.022)	(0.027)	(0.034)	(0.044)
Observations	[69950:10915]	[69950:10915]	[68914:10819]	[68914:10819]	[36857:7007]	[36857:7007]
Bandwidth	[2357:2357]	[2761:2761]	[2396:2396]	[2610:2610]	[1582:1582]	[1615:1615]
Effect. observations	[26095:7902]	[32349:8617]	[26263:7885]	[29447:8258]	[8699:3835]	[8926:3881]
Order Loc. Poly. (p)	2	3	2	3	2	3
Order Bias (q)	3	4	3	4	3	4
	(19)	(20)	(21)	(22)	(23)	(24)
	Graduated		**Final Mark**		**Apply next yar**	
Robust	0.045	0.041	0.245*	0.284*	0.404***	0.394***
	(0.032)	(0.041)	(0.136)	(0.162)	(0.019)	(0.022)
Observations	[36857:7007]	[36857:7007]	[19746:3328]	[19746:3328]	[80125:14839]	[80125:14839]
Bandwidth	[1924:1924]	[1881:1881]	[1784:1784]	[2132:2132]	[2102:2102]	[2345:2345]
Effect. observations	[11161:4337]	[10842:4282]	[5449:2000]	[6897:2228]	[25490:9913]	[29531:10560]
Order Loc. Poly. (p)	2	3	2	3	2	3
Order Bias (q)	3	4	3	4	3	4

Note: The table reports the effect of of the grant using a quadratic polynomial (odd columns), or a cubic one (even columns).
*** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Heterogeneity first year students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Immediate drop out	-0.017***	-0.016**	-0.015*	0.022	-0.020***	-0.018***	-0.010	-0.004	-0.021***	-0.017	-0.015
	(0.006)	(0.007)	(0.009)	(0.021)	(0.006)	(0.006)	(0.020)	(0.015)	(0.007)	(0.014)	(0.013)
Observations	[80125:14839]	[51477:9061]	[28648:5778]	[6894:1499]	[73231:13340]	[65512:11143]	[6078:1612]	[8535:2084]	[62790:10958]	[4553:914]	[11977:2808]
Bandwidth	[1222:1222]	[1473:1473]	[1423:1423]	[1342:1342]	[1321:1321]	[1440:1440]	[1861:1861]	[1197:1197]	[1090:1090]	[906:906]	[1211:1211]
Effect. observations	[12775:6832]	[9923:4862]	[5876:2915]	[1287:692]	[12783:6562]	[12223:5934]	[2031:914]	[1513:925]	[8537:4741]	[504:343]	[2136:1173]
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Immediate drop out-B	-0.010**	-0.006	-0.016**	0.017	-0.011***	-0.011***	-0.013	0.003	-0.009*	-0.003	-0.015**
	(0.004)	(0.004)	(0.008)	(0.019)	(0.004)	(0.004)	(0.015)	(0.010)	(0.005)	(0.007)	(0.008)
Observations	[79744:14667]	[51233:8955]	[28511:5712]	[6827:1479]	[72917:13188]	[65203:11008]	[6025:1584]	[8516:2075]	[62496:10838]	[4534:906]	[11915:2766]
Bandwidth	[1402:1402]	[1845:1845]	[1299:1299]	[1326:1326]	[1869:1869]	[1562:1562]	[1926:1926]	[1190:1190]	[1351:1351]	[1038:1038]	[1846:1846]
Effect. observations	[14956:7486]	[13263:5566]	[5235:2691]	[1237:678]	[19774:8253]	[13486:6227]	[2075:913]	[1499:917]	[11006:5474]	[592:378]	[3485:1628]
	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Never found	-0.007**	-0.011**	-0.001	0.011	-0.009**	-0.006	0.004	-0.007	-0.010**	-0.013	-0.000
	(0.004)	(0.005)	(0.005)	(0.015)	(0.004)	(0.004)	(0.014)	(0.010)	(0.005)	(0.012)	(0.009)
Observations	[80125:14839]	[51477:9061]	[28648:5778]	[6894:1499]	[73231:13340]	[65512:11143]	[6078:1612]	[8535:2084]	[62790:10958]	[4553:914]	[11977:2808]
Bandwidth	[1177:1177]	[1266:1266]	[1232:1232]	[770:770]	[1230:1230]	[1393:1393]	[1910:1910]	[1223:1223]	[1024:1024]	[901:901]	[1236:1236]
Effect. observations	[12194:6639]	[8227:4355]	[4939:2611]	[666:470]	[11735:6207]	[11701:5778]	[2086:925]	[1551:947]	[7945:4518]	[501:342]	[2187:1199]
	(34)	(35)	(36)	(37)	(38)	(39)	(40)	(41)	(42)	(43)	(44)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Enrolled same degree	0.026***	0.018*	0.039***	-0.012	0.030***	0.021***	0.081**	0.014	0.030***	-0.006	0.012
	(0.009)	(0.009)	(0.015)	(0.027)	(0.009)	(0.007)	(0.038)	(0.019)	(0.010)	(0.040)	(0.015)
Observations	[79744:14667]	[51233:8955]	[28511:5712]	[6827:1479]	[72917:13188]	[65203:11008]	[6025:1584]	[8516:2075]	[62496:10838]	[4534:906]	[11915:2766]
Bandwidth	[1120:1120]	[1438:1438]	[1189:1189]	[1412:1412]	[1240:1240]	[1497:1497]	[1948:1948]	[1307:1307]	[1022:1022]	[873:873]	[1950:1950]
Effect. observations	[11456:6336]	[9553:4740]	[4697:2515]	[1350:711]	[11788:6187]	[12768:6015]	[2100:923]	[1655:977]	[7874:4457]	[473:333]	[3700:1681]
	(45)	(46)	(47)	(48)	(49)	(50)	(51)	(52)	(53)	(54)	(55)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Enrolled other degreee	-0.015**	-0.010	-0.025**	-0.002	-0.017**	-0.009	-0.060**	-0.019	-0.021***	0.019	0.008
	(0.007)	(0.007)	(0.011)	(0.018)	(0.007)	(0.006)	(0.029)	(0.016)	(0.007)	(0.038)	(0.011)
Observations	[79744:14667]	[51233:8955]	[28511:5712]	[6827:1479]	[72917:13188]	[65203:11008]	[6025:1584]	[8516:2075]	[62496:10838]	[4534:906]	[11915:2766]
Bandwidth	[1147:1147]	[1319:1319]	[1279:1279]	[1255:1255]	[1300:1300]	[1537:1537]	[1116:1116]	[1407:1407]	[1027:1027]	[967:967]	[1817:1817]
Effect. observations	[11768:6458]	[8573:4435]	[5124:2658]	[1154:655]	[12490:6410]	[13189:6153]	[1151:610]	[1802:1028]	[7916:4477]	[540:360]	[3423:1607]
	(56)	(57)	(58)	(59)	(60)	(61)	(62)	(63)	(64)	(65)	(66)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Dropout end year 1	-0.011	0.007	-0.041**	0.033	-0.013	-0.010	-0.012	0.000	-0.015	-0.007	0.018
	(0.009)	(0.010)	(0.018)	(0.038)	(0.009)	(0.010)	(0.042)	(0.016)	(0.010)	(0.039)	(0.024)
Observations	[65371:12020]	[42085:7385]	[23286:4635]	[5200:1167]	[60171:10853]	[52839:8893]	[5606:1446]	[6926:1681]	[51334:8886]	[3659:712]	[9686:2287]
Bandwidth	[1434:1434]	[1736:1736]	[1388:1388]	[1389:1389]	[1623:1623]	[1593:1593]	[1659:1659]	[1383:1383]	[1780:1780]	[830:830]	[1691:1691]
Effect. observations	[12717:6176]	[10103:4378]	[4699:2253]	[1013:556]	[13700:6156]	[11284:5030]	[1635:750]	[1443:828]	[12920:5351]	[388:256]	[2599:1247]

Note: The table reports RDD estimates of Eq. (1) in the different sub-samples. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Heterogeneity first year students-cont

	(67)	(68)	(69)	(70)	(71)	(72)	(73)	(74)	(75)	(76)	(77)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
At least 36 credits	0.026**	0.031**	0.021	0.055	0.022	0.020	0.086**	0.001	0.024	0.101	0.021
	(0.012)	(0.013)	(0.025)	(0.047)	(0.014)	(0.016)	(0.043)	(0.048)	(0.015)	(0.082)	(0.032)
Observations	[69950:10915]	[45596:6855]	[24354:4060]	[5801:1041]	[64149:9874]	[57721:8239]	[4519:1059]	[7710:1617]	[54831:8106]	[4073:688]	[10375:2009]
Bandwidth	[1976:1976]	[1909:1909]	[1801:1801]	[1437:1437]	[1665:1665]	[1513:1513]	[1324:1324]	[1097:1097]	[1675:1675]	[581:581]	[1606:1606]
Effect. observations	[20597:7136]	[12434:4431]	[6711:2465]	[1201:522]	[14973:5843]	[11597:4649]	[1015:464]	[1244:690]	[12756:4829]	[295:192]	[2585:1096]
	(78)	(79)	(80)	(81)	(82)	(83)	(84)	(85)	(86)	(87)	(88)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Obtained enrolled credits	0.038**	0.049**	0.040	0.138*	0.035*	0.021	0.242***	0.129**	0.028	0.259**	0.045
	(0.017)	(0.024)	(0.035)	(0.072)	(0.018)	(0.021)	(0.088)	(0.065)	(0.023)	(0.102)	(0.040)
Observations	[68914:10819]	[44873:6772]	[24041:4047]	[5747:1042]	[63167:9777]	[56893:8170]	[4353:1049]	[7668:1600]	[54033:8025]	[3992:685]	[10227:1995]
Bandwidth	[2077:2077]	[1652:1652]	[1362:1362]	[1337:1337]	[2066:2066]	[1691:1691]	[538:538]	[1079:1079]	[1439:1439]	[610:610]	[2066:2066]
Effect. observations	[21620:7263]	[10056:3998]	[4728:2005]	[1092:497]	[19612:6578]	[13175:4925]	[358:212]	[1214:670]	[10336:4317]	[300:203]	[3453:1294]
	(89)	(90)	(91)	(92)	(93)	(94)	(95)	(96)	(97)	(98)	(99)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Graduated on time	0.056**	0.060*	0.036	0.192*	0.043	0.067**	0.043	-0.047	0.051	-0.083	0.061
	(0.027)	(0.035)	(0.028)	(0.107)	(0.027)	(0.033)	(0.050)	(0.085)	(0.033)	(0.140)	(0.053)
Observations	[36857:7007]	[24151:4384]	[12706:2623]	[2557:625]	[34300:6382]	[30287:5327]	[5155:1287]	[1415:393]	[29000:5187]	[1971:349]	[5417:1374]
Bandwidth	[1364:1364]	[1289:1289]	[5000:5000]	[1170:1170]	[1450:1450]	[1221:1221]	[1907:1907]	[4996:4996]	[1215:1215]	[705:705]	[1773:1773]
Effect. observations	[7242:3421]	[4269:2131]	[12704:2623]	[445:260]	[7232:3278]	[5013:2437]	[1735:749]	[1414:393]	[4829:2359]	[194:102]	[1724:766]
	(100)	(101)	(102)	(103)	(104)	(105)	(106)	(107)	(108)	(109)	(110)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Graduated	0.042	0.039	0.030	0.070	0.040	0.044	0.101	-0.047	0.045	-0.137	0.014
	(0.027)	(0.035)	(0.029)	(0.102)	(0.027)	(0.031)	(0.068)	(0.085)	(0.031)	(0.149)	(0.055)
Observations	[36857:7007]	[24151:4384]	[12706:2623]	[2557:625]	[34300:6382]	[30287:5327]	[5155:1287]	[1415:393]	[29000:5187]	[1971:349]	[5417:1374]
Bandwidth	[1329:1329]	[1230:1230]	[5000:5000]	[1265:1265]	[1460:1460]	[1284:1284]	[1235:1235]	[4996:4996]	[1282:1282]	[676:676]	[1905:1905]
Effect. observations	[7028:3348]	[4053:2059]	[12704:2623]	[491:279]	[7278:3292]	[5333:2532]	[1088:538]	[1414:393]	[5160:2455]	[185:100]	[1857:800]
	(111)	(112)	(113)	(114)	(115)	(116)	(117)	(118)	(119)	(120)	(121)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Final mark	0.177	0.159	0.161	0.205	0.197*	0.172	-0.241	1.711***	0.191*		-0.126
	(0.110)	(0.130)	(0.142)	(0.424)	(0.118)	(0.108)	(0.259)	(0.596)	(0.116)		(0.283)
Observations	[19746:3328]	[13991:2285]	[5755:1043]	[1297:298]	[18449:3030]	[16834:2682]	[2384:516]	[528:130]	[15795:2480]		[2781:651]
Bandwidth	[1377:1377]	[1330:1330]	[4999:4999]	[1168:1168]	[1297:1297]	[1501:1501]	[1541:1541]	[938:938]	[1665:1665]		[1521:1521]
Effect. observations	[3926:1660]	[2569:1161]	[5754:1043]	[227:126]	[3401:1461]	[3645:1440]	[608:263]	[83:54]	[3859:1450]		[744:324]
	(122)	(123)	(124)	(125)	(126)	(127)	(128)	(129)	(130)	(131)	(132)
	Main	Female	Male	Private	Public	Bachelor	Master	Integrated m.	Less dev	Transition	Dev
Apply again	0.404***	0.416***	0.387***	0.466***	0.398***	0.404***	0.378***	0.441***	0.405***	0.421***	0.409***
	(0.017)	(0.020)	(0.028)	(0.050)	(0.018)	(0.018)	(0.046)	(0.046)	(0.018)	(0.082)	(0.035)
Observations	[80125:14839]	[51477:9061]	[28648:5778]	[6894:1499]	[73231:13340]	[65512:11143]	[6078:1612]	[8535:2084]	[62790:10958]	[4553:914]	[11977:2808]
Bandwidth	[1253:1253]	[1418:1418]	[1376:1376]	[1595:1595]	[1217:1217]	[1520:1520]	[1851:1851]	[1244:1244]	[1407:1407]	[686:686]	[1842:1842]
Effect. observations	[13152:6970]	[9439:4751]	[5649:2853]	[1587:782]	[11592:6158]	[13084:6185]	[2015:914]	[1574:956]	[11612:5720]	[385:278]	[3507:1653]

Note: The table reports RDD estimates of Eq. (1) in the different sub-samples. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Heterogeneity by displaced students

	Immediate dropout			Immediate dropout-B			Never found		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Main	Displaced	Non disp	Main	Displaced	Non disp	Main	Displaced	Non disp
Robust	-0.017***	-0.013	-0.013**	-0.010**	-0.013*	-0.008	-0.007**	-0.000	-0.009**
	(0.006)	(0.009)	(0.006)	(0.004)	(0.007)	(0.005)	(0.004)	(0.005)	(0.004)
Obs.	[80125:14839]	[21803:3382]	[58322:11457]	[79744:14667]	[21736:3358]	[58008:11309]	[80125:14839]	[21803:3382]	[58322:11457]
Bandwidth	[1222:1222]	[1620:1620]	[1835:1835]	[1402:1402]	[1835:1835]	[1123:1123]	[1177:1177]	[1701:1701]	[1278:1278]
Effect. obs.	[12775:6832]	[4755:1925]	[15740:7036]	[14956:7486]	[5562:2070]	[8503:4921]	[12194:6639]	[5065:1976]	[9963:5475]

	Enrolled same course			Enrolled other course			Dropout end year 1		
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Main	Displaced	Non disp	Main	Displaced	Non disp	Main	Displaced	Non disp
Robust	0.026***	0.025*	0.023**	-0.015**	-0.013	-0.014*	-0.011	-0.004	-0.015
	(0.009)	(0.014)	(0.010)	(0.007)	(0.009)	(0.008)	(0.009)	(0.015)	(0.012)
Obs.	[79744:14667]	[21736:3358]	[58008:11309]	[79744:14667]	[21736:3358]	[58008:11309]	[65371:12020]	[18033:2713]	[47338:9307]
Bandwidth	[1120:1120]	[1717:1717]	[1088:1088]	[1147:1147]	[1886:1886]	[1136:1136]	[1434:1434]	[1793:1793]	[1354:1354]
Effect. obs.	[11456:6336]	[5113:1974]	[8200:4815]	[11768:6458]	[5733:2102]	[8609:4973]	[12717:6176]	[4525:1647]	[8748:4551]

	At least 36 credits			Reached all enrolled credits			Graduated time		
	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)
	Main	Displaced	Non disp	Main	Displaced	Non disp	Main	Displaced	Non disp
Robust	0.026**	-0.055*	0.047***	0.038**	-0.024	0.074***	0.056**	0.030	0.060*
	(0.012)	(0.029)	(0.016)	(0.017)	(0.043)	(0.025)	(0.027)	(0.049)	(0.031)
Obs.	[69950:10915]	[19167:2546]	[50783:8369]	[68914:10819]	[18774:2516]	[50140:8303]	[36857:7007]	[10211:1466]	[26646:5541]
Bandwidth	[1976:1976]	[1284:1284]	[1455:1455]	[2077:2077]	[1308:1308]	[1139:1139]	[1364:1364]	[1773:1773]	[1339:1339]
Effect. obs.	[20597:7136]	[3142:1239]	[10193:4483]	[21620:7263]	[3134:1241]	[7465:3743]	[7242:3421]	[2638:862]	[5258:2662]

	Graduated			Apply again		
	(28)	(29)	(30)	(31)	(32)	
	Main	Displaced	Non disp	Main	Displaced	Non disp
Robust	0.042	0.040	0.044	0.404***	0.463***	0.387***
	(0.027)	(0.056)	(0.032)	(0.017)	(0.034)	(0.020)
Obs.	[36857:7007]	[10211:1466]	[26646:5541]	[80125:14839]	[21803:3382]	[58322:11457]
Bandwidth	[1329:1329]	[1417:1417]	[1309:1309]	[1253:1253]	[1244:1244]	[1290:1290]
Effect. obs.	[7028:3348]	[1955:740]	[5118:2626]	[13152:6970]	[3390:1554]	[10085:5507]

Note: The table reports RDD estimates of Eq. (1) in the different sub-samples. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Heterogeneity analysis, first year students, receiving the grant in first and second year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Immediate dropout	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
beginning year 2	-0.027	0.005	-0.097***	-0.038*	0.059	-0.039**	-0.014	0.037	-0.024	-0.031	0.003
	(0.018)	(0.021)	(0.037)	(0.020)	(0.064)	(0.019)	(0.061)	(0.050)	(0.018)	(0.092)	(0.015)
Observations	[65371:12020]	[42085:7385]	[23286:4635]	[60171:10853]	[5200:1167]	[51334:8886]	[3659:712]	[9686:2287]	[52839:8893]	[5606:1446]	[6926:1681]
Bandwidth	[1158:1158]	[1039:1039]	[1271:1271]	[1051:1051]	[1420:1420]	[1243:1243]	[1199:1199]	[1414:1414]	[1310:1310]	[1589:1589]	[5000:5000]
Effect. observations	[9829:5272]	[5351:3046]	[4237:2113]	[8084:4449]	[1041:564]	[8209:4187]	[579:333]	[2083:1071]	[8790:4337]	[1571:733]	[6925:1681]
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Enrolled same course	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
beginning year 2	0.101***	0.039	0.201***	0.084***	0.079	0.127***	0.245	-0.011	0.098***	0.174	0.039
	(0.029)	(0.029)	(0.055)	(0.026)	(0.090)	(0.035)	(0.168)	(0.077)	(0.032)	(0.113)	(0.042)
Observations	[65371:12020]	[42085:7385]	[23286:4635]	[60171:10853]	[5200:1167]	[51334:8886]	[3659:712]	[9686:2287]	[52839:8893]	[5606:1446]	[6926:1681]
Bandwidth	[1242:1242]	[1649:1649]	[1449:1449]	[1603:1603]	[1590:1590]	[1088:1088]	[460:460]	[1395:1395]	[1240:1240]	[1467:1467]	[5000:5000]
Effect. observations	[10716:5563]	[9413:4248]	[4937:2325]	[13469:6105]	[1192:608]	[6987:3801]	[219:154]	[2041:1053]	[8215:4172]	[1433:690]	[6925:1681]
	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)
Enrolled other course	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
beginning year 2	-0.077***	-0.053*	-0.112***	-0.077***	-0.091	-0.086***	-0.189	-0.032	-0.079**	-0.085	-0.008
	(0.029)	(0.029)	(0.042)	(0.029)	(0.074)	(0.032)	(0.138)	(0.051)	(0.031)	(0.055)	(0.031)
Observations	[65371:12020]	[42085:7385]	[23286:4635]	[60171:10853]	[5200:1167]	[51334:8886]	[3659:712]	[9686:2287]	[52839:8893]	[5606:1446]	[6926:1681]
Bandwidth	[919:919]	[1135:1135]	[1649:1649]	[1021:1021]	[1337:1337]	[933:933]	[505:505]	[1732:1732]	[1116:1116]	[1892:1892]	[5000:5000]
Effect. observations	[7526:4417]	[5940:3250]	[5792:2576]	[7829:4343]	[968:537]	[5858:3359]	[240:171]	[2693:1274]	[7227:3866]	[1893:825]	[6925:1681]
	(34)	(35)	(36)	(37)	(38)	(39)	(40)	(41)	(42)	(43)	(44)
Dropout	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
end year 2	-0.028	-0.032	-0.061	-0.036	0.016	-0.057*	-0.011	0.014	-0.024	-0.167	-0.000
	(0.023)	(0.029)	(0.047)	(0.025)	(0.098)	(0.029)	(0.116)	(0.070)	(0.023)	(0.200)	(0.021)
Observations	[50648:9428]	[32504:5722]	[18144:3706]	[46879:8562]	[3769:866]	[39785:6968]	[2822:539]	[7480:1810]	[41453:7130]	[3656:956]	[5539:1342]
Bandwidth	[1745:1745]	[1437:1437]	[1667:1667]	[1644:1644]	[1358:1358]	[1244:1244]	[760:760]	[1784:1784]	[1453:1453]	[876:876]	[5000:5000]
Effect. observations	[12959:5550]	[6184:3028]	[4670:2046]	[11057:4871]	[739:414]	[6479:3274]	[284:169]	[2245:1037]	[8078:3741]	[531:308]	[5538:1342]
	(45)	(46)	(47)	(48)	(49)	(50)	(51)	(52)	(53)	(54)	(55)
Graduated	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
	0.059	0.064	0.056	0.067	0.100	0.072	-0.224	0.033	0.072	0.126	-0.072
	(0.039)	(0.052)	(0.049)	(0.045)	(0.166)	(0.047)	(0.263)	(0.105)	(0.049)	(0.120)	(0.124)
Observations	[36857:7007]	[24151:4384]	[12706:2623]	[34300:6382]	[2557:625]	[29000:5187]	[1971:349]	[5417:1374]	[30287:5327]	[5155:1287]	[1415:393]
Bandwidth	[1814:1814]	[1337:1337]	[5000:5000]	[1499:1499]	[1317:1317]	[1526:1526]	[575:575]	[1776:1776]	[1364:1364]	[1956:1956]	[4996:4996]
Effect. observations	[10365:4178]	[4451:2192]	[12704:2623]	[7541:3355]	[516:285]	[6361:2800]	[156:90]	[1726:768]	[5730:2645]	[1790:763]	[1414:393]
	(56)	(57)	(58)	(59)	(60)	(61)	(62)	(63)	(64)	(65)	(66)
Graduated on time	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
	0.074*	0.097*	0.070	0.045	0.282*	0.069	-0.090	0.206*	0.101**	0.092	-0.072
	(0.041)	(0.057)	(0.047)	(0.040)	(0.167)	(0.049)	(0.254)	(0.117)	(0.050)	(0.108)	(0.124)
Observations	[36857:7007]	[24151:4384]	[12706:2623]	[34300:6382]	[2557:625]	[29000:5187]	[1971:349]	[5417:1374]	[30287:5327]	[5155:1287]	[1415:393]
Bandwidth	[1616:1616]	[1165:1165]	[5000:5000]	[1883:1883]	[1265:1265]	[1427:1427]	[534:534]	[1213:1213]	[1337:1337]	[1994:1994]	[4996:4996]
Effect. observations	[8934:3881]	[3779:1975]	[12704:2623]	[10091:3923]	[492:279]	[5837:2661]	[145:87]	[1109:567]	[5592:2604]	[1835:769]	[1414:393]

Note: The table reports RDD estimates of receiving the grant for two years in the different sub-samples. The model is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Heterogeneity analysis, first year students, receiving the grant in first and second year-cont

	(67)	(68)	(69)	(70)	(71)	(72)	(73)	(74)	(75)	(76)	(77)
Final mark	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
	0.336*	0.244	0.322	0.346*	0.283	0.388**	0.868	-0.150	0.181	-0.418	2.907***
	(0.186)	(0.198)	(0.283)	(0.190)	(0.576)	(0.194)	(0.864)	(0.421)	(0.135)	(0.436)	(1.101)
Observations	[19746:3328]	[13991:2285]	[5755:1043]	[18449:3030]	[1297:298]	[15795:2480]	[958:155]	[2781:651]	[16834:2682]	[2384:516]	[528:130]
Bandwidth	[1006:1006]	[1158:1158]	[1520:1520]	[1046:1046]	[1166:1166]	[1330:1330]	[444:444]	[1390:1390]	[1789:1789]	[1507:1507]	[938:938]
Effect. observations	[2750:1322]	[2193:1055]	[1385:518]	[2654:1247]	[227:126]	[2921:1237]	[57:35]	[660:300]	[4584:1625]	[592:257]	[83:54]
	(78)	(79)	(80)	(81)	(82)	(83)	(84)	(85)	(86)	(87)	(88)
Apply next year	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Master	Integrated m.
	0.711***	0.669***	0.802***	0.696***	0.850***	0.702***	0.703***	0.748***	0.691***	0.832***	0.663***
	(0.032)	(0.038)	(0.067)	(0.035)	(0.110)	(0.037)	(0.136)	(0.072)	(0.033)	(0.091)	(0.049)
Observations	[65613:12136]	[42238:7456]	[23375:4680]	[60358:10953]	[5255:1183]	[51514:8965]	[3670:716]	[9733:2319]	[53026:8982]	[5650:1469]	[6937:1685]
Bandwidth	[1398:1398]	[1352:1352]	[1232:1232]	[1263:1263]	[1386:1386]	[1369:1369]	[1095:1095]	[1816:1816]	[1627:1627]	[1926:1926]	[5000:5000]
Effect. observations	[12386:6128]	[7335:3729]	[4108:2087]	[10096:5154]	[1027:565]	[9261:4526]	[530:313]	[2873:1336]	[11657:5147]	[1956:847]	[6936:1685]

Note: The table reports RDD estimates of receiving the grant for two years in the different sub-samples. The model is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8: Heterogeneity analysis, first year students, receiving the grant in first, second, and third year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Immediate dropout	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Integrated m.
beginning year 3	-0.038	-0.017	-0.028	-0.024	0.031	-0.035	0.034	-0.012	-0.028	-0.001
	(0.029)	(0.033)	(0.033)	(0.021)	(0.109)	(0.027)	(0.155)	(0.083)	(0.028)	(0.027)
Observations	[46992:8472]	[30040:5091]	[16952:3381]	[43428:7683]	[3564:789]	[36877:6295]	[2686:500]	[6936:1591]	[41453:7130]	[5539:1342]
Bandwidth	[1250:1250]	[1120:1120]	[5000:5000]	[1470:1470]	[1223:1223]	[1748:1748]	[827:827]	[1521:1521]	[1688:1688]	[5000:5000]
Effect. observations	[7744:3959]	[4092:2262]	[16951:3381]	[8720:4065]	[610:356]	[9061:3772]	[277:175]	[1662:808]	[9829:4156]	[5538:1342]
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Dropout	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Integrated m.
end year 3	-0.020	-0.037	-0.165	-0.070	0.085	-0.016	-0.189	-0.123	-0.014	0.030
	(0.036)	(0.037)	(0.135)	(0.097)	(0.199)	(0.038)	(0.238)	(0.257)	(0.091)	(0.037)
Observations	[22847:4433]	[13817:2518]	[9030:1915]	[21181:4055]	[1666:378]	[17965:3364]	[1272:236]	[3327:780]	[18778:3432]	[4069:1001]
BW Type	[5000:5000]	[5000:5000]	[1458:1458]	[254:254]	[1635:1635]	[5000:5000]	[1857:1857]	[1252:1252]	[1195:1195]	[4997:4997]
Effect. observations	[22846:4433]	[13816:2518]	[2055:950]	[603:482]	[405:201]	[17964:3364]	[368:151]	[670:331]	[2984:1522]	[4068:1001]
	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)
Graduated	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Integrated m.
	0.074	0.038	0.170	0.052	0.102	0.080	-0.206	0.162	0.096	-0.104
	(0.066)	(0.078)	(0.123)	(0.043)	(0.198)	(0.061)	(0.345)	(0.152)	(0.065)	(0.168)
Observations	[31702:5720]	[20652:3521]	[11050:2199]	[29390:5186]	[2312:534]	[24872:4255]	[1763:304]	[4687:1093]	[30287:5327]	[1415:393]
BW Type	[1242:1242]	[1132:1132]	[1181:1181]	[1795:1795]	[1519:1519]	[1810:1810]	[560:560]	[1310:1310]	[1355:1355]	[4996:4996]
Effect. observations	[5406:2641]	[2964:1589]	[1934:903]	[7945:3123]	[538:272]	[6645:2597]	[133:79]	[993:483]	[5685:2629]	[1414:393]
	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)
Graduated	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Integrated m.
in time	0.115*	0.089	0.247*	0.059	0.465*	0.070	-0.033	0.241*	0.127**	-0.104
	(0.065)	(0.081)	(0.128)	(0.049)	(0.248)	(0.064)	(0.362)	(0.141)	(0.064)	(0.168)
Observations	[31702:5720]	[20652:3521]	[11050:2199]	[29390:5186]	[2312:534]	[24872:4255]	[1763:304]	[4687:1093]	[30287:5327]	[1415:393]
BW Type	[1282:1282]	[1150:1150]	[961:961]	[1428:1428]	[1158:1158]	[1774:1774]	[507:507]	[1386:1386]	[1421:1421]	[4996:4996]
Effect. observations	[5625:2705]	[3015:1610]	[1548:759]	[5889:2671]	[389:224]	[6450:2568]	[122:74]	[1059:499]	[6012:2730]	[1414:393]
	(41)	(42)	(43)	(44)	(45)	(46)	(47)	(48)	(49)	(50)
Final mark	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Bachelor	Integrated m.
	0.268*	0.446*	0.477	0.381**	0.875	0.272	1.527	0.453	0.290	3.504**
	(0.162)	(0.256)	(0.315)	(0.185)	(0.672)	(0.206)	(1.117)	(0.374)	(0.181)	(1.475)
Observations	[17361:2810]	[12330:1922]	[5031:888]	[16177:2537]	[1184:274]	[13883:2086]	[844:141]	[2457:549]	[16833:2681]	[528:129]
BW Type	[1782:1782]	[1098:1098]	[1637:1637]	[1098:1098]	[1147:1147]	[1624:1624]	[477:477]	[1417:1417]	[1523:1523]	[941:941]
Effect. observations	[4725:1705]	[1758:868]	[1311:469]	[2389:1098]	[201:114]	[3245:1220]	[53:34]	[593:259]	[3721:1463]	[83:54]

Note: The table reports RDD estimates of receiving the grant for three years in the different sub-samples. The model is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Heterogeneity, second year students sample, RDD estimated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Immediate dropout	Main	Female	Male	Private	Public	Less dev	Transition	Dev
beginning year 2	0.008	0.011	0.005	0.028	0.005	0.004	0.026	0.017
	(0.007)	(0.009)	(0.011)	(0.022)	(0.007)	(0.008)	(0.025)	(0.018)
Observations	[18817:4179]	[12668:2724]	[6149:1455]	[2402:749]	[16415:3430]	[14029:2910]	[1185:196]	[3312:1031]
Bandwidth	[1441:1441]	[1230:1230]	[1866:1866]	[1154:1154]	[1728:1728]	[1251:1251]	[1838:1838]	[1327:1327]
Effect. observations	[3993:2047]	[2220:1208]	[1845:830]	[438:303]	[4301:1923]	[2441:1327]	[314:118]	[760:428]
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Enrolled in same course	Main	Female	Male	Private	Public	Less dev	Transition	Dev
beginning year 2	-0.007	-0.011	0.013	-0.022	-0.008	-0.004	0.002	-0.015
	(0.012)	(0.015)	(0.023)	(0.054)	(0.013)	(0.015)	(0.112)	(0.031)
Observations	[18817:4179]	[12668:2724]	[6149:1455]	[2402:749]	[16415:3430]	[14029:2910]	[1185:196]	[3312:1031]
Bandwidth	[1671:1671]	[1780:1780]	[881:881]	[401:401]	[1773:1773]	[536:536]	[1110:1110]	[1808:1808]
Effect. observations	[4806:2283]	[3483:1581]	[757:483]	[151:124]	[4456:1956]	[917:681]	[176:79]	[1066:548]
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Enrolled in other course	Main	Female	Male	Private	Public	Less dev	Transition	Dev
beginning year 2	-0.003	0.002	-0.011	-0.009	0.002	-0.002	-0.046	-0.001
	(0.010)	(0.013)	(0.016)	(0.034)	(0.011)	(0.009)	(0.109)	(0.028)
Observations	[18817:4179]	[12668:2724]	[6149:1455]	[2402:749]	[16415:3430]	[14029:2910]	[1185:196]	[3312:1031]
Bandwidth	[1691:1691]	[1608:1608]	[1546:1546]	[531:531]	[1742:1742]	[543:543]	[1088:1088]	[1684:1684]
Effect. observations	[4859:2300]	[3032:1471]	[1465:728]	[188:160]	[4353:1934]	[928:688]	[173:77]	[966:517]
	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
Dropout	Main	Female	Male	Private	Public	Less dev	Transition	Dev
end year 2	-0.023	-0.030**	-0.000	-0.019	-0.024	-0.009	-0.055	-0.039
	(0.014)	(0.015)	(0.028)	(0.047)	(0.016)	(0.019)	(0.060)	(0.034)
Observations	[17200:3710]	[11611:2411]	[5589:1299]	[2163:658]	[15037:3052]	[12872:2586]	[1069:171]	[2983:914]
Bandwidth	[1375:1375]	[1753:1753]	[1210:1210]	[905:905]	[1343:1343]	[665:665]	[1867:1867]	[1564:1564]
Effect. observations	[3560:1775]	[3205:1390]	[1009:557]	[314:223]	[2975:1440]	[1087:743]	[296:101]	[816:430]
	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)
At least 36 credits	Main	Female	Male	Private	Public	Less dev	Transition	Dev
	0.054**	0.047	0.072*	0.219***	0.028	0.079***	0.159	-0.005
	(0.027)	(0.031)	(0.038)	(0.078)	(0.027)	(0.023)	(0.136)	(0.054)
Observations	[14812:3255]	[10046:2143]	[4766:1112]	[1627:545]	[13185:2710]	[10894:2272]	[890:142]	[2803:809]
Bandwidth	[1494:1494]	[1364:1364]	[4999:4999]	[1910:1910]	[1568:1568]	[4999:4999]	[1623:1623]	[1258:1258]
Effect. observations	[3320:1626]	[2009:1012]	[4765:1112]	[539:313]	[3115:1415]	[10893:2272]	[204:79]	[605:324]
	(41)	(42)	(43)	(44)	(45)	(46)	(47)	(48)
Obtained enrolled credits	Main	Female	Male	Private	Public	Less dev	Transition	Dev
	0.040	0.048	0.079	0.208	0.028	0.110*	0.191	0.042
	(0.035)	(0.046)	(0.075)	(0.127)	(0.044)	(0.061)	(0.176)	(0.077)
Observations	[15331:3269]	[10276:2144]	[5055:1125]	[1717:551]	[13614:2718]	[11407:2294]	[963:146]	[2727:796]
Bandwidth	[1869:1869]	[1711:1711]	[754:754]	[485:485]	[1361:1361]	[537:537]	[1221:1221]	[1854:1854]
Effect. observations	[4513:1918]	[2655:1201]	[524:333]	[129:117]	[2702:1276]	[752:528]	[166:65]	[902:431]
	(49)	(50)	(51)	(52)	(53)	(54)	(55)	(56)
Graduated on time	Main	Female	Male	Private	Public	Less dev	Transition	Dev
	-0.058	-0.055	-0.051	-0.158	-0.025	-0.079	0.132	-0.041
	(0.041)	(0.049)	(0.076)	(0.146)	(0.036)	(0.065)	(0.165)	(0.087)
Observations	[15675:3269]	[10592:2139]	[5083:1130]	[1946:570]	[13729:2699]	[11758:2273]	[974:149]	[2688:810]
Bandwidth	[1195:1195]	[1217:1217]	[828:828]	[435:435]	[1842:1842]	[541:541]	[1823:1823]	[1486:1486]
Effect. observations	[2821:1442]	[1917:974]	[620:371]	[134:107]	[4140:1601]	[794:567]	[276:90]	[733:368]
	(57)	(58)	(59)	(60)	(61)	(62)	(63)	(64)
Graduated	Main	Female	Male	Private	Public	Less dev	Transition	Dev
	-0.037	-0.018	-0.063	-0.192	-0.011	-0.086*	-0.028	-0.006
	(0.030)	(0.030)	(0.069)	(0.126)	(0.027)	(0.047)	(0.109)	(0.062)
Observations	[15675:3269]	[10592:2139]	[5083:1130]	[1946:570]	[13729:2699]	[11758:2273]	[974:149]	[2688:810]
Bandwidth	[1355:1355]	[1697:1697]	[619:619]	[415:415]	[1824:1824]	[506:506]	[1749:1749]	[1854:1854]
Effect. observations	[3301:1579]	[2882:1224]	[435:283]	[125:101]	[4093:1592]	[747:538]	[264:88]	[956:442]
	(65)	(66)	(67)	(68)	(69)	(70)	(71)	(72)
Final mark	Main	Female	Male	Private	Public	Less dev	Transition	Dev
	0.087	0.118	0.129	-0.227	0.154	0.216	0.431	-0.075
	(0.152)	(0.168)	(0.277)	(0.618)	(0.162)	(0.240)	(0.716)	(0.321)
Observations	[12411:2348]	[8592:1590]	[3819:758]	[1426:380]	[10985:1968]	[9542:1679]	[722:106]	[1958:536]
Bandwidth	[1381:1381]	[1617:1617]	[978:978]	[505:505]	[1471:1471]	[545:545]	[1616:1616]	[1521:1521]
Effect. observations	[2669:1185]	[2195:912]	[556:296]	[104:97]	[2507:1043]	[641:444]	[182:59]	[547:253]
	(73)	(74)	(75)	(76)	(77)	(78)	(79)	(80)
Apply again	Main	Female	Male	Private	Public	Less dev	Transition	Dev
	0.446***	0.462***	0.436***	0.474***	0.428***	0.445***	0.048	0.488***
	(0.032)	(0.039)	(0.061)	(0.100)	(0.033)	(0.051)	(0.169)	(0.059)
Observations	[18859:4196]	[12697:2734]	[6162:1462]	[2428:760]	[16431:3436]	[14058:2924]	[1186:196]	[3323:1034]
Bandwidth	[1452:1452]	[1404:1404]	[808:808]	[565:565]	[1573:1573]	[592:592]	[793:793]	[1756:1756]
Effect. observations	[4045:2069]	[2607:1331]	[699:451]	[198:171]	[3840:1806]	[1014:751]	[126:56]	[1031:534]

Note: The table reports RDD estimates of Eq. (1) on the sample of bachelor students who apply for the grant at the beginning of their second curricular year, for the first time, and who obtained at least 36 credits in the previous academic year.Each column is a Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: Covariate Balance Checks

	Second year, all sample		**Bachelor, second year**		**Master, second year**		**Integrated m. second year**	
	Beta	se	Beta	se	Beta	se	Beta	se
A.M.L	0.014	(0.017)	0.011	(0.020)	0.021	(0.038)	0.047	(0.048)
Alentejo	0.005	(0.012)	0.009	(0.014)	0.008	(0.028)	-0.025	(0.032)
Algarve	0.015**	(0.007)	0.016*	(0.009)	0.028*	(0.015)	-0.006	(0.016)
Azores	-0.004	(0.006)	-0.007	(0.008)	-0.004	(0.014)	0.011	(0.014)
Centro	-0.019	(0.019)	-0.012	(0.023)	-0.018	(0.047)	-0.054	(0.057)
Madeira	-0.008	(0.008)	-0.004	(0.009)	0.001	(0.019)	-0.052**	(0.024)
Norte	-0.003	(0.021)	-0.013	(0.025)	-0.036	(0.052)	0.080	(0.065)
Female	0.000	(0.021)	0.011	(0.024)	-0.024	(0.048)	-0.039	(0.064)
Education	-0.006	(0.011)	-0.007	(0.011)	-0.031	(0.040)	0.000	(0.000)
Arts and Humanities	-0.018	(0.014)	-0.009	(0.018)	-0.050*	(0.029)	-0.001	(0.014)
Social sciences	0.001	(0.020)	-0.005	(0.024)	0.055	(0.049)	0.011	(0.038)
Science	0.008	(0.012)	0.009	(0.014)	0.007	(0.034)	-0.020	(0.032)
Engeneering	0.006	(0.015)	-0.015	(0.016)	-0.028	(0.036)	0.127**	(0.065)
Agriculture	0.008	(0.005)	0.009	(0.006)	0.008	(0.014)	0.005	(0.022)
Health	-0.006	(0.016)	0.005	(0.020)	0.002	(0.025)	-0.064	(0.058)
Services	0.008	(0.011)	0.013	(0.015)	0.037	(0.025)	-0.058***	(0.019)
Unkown	0.000	(0.001)	0.000	(0.002)	0.000	(0.000)	-0.001	(0.004)
Age	0.378*	(0.204)	0.525**	(0.220)	-0.559	(0.531)	-0.331	(0.291)
Public	0.032**	(0.014)	0.029	(0.018)	0.028	(0.028)	0.021	(0.033)
Bachelor	-0.058***	(0.020)						
Master	0.041**	(0.018)						
Mestrado int.	0.017	(0.013)						
Observations	29,556		20,567		6,082		2,907	

Note: The table reports DiD estimates of Equation 2 using covariates as dependent variable. Each column and row is a different regression.
*** p<0.01, ** p<0.05, * p<0.1.

Table A.11: Heterogeneity analysis, DID estimates, sample of bachelor students, second year

Immediate dropout	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
beginning year 2	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	-0.010**	-0.010*	-0.008	-0.009**	-0.013	-0.013***	-0.018	0.005	-0.001	-0.012**
	(0.004)	(0.006)	(0.006)	(0.004)	(0.019)	(0.005)	(0.015)	(0.010)	(0.007)	(0.005)
Observations	20,297	13,079	7,218	17,561	2,736	15,177	1,309	3,811	6,082	14,215
Enrolled same course	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
beginning year 2	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	0.020**	0.017	0.020	0.027***	-0.044	0.016*	0.093*	0.014	0.003	0.026**
	(0.009)	(0.013)	(0.013)	(0.009)	(0.034)	(0.009)	(0.054)	(0.024)	(0.015)	(0.011)
Observations	20,297	13,079	7,218	17,561	2,736	15,177	1,309	3,811	6,082	14,215
Enrolled other course	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)
beginning year 2	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	non-displaced
Income below*credits above 36	-0.010	-0.005	-0.012	-0.017**	0.056**	-0.003	-0.066	-0.019	-0.001	-0.013
	(0.008)	(0.011)	(0.011)	(0.008)	(0.028)	(0.007)	(0.052)	(0.022)	(0.013)	(0.010)
Observations	20,297	13,079	7,218	17,561	2,736	15,177	1,309	3,811	6,082	14,215
Dropout end year 2	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)
	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	-0.020**	-0.011	-0.032*	-0.017	-0.053	-0.034***	0.192***	-0.052**	-0.035**	-0.015
	(0.010)	(0.013)	(0.017)	(0.011)	(0.038)	(0.012)	(0.042)	(0.026)	(0.016)	(0.013)
Observations	16,882	10,927	5,955	14,637	2,245	12,632	1,076	3,174	5,055	11,827
At least 36 credits	(41)	(42)	(43)	(44)	(45)	(46)	(47)	(48)	(49)	(50)
	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	0.024	0.035	0.020	0.017	0.059	-0.001	0.262***	0.051	-0.019	0.039*
	(0.018)	(0.023)	(0.031)	(0.019)	(0.056)	(0.021)	(0.083)	(0.041)	(0.035)	(0.021)
Observations	17,185	11,222	5,963	15,059	2,126	12,918	1,066	3,201	5,225	11,960
Obtained enrolled credits	(51)	(52)	(53)	(54)	(55)	(56)	(57)	(58)	(59)	(60)
	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	0.025	0.075*	-0.031	0.033	-0.100	0.028	-0.005	0.007	0.060	0.015
	(0.028)	(0.040)	(0.039)	(0.030)	(0.085)	(0.032)	(0.124)	(0.066)	(0.054)	(0.033)
Observations	16,822	10,915	5,907	14,728	2,094	12,650	1,071	3,101	5,153	11,669
Graduated on time	(61)	(62)	(63)	(64)	(65)	(66)	(67)	(68)	(69)	(70)
	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	0.001	0.003	0.010	-0.004	0.034	0.032	-0.002	-0.096	-0.008	0.001
	(0.028)	(0.040)	(0.041)	(0.030)	(0.088)	(0.033)	(0.122)	(0.063)	(0.055)	(0.033)
Observations	13,691	8,896	4,795	11,923	1,768	10,235	861	2,595	4,123	9,568
Graduated	(71)	(72)	(73)	(74)	(75)	(76)	(77)	(78)	(79)	(80)
	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	0.023	-0.000	0.059	0.017	0.074	0.058**	-0.082	-0.070	-0.014	0.035
	(0.024)	(0.033)	(0.038)	(0.025)	(0.079)	(0.028)	(0.102)	(0.056)	(0.044)	(0.029)
Observations	13,691	8,896	4,795	11,923	1,768	10,235	861	2,595	4,123	9,568
Final Mark	(81)	(82)	(83)	(84)	(85)	(86)	(87)	(88)	(89)	(90)
	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	0.023	0.115	-0.063	0.043	-0.325	0.069	-0.320	-0.141	0.125	-0.027
	(0.135)	(0.192)	(0.196)	(0.143)	(0.425)	(0.153)	(0.659)	(0.333)	(0.266)	(0.157)
Observations	9,681	6,568	3,113	8,519	1,162	7,390	558	1,733	3,076	6,605
Apply next year	(91)	(92)	(93)	(94)	(95)	(96)	(97)	(98)	(99)	(100)
	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non-displaced
Income below*credits above 36	0.322***	0.330***	0.327***	0.301***	0.462***	0.370***	0.182**	0.219***	0.292***	0.329***
	(0.019)	(0.027)	(0.029)	(0.021)	(0.059)	(0.023)	(0.080)	(0.044)	(0.037)	(0.023)
Observations	20,336	13,101	7,235	17,575	2,761	15,208	1,309	3,819	6,085	14,251

Note: The table reports DiD estimates of Equation 2 in the different sub-samples. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: Heterogeneity analysis, student receiving the grant in second year,accounting for having received the grant also the first year

Immediate dropout	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
beginning year 2	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	-0.013***	-0.013*	-0.012**	-0.012***	-0.014	-0.015***	-0.021	-0.002	-0.004	-0.016***
	(0.004)	(0.007)	(0.006)	(0.004)	(0.020)	(0.005)	(0.016)	(0.010)	(0.007)	(0.005)
T2 only	-0.007	-0.008	-0.005	-0.005	-0.014	-0.011**	-0.017	0.010	0.001	-0.009*
	(0.004)	(0.006)	(0.006)	(0.004)	(0.020)	(0.005)	(0.016)	(0.010)	(0.008)	(0.005)
T1 only	0.004*	0.003	0.006	0.003	0.011	0.003	0.008	0.008	0.003	0.005
	(0.002)	(0.003)	(0.003)	(0.002)	(0.010)	(0.003)	(0.008)	(0.005)	(0.004)	(0.003)
Observations	20,297	13,079	7,218	17,561	2,736	15,177	1,309	3,811	6,082	14,215
Enrolled same course	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
beginning year 2	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	0.017*	0.010	0.020	0.025***	-0.053	0.016*	0.070	-0.001	0.008	0.019
	(0.009)	(0.013)	(0.013)	(0.009)	(0.035)	(0.009)	(0.057)	(0.026)	(0.015)	(0.011)
T2 only	0.021**	0.020	0.018	0.026***	-0.032	0.015	0.112**	0.015	-0.005	0.030***
	(0.009)	(0.013)	(0.013)	(0.009)	(0.035)	(0.009)	(0.057)	(0.025)	(0.015)	(0.011)
T1 only	0.010**	0.015**	0.004	0.011**	0.002	0.001	0.037	0.041***	-0.005	0.017***
	(0.005)	(0.006)	(0.008)	(0.005)	(0.017)	(0.005)	(0.029)	(0.014)	(0.007)	(0.006)
Observations	20,297	13,079	7,218	17,561	2,736	15,177	1,309	3,811	6,082	14,215
Enrolled other curse	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)
beginning year 2	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	-0.004	0.004	-0.009	-0.012	0.067**	-0.001	-0.040	0.003	-0.004	-0.002
	(0.008)	(0.012)	(0.012)	(0.008)	(0.029)	(0.008)	(0.055)	(0.024)	(0.013)	(0.010)
T2 only	-0.014	-0.010	-0.014	-0.019**	0.044	-0.004	-0.087	-0.025	0.004	-0.020**
	(0.008)	(0.012)	(0.012)	(0.008)	(0.029)	(0.008)	(0.055)	(0.023)	(0.013)	(0.010)
T1 only	-0.014***	-0.017***	-0.009	-0.013***	-0.012	-0.003	-0.042	-0.048***	0.002	-0.021***
	(0.004)	(0.005)	(0.007)	(0.004)	(0.014)	(0.004)	(0.028)	(0.013)	(0.006)	(0.005)
Observations	20,297	13,079	7,218	17,561	2,736	15,177	1,309	3,811	6,082	14,215
	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)
Drop out end year 2	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	-0.015	-0.004	-0.028	-0.012	-0.044	-0.030**	0.189***	-0.036	-0.029*	-0.010
	(0.011)	(0.014)	(0.018)	(0.011)	(0.041)	(0.012)	(0.045)	(0.028)	(0.017)	(0.014)
T2 only	-0.023**	-0.014	-0.033*	-0.021*	-0.052	-0.035***	0.192***	-0.060**	-0.041**	-0.016
	(0.011)	(0.014)	(0.018)	(0.011)	(0.040)	(0.012)	(0.044)	(0.027)	(0.017)	(0.013)
T1 only	-0.012**	-0.012*	-0.012	-0.009	-0.035*	-0.012*	0.008	-0.018	-0.008	-0.013*
	(0.006)	(0.007)	(0.011)	(0.006)	(0.020)	(0.007)	(0.024)	(0.016)	(0.009)	(0.008)
Observations	16,882	10,927	5,955	14,637	2,245	12,632	1,076	3,174	5,055	11,827
	(41)	(42)	(43)	(44)	(45)	(46)	(47)	(48)	(49)	(50)
At least 36 credits	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	0.006	0.017	0.000	0.003	0.024	-0.022	0.249***	0.050	-0.055	0.031
	(0.019)	(0.024)	(0.032)	(0.020)	(0.057)	(0.022)	(0.086)	(0.043)	(0.036)	(0.022)
T2 only	0.040**	0.050**	0.039	0.029	0.100*	0.021	0.264***	0.053	0.025	0.045**
	(0.019)	(0.024)	(0.033)	(0.020)	(0.058)	(0.022)	(0.085)	(0.042)	(0.036)	(0.022)
T1 only	0.032***	0.030***	0.038**	0.029***	0.045*	0.039***	0.031	-0.002	0.059***	0.018
	(0.009)	(0.011)	(0.018)	(0.010)	(0.026)	(0.011)	(0.039)	(0.022)	(0.017)	(0.011)
Observations	17,185	11,222	5,963	15,059	2,126	12,918	1,066	3,201	5,225	11,960
	(51)	(52)	(53)	(54)	(55)	(56)	(57)	(58)	(59)	(60)
Reached the enrolled credits	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	0.049*	0.095**	-0.002	0.062**	-0.098	0.051	-0.001	0.045	0.081	0.041
	(0.029)	(0.042)	(0.041)	(0.031)	(0.088)	(0.034)	(0.130)	(0.069)	(0.056)	(0.035)
T2 only	0.002	0.058	-0.060	0.005	-0.097	0.006	-0.029	-0.023	0.039	-0.010
	(0.029)	(0.042)	(0.042)	(0.031)	(0.088)	(0.034)	(0.128)	(0.068)	(0.056)	(0.034)
T1 only	-0.038**	-0.031	-0.043*	-0.041**	-0.022	-0.044**	0.027	-0.048	-0.040	-0.041**
	(0.015)	(0.019)	(0.024)	(0.016)	(0.042)	(0.017)	(0.059)	(0.035)	(0.027)	(0.018)
Observations	16,822	10,915	5,907	14,728	2,094	12,650	1,071	3,101	5,153	11,669
	(61)	(62)	(63)	(64)	(65)	(66)	(67)	(68)	(69)	(70)
Graduated on time	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	0.026	0.019	0.049	0.026	0.010	0.058	0.008	-0.086	0.006	0.029
	(0.031)	(0.042)	(0.046)	(0.032)	(0.096)	(0.035)	(0.134)	(0.070)	(0.059)	(0.036)
T2 only	-0.014	-0.008	-0.012	-0.023	0.045	0.012	-0.030	-0.091	-0.018	-0.015
	(0.029)	(0.041)	(0.043)	(0.031)	(0.091)	(0.034)	(0.125)	(0.064)	(0.057)	(0.034)
T1 only	-0.032*	-0.016	-0.055*	-0.039**	0.033	-0.027	0.029	-0.059	-0.011	-0.039*
	(0.018)	(0.022)	(0.028)	(0.019)	(0.052)	(0.020)	(0.074)	(0.041)	(0.032)	(0.021)
Observations	13,691	8,896	4,795	11,923	1,768	10,235	861	2,595	4,123	9,568

Note: The table reports DiD estimates of Equation 3 in the different sub-samples. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

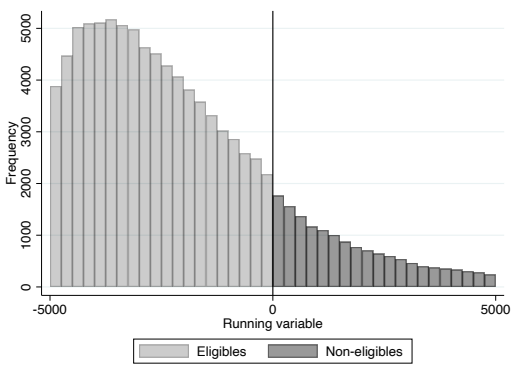
Table A.13: Heterogeneity analysis, student receiving the grant in second year,accounting for having received the grant also the first year-cont

	(71)	(72)	(73)	(74)	(75)	(76)	(77)	(78)	(79)	(80)
Graduated	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	0.048*	0.017	0.096**	0.048*	0.054	0.086***	-0.106	-0.053	0.021	0.054*
	(0.026)	(0.035)	(0.042)	(0.027)	(0.086)	(0.030)	(0.112)	(0.063)	(0.048)	(0.031)
T2 only	0.007	-0.011	0.036	-0.003	0.082	0.038	-0.091	-0.072	-0.034	0.023
	(0.025)	(0.033)	(0.039)	(0.026)	(0.081)	(0.029)	(0.105)	(0.058)	(0.046)	(0.030)
T1 only	-0.033**	-0.024	-0.044*	-0.042***	0.036	-0.034**	0.059	-0.043	-0.051*	-0.022
	(0.015)	(0.018)	(0.026)	(0.016)	(0.046)	(0.017)	(0.062)	(0.037)	(0.026)	(0.018)
Observations	13,691	8,896	4,795	11,923	1,768	10,235	861	2,595	4,123	9,568
	(81)	(82)	(83)	(84)	(85)	(86)	(87)	(88)	(89)	(90)
Final mark	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	0.037	0.078	0.032	0.061	-0.331	0.113	-0.845	-0.192	0.098	-0.000
	(0.143)	(0.201)	(0.213)	(0.152)	(0.447)	(0.162)	(0.746)	(0.351)	(0.278)	(0.168)
T2 only	0.000	0.126	-0.141	0.014	-0.288	0.026	-0.459	-0.114	0.126	-0.051
	(0.139)	(0.195)	(0.203)	(0.147)	(0.433)	(0.157)	(0.665)	(0.341)	(0.273)	(0.161)
T1 only	0.033	0.089	-0.051	0.051	-0.081	-0.006	0.537*	0.092	0.123	-0.004
	(0.071)	(0.086)	(0.126)	(0.076)	(0.198)	(0.081)	(0.312)	(0.166)	(0.129)	(0.085)
Observations	9,681	6,568	3,113	8,519	1,162	7,390	558	1,733	3,076	6,605
	(91)	(92)	(93)	(94)	(95)	(96)	(97)	(98)	(99)	(100)
Apply next year	Main	Female	Male	Public	Private	Less dev	Transition	Dev	Displaced	Non displaced
T1 & T2	0.259***	0.256***	0.275***	0.239***	0.395***	0.310***	0.113	0.144***	0.200***	0.281***
	(0.020)	(0.028)	(0.031)	(0.021)	(0.061)	(0.023)	(0.083)	(0.047)	(0.038)	(0.024)
T2 only	0.356***	0.363***	0.358***	0.332***	0.507***	0.406***	0.213**	0.241***	0.366***	0.347***
	(0.020)	(0.028)	(0.031)	(0.021)	(0.061)	(0.024)	(0.083)	(0.045)	(0.038)	(0.024)
T1 only	0.168***	0.171***	0.169***	0.164***	0.191***	0.170***	0.169***	0.160***	0.226***	0.142***
	(0.011)	(0.013)	(0.018)	(0.011)	(0.029)	(0.012)	(0.042)	(0.025)	(0.019)	(0.013)
Observations	20,336	13,101	7,235	17,575	2,761	15,208	1,309	3,819	6,085	14,251

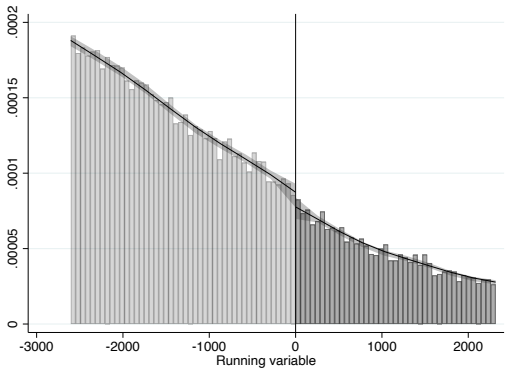
Note: The table reports DiD estimates of Equation 3 in the different sub-samples. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Additional Figures

Figure A.1: No manipulation of the running variable around the threshold



(a) Distribution of the running variable



(b) Manipulation testing plot

Figure A.2: Discontinuity in covariates -1

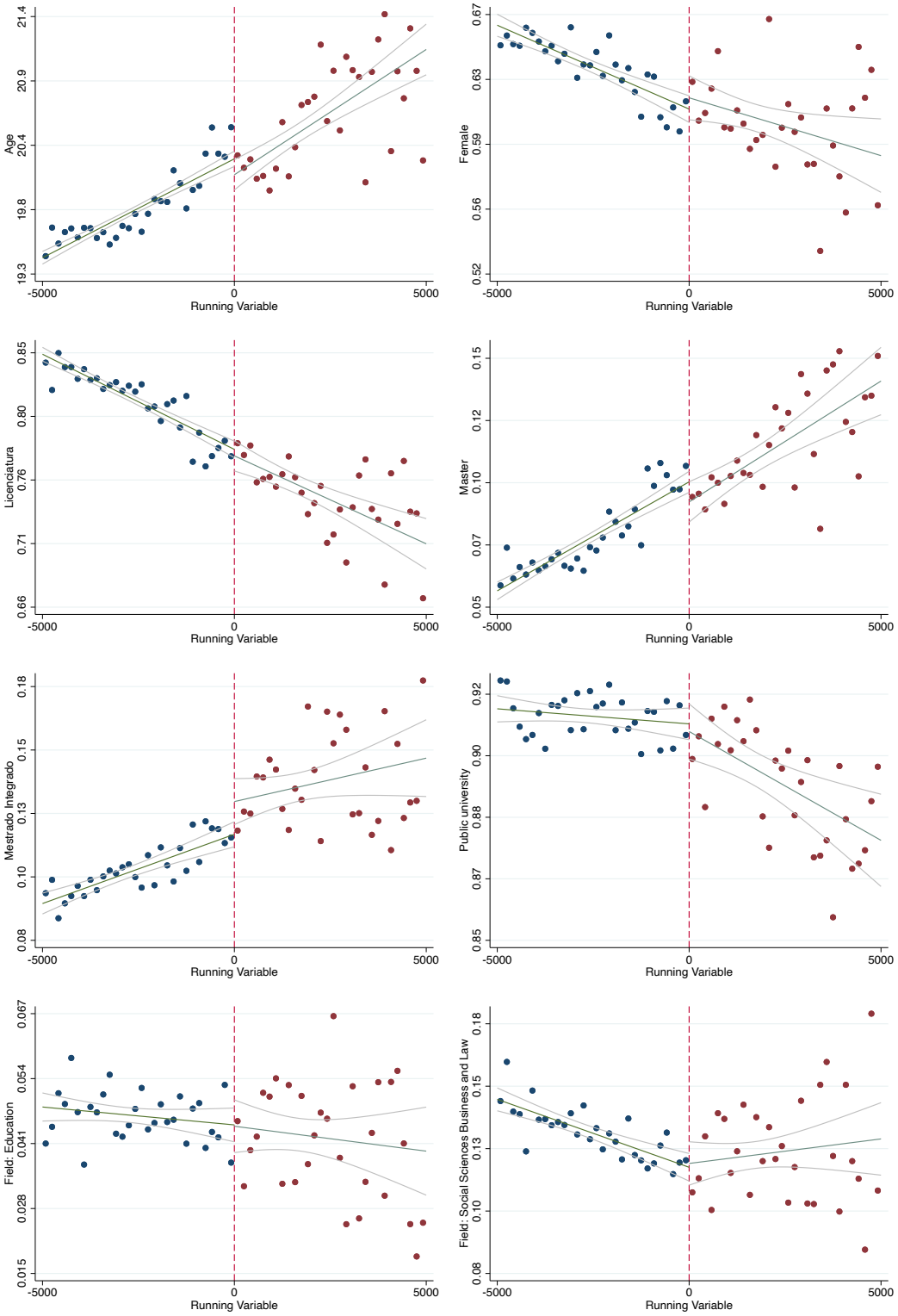


Figure A.3: Discontinuity in covariates -2

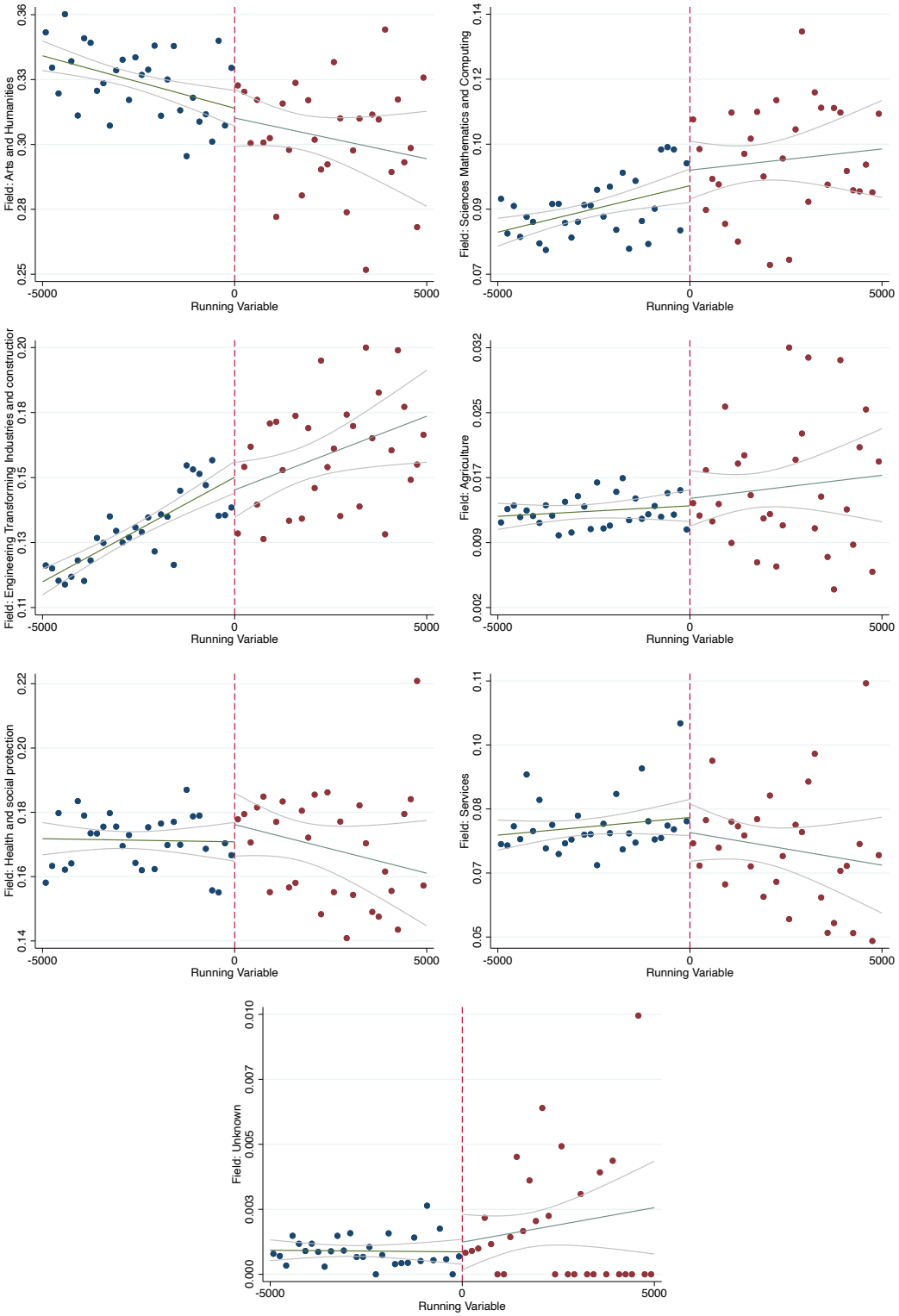
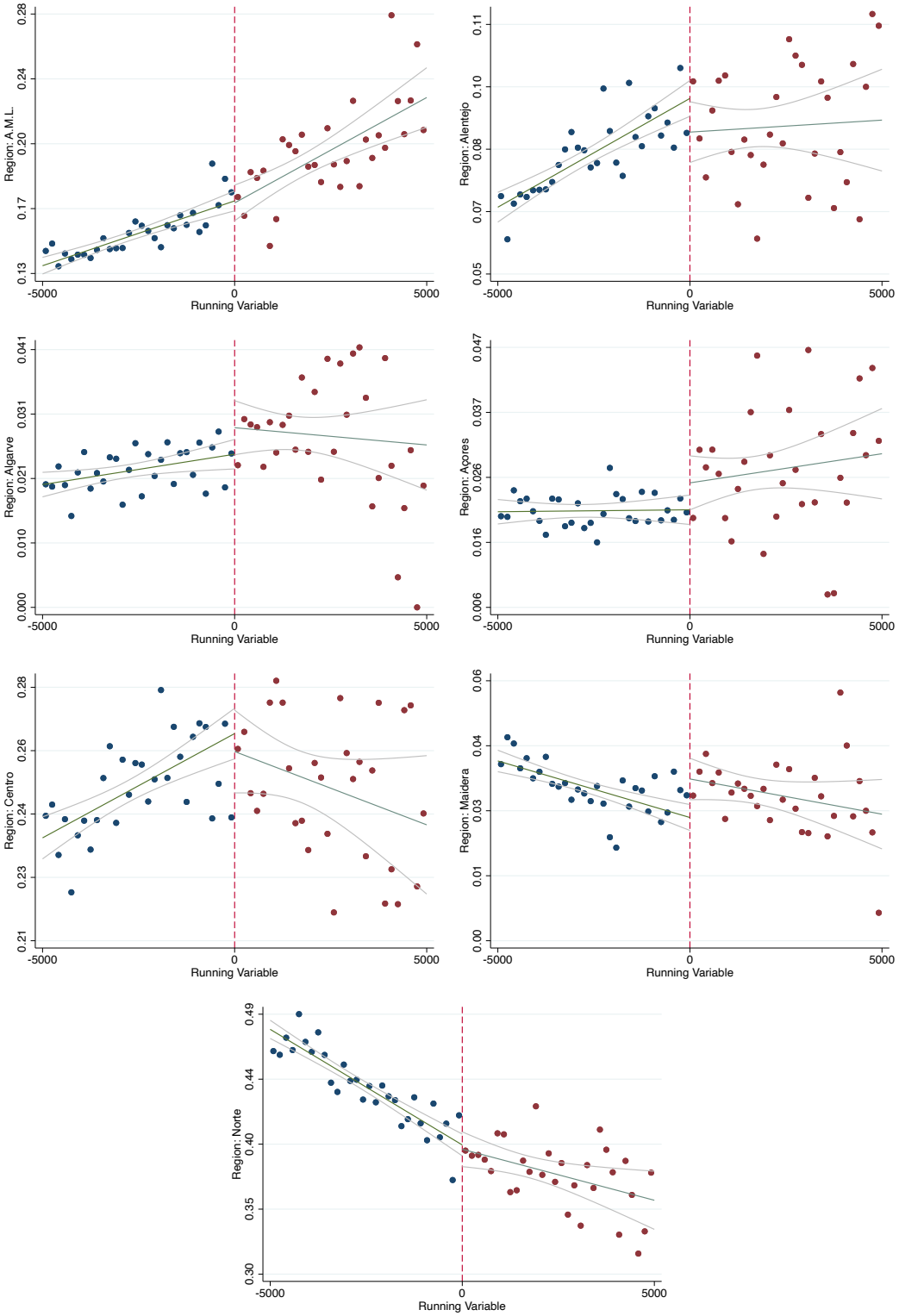


Figure A.4: Discontinuity in covariates -3



Note: Figures A.2, A.3, and A.4 display the distribution of observations around the income threshold by gender, age, type of degree, public university, field of study, region of living,

Figure A.5: Manually selected bandwidth

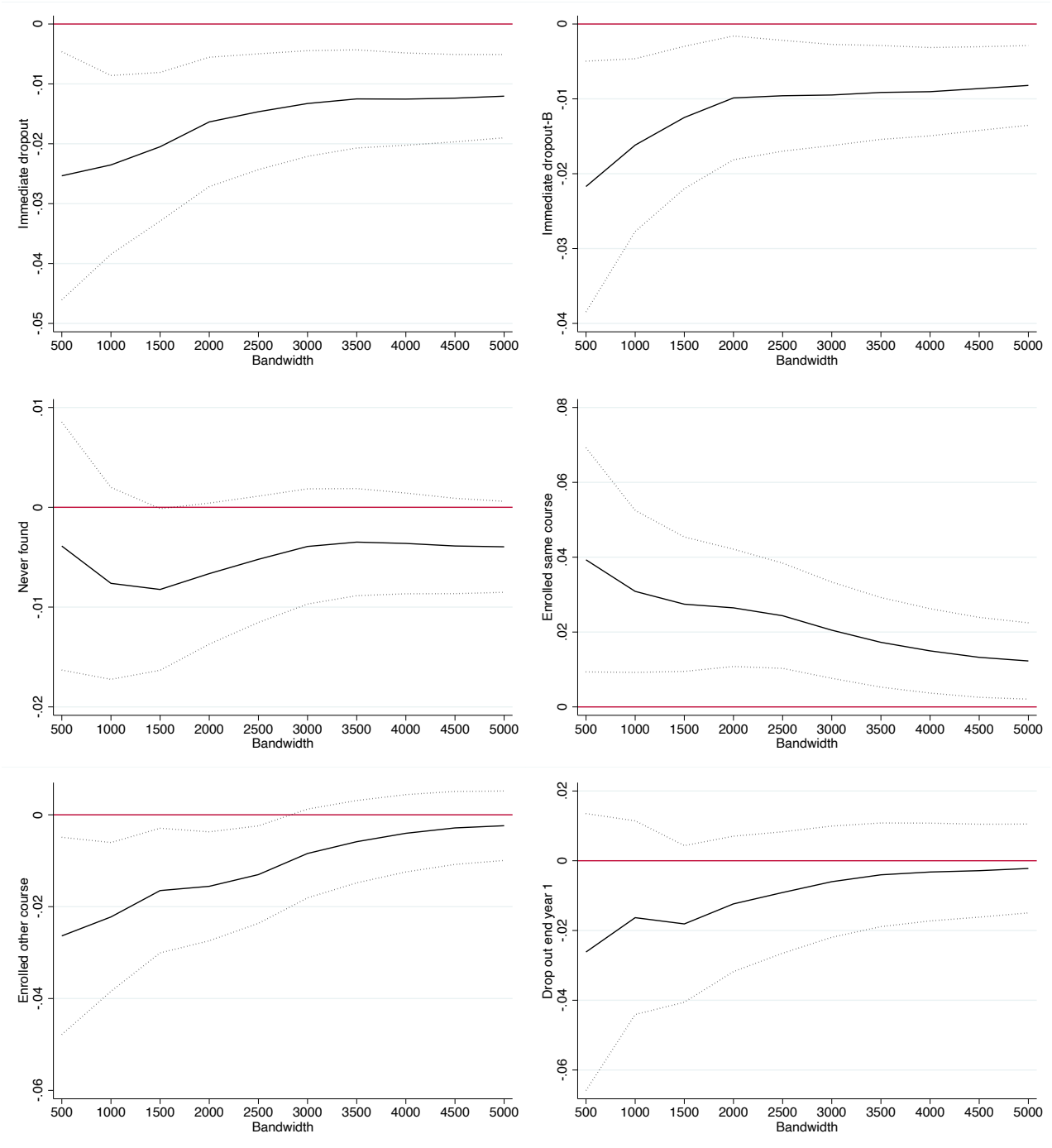
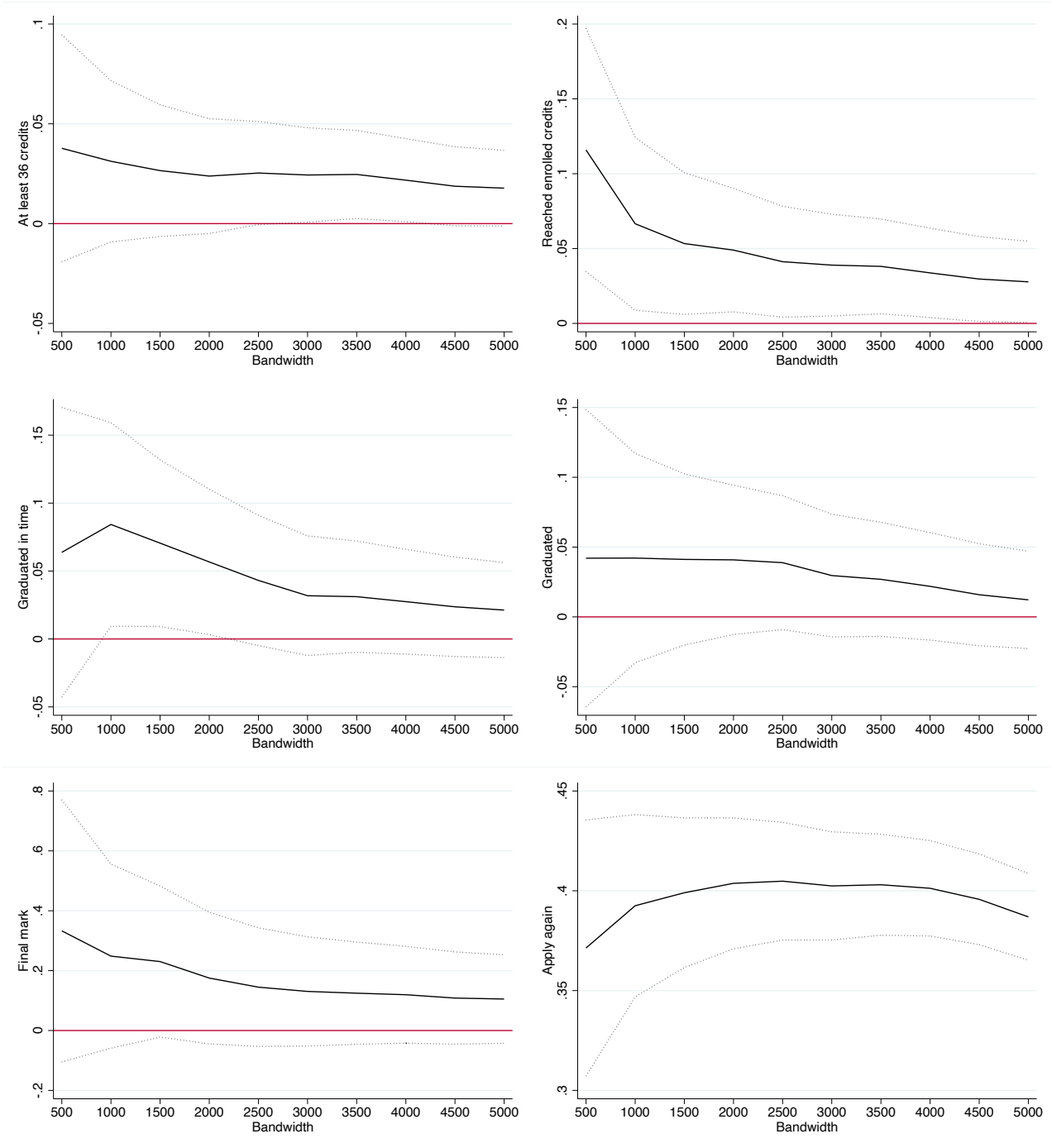


Figure A.6: Manually selected bandwidth-cont



Annexes

1 Higher Education Grant system: Maximum per capita income reference values

Below are listed the maximum per capita income values used to assess eligibility to the grant.

Table A.14: Maximum reference values of the household per capita income

Year	Reference value
2011-2012	6 868.79 €([1])
2012-2013	6 906.28 €([1])
2013-2014	6 934.80 €([1])
2014-2015	6 936.93 €([1])
2015-2016	7 770.99 €([2])
2016-2017	7 770.99 €([2])
2017-2018	7 804.59 €([2])
2018-2019	7 925.87 €([2])

([1]) 14 times the indexing of social benefits in force at the beginning of the school year, plus the amount of the tuition fee set for the 1st cycle of studies of public higher education, according to Despacho 12780-A/2011, September 23.
([2]) 16 times the indexing of social benefits in force at the beginning of the school year, plus the amount of the annual tuition fee set for the 1st cycle of studies of public higher education, as amended by Despacho 7031-B/2015, June 24.

2 Higher Education Grant system: Award conditions

Grants may be awarded to students under the following conditions:

1. Students who are:
   - To be citizens nationals of Member States of the European Union with the right to a permanent residence in Portugal, and their families;
   - To be third country nationals: holders of a permanent residence permit; beneficiaries of long-term resident status; coming from States with cooperation agreements providing for the application of such benefits;
   - To be stateless people;
   - To be political refugees.
2. To students attending Professional Higher Technical Courses, Degree, Integrated Master and Master courses, in Portuguese higher education institutions. Graduates of degree or master courses can also be awarded a grant when, in the period of 24 months after obtaining the degree, undergoing professional training for the exercise of a profession.
3. To students not holding a degree or diploma similar or higher in relation to the one which attends.
4. To be enrolled in a minimum of 30 ECTS credits, with some exceptions (to be completing the course or enrolled in a thesis).
5. To have had academic success in the previous school year (at least 36 credits, if enrolled in more than 36 or the total amount of credits, if enrolled in less).
6. To be able to complete the course within its normal duration plus 1 or 2 years, depending on the normal duration of the course.
7. To have a household per capita income less or equal to 16 times the indexing of social benefits in force at the beginning of the school year, plus the amount of the annual tuition fee set for the 1st cycle of studies of public higher education.
8. To have, as of December 31 of the year prior to the beginning of the school year, movable assets not exceeding 240 times the indexing of social benefits.
9. To present the tax and contributory situation regularized.

3 Higher Education Grant system: Reasons for not awarding a grant

— Submission of the application outside the deadlines

— Process not complete

— Holder of a degree or diploma similar or higher in relation to the one which attends

— Household per capita income higher than 16 times the indexing of social benefits in force at the beginning of the school year, plus the amount of the annual tuition fee set for the 1st cycle of studies of public higher education

— Not matriculated in a higher education institution and not enrolled in a course

— Completion of the course outside the established period

— Providing false information or omission of data

— Household with no income or with not perceptible income sources

— Fraud application

— Movable assets exceeding 240 times the indexing of social benefits

— Nationals of Member States of the European Union without the right to a permanent residence in Portugal

— Third country nationals without a regular permanence in Portugal

— To be enrolled in less than 30 ECTS credits

— To be enrolled simultaneously in several courses

— Professional training not covered

— Withdrawal of the application

— Student without the tax and/or contributory situation regularized

— Lack of academic success in the previous school year

— International student status

— Institution and/or course not covered

— One-person household with an income of less than 6 times the indexing of social benefits

4 Higher Education Grant system: Process and financing

The grant award conditions are common for both public and private higher education, although:

— In public higher education, analysis and decision on the applications fall within the responsibility of higher education institutions;

— In private higher education, analysis and decision on the applications fall within the responsibility of DGES.

Payment is ensured, in all cases, by DGES. Regions of North, Center and Alentejo are co-founded by the EU funds and students from other regions are financed by the State budget.

The grant application for an academic year must be regularly submitted:

— Between 25 June and 30 September;

— Within 20 working days following registration, when registration occurs after September 30;

— Within 20 working days following the initiation of internship in the case of graduates or masters who are undertaking professional internship.

First year students usually enroll into higher education in September. Students can apply to the grant even before enrolling in higher education, but they need to be enrolled in order to get it. By law students should know the result of their grant application within 30 working days. However, since it only starts from the moment the process is complete with academic and financial information, it varies. And as the financial information is only available from September onwards (also when most of the academic information is loaded), in fact, the deadline only starts to run in mid-September, therefore in ending in November or December. Besides, in all cases of document requests or student hearing, the deadline is extended, so it can even exceed December While most of students do not know the results of the application when they start the academic year (September),when we measure the first outcomes - December of the first year- around 80 % of the students know the results of their application.

5 Higher Education Grant system: Amounts

The reference grant equals 11 times the value of the indexing of social benefits in force at the beginning of the school year, plus the amount of the tuition fee actually paid (which can never be higher than the maximum amount fixed annually for the 1st cycle of studies of public higher education). The annual base grant equals the difference between the respective reference grant and the per capita income of the household. The minimum grant guaranteed to all students is equal to the tuition fee they paid (up to 125% of the maximum amount fixed annually for the 1st cycle of studies of public higher education.) This implies that, the further the students' are from the per capita income threshold, the higher would their grant be,and that for students close to the threshold, the grant consist only a tuition fee waiver of around 1000 euro. For those whose per capita income is further away from the threshold, in addition to the tuition fee waiver additional cash is also provided.

$$Grant = (11 * IAS + PE) - C \tag{4}$$

Where, IAS is the index of social benefits(which is equal to 419,22 from 2012 to 2016; 421,32 in 2017)[32]; PE is the fee actually paid by the students (or the maximum fee fixed in public education) and C is the per capita income. Assuming the amount fixed annually for the 1st cycle of studies of public higher education is set at 1000 (as it was in all the academic year considered), we can calculate how far one students needs to be from the per capita income threshold so to get additional cash with respect to the tuition fee amount.

If we take for example academic year 2012, we know that the threshold to receive the grant is set to 6906.28, the maximum fee was 1038, so the reference grant for someone paying the maximum grant was 5648 (419.22 * 11 + 1038) euro. The grant received would equal to the maximum between the 1038 and the difference between 5648 and the per capita income. So all those whose per capita income (enrolled in course charging maximum fee)is above 4610 euro will get 1038 euro, and all those below will get 1038, plus additional cash according to their percapita income. Since the threshold to get the grant is 6906.28, we can conclude that all those whose per capita income is between 4610 and 6906.28 will get more or less the same amount (they will get the reimbursement of the tuition fee). This is plotted in Figure A.7, for the sample of Bachelor students enrolled in public universities, for the 6 academic year included in our analysis. Per capita income is rounded to the decine, and average grant received in that decine is plotted [33]

The following supplements may also be awarded:

1. Accommodation supplement for displaced students;
2. Transport benefits for students displaced from or to the autonomous regions;
3. Supplements for students taking mobility periods;
4. Supplements for students with special educational needs.

The Regulation for the Allocation of Grants to Higher Education Students provides for the definition of a calendar that sets the payment dates for the grants. According to the regulation:

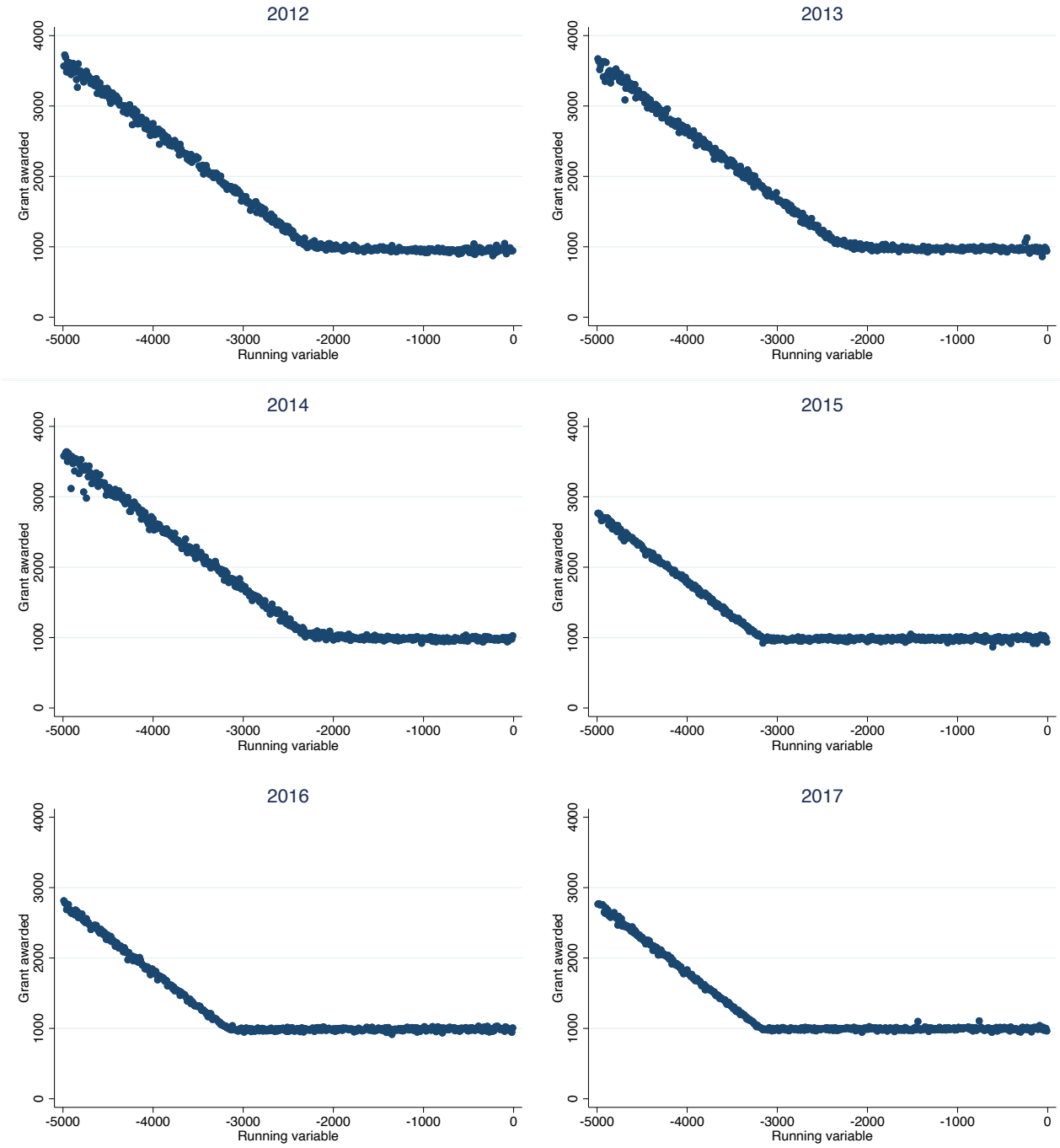
— The grant payment is made, monthly, directly to the student through bank transfer

— When making a monthly payment, compensation can be made in order to adjust the amounts delivered or to be delivered, to the annual value of the grant awarded.

[32] https://www.dgaep.gov.pt/index.cfm?OBJID=3E74CF19-DA87-4B8F-81E2-51E0649AAA9F

[33] The data used to create this graphs were shared with JRC at the beginning of the project, but are not the ones used for the rest of the analysis, as it only contains info from the application files, and no information on the outcomes are available in this set of data.

Figure A.7: Grant awarded according to the running variable



6 Empirical strategy for the measurement of the impact on the full set of students

This annex describes the methods that will be implemented to study the impact of the grant on the full set of students, that is including the impact for students in higher years than the first year of degree followed. Students who apply for the grant in their second year of Master or second or third year of Bachelor have to comply with a second eligibility condition; having obtained a certain number of credits in the previous year. This second eligibility condition is used in addition to the income eligibility criteria to measure the impact of the grant on academic success.

Formally, one aims to analyse the effect of the higher education grant on academic success. Our identification strategy relies on two running variables:

— s_{1i}, "household income", which is defined as the household income used for the assessment eligibility of

student i. Student i qualifies for the grant if her household income is below the income cut-off, c_{1i}. We define the dummy variable indicating whether student i has an household income below the eligibility cut-off as $d_{1i} = I(s_{1i} < c_{1i})$.

— s_{2i}, "credits the previous year", which is defined as the number of credits obtained and used for the assessment eligibility of student i. Student i qualifies for the grant if the number of credits she obtained is above the cutoff, c_{2i}. The corresponding dummy variable can be written as $d_{2i} = I(s_{2i} > c_{2i})$.

Following Choi and Lee (2018) and Cattaneo et al. (2020c), the model can be written as:

$$E(y_i/s_{1i}, s_{2i}) = \beta_0 + \beta_1 d_{1i} + \beta_2 d_{2i} + \beta_3 d_{1i} * d_{2i} \tag{5}$$

where $y_i/s_{1i}, s_{2i}$ is the outcome variable. β_3 is the parameter of interest, that is the impact of meeting both eligibility criteria, also written as $D_i = d_{1i} * d_{2i}$. In our sample, all eligible students are receiving the grant. Thus, D_i corresponds to the treatment variable - holding the higher education grant. β_1 and β_2 measure both partial effects, that is meeting only one of the eligibility criteria on academic success.

7 Information on students' situation

From the database provided by Directorate-General for Statistics on Education and Science (DGEEC), we retrieve the student situation in the academic years from 2012/2013 to 2018/2019. For each of the academic year, each student could be classified in 8 possible categories. In 2015/2016 for instance, the student could be:

1. Graduated in the program (HEI/course) associated with the original grant application in the previous scholar year (2014/2015)
2. Didn't graduate, but is still enrolled in the program associated with the grant application;
3. None of the above, but left the program with an intermediary diploma corresponding to partial completion of the program, obtained in 2014/15 (e.g. a diploma for the course part of a masters program);
4. None of the above, but graduated in another program (HEI/course) with the same ISCED level (or above) as the program associated with the grant application (in the scholar year 2014/15);
5. None of the above, but is still enrolled in another program (HEI/course) with the same ISCED level (or above) as the program associated with the grant application;
6. None of the above, but graduated in another program (HEI/course) with lower ISCED level than the program associated with the grant application (in the scholar year 2014/15);
7. None of the above, but is still enrolled in another program (HEI/course) with lower ISCED level than the program associated with the grant application;
8. Was not found in any HEI database of graduates or enrolled students in this year.

Whenever a student is classified in situation 1 or 4, information about the final grade of the student at graduation, in the scale 10-20- is provided. Whenever the student is classified in situation 2, we known whether that is the first year of enrollment and the number of credits obtained at the end of the first year.

8 Credit variable- first year students

The information on the number of credits obtained at the end of the first year, come from two sources:

— Information retrieve from the application to the grant done by the same student at the end of the first year. This is available only for the students who apply again in the year following year t

— Information form the DGEC data, which indicates the number of credits completed so far by the students, which for students at the end of the first year coincide with the number of credit completed in the first year. This information is available only for students who in December of year t+1 from the application are still enrolled in the same course for which they apply to the grant.

The first year students' sample is composed by 94,964 students, of which 78,601 treated and 16,363 non treated. Of this 94,964 students, 17,215 are first year students in academic year 2017, which means that for those students the information on credits come only from the application file of academic year 2018, as the last outcome available from the DGEC information is from December 2017. Table A.15 report the number of students by academic year, for which the information about credits obtained is available, from either one of the two sources.

Table A.15: Number of students with missing credits information

Academic year	Credit available	Credit Not available	Total
2012	14,992	1,697	16,689
2013	12,59	1,168	13,758
2014	14,468	1,401	15,869
2015	14,438	1,217	15,655
2016	14,626	1,152	15,778
2017	13,056	4,159	17,215
Total	84,170	10,794	94,964

Note: The table summarize the number of students per each academic year for which any information about the credit obtained at the end of the first year is available.

So, for some of the students there is no information on the number of credits obtained at the end of the first year, this can be due to:

— Students actually dropout during the first year, so they cannot have credits.

— Students did not apply again to the grant at beginning of year two and were not enrolled in the same course of application at the end of year 1.

For the 84,170 students for which the credit variable is available, we compared the two sources of information. for 54,401 students we have info from both sources, while from 29,769 only from one of the two. If information is present only in one source, we use the available one. Among the 54,401 students which have info from both sources, for 40,958 (75%) the variables take the exact same values, while for 13,443 the two sources provided different information. For the 13,443 where information is not the same, we checked if both variables were above (below) 36 credits, and if both variables were above (or below) the number of credit enrolled. This is true for 10,138 for what regards the 36 credit threshold and 9,006 for what regards the enrolled credit threshold. This means that we cannot use respectively 3,305 and 4,437 students out of 84,170 when estimating effects of the grant on these two outcomes. This is summarized in Table A.16 by academic year

Table A.16: Number of students with missing credits information

		At least 36 credits		Obtained enrolled credits	
Academic year	(1)	(2a)	(3a)	(2b)	(3b)
2012	14992	1611	13381	1385	13607
2013	1590	487	12103	765	11825
2014	14468	373	14095	656	13812
2015	14438	467	13971	875	13563
2016	14626	367	14259	756	13870
2017	13056	0	13056	0	13056
Total	84170	3305	80865	4437	79733

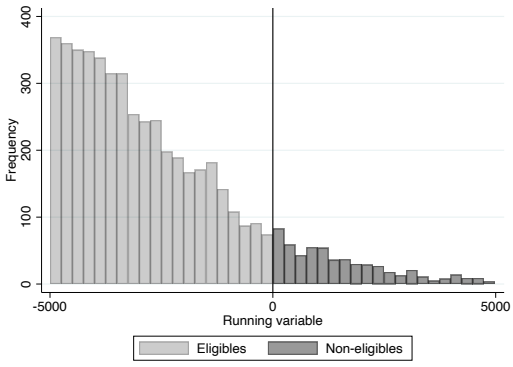
Note: The table summarize the number of students per each academic year for which we will use infomation about the credit variable to build the outcome variables "Student obtained at least 36 credit at the end of the first year" (Columns 2a and 3a) , and "Student obtained all the credits he was enrolled in at the end of the first year"(Coumns 2b and 3b).Columns (1) report the number of students for which at least one source of information regarding the credit is available — also reported in column (1) of Table A.15— columns (2a) and (2b) report the number of students for which the information will not be used, since the two sources provide contrasting information, and columns (3a) and (3b) report the number of students for which the information can be used since the two sources provide the same outcome.

9 Analysis on the first year students enrolled in the Curso técnico superior profissional (TESP)

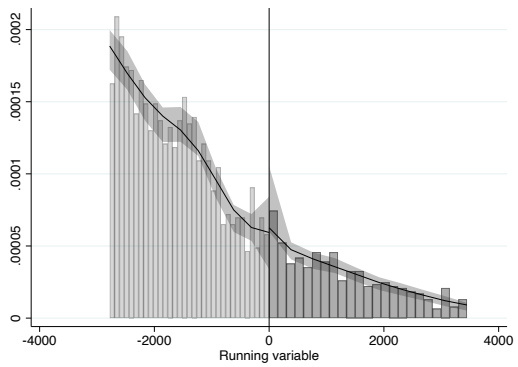
In this Annex, we replicate the analysis presented in Section 4 for the students enrolled in the Curso tecnico superior profissional (TESP). The number of first year, first applicants is 5,111, applying for the grant between 2015 and 2017. For this set of students we will only focus on short term outcomes, as outcomes related to graduation are not observable -yet- for most of the students. Of the 5,111 students 4,496 have income lower than the threshold and are treated, 565 have income above the thresholds and are not treated, and 50 have income below the threshold but do not receive the grant. Given the -small- non compliance we follow the same approach as in the main section and we apply a fuzzy approach.

We first tested for manipulation of the running variable around the threshold, first by plotting the distribution of per capita income around the threshold and second by using the density test. Figure A.8 plot the two graphs. Graph (a) of Figure A.8 displays no jump at the threshold. In addition, the presence of a discontinuity in the density function at the cut-off point is tested and rejected using tests proposed by Cattaneo et al. (2020b). Graph (b) of Figure A.8 reports the estimate of the local polynomial density estimation test (unrestricted model) by Cattaneo et al. (2020b). We can see that there is no significant discontinuity in the distribution of the income i.e. the running variable around the eligibility threshold. The corresponding coefficients are the following: the robust estimate equals -1.371 with p-value 0.170, (optimal bandwidth selection, default settings: $(p) = 2$ and $(q) = 3$).

Figure A.8: No manipulation of the running variable around the threshold - TESP sample



(a) Distribution of the running variable - TESP sample



(b) Manipulation testing plot -TESP sample

Table A.17: Discontinuity in covariates - TEPS sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	**A.M.L**	**Alentejo**	**Algarve**	**Azores**	**Centro**	**Madeira**	**Norte**
Robust	0.027	0.092	0.008	0.040**	-0.151*	-0.028	-0.002
	(0.064)	(0.060)	(0.019)	(0.020)	(0.086)	(0.018)	(0.080)
Observations	[4505:559]	[4505:559]	[4505:559]	[4505:559]	[4505:559]	[4505:559]	[4505:559]
Bandwidth	[1159:1159]	[1115:1115]	[985:985]	[1576:1576]	[1399:1399]	[1047:1047]	[1793:1793]
Effect. observations	[447:269]	[418:261]	[350:233]	[728:342]	[607:307]	[380:246]	[877:368]
	(9)	(10)	(11)	(12)	(13)	(14)	(15)
VARIABLES	**Less dev.**	**In transition**	**Dev.**	**Female**	**Age**	**Arts**	**Social Sciences**
Robust	-0.034	-0.028	0.027	0.108	-1.319	0.051	0.024
	(0.060)	(0.025)	(0.064)	(0.092)	(0.857)	(0.042)	(0.072)
Observations	[4505:559]	[4505:559]	[4505:559]	[4546:565]	[4546:565]	[4546:565]	[4546:565]
Bandwidth	[1479:1479]	[868:868]	[1159:1159]	[1134:1134]	[1259:1259]	[1746:1746]	[1592:1592]
Effect. observations	[666:321]	[294:210]	[447:269]	[432:268]	[509:295]	[850:366]	[750:347]
	(16)	(17)	(18)	(19)	(20)	(21)	
VARIABLES	**Sciences**	**Engineering**	**Agriculture**	**Health**	**Services**	**Public**	
Robust	-0.048	-0.030	0.053	-0.055	0.001	-0.084	
	(0.068)	(0.064)	(0.048)	(0.078)	(0.067)	(0.058)	
Observations	[4546:565]	[4546:565]	[4546:565]	[4546:565]	[4546:565]	[4546:565]	
Bandwidth	[1191:1191]	[1509:1509]	[888:888]	[986:986]	[1417:1417]	[1756:1756]	
Effect. observations	[469:283]	[690:334]	[313:216]	[353:238]	[619:312]	[856:369]	

Second, we checked for no discontinuity of predetermined covariates at the threshold.[34] Results are reported in Table A.17: overall there are no sign of discontinuity of covariates, with the exception of 2 regions: Azores and Centro. However if we group the students, following the division into “Less developed regions”, “In transition

[34]There are no students enrolled in the Education field

regions" and "Developed regions" there are no more differences at the threshold. Evidences provided in Table A.17 and Figure A.8 support the use of regression discontinuity design on this sample of TEPS students. We therefore proceed with the analysis of the effectiveness of receiving the grant among those students.

Table A.18: Main results -TEPS students

	(1)	(2)	(3)	(4)	(5)
	Immediate dropout	**Immediate dropout B**	**Never found**	**Erolled same course**	**Enrolled other course**
First stage	0.958***	0.947 ***	0.961***	0.955***	0.971***
	(0.031)	(0.038)	(0.026)	(0.030)	(0.020)
Robust	-0.022	-0.005	-0.000	0.006	-0.016
	(0.027)	(0.004)	(0.027)	(0.029)	(0.022)
Observations	[4546:565]	[4491:549]	[4546:565]	[4491:549]	[4491:549]
Bandwidth	[980:980]	[702:702]	[1201:1201]	[1017:1017]	[1690:1690]
Effect. obs.	[351:237]	[231:172]	[479:286]	[355:236]	[792:349]

	(6)	(7)	(8)	(9)
	Dropout end year1	**At least 36 credits**	**Obtained enrolled credits**	**Apply again**
First stage	0.944***	0.969***	0.971***	0.973***
	(0.037)	(0.024)	(0.023)	(0.019)
Robust	-0.067	0.017	0.032	0.459***
	(0.076)	(0.050)	(0.107)	(0.067)
Observations	[2884:326]	[3781:334]	[3721:332]	[4546:565]
Bandwidth	[1417:1417]	[1112:1112]	[1244:1244]	[1750:1750]
Effect. obs.	[379:182]	[352:162]	[409:177]	[855:367]

Note: The table reports RDD estimates of Eq. (1). on the sample of TEPS students. Eq. (1) is estimated with the optimal bandwidth, triangular kernel, and local linear polynomial. The coefficients reported are bias-corrected and robust standard errors are in parentheses. Each column is a different regression. *** p<0.01, ** p<0.05, * p<0.1.

Results are reported in Table A.18. There are no significant effects of receiving the grant on any of the outcomes considered. The only effect is found on the probability of applying again for the grant the following year. Results are stable to the inclusion of covariates.

We also tried to perform heterogeneity analysis by gender, public or private university and regions of residence, but no results are found in any of the subgroups.

GETTING IN TOUCH WITH THE EU

In person

All over the European Union there are hundreds of Europe Direct information centres. You can find the address of the centre nearest you at: https://europa.eu/european-union/contact_en

On the phone or by email

Europe Direct is a service that answers your questions about the European Union. You can contact this service:

- by freephone: 00 800 6 7 8 9 10 11 (certain operators may charge for these calls),

- at the following standard number: +32 22999696, or

- by electronic mail via: https://europa.eu/european-union/contact_en

FINDING INFORMATION ABOUT THE EU

Online

Information about the European Union in all the official languages of the EU is available on the Europa website at: https://europa.eu/european-union/index_en

EU publications

You can download or order free and priced EU publications from EU Bookshop at: https://publications.europa.eu/en/publications. Multiple copies of free publications may be obtained by contacting Europe Direct or your local information centre (see https://europa.eu/european-union/contact_en).

KJ-NA-31031-EN-N

The European Commission's science and knowledge service

Joint Research Centre

JRC Mission

As the science and knowledge service of the European Commission, the Joint Research Centre's mission is to support EU policies with independent evidence throughout the whole policy cycle.

EU Science Hub

ec.europa.eu/jrc

@EU_ScienceHub

EU Science Hub - Joint Research Centre

EU Science, Research and Innovation

EU Science Hub

Publications Office of the European Union

doi:10.2760/95533

ISBN 978-92-76-49649-6