

The Effect of Grants on University Dropout Rates: Evidence from the Italian Case

Francesca Modena
Bank of Italy

Enrico Rettore
*University of Padua and Research Institute for the Evaluation
of Public Policies*

Giulia Martina Tanzi
Bank of Italy

In this paper we evaluate the impact of need-based grants on university outcomes, using student-level administrative data from all Italian universities. We compare students receiving the grant to those who were eligible but not awarded the grant. We estimate the average treatment, using blocking on the propensity score with regression adjustment. We show that around one-third of student recipients of grants would have left university at the end of the first year in absence of the aid. Moreover, grants have a relevant impact on the probability of completing college education.

I. Introduction

The aim of this paper is to evaluate the causal effect of need-based grants on college persistence among low-income university students in Italy. Household economic conditions and credit constraints may be reasons for being unable to afford university and for abandoning studies. Obtaining a grant that covers university fees and living costs may reduce the

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dropout probability by decreasing the direct and indirect costs of university attendance. However, financial aid may not be effective in long-term educational attainment, because there may be factors other than credit constraints that drive the decision to drop out from college (Stinebrickner and Stinebrickner 2008). Moreover, the fact that the costs of attendance are artificially discounted by the grant may induce students with a low probability of academic success to enroll, with negative effects on college persistence and on the attainment of higher education.

How to increase college persistence is a matter of increasing concern: higher enrollment translates into a higher stock of human capital only if the propensity to quit before completion is low (Cappellari and Lucifora 2009; Zotti 2015). This issue is particularly important in the Italian context. Italy has one of the lowest percentages of university graduates among European Union countries, as a result of both a low enrollment rate¹ and high dropout rates (Di Pietro 2006; Cingano and Cipollone 2007). In recent years the percentage of students dropping out has fallen,² but it is still very high: the completion rate was around 60% in 2017 (70%, on average, across Organisation for Economic Cooperation and Development countries; OECD 2019; ANVUR 2019). Significant numbers of dropouts occur during the first year of study (Mealli and Rampichini 2012; Gitto, Minervini, and Monaco 2015; Zotti 2015); between 2003 and 2014, on average, about 15% of new entrants to first-level tertiary education³ did not enroll for the second year, with a declining trend (from 16% in 2003 to about 12% in 2014; ANVUR 2016; De Angelis et al. 2016).

We measure the impact of need-based aid on university dropout rates in the first year of enrollment and on the probability of obtaining a college degree; we use student-level administrative data over the period 2003–13 that cover the entire population of Italian university students. The data follow the student from his or her enrollment to graduation/dropout and provide several items of information on the student's academic career and educational background.

The key source of variation we exploit to identify the causal effect comes from funds rationing; some students eligible for the grant do not receive funds because of limitations in the amount of funds. In fact, within each university, students are ranked according to an index of their family's economic condition; those below the cutoff for eligibility are awarded a grant until the available funds are exhausted. Unfortunately, the Equivalent Economic Situation Indicator (ISEE) index assigned to each student is not

¹ Between 2007 and 2015, new entrants to first-level programs dropped by roughly 10% (De Angelis, Mariani, and Torrini 2017).

² The reduction was partly a consequence of the 2001 reform (the “3+2” reform; Bratti, Broccolini, and Staffolani 2006; D’Hombres 2007; Di Pietro and Cutillo 2008; Cappellari and Lucifora 2009). Indeed, one of the goals of the reform was to improve the performance of Italian university students, in terms of reducing both dropout rates and age at graduation (Bratti, Broccolini, and Staffolani 2010).

³ First-level courses include 3-year and 5-year bachelor degrees.

observable in our data, but we know whether the student is eligible for the grant and whether he or she is a grant beneficiary. Our methodology therefore consists of comparing—within each university and controlling for the set of available observables—grant beneficiaries, the treated group, with eligible nonbeneficiaries, the control group. We do this in two steps.

First, we estimate the propensity score, defined as the probability of receiving treatment, the grant, given some student and university covariates. Then we partition the sample into blocks according to the propensity score, and we compare beneficiaries with nonbeneficiaries within each block, controlling for the same set of observables included in the propensity score. In this way we reduce the differences between the two groups with regard to observables by blocking on the propensity score; then any minor remaining differences are accounted for via regression (Rosenbaum and Rubin 1983, 1984; Imbens and Rubin 2015).

The use of administrative data over a long time span constitutes one of the major advantages of this paper with respect to previous works and provides a nice contribution to the literature. Moreover, information available in our database and the applied estimation strategy allow us to address several endogeneity concerns that could arise when investigating the causal impact of a grant on college persistence.

One of the main issues is the difficulty in separating the unique effect of the grant from all of the other factors that influence whether students succeed in college (Bettinger 2007). In particular, family financial conditions determine the access to aid and are also directly associated with student outcomes.⁴ However, in our setting, beneficiaries and eligible students had similar family characteristics; to be eligible for a grant, certain thresholds, in terms of the family's yearly income and assets, must not have been exceeded.

Second, if the assignment of the grant is known before enrollment, as happens in many countries, the notice of acceptance may affect the decision to enroll or not to enroll at a specific university. In particular, beneficiary students with low motivation and low ability may be encouraged to enroll because their costs are artificially lowered, while nonbeneficiaries are less likely to enroll. We can exploit the fact that in Italy, an application for a grant is submitted after enrollment and the notice of acceptance is, in general, communicated a few months later. This helps in correctly evaluating the impact of the grant on college persistence, since eligible nonbeneficiaries and beneficiaries decide to enroll whether they will be awarded the grant or not. To our knowledge, only Goldrick-Rab et al. (2016) showed the same advantage, since they exploited a private program that used a lottery to select eligible students from first-year students.

⁴ Students from the poorest families tend to attend lower-quality high schools, have fewer resources for learning, and, in general, have parents who provide less support for their education.

Another endogeneity problem may arise when scholarships are (also) merit based. In this case the estimated effect on college persistence can be biased because students with scholarships perform better, on average. For this reason, we considered only first-year grants, which are assigned only on the basis of the household's financial situation; in this way beneficiaries should not be *ex ante* different in terms of a student's merit and abilities. Introducing a rich set of covariates into our estimation strategy enabled us to control better for the remaining differences in terms of skills.

We find that by lowering economic barriers for low-income students, financial aid can promote persistence and the likelihood of college graduation. Being the recipient of a grant reduces the probability of dropping out among low-income students by 2.7 percentage points (from 9.6%). Several robustness checks confirm this result: the estimated coefficients in the different specifications range from -2.7 to -4.3 percentage points. As a further result, we show that financial aid, in addition to fostering college retention in the first year, encourages low-income students to graduate and to finish their studies within a set time, thus increasing their academic success and improving efficiency for the university overall. However, even if need-based grants seem to be effective in diminishing economic disparities in the attainment of a degree, it is also worth noting that a nonnegligible fraction of students drop out even after the assignment of the grant. For these students the costs of education remain higher than the benefits. This can be due, on the one hand, to the fact that the aid is not sufficiently generous. This can be an important issue since in Italy students are not given access to loans during school. On the other hand, it may also be that factors other than credit constraints are crucial in determining the perceived benefits of education (e.g., returns to education, risk aversion, or the taste for education). Our analysis also shows that the impact of the grant is heterogeneous depending on students' characteristics (area of residence, type of high school, and final grade attained at high school). Thus, reconsidering the redistribution of funds can further improve the effectiveness of the financial aid policy and narrow economics-based inequality in college persistence.

The rest of the paper is organized as follows. After a brief review of the literature, section III describes the Italian institutional setting and the grant assignment rule, while section IV presents the data. Section V describes the empirical strategy and discusses the identification issues; the results are set out in section VI. Section VII concludes.

II. Literature Review

Financial aid plausibly affects several margins of college students' behavior: the decisions to enroll, to persist, and to complete the degree. According to the standard human capital model (Becker 2009), students will enroll in college if the perceived present discounted value of the benefits of higher education exceeds its costs. By reducing the cost of going to college,

financial aid may lower the real or perceived cost of attendance of low-income students, positively influencing their college-going decisions. Need-based grants may thus help reduce the gaps in enrollment with respect to more affluent students. According to another strand of literature, students from disadvantaged backgrounds do not enroll in college not because of liquidity constraints but because of long-term factors associated with a parental background and family environment that promote cognitive and noncognitive ability (Cameron and Heckman 2001; Carneiro and Heckman 2002).

The empirical evidence has so far been unable to provide a definitive answer regarding the effectiveness of need-based grants on the enrollment decision: the majority of papers found that need-based grants positively affect the decisions of students from low-income families to enroll (Ellwood et al. 2000; Lauer 2002; Dynarski 2003; Kane 2003; Deming and Dynarski 2009; Nielsen, Sørensen, and Taber 2010; Steiner and Wrohlich 2012), but other works find no significant effects (Baumgartner and Steiner 2006; Bruckmeier and Wigger 2014; Vergolini and Zanini 2015).

Moving from enrollment to persistence and college success, the economic theory states that grants may enhance college persistence by providing income that allows students to allocate more time to school activities instead of work, increasing the study time and thus the students' performance (DesJardins, Ahlburg, and McCall 2002). However, financial aid may not be as effective in long-term educational attainment. There may be factors other than credit constraints that are important in determining the substantial dropout rates of students from low-income families. According to Stinebrickner and Stinebrickner (2008), a large majority of students' attrition would remain even under generous policies aimed at relaxing credit constraints. According to other authors, financial aid may have negative effects on college success because it induces students with a low probability of academic success to enroll because the financial costs they incur for their education are artificially lowered. An expansion of the access to public higher education of low-income students is often associated with a reduction in the quality of college graduates and in the college wage premia, because these students are drawn from the lower tail of the ability distribution (Juhn, Kim, and Vella 2005; Bratti, Checchi, and De Blasio 2008; Carneiro and Lee 2011; Keng, Lin, and Orazem 2017).

The majority of the empirical results find that need-based grants mitigate the effect of college costs and positively affect low-income students' persistence and degree completion (DesJardins, Ahlburg, and McCall 1999, 2002; Singell 2004; Bettinger 2007; Bettinger et al. 2012; Arendt 2013; Castleman and Long 2016). Positive effects of financial aid are also found in other aspects of academic success: grades (Cappelli and Won 2016), time taken to complete a degree (Glocker 2011), and initial earnings (Denning, Marx, and Turner 2017).⁵

⁵ Some other works look at the effect of an increase in college fees on different measures of academic success (Garibaldi et al. 2012; Fricke 2018).

While we consider need-based grants, other studies focus on the impact of monetary incentives (De Paola, Scoppa, and Nisticò 2012) or merit-based grants (Dynarski 2008; Scott-Clayton 2011; Dynarski and Scott-Clayton 2013; Sjoquist and Winters 2015). These grants target a population of students different from the one targeted by need-based grants; merit aid recipients are a select group of students who may be likely to attend college with or without the existence of a state merit scholarship program. For this reason, the size of the effect can be different. Interestingly, some works compared and tested the effectiveness of various college subsidy schemes on college persistence (Singell 2004; Mendoza, Mendez, and Malcolm 2009). Hanushek, Leung, and Yilmaz (2014) explore the implications of different forms of financial aid from both an efficiency perspective and an equity perspective (income distribution and intergenerational income mobility).

This work contributes to the literature on the effect on students' persistence, using an administrative data set that covers all Italian students in the period 2003–13. This offers a unique opportunity to determine the causal relation stemming from the policy. As a result of lack of data, a high fraction of the empirical works focused on specific universities or specific years; these results are more difficult to generalize. In regard to the Italian case, both Mealli and Rampichini (2012) and Sneyers et al. (2016) considered first-year students in a specific academic year for a selected sample of Italian universities. Both works suggested that financial aid positively affects students' performances and completion in a substantial and statistically robust way.

III. The Institutional Setting

The Italian financial aid system for higher education is mainly based on the *Diritto allo Studio Universitario* (DSU; “the right to study”) program, intended to encourage enrollment and attendance by students from more disadvantaged families. The main objective of the DSU is to enable motivated students to obtain higher education, irrespective of their income (Republic of Italy 2001). The main benefits offered by the DSU are student grants. After the 2001 constitutional reform, the DSU became part of the exclusive competence of regional legislation; grants are generally managed by regional agencies, with some administrative tasks assigned to universities.⁶

Funds come from regional governments, the central government (Fondo Integrativo Statale), and a specific tax paid by noneligible students. The amount of funding available for these grants thus differs among regions and years and also among universities within regions. There are remarkable differences between geographical areas because of the lower amount of funding available for the regions in the south of Italy: in 2013 the

⁶ Calabria and Lombardy are the only regions where grants are managed entirely by universities.

coverage rate was 90% in the north and 56% in the south (ANVUR 2016). The percentage of eligible students who actually received the grant declined during the Great Recession: it was about 82% in the period 2006–08, it reached the minimum in 2011 (69%), and then it increased to 76.5% in 2013.

In the first year of enrollment, eligibility for a grant is exclusively based on the student's family economic condition.⁷ Applicants are ranked according to an index (the ISEE), computed on the basis of the family's yearly income and assets and also taking into account the family's composition. The allocative algorithm of the grant is thus a continuous function of this index, but a maximum threshold is set at a national level that guarantees that only students from low-income families are eligible. This eligibility threshold is fairly low, making the students comparable in term of financial condition even if an eligible student's index may fall close to or far from the threshold. As an example, in 2008 the ISEE cutoff for eligibility was around €19,000. For a household with both spouses and one child, with zero assets, this is equivalent to an after-tax yearly income as high as €27,000, which in turn is approximately equivalent to 77% of the average Italian yearly income that year for a household of that type.

The ISEE index assigned to each student is not observable in our data, but we have information about the eligibility of the student and whether he or she was awarded the grant. In fact, not all eligible students receive grants because of the lack of funds in some universities and for certain years. This constitutes the source of variation that provides identification in our paper and that allows us to generate a treatment group (those who actually received the grants) and the control group (eligible but not beneficiaries). Note, however, that beneficiaries are slightly poorer than eligible students not receiving the grant (because, as noted above, applicants are ranked according to the ISEE index). This implies that if our identification strategy were not sufficient to compensate for the selection bias, our estimate would likely be biased toward zero, that is, against finding an impact of the grant on college retention.

The timing of the grants' assignment and the type of information available to students may cause selection along different dimensions, which must be taken into account in the analysis. First, if the assignment of the grant is known beforehand, the receipt of the grant may encourage enrollment by students with a low probability of academic success simply because the financial costs that they incur for their educations are artificially lowered. However, in Italy the application for a grant is submitted after enrollment to the regional agency where the university is located;⁸ notice of acceptance is generally communicated a few months after enrollment. This constitutes a main advantage in correctly evaluating the impact of

⁷ The second payment of the grant is conditional on the achievement of a minimum level of credits (established by the regions after consulting the universities, up to a maximum of 20 credits; Republic of Italy 2001).

⁸ In Calabria and Lombardy, the application is submitted directly to the university.

the grant on college persistence, because eligible nonbeneficiaries and beneficiaries decide to enroll regardless of whether they will be awarded the grant.

Second, because the probability of receiving a scholarship varies across regions and years, in principle there is room for students to strategically self-select universities with a higher coverage ratio. In practice, this strategic behavior is precluded because the coverage rates are not known to the public before the moment of enrollment, because of the delayed notice of acceptance. Students' strategic behavior is based on past information, but the coverage rate varies widely over time because it depends on the availability of public funds and on political choices. Moreover, since we control for university-time fixed effects, this selection would have been a concern only if beneficiaries and eligible students within the same university in the same period had had a different set of information about coverage rates, that is, if students' strategic behavior had been correlated with ISEE scores. Conversely, regarding transfers between universities or fields of study after enrollment, students cannot move in order to improve their financial situation, since the application for a grant can be submitted only at the beginning of the academic year and the assignment of grants does not follow a specific field pattern.

In the first year of enrollment, there are no merit requirements for grant eligibility, while for maintaining the grant in the following years, it is necessary to achieve a minimum level of credits. The amount of the grant (which we do not observe) depends on the ISEE index, on the region of enrollment, and on whether students reside in the city where the university is located, whether they are daily commuters, or whether they are out-of-site students.⁹ In 2013 the average yearly amount was about €3,400.¹⁰ There are no restrictions on how the grant is spent.

Even if not all of the eligible students are awarded the grant, these students are all exempted from the payment of tuition fees. In 2013 the average yearly amount of tuition fees at state universities was about €1,000 (about €700 in the south and €1,400 in the north), and it was lower for students from low-income families (the lowest bracket was €200; ANVUR 2016). Summing up, the average size of the grant is at least three times larger than the average fee from which eligible students are exempted.

IV. Data

We exploited the Anagrafe Nazionale Studenti (ANS), a unique data set that contains administrative records on enrollments, students' school backgrounds, and their academic careers in Italian universities. By far,

⁹ Every year the Ministry of Education sets the minimum amount for the grants, but the differences over time are very small. In 2013 the minimum yearly amount for a grant was €1,904 for students who are residents in the city where the university is located, €2,785 for daily commuters, and €5,053 for out-of-site students.

¹⁰ Source: *Osservatorio Regionale per l'Università e il Diritto allo Studio Universitario del Piemonte* (<http://www.ossreg.piemonte.it>).

TABLE 1
DESCRIPTIVE STATISTICS OF THE WORKING SAMPLE BY YEAR OF ENROLLMENT

	2003–06	2007–10	2011–13
Dropouts (%)	8.2	7.6	6.7
Recipients of grants (%)	68.1	73.6	72.6
Females (%)	63.3	62.8	62.1
Residents in the north (%)	26.8	32.5	31.4
Residents in the center (%)	15.4	17.6	16.5
Residents in the south (%)	57.8	49.9	52.1
High school grade	85.0	82.8	83.5
From <i>licei</i> (%)	51.7	59.5	62.3
Out of site (%)	13.9	18.0	21.3
Living in an urban local labor system (%)	39.8	39.8	40.0
Foreign students (%)	1.4	3.4	4.4
	N (Annual Average)		
	20,918	19,149	14,985

Source.—Our calculations based on ANS data.

Note.—The working sample includes students between the ages of 18 and 20, enrolled for the first time at an Italian university, who were eligible for the grant (all of them were exempted from paying tuition fees). High school grade consists in the grade reported at the final exam of high school: the passing level is 60; the top level is 100. *Liceo* is a nonvocational upper-secondary school designed to give students the skills to progress to any university or higher-educational institution.

the main advantage of our database is that it covers the entire population of university students in Italy over a long period of time. We focused on students between the ages of 18 and 20,¹¹ enrolled for the first time at an Italian university over the period 2003–13. Our working sample included first-year student recipients of grants, the treatment group, and those who were eligible but were not awarded the grant, the control group.¹² On average, 19,000 students per year were recorded. Descriptive statistics of the sample are shown in table 1. We defined dropout students as those enrolled as first-year students in the academic year who did not enroll at any university in the following academic year $t + 1$ (ANVUR 2016; De Angelis et al. 2016; De Angelis, Mariani, and Torrini 2017). The dropout rate was, on average, 7.6%, with a downward trend; recipients of grants represented about 70% of all eligible students. Unfortunately, we do not have information on the amount of the grant; we know only whether the student was eligible and whether he or she was awarded the grant. Column 3 of table 2 reports the mean differences between the two groups (beneficiaries and eligible nonbeneficiaries), with respect to dropout rates and

¹¹ The rationale for this is to avoid problems of comparability between students who started university immediately after completing high school and those who started an undergraduate program later on.

¹² A potential problem with our comparison group is that it also includes students not eligible for the grant but exempted from paying the tuition fees for other reasons. On the basis of the information available in the archive, we are unable to separately identify these students in order to exclude them from the analysis. Luckily, on the basis of collateral evidence we can conclude that the resulting degree of contamination of the comparison group is negligible (we estimate that they might be approximately 1%–2% of the students included in the comparison group).

TABLE 2
DESCRIPTIVE STATISTICS FOR TREATED AND NONTREATED GROUPS

	Beneficiaries	Nonbeneficiaries	Differences	Differences Controlling for Propensity Score
Dropouts (%)	6.9	9.6	-2.7*** (.1)	-1.0*** (.2)
Females (%)	63.8	60.6	3.2*** (.2)	.8*** (.3)
Residents in the north (%)	32.3	24.1	8.2*** (.2)	2.4*** (.3)
Residents in the center (%)	17.9	12.8	5.1*** (.2)	-1.2*** (.2)
Residents in the south (%)	49.8	63.1	-13.3*** (.2)	-1.2*** (.3)
High school grades	83.3	85.3	-2.0*** (.1)	-.2* (.3)
From <i>licei</i> (%)	55.2	61.3	-6.1*** (.2)	-.6* (.3)
Out of site (%)	21.5	6.11	15.4*** (.2)	2.5*** (.2)
Living in an urban local labor system (%)	38.7	42.5	-3.8*** (.2)	-1.0*** (.3)
Foreign students (%)	3.5	1.0	2.5*** (.1)	.4*** (.3)
Propensity score	84.9	37.2	47.8*** (.1)	2.3*** (.0)
Observations	146,005	59,219		

Source.—Our calculations based on ANS data.

Note.—Years 2003–13. The working sample includes students between the ages of 18 and 20, enrolled for the first time at an Italian university, who were eligible for the grant. Nonbeneficiaries are eligible but not beneficiary students. Columns 3 and 4 show ordinary least squares estimations; dependent variables are shown in rows. Column 4 shows estimation controlling for dummies for propensity score intervals (0.2–0.4–0.6–0.8). Propensity score is estimated in eq. (2). Standard errors are shown in parentheses.

* $p < .10$.

*** $p < .01$.

to some individual characteristics possibly affecting dropout rates (e.g., gender, type or area of residence, school grades and type). The dropout rate is statistically lower for treated students. In addition, the other mean differences between treated and control groups are significantly different for all of the considered variables. We will obviously take them into account, both in the propensity score matching and in the regressions, to compensate for the selection bias.

V. Estimation Strategy

We were interested in estimating the following equation on the sample of treated and control students:

$$Y_{iut} = \alpha S_{iut} + \beta X_{iut} + \gamma D_{ut} + \epsilon_{iut}, \quad (1)$$

where student, university, and year are indexed by i , u , and t , respectively; Y_{iut} is a measure of academic success. Our main dependent variable is a dummy variable taking the value of 1 if the student enrolled in the first year of college does not enroll in the second year. We also consider other outcomes, such as the probability of obtaining a degree, the probability of graduating within x years, and the degree grade. The dummy variable S_{iut} is a binary treatment status denoting recipients of a grant and takes the value 1 if the student received a grant and 0 if the student did not receive it despite being eligible. As noted in section IV, we do not have any information on the amount of the grant, only whether the student receives a grant. The variable X_{iut} represents individual characteristics possibly relevant for dropout rates, namely, gender, nationality, area of residence, a dummy for studying in a macro area different from the area of residence, high school type and grade, and a dummy for the local urban labor system of residence (Di Pietro 2004; Adamopoulou and Tanzi 2017). Tables 1 and 2 report some descriptive statistics from our sample. Finally, D_{ut} is a university dummy interacted with time dummies, in order to capture university-time-specific patterns. Including these university-time fixed effects in the regression, we identify the causal parameter exploiting only the within-university-time variability in the treatment status.

Our parameter of interest is α , the average impact of need-based financial aid on the dropout probability. Endogeneity issues may arise in the estimation of α . A classic problem in this literature is an ability bias due to selection into treatment of more able students. This would be a major concern if the grant were awarded (also) on the basis of students' merit. As explained, this is not the case for students enrolled in the first year of the program; in their case, it is only the family economic circumstances that matter for the assignment of the grant. In addition, we were able to control for some factors relating to students' abilities and merits (high school type and grades). Another endogeneity issue that has frequently emerged in the literature relates to the fact that application for a grant is voluntary and the propensity to apply may depend on a set of observable and unobservable individual characteristics, possibly correlated with the outcome. This concern did not apply in our setting, because both the treated and control groups were students who had voluntarily applied for the grant.

Another frequent issue that creates difficulties in measuring the impact of aid on persistence is related to the fact that the aid may first affect the choice to enroll, generating selection concerns. Since in Italy the notice of grant acceptance is generally communicated a few months after enrollment, both beneficiaries and nonbeneficiaries decide to enroll whether they will be awarded the grant or not.

The main problem we face is how to control for the residual differences in the economic circumstances of the student household after selecting our sample conditioning on eligibility for the grant. The assignment of the grant is based only on the ISEE index, which, unfortunately, we do

not observe in our data set. Hence, all household characteristics correlated with ISEE and with the outcome are confounders for our problem. What we do is control for the set of characteristics of the student, of his or her household, and of his or her university (including a university-time fixed effect). It should be noted, however, that if our strategy was not enough to net out the differences between the two groups with respect to financial conditions, the resulting estimate is likely biased toward zero, that is, against finding an impact of grant on college retention.

To implement our estimator, we proceed in two steps. First, we estimate the propensity score, defined as the probability of receiving treatment, given some students' and universities' covariates (X_{iut} and D_{ut}):

$$e(X, D) = E[S_{iut}|X_{iut}, D_{ut}] = \Pr(S_{iut} = 1|X_{iut}, D_{ut}), \quad (2)$$

where the estimator is based on a logit model.

Then the empirical strategy is based on blocking on the propensity score combined with a regression adjustment. The idea behind this method, proposed by Rosenbaum and Rubin (1983, 1984), is to split the sample into subclasses according to the propensity score and then run the regression of the outcome on the treatment status as well as on the list of controls included in the propensity score within each subclass. The two main advantages of this estimator are as follows (Imbens 2015): first, the subclassification approximately averages the propensity score within the subclasses, smoothing over the extreme values of the propensity score, and, second, the regression within the subclasses adds a large amount of flexibility compared with a single weighted regression.

Following Imbens (2015) and Imbens and Rubin (2015), we need to partition the range $[0, 1]$ of the propensity score into J intervals $[b_{j-1}, b_j]$, for $j = \{1, \dots, J\}$, where $b_0 = 0$ and $b_J = 1$. Let $B_i(j) \in \{0, 1\}$ be a binary indicator where the estimated propensity score for unit i , $\hat{e}(x)$, satisfies $b_{j-1} < \hat{e}(x) < b_j$. In particular, we choose to partition the sample into five blocks according to the following propensity score values: $j = 1$ if $0 \leq \hat{e}(x) < 0.2$; $j = 2$ if $0.2 \leq \hat{e}(x) < 0.4$; $j = 3$ if $0.4 \leq \hat{e}(x) < 0.6$; $j = 4$ if $0.6 \leq \hat{e}(x) < 0.8$; $j = 5$ if $0.8 \leq \hat{e}(x) \leq 1$.

Within each block, the average treatment effect is estimated using linear regression, with all of the covariates X_{iut} and D_{ut} described in equation (1), and including an indicator for the treatment. By including in each regression the university-time fixed effects, we identify the average causal effect relying only on the within-university-time variability of the treatment status: the comparison group is made up of students enrolled at the same university in the same period as the treated one. This leads to J estimates $\hat{\alpha}_j$, one for each block. These J within-block estimates are then averaged over the J blocks, using the proportion of treated units in each block as the weights:

$$ATT = \alpha_{\text{block,treat}} = \sum_{j=1}^J \frac{N_{\text{treat}_j}}{N_{\text{treat}}} \cdot \hat{\alpha}_j. \quad (3)$$

The coefficient $\alpha_{\text{block,treat}}$ is the estimated value of the average effect of the grant on the probability of dropping out for those receiving the grant; that is, we estimate the average treatment effect on the treated group (ATT). Of course, to explore the degree of heterogeneity of the causal effect, one could also evaluate the weighted average with respect to a different set of weights, for example, the proportion of untreated units in each block, so as to get the average treatment effect on those not receiving the grant (ATNT) or the proportion of units in the block to get the average treatment effect on the population (ATE).

VI. Results

Figure 1 plots the distribution of the propensity score for the two groups. A large difference between the two groups is apparent, with treated units closely concentrated just below 1 and untreated units more evenly distributed over the whole support with a mode of around 0.2. The mean (median) value of the propensity score is 0.85 (0.95) for treated students and 0.37 (0.29) for untreated ones (table 2).

The large difference between the two distributions might raise a concern about the lack of common support. Note, however, that the large

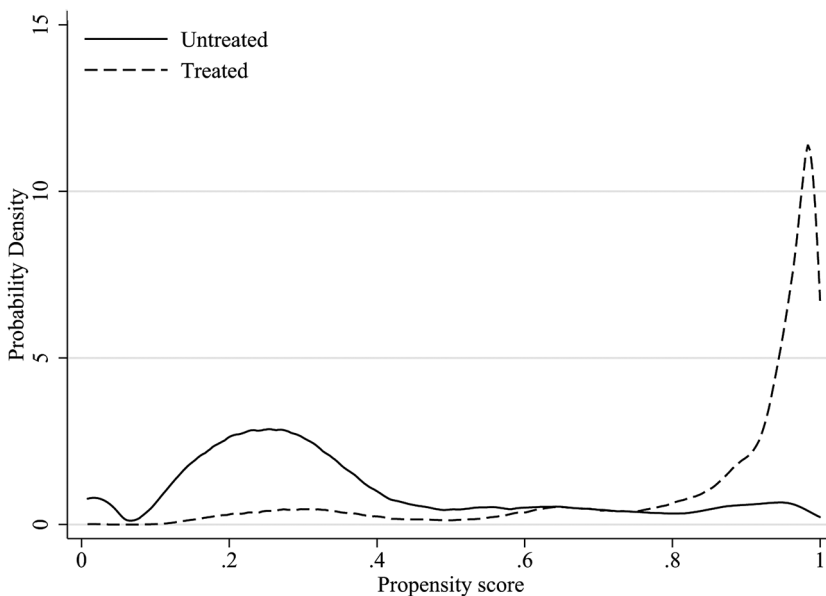


Figure 1.—Distribution of the propensity score in the treated group and the nontreated group. The following controls are included in the propensity score: female, area of residence (north, center, south of Italy), foreign, a dummy for studying in an area different from that of residence, high school type (dummies for different types) and grade (categorical variable with five classes), a dummy for residing in an urban local labor system, and university dummies interacted with time dummies. Source: Our calculations based on ANS data.

number of units available in both groups makes the comparison feasible essentially everywhere on the support of the propensity score. In particular, in the last block, where the proportion of nontreated units is the smallest, we have about 6,100 students in the comparison group. The main driver of this large difference between the two distributions is the university-time fixed effect (see also sec. VI.B). As explained in section V, there is a strong case for including these fixed effects among the control variables; in this way, in fact, we can force the composition of the comparison group with respect to university-time to be exactly the same as that of the treatment group. In column 4 of table 2 we show that controlling for propensity score (by including block dummies) drastically reduces the differences between treated and control groups in the observables. Still, these small residual differences cannot be overlooked. To account for them, we run a regression separately in each block of the outcome variable on the treatment status, controlling for the very same set of covariates we included in the propensity score.

Table 3 reports the estimated effect of the grant on dropout for each block ($\hat{\alpha}_j$) and the weighted average effect (ATT), while table 4 presents the estimated coefficients for all of the variables included in the regression in each block. We find that need-based aid positively affects college

TABLE 3
ESTIMATED EFFECT OF GRANTS ON DROPOUT RATE ($N = 205,147$)

Block	Weight	α_j	Standard Error
$j = 1$.0158	.0256***	.0092
$j = 2$.0762	.0008	.0073
$j = 3$.0382	-.0047	.0067
$j = 4$.0916	-.0236**	.0104
$j = 5$.7781	-.0323***	.0085
ATT		-.0270***	.0067
Robustness Check with Block 5 Split			
$j = 5$.1180	-.0228*	.0122
$j = 6$.6601	-.0391***	.0090
ATT		-.0303***	.0062
Robustness Check with Block 6 Split			
$j = 6$.1610	-.0247**	.0116
$j = 7$.4992	-.0530***	.0115
ATT		-.0350***	.0063

Source.—Our calculations based on ANS data.

Note.—The average effect (ATT) is computed as the weighted average over the J blocks, using the proportion of treated units in each block as weights (eq. [3]). Each within-block regression includes the following controls: female, area of residence, foreign, a dummy for studying in an area different from that of residence, high school type and grade, a dummy for residing in an urban local labor system, and university dummies interacting with time dummies. Residuals are clustered at the university-year level.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

TABLE 4
ESTIMATED EFFECT OF GRANTS ON DROPOUT RATE: FULL REGRESSION
(Dependent Variable: Dummy Dropout)

	Block 1	Block 2	Block 3	Block 4	Block 5
Grant	.026*** (.009)	.001 (.007)	-.005 (.007)	-.024** (.010)	-.032*** (.009)
Female	-.005 (.007)	-.016*** (.004)	-.007 (.005)	-.004 (.005)	-.007*** (.002)
Resident in the center	.046 (.031)	-.032* (.017)	.035** (.016)	-.004 (.013)	.023*** (.004)
Resident in the south	.009 (.029)	.012 (.019)	.004 (.020)	.004 (.018)	-.002 (.003)
Foreign student	.034 (.063)	-.021 (.015)	-.013 (.034)	-.027 (.017)	-.029*** (.004)
Out-of-site student	-.086*** (.023)	-.025* (.014)	-.003 (.010)	-.025*** (.009)	-.007** (.003)
High school grade	-.036*** (.002)	-.033*** (.002)	-.027*** (.003)	-.022*** (.003)	-.021*** (.001)
Vocational high school	.116*** (.007)	.091*** (.007)	.083*** (.009)	.054*** (.007)	.050*** (.003)
Other high school	.137*** (.014)	.126*** (.014)	.076*** (.009)	.063*** (.008)	.065*** (.004)
Living in an urban local labor system	-.002 (.004)	.011** (.004)	.001 (.007)	.007 (.006)	.006*** (.002)
University-time fixed effect	Yes	Yes	Yes	Yes	Yes
R^2	.076	.059	.065	.063	.054
Observations (treated)	2,313	11,124	5,575	13,373	113,577
Observations (total)	16,749	38,247	11,822	18,607	119,722

Source.—Our calculations based on ANS data.

Note.—Omitted categories are high school *licei* and students residing in the north of Italy. High school grade is a categorical variable with five classes. Standard errors clustered at the university-year level are shown in parentheses.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

retention for the treated students: the estimated average effect is a reduction of 2.7 percentage points in the probability of dropping out. This is very close to the crude difference in the dropout rate that we observe between the two groups in table 2, meaning that the large differences with respect to observable characteristics summarized by the propensity score in this instance do not raise any substantial selection bias. Since most of these observable characteristics do matter for the dropout rate (see, in particular, col. 5 of table 4), it must be that the selection bias separately due to each of these observable characteristics overall cancels out.

The magnitude of the estimated coefficient is significant: the dropout rate for those who received the grant would have increased from 7% to about 10% in the absence of a grant. In regard to the within-block estimates, the average effect is driven, as expected, by the fifth block (which includes 78% of treated students). On the contrary, the coefficients of the first three blocks are positive or not significantly different from zero. This may be explained by students' characteristics: in particular, in these

blocks there are higher percentages of students from *licei*¹³ and who reported high grades at school (as we will explain in sec. VI.A, the effect of the grant is smaller for these students). The positive sign of the coefficient in block 1 is also driven by students enrolled at the University of Genoa, for whom we found a measurement error in the classification of eligible students (see sec. VI.B for further details). When we exclude these students from the working sample, the estimated coefficient in the first block becomes negative and not statistically significant, but the results in the other blocks remain substantially unchanged (results available upon request).

As a robustness check, we further split the last block (table 3, bottom panels): first, we halved it, and we obtained an average impact as large as -3.0 percentage points; we then further divided the last block in half, resulting in an average total effect as large as -3.5 percentage points.

In the baseline estimates, we include university-time fixed effects, grouping together contiguous years.¹⁴ However, results are robust to windows of different years: in particular, controlling for university-year fixed effects, we obtain an even higher average effect. We prefer to present the most conservative estimates (results for different time periods available upon request). Standard errors are corrected for the potential clustering of residuals at the university-year level, since students attending the same university in the same year would have similar outcomes; the results (available upon request) are robust to alternative treatments of the error terms, such as university or field of study.

In order to establish the validity of our inferences, we strengthen the analysis by using two alternative methodologies: kernel matching and propensity score reweighting. In both cases we included the X_{it} and D_{it} controls described in equation (1). The results are reported in the top panel of table 8. Using the kernel-matching method¹⁵ (with a bandwidth of 0.06 and with bootstrap standard error¹⁶), the estimated average treatment effect on the treated group is -4 percentage points; following the propensity

¹³ In Italy, the upper-secondary education system is classified into three types. First, the *liceo* is designed to give students the skills to progress to any university or higher-educational institution; the education received is mostly theoretical, with a specialization in a specific field of studies (humanities, science, languages, or art). Second, the vocational/technical institute offers both a wide theoretical education and a specialization in a specific field of studies (e.g., business administration, humanities, administration, law, technology, tourism); these institutes qualify students for all jobs in all sectors of the economy. Third, the professional institute offers a form of secondary education oriented toward practical subjects (engineering, agriculture, gastronomy, technical assistance, handicrafts) and enables students to start searching for a job as soon as they have completed their studies; some schools offer a diploma after 3 years instead of 5 years, although it is considered a lower level of school compared to the others. Any type of secondary school that lasts 5 years grants access to the final exam (*esame di maturità*), which allows enrollment to university.

¹⁴ We consider the following periods: 2003–06, 2007–10, and 2011–13.

¹⁵ The extent of balancing between the two samples significantly increases after matching is carried out. After matching, the pseudo- R^2 reduces to 0.05 from 0.43 and the mean bias to 3.0 from 9.5.

¹⁶ We replicated the analysis with bandwidths of 0.08 and 0.04, and the results remain unchanged.

score reweighting (where weights equal to 1 for treated students and $\hat{\varepsilon}(x)/(1 - \hat{\varepsilon}(x))$ for the control group), the estimated effect of a grant is -3.9 percentage points. These are basically the values of the estimated ATT that we presented in table 3 when breaking down the fifth block into three subblocks.

So far, we have consistently shown that need-based aid has increased the probability of enrolling in the second year. However, the literature suggests that financial aid may not be effective for long-term educational attainment, since there may be factors other than credit constraints that drive the decision to graduate (Stinebrickner and Stinebrickner 2008). To refine our analysis, we address the effect of grants on the probability of graduating and the degree grade.¹⁷ We replicated the same empirical strategy based on block-propensity score matching and covariate adjustments within blocks. We find that need-based aid has a positive impact on the probability of obtaining a college degree: the estimated average effect is an increase of 7.8 percentage points in the completion rate for treated students (col. 1 of table 5). As a matter of fact, the receipt of the grant in the first year is highly correlated with receiving the grant in subsequent years. Then the effect of the grant on the probability of graduating reported in table 5 is an average cumulative effect of all of the financial aid received during the academic career. Moreover, we also find that beneficiary students are more likely to graduate on time (cols. 2–4), in line with other findings in the literature (Glocker 2011). This is an important result in terms of policy implications, because Italian students have an abnormal tendency to extend their presence in a university program beyond the normal completion time, as documented in Garibaldi et al. (2012).

On the contrary, treatments and controls do not differ, on average, with respect to the final degree grade (col. 5). This could be interpreted as the result of two opposite effects that counterbalance each other. On the one hand, there is the effect on the grade of students who would graduate even in the absence of the aid. The grant may allow them to allocate more time to school activities instead of work, increasing their performance. This is true for the subgroup of students for whom a meaningful causal effect of the grant on the final grade is defined. On the other hand, the grant induces graduation of students who would not graduate in the absence of the grant. If their grade in the presence of the grant is, on average, lower than the grade of students graduating even in the absence of the grant (see sec. II), the comparison between the two groups is biased by a differential composition. Following Lee (2009), we assume that students who would not graduate in the absence of the grant would graduate in the presence of it with a grade that systematically belongs to the left tail of the distribution. Then, in each block, we remove from the left tail of the distribution of grades a fraction of the treated students as large as the impact of the treatment on the dropout rate in that block. After removing

¹⁷ We thank an anonymous referee for this insightful suggestion.

TABLE 5
ESTIMATED EFFECT OF GRANTS ON GRADUATION

	Graduate	Graduate within			Degree Grade
		1 Year	2 Years	3 Years	
ATT	.0784*** (.0177)	.0532*** (.0171)	.0692*** (.0178)	.0755*** (.0179)	.1492 (.2556)
Observations	172,189	172,189	172,189	172,189	103,431

Source.—Our calculations based on ANS data.

Note.—The average effect (ATT) is computed as the weighted average over the J blocks, using the proportion of treated units in each block as weights (eq. [3]). Each within-block regression includes the following controls: female, area of residence, foreign, a dummy for studying in an area different from that of residence, high school type and grade, a dummy for residing in an urban local labor system, and university dummies interacting with time dummies. Residuals are clustered at the university-year level. In col. 1 the dependent variable is a dummy equal to 1 if the student obtains a degree. In cols. 2–4 the dependent variables are dummies equal to 1 if the student graduates within x years of the legal duration of the course. In col. 5 the dependent variable is the degree grade, which ranges from 66 to 110.

*** $p < .01$.

this subset of treated students, we estimate the impact of the grant on the final grade. The estimate we get in this way is an upper bound on the average causal effect of the grant on grades of students graduating even in the absence of the grant. Results show that this effect is negligible even if statistically significant, resulting in an increase of the grade as large as 0.48, where the degree grade ranges from 66 (sufficient) to 110 (excellent; results available upon request).

To conclude, we show that first-year grants, in addition to reducing the dropout rate immediately, also encourage low-income students to graduate and to finish their studies within a set time, thus increasing their academic success and improving the university's overall efficiency.

A. *Heterogeneous Effects*

To refine our analysis, we assess whether financial aid has boosted college retention disproportionately more for students with certain characteristics. Previous research has investigated whether the effects vary according to race, ethnicity, gender, parental education, precollege academic preparation, institutional selectivity, or depth of familial poverty the students face. Also on the basis of data availability, we chose to focus on some dimensions of heterogeneity related to the characteristics of the students and of the Italian tertiary education supply.

We first consider whether the effect of the grant depends on the gender of the student (table 6). In Italy, in fact, there exist significant differences in attitudes between men and women toward university studies: according to public statistics, women graduate at a higher rate and more quickly than men. At the same time, women have a stronger preference for staying at the local university than do men (Rizzica 2013). Such differences may suggest that the responses of men and women to financial aid could be different as well. In order to check this, we interacted the treatment status with the female dummy. The coefficient of the interaction

TABLE 6
ESTIMATED EFFECT OF GRANTS ON DROPOUT: INTERACTION TERMS ($N = 205,147$)

	Estimated Average Impact (ATT)			
	(1)	(2)	(3)	(4)
Treatment	-.0315*** (.0083)	-.0123 (.0084)	-.0455*** (.0098)	-.0355*** (.0082)
Treatment \times female	.0075 (.0070)			
Treatment \times resident south		-.0311*** (.0117)		
Treatment \times <i>licei</i>			.0335*** (.0085)	
Treatment \times high school grade				.0263*** (.0077)

Source.—Our calculations based on ANS data.

Note.—The average effect (ATT) is computed as the weighted average over the J blocks, using the proportion of treated units in each block as weights (eq. [3]). Each within-block regression includes the following controls: female, area of residence, foreign, a dummy for studying in an area different from that of residence, high school type and grade, a dummy for residing in an urban local labor system, and university dummies interacted with time dummies. Residuals are clustered at the university-year level.

*** $p < .01$.

term was not statistically significant, meaning that there were no gender differences in the impact of need-based aid.¹⁸

We then explored whether a heterogeneous effect exists according to students' school curriculum (type of high school and high school grade); in Italy, students who attend high schools named *licei* show a much higher level of academic preparation than do those coming from vocational schools. Moreover, Italy is characterized by a historically poor presence of short and professional paths that are more able to satisfy the needs of students with a less academic profile or with low abilities (De Angelis et al. 2016). As found in the literature (Goldrick-Rab et al. 2016), our data also show that the aid effect is greater for students with less academic preparation and lower odds of success. These students benefit more from the income provided by grants, for example, reducing their workload and allowing them to focus more of their time and energy on school. Without the grant, the dropout rate would increase from 4.3% to 5.5% for students from *licei* and from 10% to 14.5% for students from vocational studies. Moreover, more able students (higher grades in high school) are less likely to drop out, irrespective of the grant, because they have higher expected benefits from obtaining a university degree; without the grant, the dropout rate would increase from 3.8% to 4.7% for students who reported a high grade at the final exam of high school and from 8.7% to 12.2% for low-grade students. Thus, the effectiveness of the grant varies according to how prepared students were for college.

¹⁸ The literature reported gender differences in the impact of merit-based aid, finding a stronger impact among women (Dynarski 2008).

Third, we wanted to assess whether there are any differences in the impact according to the area of residence, given the strong economic and social gap between the north and the south of Italy. The coefficient on the interaction term revealed that students residing in the south gain more from financial aid than students residing in other areas. In particular, the dropout rate would increase in absence of the grant from 6.5% to 10.8% for southern students; the effect for those residing in the center or the north is negative, but the coefficient is lower and is not precisely estimated. A possible explanation is that budget and credit constraints are more likely to be binding in the south, which is characterized by lower average household income than the north and by lower employment opportunities (Ciani and Torrini 2019).

Finally, we also analyzed whether the impact of the aid varies according to the share of eligible students who actually receive a grant. In fact, marginal recipients enrolled at universities where the coverage rate is low can be poorer than those enrolled at universities where almost all eligible students receive a grant; therefore, the average impact of the grant on these students is likely to be larger.¹⁹

In order to check this hypothesis, we interacted the treatment dummy (S_{iut}) with $(CR_{ut} - CR_{avt})$, which is the difference between the coverage rate at university u in period t and the average coverage rate.²⁰ However, results show that the average effect of the interaction term is not statistically significant, suggesting that the impact of the grant does not vary according to the university coverage ratio (results available upon request).

B. Robustness

We now present a set of robustness analyses in order to check whether our results hold over a variety of specifications and sample selection criteria. The first two robustness checks were connected to the estimation of the propensity score (eq. [2]). First, as shown in figure 1, the distribution of the propensity score is highly unbalanced in the two treatment arms, as a result of the inclusion of university-time fixed effects that capture most of the variability in the treatment status. If we remove these fixed effects and include only time fixed effects, we obtain a more balanced distribution (fig. 2). The average impact of a grant on dropout rate for the treated group (ATT) is still negative and statistically significant, even if the magnitude is lower (1.15 percentage points; table 7). It is important to note that in the baseline model presented in table 3, the composition of the

¹⁹ According to the literature, need-based grants are not equally conducive to the college persistence of students from various economic strata because larger positive benefits are found for the students in the bottom half of the income distribution (Alon 2011).

²⁰ In this specification of the model, the coefficient on the treatment dummy represents the causal effect of a grant for students in a university period with a coverage ratio at the average level, while the coefficient on the interaction term represents the change in the causal effect of a grant induced by a marginal variation of the coverage rate with respect to the average.

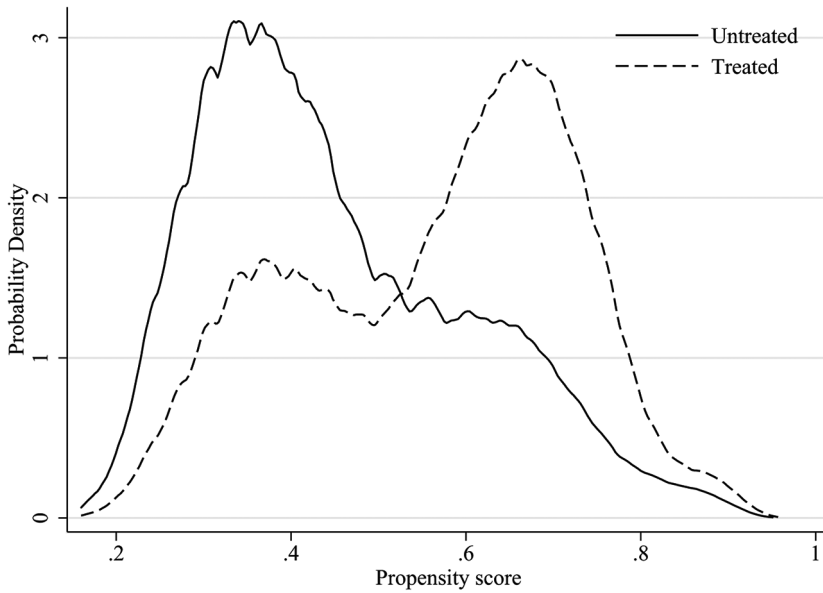


Figure 2.—Distribution of the propensity score: robustness. We included the following controls: female, area of residence, foreign, a dummy for studying in an area different from the one of residence, high school type and grade, a dummy for residing in an urban local labor system, and year fixed effects. Source: Our calculations on ANS data.

comparison group with respect to the university period is forced to be the same as that of the treatment group. This is no longer the case when we drop the university fixed effect, leaving only the time fixed effect.

Second, in the baseline model the treated and control units are enrolled in the same university and in the same period but possibly in different

TABLE 7
ESTIMATED EFFECT OF GRANTS ON DROPOUT: ROBUSTNESS ($N = 340,205$)

Block	Weight	α_j	Standard Error
$j = 1$.0018	-.0157*	.0093
$j = 2$.2110	-.0220***	.0046
$j = 3$.2951	-.0114*	.0062
$j = 4$.4495	-.0076	.0073
$j = 5$.0425	-.0017	.0134
ATT		-.0115***	.0039

Source.—Our calculations based on ANS data.

Note.—The average effect (ATT) is computed as the weighted average over the j blocks, using the proportion of treated units in each block as weights (eq. [3]). Each within-block regression includes the following controls: female, area of residence, foreign, a dummy for studying in an area different from that of residence, high school type and grade, a dummy for residing in an urban local labor system, and year dummies. Residuals are clustered at the university-year level.

* $p < .10$.

*** $p < .01$.

fields of study. Funds rationing does not follow some specific pattern across fields, but it could be important to control for field-specific characteristics. Therefore, we expanded the fixed effects and made them university \times time \times field specific, considering four fields: sanitary, scientific, social, and humanities.²¹ Results are confirmed: the estimated ATT is -0.0276 .

The third robustness check examined the presence of possible measurement errors in treatment status. According to the statistical office of the Ministry of Education, University, and Research (MIUR), and considering all enrolled students, the rate of students with grants was, on average, 7.4% over the period 2003–13 (ANVUR 2016), while, according to ANS data, the rate was lower, about 5% of all enrolled students.²² The difference could mainly be due to the fact that data on grants are collected from different sources. ANS data are administrative data reported by universities, while MIUR data are provided by the regional agencies that manage grants. These differences in the data could generate two problems relating to possible measurement error in our treatment variable. The first is a nonrandom selection of the students awarded grants that occurs if the students with grants that are not reported in our database are not randomly selected in terms of students' or universities' characteristics. Because we are able to control for a large set of variables at the individual and university level, we do not think that this issue compromises the validity of our results. The second problem is contamination, and it occurs if the control group includes some treated individuals; this would imply that we are underestimating the impact of a grant on the dropout rate. To deal with this issue, we restricted the sample of our analysis in order to minimize the gap between ANS and MIUR data. In particular, we restricted the sample by considering only university-year pairs for which the difference between the two data sources was minimal (in particular, we kept only the universities for which the difference between the two databases in the number of students awarded grants was lower than 5%). Table 8 shows the results: the negative and statistically significant impact of grants is confirmed, with an average effect of -4.3 percentage points. Considering all of the results yielded by our analysis, the estimated impact of grants on beneficiaries is a reduction in the dropout probability that ranges from -2.7 percentage points in the baseline analysis to -4.3 percentage points in the most stringent specification.

Another measurement error refers to, as mentioned in section VI, the classification of the eligible students at the University of Genoa: with respect to the available public data, our data show a higher number of eligible

²¹ The sanitary field includes medicine, pharmacy, and veterinary medicine; the scientific field includes math, physics, statistics, geology, biology, engineering, architecture, and computer science; the social field includes political/social sciences, law, economics, and management; the humanities field includes literature, languages, history, and geography.

²² Unfortunately, we cannot make these comparisons on grants for our working sample because there are no publicly available statistics for the sample of 18–20-year-old students enrolled for the first time in Italian universities.

TABLE 8
ESTIMATED EFFECT OF GRANTS ON DROPOUT

	α	Standard Error
Different Estimation Methods		
Kernel matching	-.0397***	.0037
Propensity score reweighting	-.0389***	.0076
Observations	205,147	
Different Subsamples		
Universities/years with low gap	-.0431***	.0110
Observations	93,028	

Source.—Our calculations based on ANS data.

Note.—Show are robustness checks with different estimation methods and different subsamples. We included the following controls: female, area of residence, foreign, a dummy for studying in an area different from that of residence, high school type and grade, a dummy for residing in an urban local labor system, and universities dummies interacted with time dummies. Residuals in the propensity score reweighting are clustered at the university-year level. For the top panel, kernel matching is estimated with a bandwidth of 0.06 and with bootstrap standard error. For the bottom panel, the analysis is based on blocking with regression adjustments. The average effect ($ATT = \alpha$) is computed as the weighted average over the J blocks, using the proportion of treated units in each block as weights.

*** $p < .01$.

nonbeneficiaries. This means that the control group is wrongly including students who are not eligible for the grant. As a result, these false-eligible students end up in the first block, precisely because their propensity score—that is, their probability of receiving the grant—is small. In fact, these students constitute a large fraction of the control sample in the first block. In addition, these students seem to have a lower dropout rate, with respect to the real group of eligible students, because they are students with better economic conditions. In fact, data show that for the University of Genoa, the dropout rate of eligible students is lower than the dropout rate of beneficiaries (on the contrary, for the whole sample, the dropout rate of eligible students is greater than that of beneficiaries, as shown in table 2). When we omit the students enrolled in the University of Genoa from the sample, the estimated coefficient in the first block becomes negative and not statistically significant while results in the other blocks are confirmed (estimates without the University of Genoa available upon request).

As an additional analysis, we computed the average treatment effect on those not receiving a grant (ATNT) and the population-wide average treatment effect (ATE), which would be the average causal effect if eligible individuals were assigned at random to treatment. In fact, in a heterogeneous response model, the treated and nontreated may benefit differently from being awarded a grant; therefore, the effect of the treatment on the treated will differ from the effect of the grant on the untreated, hence from the ATE. To explore the degree of heterogeneity of the causal effect, we computed the effect of the grant by using different weighting

strategies. We first used the proportion of untreated units in each block as a set of weights to get the ATNT; in this way, we gave most weight to the left tail of the propensity score distribution and, in particular, to the second block (see fig. 1), where the coefficient of the treatment dummy is not statistically significant (see tables 3, 4; see also sec. VI). Consequently, the average coefficient becomes approximately zero and statistically not significant. To compute the ATE, we used the share of units in each block as a set of weights to average out block coefficients, and we found that the effect of the grant on the whole population of low-income students is a statistically significant reduction in the dropout rate of 1.9 percentage points.

VII. Conclusions

In this paper we investigated whether need-based grants influence students' educational outcomes. The study of the effect of student financial aid is particularly important, given the increasing public policy expenditures on these programs. In Italy, whether and how much need-based grants are effective in boosting higher education is a key policy question. In fact, only about 60% of students who enroll obtain a university degree (Gitto, Minervini, and Monaco 2015), and the majority of dropouts occur at the end of the first year of enrollment (Mealli and Rampichini 2012).

The main advantage of our analysis is that it is based on a unique database covering the entire population of university students in Italy. The paper addresses endogeneity issues by restricting the sample to eligible students and by exploiting the fact that, because of insufficient funds, some of these students are not awarded a grant. Our estimation strategy is based on block-propensity score matching and covariate adjustments within blocks.

We found that need-based aid positively affects college retention in the first year of enrollment: the dropout rate for low-income students would rise from about 7% to 10% as a consequence of not receiving a grant. The result is quite robust to different estimation methods and also holds when we restrict the sample for further robustness checks. We observe a great heterogeneity among population groups: the aid is more effective for students with a relatively low high school background and for those residents in the south of Italy. Moreover, the grant also has an impact on the probability of completing a college education: the estimated average effect is an increase of 7.8 percentage points in the completion rate for treated students. Our results support the evidence that a grant program truly prevents low-income students from dropping out and is effective for long-term educational attainment, resulting in higher human capital accumulation.

Establishing the impact of financial aid on college persistence is important for policy purposes. As far as private returns on schooling are concerned, university education has positive effects on employment, earnings, and social outcomes, such as health and life satisfaction (Ciccone, Cingano, and Cipollone 2004; OECD 2016). University completion is particularly important in Italy, given the legal value of university degrees (in terms

of access to public sector jobs and to specific regulated occupations) and the honorific title of *dottore*, which conveys an important social status (Cappellari and Lucifora 2009). At the social level, education creates a variety of benefits that are shared by society in general: human capital spillovers can increase productivity and wages, reduce criminal participation, and improve voters' political behavior (Cicccone, Cingano, and Cipollone 2004; Moretti 2004). In addition, reducing the dropout rate among the poorest students has important implications from an equity point of view, leading to an increase in intergenerational mobility and a reduction of inequality (Hanushek, Leung, and Yilmaz 2014).

However, our empirical analysis detects that a nonnegligible fraction of students drop out at the end of the first year irrespective of whether they have a grant, suggesting that liquidity constraints are one important explanation for low college completion rates but not the only one. Thus, reconsidering the redistribution of funds and understanding the most appropriate mechanism for awarding grants can further improve the effectiveness of the financial aid policy and narrow economic-based inequality in college persistence. Some recent works assess how different assignment rules that target different students are more or less effective in reducing dropout rates (Hanushek, Leung, and Yilmaz 2014; Modena, Pereda Fernández, and Tanzi 2019). Taking these differences into consideration is fundamental for policy makers to appropriately design the assignment rule, mainly in countries such as Italy, where the availability of public grants is limited compared with neighboring countries.

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