Increasing College Going by Incentivizing Savings: Evidence from a Randomized Controlled Trial in Italy

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Abstract

*We estimate the impact of a matched savings account program on high school students' college enrollment and persistence, through a randomized controlled trial carried out in Italy. The tested program (*Percorsi*) provided low-income high school students with a 4:1 match rate for savings dedicated to higher education expenditures and required that they attended financial education classes. The program increased rates of enrollment and persistence in university by about nine percentage points. Effects were even larger for vocational school students, who have poorer social backgrounds and lower academic preparation. Incentivized savings programs have potential to reduce social disparities in higher education participation, though the lower saving capacity of poorer households can generate regressivity in program design.*

Keywords: Higher Education, Financial Aid, Children's Savings Accounts, Randomized Controlled Trial
JEL Codes: C93, D14, I22, I23, I24, I28

INTRODUCTION

Despite years of educational expansion and policy efforts to make education more affordable, social disparities in higher education participation are still prominent and pervasive in most countries (Narayan et al., 2018). The family of origin greatly affects young people's likelihood of enrolling in college, persisting in college, and graduating from college. Children of socioeconomically deprived families attain lower levels of education because of a mix of inadequate financial resources and low educational expectations (Buchmann & Dalton, 2002; Cohen, 1987; Scott-Clayton, 2015).

Across many countries, the two major financial aid programs are need-based grants and loans (OECD, 2018). These two types of aid, however, have limited potential to effectively curb social inequality in higher education participation, for two reasons. First, because the monetary amount is often inadequate to cover all education-related costs. Second, and perhaps even more importantly, because students and their families become involved too late: they have to apply to college before even knowing whether they will benefit from financial support. This leaves students and their families, especially those that are most vulnerable, with a lot of uncertainty about the real sustainability of education investments up to the time of college application and admission (Elliott & Lewis, 2018). Incentivized savings programs, such as Children's Savings Accounts (CSAs), may represent an additional approach to encouraging the college participation of students from disadvantaged socioeconomic backgrounds (Elliott & Sherraden, 2013). On top of tackling the economic constraints faced by families in a more thorough way by improving families' financial preparation, they also foster students' (and parents') educational expectations (Beverly, Elliott, & Sherraden, 2013; Kim et al., 2015). Because they start well before the time for formal university application and involve the entire family in a process of saving toward a

long-term goal, incentivized savings programs can enhance a family's investment and planning capability. Moreover, by providing families with more certainty about the real financial sustainability of education investments, they can enhance families' confidence and expectations regarding the concrete possibilities of achieving long-term educational goals.

CSAs are increasingly popular in the United States. As of the end of 2018, 65 programs were active across 34 States (Quezada, Markoff, & Copeland, 2019). While there is abundant correlational evidence in support of these kinds of programs, robust experimental evidence exists only in respect to important, yet intermediate, outcomes such as savings behaviors, child social and emotional well-being, and parental educational expectations (Elliott & Lewis, 2018; Markoff, Loya, & Santos, 2018). Very little is known about the impacts of CSAs on postsecondary outcomes (Long & Bettinger, 2017).

The present study contributes to the literature by providing experimental evidence on the impact of an incentivized savings program on low-income students' postsecondary education participation. The study is based on the results of ACHAB (Affording College with the Help of Asset Building), a demonstration financed by the European Union and carried out in Italy between 2014 and 2017. Relative to the United States—to which most of the evidence has so far been confined—as well as to many other OECD countries, Italy has some peculiar institutional characteristics. First, Italy has one of the lowest rates of people holding a postsecondary degree and one of the highest levels of social disparity in postsecondary education attainment among OECD countries. Second, college costs are modest (with respect to countries such as the United Kingdom and the United States) as public universities charge relatively low tuition fees (averaging €1,000 per year), but financial aid—which mostly consists of needs-based grants and tuition waivers—is often underfunded, and hence, unreliable.

The tested program (*Percorsi*) was run by a private foundation (*Ufficio Pio, Compagnia di San Paolo*) in the metropolitan area of Turin (Northwest Italy). 716 students participated in the study and were randomly allocated to either the treatment or the control group. Program participants received an unusually generous subsidy in the form of a match rate on family savings. The savings match was strictly conditioned on education-related purchases (i.e., a 4:1 match rate for college-related expenses and a 2:1 match rate for education-related expenditures incurred during high school). The total amount of savings was capped at 2,000 euros, so that each participant had available a maximum of 10,000 euros. Moreover, participants were offered targeted financial education classes.

The experiment results point to sizeable and significant impacts of the program on university enrollment (+8.7 percentage points; pp, henceforth) and persistence to the second and third year (+8.9 pp and +11.3 pp, respectively). Data on completion are not available but it is argued that persistence is a good proxy for completion, considering that, in Italy, the largest number of university dropouts take place during the first year (ANVUR, 2018). Heterogeneity analysis reveals that the program's impacts were significantly larger for students coming from vocational schools, which are the least college-oriented school track in the Italian secondary education system. Cost-effectiveness estimates further support the conclusion that the program worked best for the most disadvantaged students: To induce one additional student to enroll at university, the program had to support 7.7 students who would have gone to college anyway, versus only 2.2 when restricting the focus to students from vocational schools. Finally, even if the estimated treatment effects for higher versus lower income families are not statistically distinguishable, the results show that subjects from households whose income fell below the

sample median saved less and gained less from the program than their counterparts with higher income, pointing to a possible issue of regressivity in the program.

THE MOST COMMON FINANCIAL AID POLICIES

Governments around the world typically employ two broad financial aid approaches to try to make university education more affordable for everyone and thus support low-income students in paying for the living and educational costs associated with higher education participation. The first option is providing students with direct financial aid in the form of need-based grants. The second option is providing students with the opportunity to borrow money at low interest rates by giving them access to subsidized loans (OECD, 2018).[1]

Experimental and quasi-experimental evidence on the effectiveness of these financial aid tools is mixed (Deming & Dynarski, 2010; Dynarski & Scott-Clayton, 2013; Herbaut & Geven, 2020; Page & Scott-Clayton, 2016; Scott-Clayton, 2015). Concerning need-based grants, most studies based in the United States point to positive but limited impacts on college enrollment, persistence, and completion (Bettinger, 2015; Castleman & Long, 2016; Denning, Marx, & Turner, 2019; Goldrick-Rab et al., 2016). A common conclusion is that the amount of the grant is a key aspect for financial aid effectiveness (Herbaut & Geven, 2020). This conclusion is also reached when looking at evaluation studies conducted in Europe, where tuition fees are typically lower. Relatively generous need-based grants were found to increase enrollment rates in Germany (Lauer, 2002; Steiner & Wrohlich, 2012), France (Fack & Grenet, 2015), and the UK

[1] Tax benefits, public university funding, and merit-based grants are additional methods employed by governments to reduce college costs for individuals. They are not treated here because they are generally considered to have a lower effectiveness potential to redress inequality and they are often regarded as possibly regressive tools (Bergman, Denning, & Manoli, 2019; Elliott & Lewis, 2018). Merit-based grants are not considered because, by being conditional on past school outcomes, which are linked to social origins, they may increase inequalities (Orfield, 2002).

(Dearden, Fitzsimons, & Wyness, 2014), as well as to reduce first-year dropout rates in Italy (Mealli & Rampichini, 2012; Modena, Rettore, & Tanzi, 2020).

Regarding loans, the available evidence points to some short-run positive impacts on enrollment, persistence to the second year (McKinney & Burridge, 2015; Solis, 2017), and performance in terms of GPA and credits (Marx & Turner, 2019; Wiederspan, 2016).[2] Wiederspan (2016) also finds loans have positive effects on math and science course completion. Other studies, however, find that loans have null effects on completion (Dowd & Coury, 2006; Malcom & Dowd, 2012). Loans are often seen as a palatable financial aid option as they shift costs from government to the individual and thereby make it possible to reach a larger number of students. However, there is evidence that—if the repayment plan is not adequately income-driven—they may lead to a spiral of debt and debt delinquency, especially for households in financial distress (Akers & Chingos, 2018).

Financial aid programs are of course very different in form and come with specific, differing features. It is far beyond of the scope of this study to attempt to review them comparatively. However, some common limitations can be identified (Elliott & Lewis, 2018). Current financial aid programs focus only on the liquidity constraints faced by university students (and their families) in order to pay for college without intervening on students' and families' financial preparation and educational expectations. This happens because both loans and grants are activated too late, often when students formally apply to university. Up until that moment, low-income households are not supported to invest in children's education. These families are often also families with low levels of education and their educational expectations

[2] Barr, Bird, and Castleman (2019) show that an intervention aimed at reducing loan borrowing led to negative consequences, reducing academic performances and increasing the risk of default.

are low and will remain low if no external support or promise of support is given.[3] Furthermore, families that do not commit early to education investment plans are not financially ready for college and hence will continue to see long-term educational investment as too risky and uncertain (Beverly, Elliott, & Sherraden, 2013; Elliott & Lewis, 2018). A similar conclusion is also reached by Herbaut and Geven (2020, p. 10), who, at the end of their review of financial aid programs, state that "an early commitment of aid, while students are still in high school, leads to much larger impact on higher education access."

AN ASSET-BASED APPROACH TO FINANCIAL AID

An answer to the needs unmet by current financial aid may come from Individual Development Accounts (IDAs). IDAs are financial vehicles aimed at fostering low-income families' long-term development goals (Sherraden, 1991). These programs provide low-income families with incentives to save small amounts of money regularly toward long-term asset purchases such as business capitalization, purchasing a home, and postsecondary education. The incentive provided is typically a match rate mechanism, which works in such a way that every dollar saved by the participant is topped-up by the program, with the restriction that the money is used to pay for the allowed expenses.[4]

Over recent years, a specific form of IDA, known as Children's Savings Account (CSA), has spread across the United States (Quezada, Markoff, & Copeland, 2019) and other countries,

[3] It is worth mentioning that experimentations in the vein of making college free (a.k.a. Promise Programs) are blooming in the United States. These programs show some potential partly also because they clearly convey the message to families that their children's college will be affordable well in advance of the college enrollment decision (Andrews, Des Jardinis, & Ranchhod, 2010; Bozick, Gonzalez, & Engberg, 2015; Deming, 2019; Gurantz, 2020).

[4] Other savings incentives typically used are initial deposits (seed), savings targets, and automatic progressive subsidizes (Elliott, 2018; Markoff, Loya, & Santos, 2018; Sherraden, Clancy, & Beverly, 2018).

such as Canada and Singapore (Sherraden et al., 2018). As of 2018, over 450,000 children in the U.S. were enrolled in a CSA, a 46 percent increase from 2016 (Quezada, Markoff, & Copeland, 2019).[5] These programs provide participants with the same incentivized savings mechanisms as IDAs but differ insofar as they specifically target children (typically from very early ages, even birth) and have the specific purpose of increasing family investments in children's postsecondary education (Elliott & Lewis, 2018).

Matched savings programs can have two main comparative advantages over the more classical forms of financial aid, such as grants and loans. First, by stimulating stronger and longer-lasting family commitment, they are expected to trigger parents' expectations, nurture children's attitudes toward education, and make the entire family more confident about the actual sustainability of long-term education plans (Assets and Education Initiative, 2013; Beverly, Elliott, & Sherraden, 2013). Second, these programs impose a strong conditionality in the use of the monetary benefits, relying on the assumption that constraining families' use of the provided financial resources for postsecondary education expenses is a more effective approach to reach that particular investment goal than the alternative option of leaving beneficiaries free to use their savings for multiple purposes.

More analytically, the pathways from matched savings programs to college success are depicted in Figure 1. The "financial preparation" channel comprises aspects connected to liquidity constraints as well as a family's capacity to save and use the accumulated money for long-term educational investment goals. Typically, these programs also offer financial education

[5] CSAs should not to be confused with tax-advantaged education savings plans (e.g., the 529 plans in the United States), even if some CSAs in the United States exploit the 529 plan infrastructure (Quezada, Markoff, & Copeland, 2019; Sherraden, Clancy, & Beverly, 2018). If not well targeted or not providing *ad hoc* incentives, plans like the 529 savings plans are typically accessed by wealthier households (Dynarski, 2004) as low-income families see lower benefits from tax incentives.

classes to build families' saving capacity further. A second channel, instead, operates on both parents' and students' college expectations through the so-called college-bound identity (Beverly, Elliott, & Sherraden, 2013; Elliott et al., 2011; Oyserman, 2013).[6] If children and their parents know they have money set aside for their future higher education expenses, they may develop higher and more realistic expectations regarding their college participation and put into practice the actions needed to purse this goal. The development of the college-bound identity could also translate into increased higher education participation indirectly, by positively affecting children's academic preparation (e.g., high school results), which are strong predictors of college success.

[FIGURE 1, ABOUT HERE]

Evidence on the effectiveness of CSAs has bloomed in the past few years (Elliott & Lewis, 2018; Markoff, Loya, & Santos, 2018). Clancy et al. (2016) analyze data from the SEED (Saving for Education, Entrepreneurship, and Downpayment) for Oklahoma Kids (SEED OK) demonstration and find that the program tested—which provided families with an incentive in the form of an initial deposit—increased households' likelihood of opening a savings account (namely a 529 college savings account) and significantly increased savings for college. Similar results were found by Long and Bettinger (2017) in an experimental study aimed at assessing the Early College Planning Initiative conducted in Boston public schools. The authors show that offering families assistance in opening a 529 college savings plan did not affect their engagement in saving for children's college. Instead, what made a difference to savings behavior was providing families with the $50 initial deposit (coupled with assistance) required to open a savings plan.

[6] College-bound identity is a concept developed as an extension of identity-based motivation theory (Oyserman, 2009) and can be defined as "an identity rooted in an expectation of attending college" (Assets and Education Initiative, 2013, p. 31) or an education-focused expectation that can be activated or nurtured by institutions.

Families receiving this incentive were about 22 percent more likely to open an account and 7 percent more likely to sign up for automatic monthly contributions on the account. Positive impacts of a CSA program on accumulated savings and frequency of deposits were also found in the *Bridges to the Future* randomized controlled study, which assessed a matched savings account program in a very different context, namely AIDS-affected children in Uganda (Wang et al., 2018).

The results from the SEED OK study also indicate that CSAs can have a positive impact on children's social-emotional development (Huang et al., 2014) and that they can enhance parents' educational expectations (Kim et al., 2015; Kim et al., 2018). Positive impacts of CSAs on children's educational plans and school marks were also found in the *Suubi* project, a randomized controlled trial study conducted on a sample of orphan children in Uganda (Curley, Ssewamala, & Han, 2010).

Evidence of the effectiveness of CSAs on postsecondary education access and attainment is virtually non-existent, with the only exception being the above-mentioned study conducted by Long and Bettinger (2017). The authors find that young people receiving the savings incentive show similar college enrollment rates as youths in the control group but have a higher—although insignificant, possibly due to sample size—likelihood of enrolling at a four-year college rather than a two-year college, among those who enroll. The fact that the program had no impact on enrollment but a possible impact on the type of college chosen may be due to the fact that the program was open to all families in the Boston public school system, regardless of their income. Affluent families participating in the program may have had very high and consolidated college expectations, thus limiting the room for the program to impact on their decisions. Even under these circumstances, though, the authors conclude that college savings could "lead to more

expensive, and perhaps better, investments in postsecondary education" (Long & Bettinger, 2017, p. 18).[7]

Research has also pointed out some potential weaknesses of CSAs. The first concern is connected to the risk that the poorest households would not be capable of saving even small amounts of money. To counteract this possibility and ensure maximal inclusiveness and progressivity of these tools, practitioners and researchers have experimented with *ad hoc* incentives and support measures such as providing match rates inversely proportional to household income, transferring seed deposits to the poorest households, or linking the savings accounts to rewards cards (Elliott, 2018; Markoff, Loya, & Santos, 2018). Second, as pointed out in the literature on unconditional vs. conditional cash transfers, imposing a conditionality in the use of the matched benefits could divert family resources from other important investment goals and this could have unintended consequences on other relevant outcomes (Baird, McIntosh, & Özler, 2011). Participating in a CSA could, for example, limit family investment in children's early education, which is known to have long-term positive effects on postsecondary education as well as other life outcomes (Deming, 2009; Dynarski, Hyman, & Schanzenbach, 2013). This criticism holds true especially in contexts where the price of high-quality childcare and preschool education is high. Third, saving, in itself, could reduce families' liquidity and thereby their ability to smooth over income shocks. This might increase the likelihood of the family

[7] Evidence showing the potential of incentivized savings mechanisms for individuals' postsecondary education attainment is available for IDAs. These programs, however, are different from CSAs, mainly because they are typically targeted at adults and have multiple purposes (i.e., postsecondary education, purchasing a home, and business capitalization). The existing evaluation studies on IDAs refer to programs implemented in the United States, e.g., the Asset for Independence Initiative (AFI) program (Mills et al., 2019) and the American Dream Demonstration (Grinstein-Weiss et al., 2013), or, in Canada, the Learn$ave Demonstration (Leckie et al., 2010). Overall, these programs show relevant impacts on savings behavior (Elliott & Sherraden, 2013; Leckie et al., 2010; Mills et al., 2019) as well as positive effects on postsecondary education enrollment (Grinstein-Weiss et al., 2013; Leckie et al., 2010).

experiencing financial distress or material hardship, which can also have long-term consequences on children. To avoid this, matched savings programs often allow unmatched savings withdrawals to allow families to face emergency expenses (Mills et al., 2019). A further potential criticism concerns the fact that the savings accumulated in the account may count as financial assets when eligibility for other public social benefits is determined, and this may discourage families' participation or prevent them from accessing important social support (Ratcliffe, et al., 2016). In this regard, it is important that program regulations and the relevant legislation explicitly state that the savings accumulated within these programs do not count when establishing eligibility for other means-tested benefits.

THE ITALIAN CONTEXT

The Italian education system is divided into four stages. Primary school starts at the age of six and lasts five years; it is compulsory and designed to offer the same curriculum to all. Secondary education is composed of two levels. Lower secondary education lasts three years and its curriculum is still undifferentiated. At the age of 14, students have to choose which school type to attend for upper secondary education. The available tracks are academic (*liceo*), technical (*istituto tecnico*), and vocational (*istituto professionale*). All tracks last five years and end with a final exam. Passing this exam serves as a formal entitlement to enroll in tertiary education irrespective of the type of school attended.[8] However, transition to university differs widely across tracks. While 92.2 percent of academic track students enroll at university, this falls to 40 percent for technical school students and drops even further to 20.9 percent for students from

[8] In addition to these three school tracks, there are also some vocational training programs, managed by local governments, that offer shorter, typically three-year long, programs that do not grant access to university.

vocational schools (ISTAT, 2016). The choice of high school is therefore very significant for young people's future outcomes. It is no surprise that this choice is also closely linked to family background: 70.7 percent of students with tertiary educated parents choose an academic track against only 44.6 percent of pupils whose parents have at least an upper secondary qualification, and 24.8 percent of those whose parents only completed compulsory education (Contini & Triventi, 2016).

Italian tertiary education is based on a sequential system that comprises a three-year Bachelor's degree (*laurea*) and a two-year Master's degree (*laurea magistrale)*.[9] This design emerged as one the outcomes of the Bologna Process, an attempt to harmonize the structure of tertiary education across European Union Member States. Actually, the Bologna process left out of the 3+2-year system programs such as Veterinary, Dentistry, Pharmacy, Architecture, and Law school, which follow a single-cycle five-year degree, while medical schools offer a single-cycle six-year degree and Primary Education Science offers a four-year degree. This is the main reason why, with the current available data, we cannot yet investigate the impact of *Percorsi* on college completion. Italian public universities' tuition fees are a small fraction of those charged in other countries (e.g., the U.S.), with tuition averaging €1,000 euro per year (OECD, 2018; European Commission/EACEA/Eurydice, 2018).[10] However, these low tuition fees, combined with other costs (such as books, transportation, software, and Internet access), leaving aside foregone earnings, result in an average annual cost ranging between €2,500 and €3,000, which might not be affordable by families in financial distress (Barone, Abbiati, & Azzolini, 2014).

[9] Postsecondary non-tertiary courses exist (i.e., *Istruzione e formazione tecnica superiore*, higher technical education and training) but are attended by only 14,000 students, while Bachelor-level students number more than one million (ANVUR, 2018).

[10] Private universities also exist and enroll about 12 percent of the entire university student population (source: Italian Ministry of Education; http://ustat.miur.it).

In Italy, the main program for funding university participation is the so-called *Diritto allo Studio* ("Right to Education"). The program is regulated at the national level and is co-financed by regional governments. It provides eligible students with a complete tuition fee waiver and with grants whose amount varies depending on family income. Eligibility is primarily based on family economic conditions, although, to maintain the grant, university students are also required to meet certain exam and credit requirements. Two main concerns regarding *Diritto allo Studio* are, first, that the program is accessed only by those who have already decided to enroll in a university (hence, it can hardly have an impact on enrollment decision) and, second, that its funding is low, geographically heterogeneous, and unpredictable from one year to the next. In other words, being eligible does not mean automatically being a beneficiary. In the 10 academic years since 2007/2008, the yearly share of grant recipients among eligible students has ranged between 68 percent and 95.7 percent at the national level. Variability has been even larger in the region where *Percorsi*, the matched savings account program evaluated in this paper, was implemented (i.e., Piemonte), where it ranged from a minimum of 30.8 percent to a maximum of 100 percent.[11] Beyond this national-regional scheme, there are only a few small student aid programs funded by local governments or private foundations. However, these interventions are not systematic and are rather scattered throughout the country. Finally, in contrast to countries such as the United States, the United Kingdom, or Sweden, fewer than 1 percent of Italian students benefit from a subsidized loan (European Commission/EACEA/Eurydice, 2018).

The limitations of the Italian financial aid system are coupled with an overall picture of low educational attainment in the country. Italy ranks last among OECD countries in respect to the share of the adult population with a tertiary education degree (OECD, 2018). In 2018, less

[11] Data from *Osservatorio Regionale per l'Università e per il Diritto allo studio Universitario* (Regional Observatory for University and Right to Education). Website: http://www.ossreg.piemonte.it/default_en.asp.

than 30 percent of the population aged 30 to 34 had a tertiary degree, while the EU-28 average is about 41 percent. This fact is also linked to high university dropout rates: Ten years after enrollment, 4 out of 10 students leave or have left the university without a degree (OECD, 2018). The problem of dropout is particularly severe in the first years of university. According to administrative data, about 20 percent of enrolled students leave during the first two years (ANVUR, 2016). Students from disadvantaged socioeconomic backgrounds tend to drop out more frequently (Aina, 2013), while the type of high school attended has been found to affect the risk of dropping out even more than the decision to enroll (Di Pietro, 2004). Social disparities in college attainment in Italy are very high: The country has one of the highest gaps in higher educational attainment between individuals with at least one university-educated parent and those whose parents did not attend higher education among the OECD countries.[12]

THE PROGRAM

The ACHAB demonstration was built upon an already existing program called *Percorsi*. *Percorsi* is a matched savings account program implemented since 2010 by the *Ufficio Pio* of the *Compagnia di San Paolo*, a foundation based in Turin (Northwest Italy). The primary aim of *Percorsi* was to help families hit by the economic crisis by supporting them with the costs of their children's education. The program was slightly adapted for the ACHAB experimentation, which took place between 2014 and 2017. During that period, the program was targeted at students residing in the metropolitan area of Turin who were enrolled in the final two years of high school (grades 12 and 13) and who came from low-income families. The income threshold

[12] This information has been obtained by the authors' own elaboration of data from the OECD.Stat data warehouse. More precisely, we used the "Intergenerational mobility in education" data, which can be retrieved via the following link: https://stats.oecd.org/Index.aspx?DataSetCode=EAG_MOB.

was set at 25,000 euros of household equivalent annual income (ISEE index),[13] which corresponds to approximately 150 percent of the poverty level for four-person households. Table 1 provides an overview of the main features of the program.

[TABLE 1, ABOUT HERE]

Each participant had the chance to open a dedicated bank account and was expected to save between five and 50 euros each month for a maximum period of six years.[14] Participants were obligated not to skip any more than two consecutive months and to attend three modules of financial education. The maximum deposit allowed was 2,000 euros, which could be matched at a 4:1 rate if used for university-related expenses (e.g., fees, transport, computer and internet, study materials, etc.) or at a 2:1 rate for the same types of education-related expenses incurred during high school. Hence, savings could be supplemented by a maximum match of 8,000 euros, so that the funds available to pay for university could reach 10,000 euros. This amount is above the available estimates of the average costs of completing a three-year university level in Italy (excluding rent).[15]

Several of the program's features were meant to enhance participants' savings behavior. First, the program aimed at fostering the habit of saving by imposing a fixed savings scheme with regular deposits. Second, in order to discourage opportunistic behaviors, the program included "waiting time" mechanisms between deposits and purchases—i.e., participants could access the matched savings only four months after program entry and, afterwards, always had to

[13] *Indicatore della Situazione Economica Equivalente* (ISEE) is the national index of households' equivalent economic condition, which is used to define eligibility for social benefits. For the sake of simplicity, throughout the paper we sometimes use the term income to refer to this index.
[14] Each yearly call allowed only one application per household, but families could apply multiple times in different years for different siblings.
[15] Practically, in order to access the matched payments, participants have to fill in a formal request on the program's dedicated web page, specifying the amount and the purpose of the expense. The program staff examine the request and notify participants about the eligibility of the request. If there is a positive response, participants can access the requested sum and are allowed to spend it.

wait for two months before using their savings and exploiting the match rate mechanism. Third, the program aimed to increase participants' awareness of the importance of saving to purchase long-term assets by requiring students and their families to participate in dedicated financial instruction classes. Furthermore, as an additional way to foster the habit of saving and financial management competences, participants were able to monitor their savings anytime they wanted by accessing the account web page, where they had to submit all their purchase requests and the required documentation.

Savings could not be withdrawn before the end of the project except for permitted educational expenditures, but deposits were at participants' disposal at the end of their program participation. Importantly, participation in *Percorsi* did not prevent participants from being eligible for standard financial aid and, as a matter of fact, all participants in *Percorsi* were also eligible for *Diritto allo Studio*. The educational expenditures funded by the program could not benefit from tax deductions and the accumulated savings counted as financial assets and hence, in principle, reduced subjects' opportunity to access other means-tested benefits. However, the amount of savings was not high enough to represent a real concern in this regard.

THE DESIGN OF THE RCT

Recruitment and Targeting

Potentially interested households were contacted via a massive social media and school-based recruitment campaign that took place in Fall 2014 for the first experimental cohort (made up of 12th and 13th graders) and in Fall 2015 for the second cohort (composed of 13th graders only).

To sign up to the program, students had to fill in an application form that also served as the study's baseline survey. Overall, 1,340 (1,185 valid) applications were collected.[16]

Program oversubscription provided room to implement a targeting strategy aimed at selecting, among the pool of all eligible applicants, those who were expected to be more likely to benefit from the program and excluding those who were less likely to benefit from it. In other words, targeting was meant to give priority access to the study to those who were more at risk of giving up their higher education plans because of economic reasons.

The targeting strategy applied aimed to identify two groups of students: Those who would not go to college even if treated (hereafter labeled as "never enrollees") and those who would have gone to college even if assigned to the control group (hereafter "always enrollees"). Never enrollees were identified based on how they answered on two application form questions related to higher education enrollment intentions. More precisely, never enrollees were those students who declared no intention to enroll at university and those who were undecided for reasons that were not economic, but related more to a lack of interest or the low value they attached to postsecondary education. The *a priori* expectation was that relatively few never-enrollees would be intercepted. However, because in addition to the 4:1 multiplier, the program also provided a lower multiplier of 2:1 for school-related expenditure while in high school, the presence of applicants with no explicit intention to enroll at university but who were still interested in the program could not be ruled out. In total, 52 students (about 4 percent of all valid applicants) were dropped from the analysis as never enrollees.

[16] 130 applications (110 in the first cohort and 20 in the second cohort) were rejected due to formal errors. The largest fraction was eliminated before the first randomization, while 54 were identified and eliminated during the randomization process and excluded from that moment on from the study. A further 25 students belonging to cohort 1 and assigned to the control group repeated their application in the subsequent school year: 14 of them ended up in the control group again, while 11 ended up in the treatment group. We consider these 11 cases as crossovers.

The identification of always enrollees was achieved through the prediction of each applicant's probability of enrolling in university. External microdata were used to model university enrollment and identify its main determinants in the population of high school students (the full model is included in Table A1 in Appendix A).[17] The estimated coefficients were applied to each ACHAB applicant's characteristics in order to obtain individual predictions of university enrollment. Finally, ACHAB applicants were ranked in ascending order by their predicted enrollment probability. Based on budget capacity, the first 716 students were admitted to the study and randomized, while the remaining 417 students (i.e., those with higher predicted probability of enrollment) were excluded. Thus, the enrollment probability thresholds for admission were obtained empirically based on the number of available slots in the two experimental cohorts and varied from 0.675 for the first cohort to 0.826 for the second.

Randomization

The 716 students retained after the targeting procedure were subject to randomization with a treated/control ratio of 0.68. To reduce unexplained variation in the outcomes of interest and reduce the risk of unhappy randomization, a blocking randomization design was implemented (Bloom, 2006). Nine blocks were formed based on experimental cohort (2014 and 2015), grade (12th or 13th), and school track (academic, technical, or vocational). The choice of high school track was mainly due to its strong predictive power of students' university enrollment. To guarantee that the treated cases had a balanced distribution across blocks, subjects were randomized within each block to reach block-specific targets.

[17] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Data Collection

Outcome data were collected with follow-up surveys conducted via CATI (Computer-Assisted Telephone Interviewing) by an external agency. These surveys took place in the Spring and Fall of 2016 and 2017 (Table C1 in Appendix C)[18] and collected information on: i) enrollment (i.e., university enrollment right after high school); ii) persistence to the second year (i.e., enrollment in the second year); ii) persistence to the third year (i.e., enrollment in the third year). These outcomes are coded as dichotomous variables. University enrollment takes the value of one if the student is enrolled at the university and zero otherwise.[19] The two persistence indicators take the value of one if the student is enrolled in the second (third) year and value zero for those who dropped out or did not enroll at all at university after high school. This means that the analyses in which persistence is used as the outcome variable are not conditional on enrollment.

Given the high incidence of university dropouts in the country, results regarding graduation could, in principle, be different from those regarding persistence. Data on graduation are not available for this study, but we argue that the risk of having diverging results is limited, for two main reasons. First, according to ANVUR (i.e., the Italian National Agency for the Evaluation of Universities and Research Institutes) data, the largest number of dropouts occurs in the first year (14 percent). In the following years, dropouts increase, but at a much lower pace (at an average rate of 3.5 percent per year). Moreover, using external microdata (ISTAT, 2015, survey on high school leavers: *Percorsi di Studio e di Lavoro dei Diplomati*), we calculated that the likelihood of still being enrolled or having completed university four years after enrollment

[18] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[19] Enrollment in postsecondary non-tertiary courses is marginal and not counted as participation in higher education, as the 4:1 matching grant provided by *Percorsi* was strictly limited to attending university institutions.

of a comparable sample of students to the ones participating in our study who persisted to the second year is about 90 percent.[20]

Integrity Checks

The assessment of the statistical equivalence of the treatment and controls groups is based on a number of individual and family characteristics collected in the application form. Table B1 (see Appendix B) displays the balancing tests for the full sample. The first two columns of Table B1 report the mean values for each of the characteristics, while the third column reports the p-values of the t-tests of the differences. The two groups appear to be well balanced, with the only exception of the lower secondary school final mark, on which control students performed slightly better.[21] The estimation models include covariates to adjust for possible differences between the two randomized groups.

The CATI interview process went smoothly: The non-response rates were low and the treated-controls differential attrition was well below the usual standards in the field (What Works Clearinghouse, 2014) (see Tables C1 and C2 in Appendix C). Nonetheless, the balancing test shown in Table B1 was conducted again on the Follow-up 1, Follow-up 2, and Follow-up 3 respondent samples and confirmed the overall equivalence of the two randomized groups (see Appendix B).

Figure 2 summarizes the experiment flowchart, showing the path from the application to the Follow-up surveys.

[20] See Online Appendix G for details. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[21] This unbalance is there for academic track students and, marginally, also for 13th graders, but not for the vocational and technical track students or 12th graders (see Tables B2 to B6 in Appendix B).

[FIGURE 2, ABOUT HERE]

Estimation

The program's impacts are estimated through linear probability models based on the following specification:

$$Y_i = \beta_0 + \beta_1 \cdot Z_i + \beta_2 \cdot B_i + \beta_3 \cdot X_i + \varepsilon_i \ ,$$

where *Y* is the outcome of interest, *Z* is the treatment assignment, *B* stands for the blocking variables (school track, call, and cohort of entry), and *X* stands for the covariates included to increase the precision of the estimates.

The parameter of interest in the above equation (β_1) identifies an intent-to-treat (ITT) effect. Because non-compliance with the treatment assignment was very low (i.e., only 11 crossovers and zero no-shows), local average treatment effect (LATE) estimates obtained using a Wald estimator would be very similar to the ITT estimates.

One possible threat to identification is posed by the presence of potential spillover effects that may lead to the violation of SUTVA (Stable Unit Treatment Value Assumption). We argue that this threat is limited for two reasons. First, students in the study came from 125 different schools (unfortunately, with our data, we cannot identify the classes). About 10 percent of control students attended schools with no students in the treatment group and about 60 percent of controls attended schools with at most four treated students. Second, the potential spillover effects are weak considering the nature of the program, which gives access to non-transferrable savings, thus limiting the room for contamination to expectations and motivations.

RESULTS

Implementation

Table 2 shows descriptive statistics that are informative about the extent to which participants actually took part in the various services provided by the program. All subjects assigned to the treatment group complied with the conditions and opened the *Percorsi* bank savings account. Nearly all of them (94 percent) also made at least one deposit on the account and saved, on average, 33 euros every month, accumulating 1,088 euros.[22] Hence, even if the available data do not make it possible to investigate the possible savings shifting from other accounts to the account funded by the program, these figures suggest that the program was successfully implemented and that participating subjects made effective use of the savings account provided.

[TABLE 2, ABOUT HERE]

Average matched grants are mean per-person euros that the program paid to match participants' accumulated savings. This estimate is computed as the sum of (a) the matched money actually spent by May 2019 and (b) the amount of money that students still enrolled in the program would receive if they continued to save at the same rate as the previous 12 months of program participation until the end of the project (up until they reach the program's cap) and used all their savings for education expenses. Note that this sum does not equal the maximum theoretical sum that the program could pay, because 26.3 percent of participants used the 2:1 match rate during high school (corresponding to 5.7 percent of the total admitted expenditures). Also, our calculations could be overestimated, because we assume that beneficiaries would exploit all available program resources in the remaining months of the program's activity—i.e., they would

[22] Because the six-year (72 months) program duration had not yet expired at the moment when the program administrative data were extracted (May 2019), these estimates refer to the first 53 or 41 months of program participation, depending on whether subjects belonged to cohort 1 or 2.

actually remain enrolled at university and purchase eligible educational goods or services.[23] In addition to savings and expenditures, participants also showed very high levels of participation in the financial education classes. 96 percent of beneficiaries took part in at least one class of at least one of the three modules offered and, on average, they attended 2.2 modules out of three. Considering that data on the third module was not yet available for the second cohort, these figures may be underestimates of the actual financial education participation.

Main Treatment Effects

All impact estimates are presented as regression-adjusted differences in means. We start with the overall impact of the program on college enrollment and persistence and then move on to analyze the program's heterogeneous impacts, cost-effectiveness and the issue of regressivity.

Figure 3 reports the overall impacts of the program on university enrollment and persistence. The impact on university enrollment is estimated at 8.7 percentage points (pp). This estimate is statistically significant and sizeable. Considering the control average of 67.1, the program increased enrollment rate by about 13 percent. The effect of the program remains positive and significant even when we consider persistence to the second (+8.9 pp) and to the third year (+11.3 pp). It is important to stress that the program's effects do not vanish over time and are found on both enrollment and persistence, as one of the criticisms directed toward financial aid programs is that they may increase the college enrollment rate of students with a high risk of dropping out because of a mix of low motivation, bad preparation, or economic reasons.

[23] Alternative estimates not shown here (e.g., the consideration of the savings patterns of the entire program's duration instead of the last 12 months, or the use of median instead of mean values) lead to substantially similar results.

[FIGURE 3, ABOUT HERE]

A further possible concern is that the program induced participants to enroll in "easy" or less rewarding college programs. Table E1 in Appendix E provides some evidence against this hypothesis, showing that there are no significant differences in program impacts across university fields of study.[24]

Heterogeneous Treatment Effects

A substantial high school track heterogeneity is hidden behind the program impact estimates (Figure 4). Impacts on enrollment are stronger for students from the vocational track (+20.5 pp) than for those from an academic one (+9.1 pp), while the effect for the technical track is not statistically significant. Similar results are found when considering persistence to the second year, where a significant impact is found for students from vocational schools only. Differences between the effects for vocational school students and the effects for the two other types of schools are statistically non-significant for enrollment and marginally significant for persistence.[25] Hence, the program worked well for students from the vocational track, who usually show the lowest likelihood of enrolling at university due to of a mix of disadvantaged family backgrounds and weak academic preparation. Once again, it is encouraging that the program had an impact not only on college enrollment but also on persistence for this

[24] By reducing households' financial constraints, the program may also have had an impact on students' decision to enroll in a university outside the region, as also found for other financial aid programs in Italy (Vergolini & Zanini, 2015). Overall, the share of "movers" (i.e., sample students who enrolled at any university outside the region) is relatively small (i.e., 6.8 percent). Consistent with our expectation, we observe that treated students enrolled at a higher rate outside the region—8.5 percent vs. 5.5 percent among the controls. However, we do not have the statistical power to state that this difference is significantly different from zero.

[25] Regarding persistence to the second year, the p-values for the comparison between vocational track and the academic and the technical ones are 0.062 and 0.057, respectively. We do not consider persistence to the third year, because this outcome is available only for 13th graders in cohort 1 and the sample size is too small to run further stratified models.

particularly weak segment of the student population.

[FIGURE 4 ABOUT HERE]

An unexpected result is the null effect for technical track students. These students are placed midway between vocational and general school students on a long list of relevant predictors of college participation from academic preparation to family background. Hence, we would have expected a positive effect sized somewhere in-between those estimated for academic and vocational tracks for these students. The reason for the null effect may be related to the good match between technical school students' competences and the local labor market demand, which is characterized by a developed industrial sector. But more research would be needed to investigate this result further.

Another meaningful source of heterogeneity is provided by the grade during which participants entered the program. All else being equal, those who entered the program while attending the 12th grade should be able to accumulate more savings and, consequently, gain more from the program in terms of college enrollment relative to those who started the program one grade later. Heterogeneous models by length of program exposure during high school would seem to confirm this expectation, even if, because of the limited sample size of 12th graders, the evidence is not very strong. Moreover, because the starting grade was not randomized, and because all 12th graders belong the first experimental cohort, any difference in program impacts across grades is likely to hide some compositional effects.

Finally, as reported in Table F1 (Appendix F), the program's impacts are found to be weaker for participants with a higher predicted probability of enrollment relative to those with a lower predicted likelihood of enrolling in college. Even if the evidence is weak, due to the limited sample size, these results are in line with our expectation that the program is more

effective for the subgroup of students with a lower "natural" likelihood to continue their education after high school.

Cost-Effectiveness

Because resources are limited, decisionmakers need to compare alternative programs by considering not only their effectiveness but also their cost-effectiveness. Once it is established that a program has had an impact, its cost-effectiveness—defined as the cost needed to produce a one-unit impact—depends on three factors: the size of the impact, program costs and the so-called "deadweight." In our case, the deadweight is captured by the natural college enrollment rate (i.e., the enrollment rate observed among the controls) and expresses the percentage of students who would have enrolled at university even in the absence of the program. Hence—if the program's impact size and costs are held fixed—the higher the deadweight, the lower the program's cost-effectiveness.

We compute two cost-effectiveness indices. The first is the simple ratio between the program's deadweight and the impact. The value this index takes expresses the number of students that the program needs to support in order to induce one additional student to enroll at university. The lower this number, the better the program's cost-effectiveness. Overall, the program had to support 7.7 students to induce one additional student to enroll at university (Table 3).[26] In other words, 7.7 students received the program's support to do something that they would have done anyway. When compared with other financial aid programs (see the review by Herbaut & Geven, 2020), the program performs substantially better than the median

[26] We calculate cost-effectiveness only for the first outcome, i.e. enrollment. However, the same estimate can also be replicated for university persistence.

program.[27] Furthermore, the estimated index is clearly smaller for vocational school students (2.2) and larger for those from academic schools (8.5).[28] This heterogeneity is the result of a combination of larger impacts and lower deadweight for the vocational student population.

[TABLE 3 ABOUT HERE]

The second cost-effectiveness indicator adds program costs into the calculation and is obtained by simply multiplying the estimated average matched euros by the "cost-effectiveness I" indicator.[29] The resulting cost for one unit of impact (i.e., one additional student enrolled), obtained by adding the mean per-person cost, amounts to 41,908 euros. The same cost turns out to be substantially smaller for vocational school students (10,601 euros), reinforcing once again the conclusion that targeting vocational school students would be the best choice for the program's cost-effectiveness. However, part of this cost-effectiveness gain may be due to vocational school students' lower savings capacity, as suggested by the comparison of the mean per-person costs of vocational school students (3,364 euros) vs. those of academic school students (5,732 euros). This result opens a question regarding equity, which is addressed thoroughly in the next section.

Regressivity

A well-known issue of asset-building programs is related to the lower savings capacity of low-income households. This problem is largely taken into account by setting strict income-based

[27] The financial aid programs included in the cited review show a median cost-effectiveness index of 17.2. Unfortunately, cost estimates for all programs are not available, thus making it impossible to compare program costs. More details on this comparison are provided in Appendix H. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[28] The index for technical school students is not relevant, as the program had no impact on this subgroup.

[29] When calculating program costs, we do not consider other fixed costs (e.g., recruitment, management, personnel, financial classes, etc.) (Schreiner, Ng, & Sherraden, 2006; Ssewamala et al., 2018) because these costs can vary widely across organizations and contexts and hence would make comparability with other programs more difficult.

eligibility criteria, aimed at excluding students from more advantaged socioeconomic backgrounds. Yet, even among low-income households, there may be systematic heterogeneity in savings. Table 4 shows that students from wealthier families (i.e., families with a household equivalent income above the sample median) save more than their poorer peers. More precisely, they deposit more money in the account (37.4 vs. 28.8 euros on a monthly basis) and, therefore, benefit more from the program. Students above the income median spend about 5,700 euros on education: about 43 percent more than their lower income peers.

[TABLE 4 ABOUT HERE]

The ACHAB experiment shows that students with a higher relative family income benefit more from the program in terms of enrollment and persistence probability (Table 5). Relatively richer students show a 10 pp significant impact, while those with an income below the median show 6 to 7 pp non-significant impacts. The differences between the two groups' estimates are not statistically significant, hence the existence of real differing impacts across income levels is not confirmed.

[TABLE 5 ABOUT HERE]

Taking the results in Tables 4 and 5 together, a mixed picture emerges, in which lower income households clearly exploit the savings incentives to a lesser extent—possibly due to higher financial constraints on regular savings—but at the same time seem to benefit from the program, in terms of higher college participation, to the same extent as wealthier families. These results could be interpreted in three ways. First, poorer households bear more financial distress in order to achieve the same educational goal (i.e., enrolling in college). Second, lower income students concentrate their savings on strictly necessary educational expenses. This interpretation seems to be confirmed when comparing the expenditures that actually occurred. Lower income students

spend roughly the same as higher income students for purchases such as fees, transportation, meals, and books, while they claim lower expenses on PC/internet, rent (possibly as they are less likely to move out from home), and additional training. These two possibilities may have some unintended consequences on the quality of the educational careers and should be researched further. Third, lower income households are also more often families with lower educational expectations and hence are expected to benefit from the program not only in terms of a reduction of financial constraints but also through boosted educational expectations. The next section attempts to provide more insights into the mechanisms behind the program's impacts.

Mechanisms and Program Components

Theory predicts that CSAs increase postsecondary education outcomes through a reduction in families' economic constraints and increased educational expectations. While the experimental data presented above provide compelling evidence on the overall impacts of the program, they do not allow the two main mechanisms at play to be disentangled.

The existing literature consistently finds positive impacts of CSA programs on educational expectations. However, because of the particular features of the program tested in this paper—i.e., the higher age of participants, the limited time between program enrollment and college enrollment, and the possible positive self-selection of students enrolling in the program—it could be argued that little room was left for the "expectations" channel to operate. To investigate this aspect further, 21 qualitative interviews were conducted between 2017 and 2018 with students belonging to the treatment group who enrolled in college. Given the salience of school track as a predictor of college participation, the sample was equally split across the three high school types. Interviews were conducted in a non-structured format in order to piece

together individuals' biographic narratives, better capturing their perceptions and feelings, and, ultimately, investigating the role of aspirations and economic constraints that shaped educational decisions.

The interviews confirmed the role of economic factors as constraints on educational decisions. Some interviewees referred to older siblings who did not participate in *Percorsi* and had to give up their university plans because of family financial constraints. The students interviewed perceived the standard financial aid (*Diritto allo Studio*) as unreliable and arriving too late. Instead, they thought that the support coming from *Percorsi* would be enough to pay for college and that would allow them to save the money coming from the standard financial aid scholarship without having to depend on their parents. Also, some students said they would have had to work and study had they not been admitted to *Percorsi*, and this would have increased the risk of dropping out. Moreover, they stated that the program was influential in helping them not only to decide whether to embark on a university course or not, but also on what and where they ultimately chose to study, since attending a longer, more challenging degree course would otherwise be impractical for those who needed to hold down a job. Finally, what students seem to have appreciated about *Percorsi* was that they could always check and know how much money they had available for their educational expenses, which gave them a sense of control over their financial means.

In-depth interviews also provided insights about the role played by expectations as possible mediators of the program's impact on college participation. First, interviewees reported that their parents were enthusiastic about the opportunity to participate in the program. In particular, parents were reported to see college as a realistically reachable goal, thanks to the program. Also, students reported developing more realistic expectations. Some students revealed

that being admitted into the program made them feel as though they had been given a second chance or that it was their first taste of success in a school career marked by a lack of encouragement and a history of bad decisions. This sense of achievement then acted as a springboard for further success, providing an important source of motivation for them to go on to university and complete their required exams. The change in students' aspirations was found to be more widespread among students from lower social backgrounds and those who attended vocational schools, who reported having started seriously to consider college only after being admitted to the program. Academic school students, in contrast, had typically already formed some college aspirations and expectations and may have benefited only from the financial channel and the increased self-confidence in pursuing a college career.

A discussion of the role of the different program components (i.e., the account with the savings incentives and the financial planning classes) is also needed. The experimental data fall short in providing insights regarding the independent effect of the two program components, as the intensity or mode of use of the match savings mechanisms or the degree of attendance of the financial courses are endogenous. An investigation of the role of the different treatments is also difficult because, as shown in Table 2, the take-up of the two treatments was very high: 94 percent of students made use of the savings account by making at least one deposit and 96 percent attended at least one class of the three modules offered. Yet, correlational evidence shows that treated students who attended a higher number of classes also showed higher college participation. At the same time, reverse causality is also likely as some classes took place after college enrollment, hence those who did not enroll were not even invited to the classes. Regarding the use of the savings incentives, students who made use of the lower match rate during high school (about one fourth) showed lower college participation. This may signal either

the fact that some students had already decided not to enroll in college and wanted to make use of the benefits made available by the program or the fact that some households faced financial constraints during high school and were compelled to use the lower multiplier, with negative consequences on their capacity to save for college.

In summary, the study does not make it possible to isolate specific mechanisms, nor the independent effects of the two program components. In line with the bulk of studies on CSA conducted in the United States, the qualitative evidence provides hints about the importance of the moderating role of educational expectations beyond economic constraints, especially among students who entered the program with lower college expectations (i.e., vocational students). Regarding the separate impact of the different CSA components, the research is still in its infancy and this study only provides correlational and qualitative evidence concerning the independent effects of the savings incentives and financial education.

CONCLUSIONS

In recent years, Children's Savings Account (CSA) programs have flourished across the United States and in other countries. CSAs are based on the supposition that providing low-income families with conditional savings incentives would raise their children's postsecondary educational attainment. Most existing evidence on the effectiveness of these programs, however, is confined to important yet intermediate outcomes, such as children's academic preparation and parents' educational expectations.

This paper presents fresh experimental evidence on the impacts of a matched savings account program on young people's college participation in Italy. The evaluation results indicate that the program (*Percorsi*) increased university enrollment by 13 percent, persistence to the

second year by 15 percent, and a persistence to the third year by 24 percent. These results suggest that the program had long-lasting effects on students, and did not lead to students enrolling and then dropping out. The analysis also revealed notable heterogeneity in treatment effects with the largest impacts found for vocational school students. Because these students have the lowest likelihood of enrolling at university, due to a mix of poorer academic and social backgrounds, the findings suggest not only that the program worked for low-income families, but that it worked even better for the weakest students in this sub-population. Cost-effectiveness analysis further reinforces this conclusion. To induce one more student to enroll in college, the program would have had to support 7.7 students who would have gone to college anyway—a relatively good performance compared to other financial aid programs (Herbaut & Geven, 2020)—which is further improved (2.2 students) when restricting the focus to vocational school students.

Hence, the ACHAB demonstration clearly shows the potential for matched savings mechanisms to enhance the effectiveness of higher education financial aid and, ultimately, reduce social disparities in higher education participation. As usual, the study does not come without limitations and opens questions for future research.

First, the research draws attention to the issue of regressivity that arises as a consequence of the fact that—even if there is no evidence of significantly different impacts on college participation according to family income—lower income households struggled to save to the same extent as relatively wealthier households and hence received lower amounts of matched funds. The open question regards identifying the conditions—or the program services and targeted support measures—that need to be put in place in order to make sure that matched savings programs are effectively fully inclusive and progressive tools. The literature on CSAs

provides several suggestions for this, such as letting the match rate vary by income, providing lower income households with an initial seed deposit (seed), activating automatic progressive subsidizes, offering targeted savings options, or applying savings caps.

One limitation of the study relates to its external validity. The ACHAB participants underwent three levels of selection: First, only students from low-income households were entitled to apply; second, the program adopted an opt-in enrollment procedure that discouraged applications from students who were not at all interested in postsecondary education; third, applicants with a high predicted probability of enrollment were considered as "always enrollees" and excluded from the study, with the assumption being that the program would not have had an impact on them as they would have gone to college anyway. Taking the first two selections together, the results can be generalized to high school students from low-income households with at least some college aspirations, as they decided to apply to the program. All in all, this makes participants very similar to standard financial aid beneficiaries, as the income threshold is similar and standard aid is also an opt-in program. On the other hand, the implementation of a targeting mechanism based on applicants' predicted probability of enrollment narrowed down the reference population to the lower tail of the overall financial aid beneficiary population (i.e., students with the lowest predicted probability of college enrollment). In this regard, an important recommendation of the study is that proper targeting strategies are put in place in order to, first, target the program to students who are really "in need" and, second, increase the program's cost-effectiveness, avoiding "wasting resources" on students who are not responsive to the program's incentive and would go to college anyway. This conclusion is in line with the heterogeneous impact models, which show that the program was more effective on the subgroup of students with the lowest prospect of college enrollment (i.e., vocational school students).

A second dimension on which to assess the study's external validity relates to the transportability of the results to contexts that are characterized by different (higher) costs of higher education or different (more reliable) financial aid systems. On the one hand, the ACHAB results can be linked to the growing experimental results coming from the United States and developing countries, which find consistent impacts of different kinds of incentivized savings programs (i.e., CSAs, IDAs, and college savings plans) on intermediate educational outcomes and, in some cases also, on postsecondary education, suggesting that incentivized savings programs may work to enhance postsecondary outcomes of children of low-income families in very institutionally and socioeconomically different contexts. We might speculate that, in contexts where the price of college is high (such as the United States), programs may be more effective if they allow low-income families to accumulate more savings either by starting earlier or by providing stronger and more substantial incentives. In general, an increased effort to improve the (international) comparability of programs (which employ different incentive mechanisms along with a variety of other different components) is in order to enhance existing knowledge and to provide a better basis to inform future research and policymaking.

Further, the small sample size and the unavailability of data on college graduation require some caution to be exercised in the interpretation of the results, and call for more research or replication studies to be undertaken in the future. The small sample size limited the possibility of exploring in a robust way the existence of heterogeneous impacts and, even if it is argued that, based on dropout patterns in Italy, persistence is a good proxy for completion, our measure of second-year persistence was collected only on one of the two experimental cohorts.

Finally, the experimental data could not help us disentangle the channels behind the estimated impacts. Qualitative interviews suggest that students benefited from a reduction in

economic constraints but that the program also contributed to increasing and corroborating their and their parents' educational expectations, especially among vocational school students. More research, employing mediation analysis methods or multi-arm RCT designs, is needed to improve our understanding of the specific mechanisms through which matched savings programs enhance beneficiaries' higher education participation, as well as to identify the independent effects of the program's components (i.e., savings incentives and financial education).

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TABLE AND FIGURES

Table 1. Main features of the *Percorsi* program.

Eligibility	• High school students (12th and 13th grades) • Low-income: below 25,000 euros of family equivalent annual income (ISEE index) • Residing in Turin metropolitan area
Recruitment and Enrollment	Opt-in: recruitment campaign & application
Allowable match uses	Only education-related purchases validated by program's staff
Program-provided Initial Deposit (Seed)	No
Savings period	• Min: 4 months • Max: 6 years
Monthly deposits	• Min: 5 euros • Max: 50 euros
Savings cap	2,000 euros
Match rate	4:1 university, 2:1 high school
Emergency (unmatched) withdrawals	Not allowed
Financial education	27 hours split in three modules

Table 2. Key figures on program implementation.

Take-up statistics and program's services usage	Value
Opened the savings account	100%
Made at least one deposit	94%
Average (median) monthly deposit [a]	33 (36) euros
Average (median) total deposit [a]	1,088 (1,050) euros
Average (median) matched grants (estimate)	4,810 (5,696) euros
Euros spent with the 2:1 match rate as a percentage of total money spent	5.7%
Expenditures breakdown	
Tuition fees	33%
PC/internet	28%
Transportation	16%
Books	9%
Other	14%
Financial education participation	
Attended at least one module	96%
Average (median) number of modules attended	2.2 (2)

Note: [a] The savings period considered varies between the two cohorts.

Table 3. Cost-effectiveness of the program on college enrollment.

	Overall	Academic	Technical	Vocational
Deadweight (a)	0.671	0.777	0.622	0.441
Impact (b)	0.087	0.091	0.047	0.205
Cost-effectiveness I (c=a/b)	7.7	8.5	13.2	2.2
Mean per-person matched savings (€) (d)	4,810	5,732	4,231	3,364
Cost-effectiveness II (€) (e=c*d)	37,098	48,943	55,999	7,237

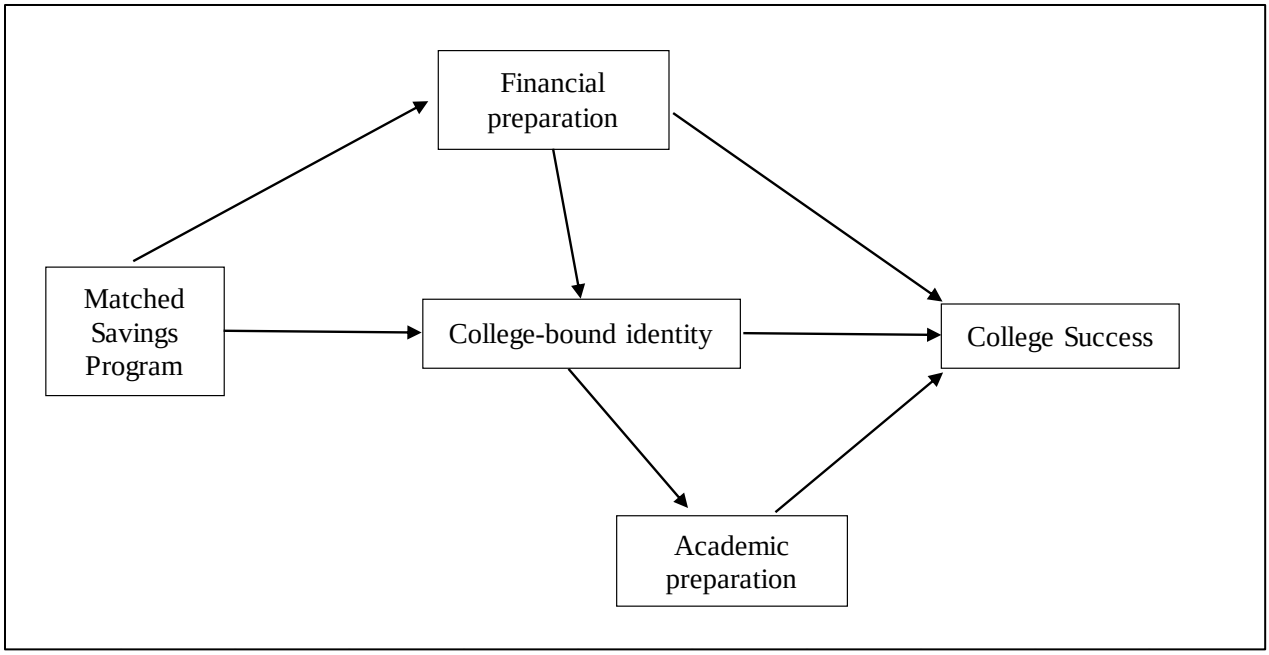
Table 4. Monthly and total deposits and total matched savings, by household equivalent income levels.

	Below the median	Above the median	P-value t-test
Monthly average deposit (€)	28.8	37.4	0.000
Average total deposits (€)	882	1,307	0.000
Average total matched savings (estimate) (€)	3,971	5,696	0.000

Table 5. Heterogeneity of impacts, by household equivalent income levels.

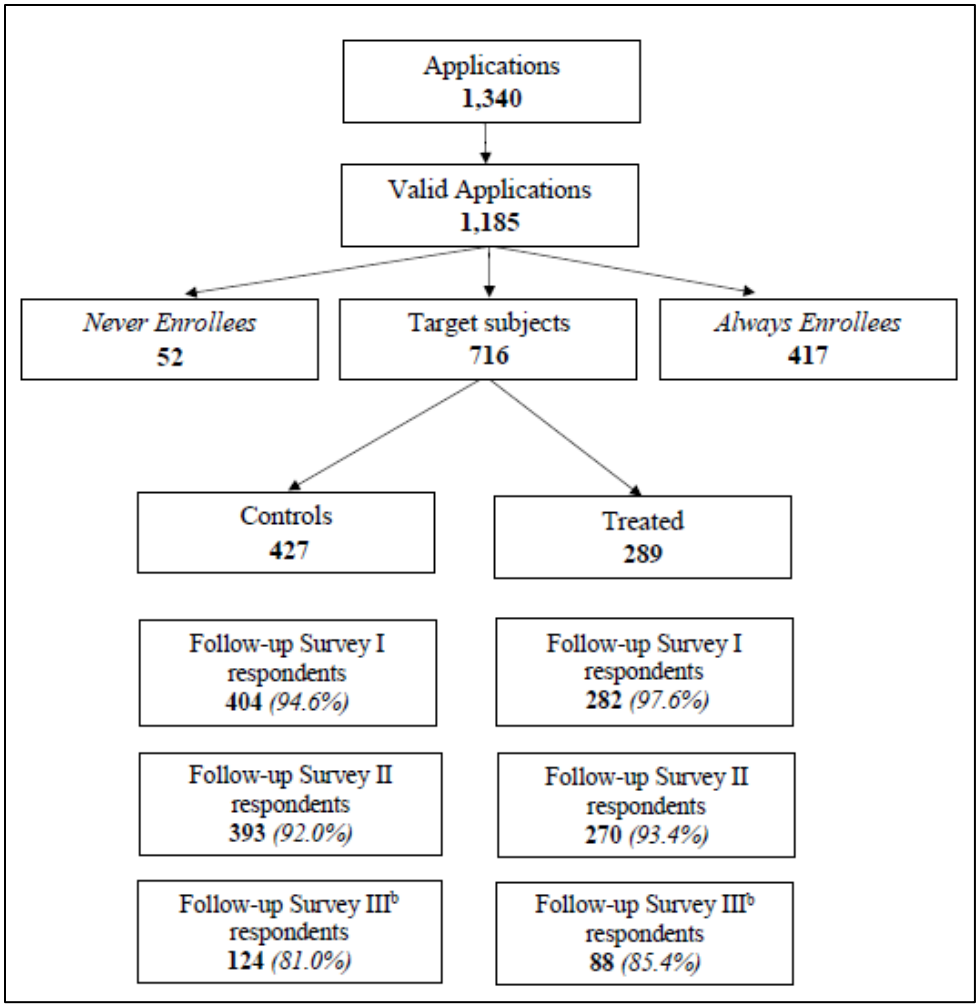
	Enrollment				Persistence to the second year			
	Controls	ITT	S.E.	N	Controls	ITT	S.E.	N
Below the median	0.599	0.062	0.050	337	0.519	0.073	0.052	327
Above the median	0.736	0.104**	0.042	349	0.660	0.100**	0.047	336

Notes: Linear probability models controlling for blocking variables, sex, and school career (failure and remedial courses). Robust standard errors are calculated.
*p<0.10; **p<0.05; ***p<0.01.



Source: Beverly, Elliott, and Sherraden (2013).

Figure 1. The Theory of Change of Matched Savings Account Programs.

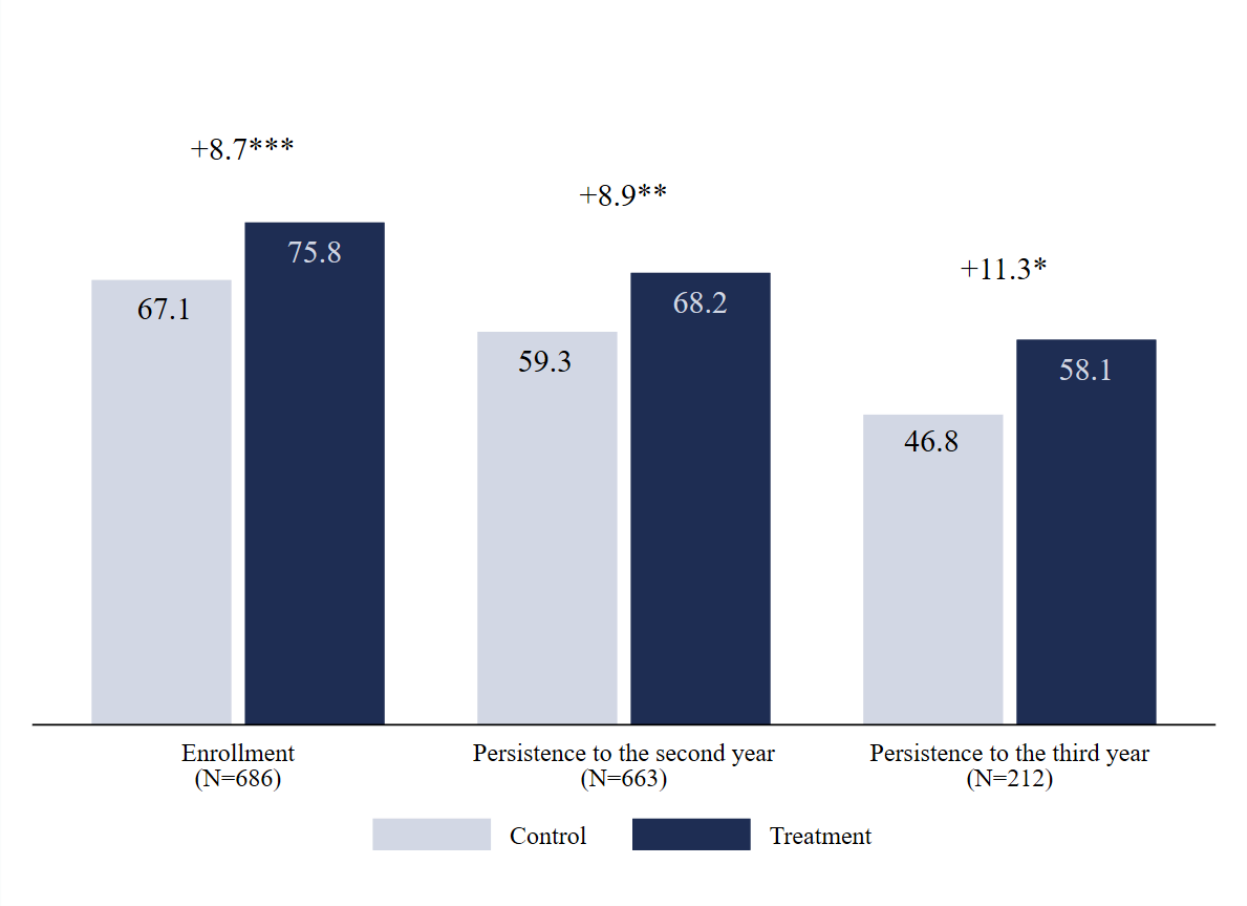


Notes:

[a] Some of the invalid applications were excluded after randomization because of formal irregularities (see Footnote 16).

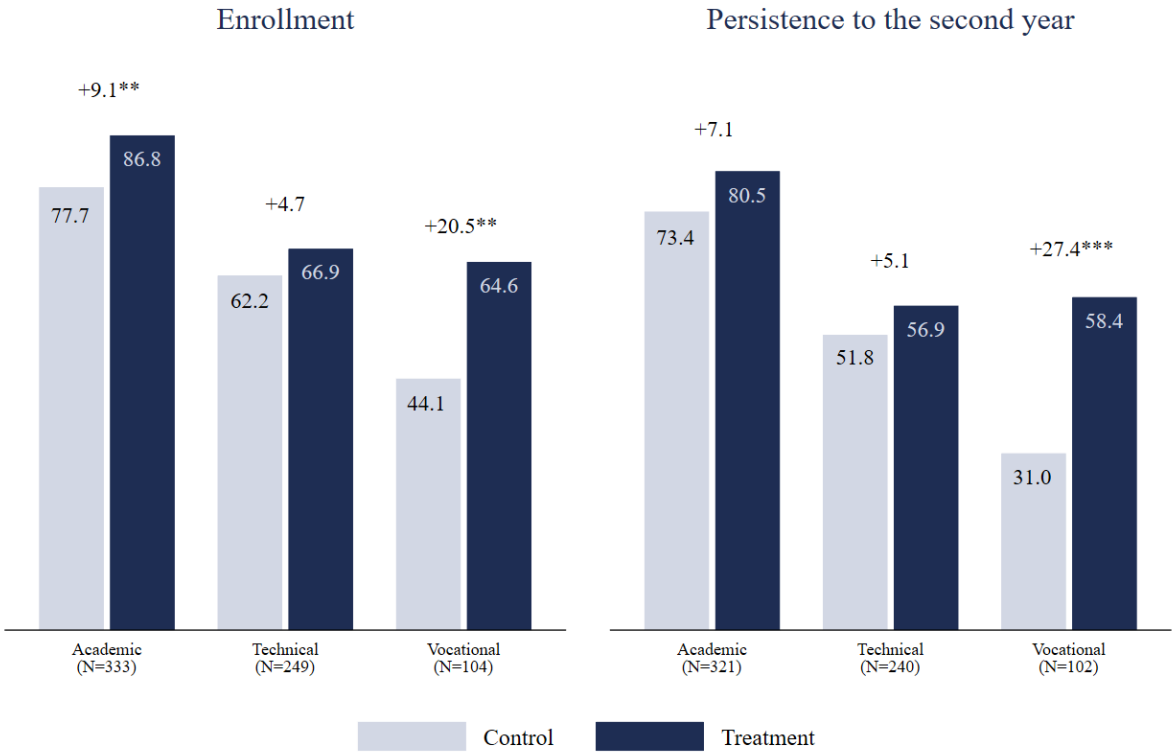
[b] Only the first cohort took part in the third follow-up survey.

Figure 2. ACHAB Experiment Flowchart.



Notes: Mean values for the control group (observed) and mean values for the treatment group (obtained by adding regression-adjusted ITT estimates to control means). Program's impacts were estimated through linear probability models controlling for blocking variables, sex, ISEE (index of households' equivalent economic condition), and school career (failure and remedial courses in earlier grades). The models are shown in Appendix D (Model 3 in Table D1). Persistence outcomes are unconditional on enrollment. These estimates are robust to the inclusion of different combination of covariates (Table D1). The different sample size between "Enrollment" and "Persistence to the second year" is due to attrition, while the lower sample size for the "Persistence to the third year" outcome is due to attrition and to the fact that this particular outcome was collected only for students enrolled in grade 13 in the first cohort (Table C2.) Lower and upper bound estimates of program impacts computed following the Lee's (2009) approach (Table D3) provide evidence that these results are robust to differential attrition. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com. *p<0.10; **p<0.05; ***p<0.01.

Figure 3. Program's Impacts.
[Color figure can be viewed at wileyonlinelibrary.com]



Notes: Mean values for the control group (observed) and mean values for the treatment group (obtained by adding regression-adjusted ITT estimates to control means). Program's impacts were estimated through linear probability models stratified by school track and controlling for blocking variables, sex, ISEE (index of households' equivalent economic condition), and school career (failure and remedial courses in earlier grades). The models are shown in Appendix D (Model 3 in Table D2). Persistence outcomes are unconditional on enrollment. These estimates are robust to the inclusion of different combinations of covariates (Table D2). The different sample size between "Enrollment" and "Persistence to the second year" is due to attrition. Results on "Persistence to the third year" are not shown due to the limited sample size (see Table C2). Lower and upper bound estimates of program impacts computed following the Lee's (2009) approach (Table D4) provide evidence that these results are robust to differential attrition. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com. *p<0.10; **p<0.05; ***p<0.01.

Figure 4. Program's Impacts by High School Track.
[Color figure can be viewed at wileyonlinelibrary.com]

APPENDIX A

Applicants' probability of enrolling at university after completing high school was predicted using microdata from the *Indagine sui diplomati trentini* (Survey on High School Graduates, henceforth SHSG) carried out in the province of Trento (Northeast Italy). We chose these data over two alternative available options—the Bank of Italy's "Survey of Household Income and Wealth" (SHIW) and the Italian National Institute of Statistics' survey on high school leavers (*Percorsi di Studio e di Lavoro dei Diplomati*)—for two important reasons. First, the SHSG data provide richer and more detailed information on several key aspects, including (a) enrollment intentions and actual enrollment; (b) sociodemographic characteristics; (c) social origins (parental education and parental occupational class); (d) school career (school type attended, mark obtained on the 8th grade final exam; failure experience; and attendance of remedial courses). Second, the SHSG data cover four high school graduate cohorts, while SHIW is a household survey representative of the entire population. The second survey would have had the advantage of being addressed to those who had high school qualifications and contained a rich set of covariates.

Determinants of enrollment probability are analyzed by the means of a probit regression model, whose estimates are shown in Table A1. Confirming findings from prior research, the main determinants of university enrollment probability are the type of school attended and family background. The coefficients on type of high school turn out to be largest, motivating our decision to use school tracks as a randomization blocking variable.

Table A1. Probit regression for university enrollment probability. Average marginal effects (AME) and standard errors (S.E.).

	AME	S.E.
Sex		
Female	(ref. category)	
Male	- 0.047***	0.010
Migration background		
Native	(ref. category)	
Mixed parents	- 0.016	0.021
Two migrant parents	0.073***	0.027
Lower secondary education final grade		
Sufficient	(ref. category)	
Good	0.057***	0.017
Very good	0.118***	0.019
Excellent	0.176***	0.023
Grade failure		
Yes	(ref. category)	
No	0.026**	0.013
Remedial courses		
Yes	(ref. category)	
No	0.113***	0.011
Upper secondary school track		
Academic	(ref. category)	
Technical	- 0.256***	0.012
Vocational	- 0.432***	0.020
Parental occupational class		
Service class	(ref. category)	
White collar	- 0.147***	0.014
Self-employed	- 0.113***	0.017
Working class	- 0.210***	0.016
Parental education		
Up to lower secondary school	(ref. category)	
Upper secondary school	0.038***	0.011
Tertiary degree	0.053***	0.018
Family size		
Below 5 members	(ref. category)	
5 members or more	-0.031**	0.012
N	7,642	
Pseudo-R^2	0.291	

Source: authors' elaboration on the Survey on High School Graduates (SHSG).
Note: *p<0.10; **p<0.05; ***p<0.01.

APPENDIX B

Table B1. Balancing test for the randomized sample.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.541	0.597	0.138
ISEE	9567.18	9905.04	0.570
Parental occupational class			
Service and white collar	0.373	0.353	0.598
Self-employed	0.135	0.140	0.836
Working class	0.493	0.507	0.714
Parental education			
Up to lower secondary degree	0.399	0.437	0.314
Upper secondary degree	0.462	0.447	0.694
Tertiary degree	0.139	0.117	0.372
Migration background			
Native	0.791	0.800	0.766
Mixed parents	0.063	0.040	0.186
Two migrant parents	0.147	0.160	0.624
Household size (>5)	0.106	0.103	0.917
Low. Sec. Educ. Final Mark			
Excellent	0.291	0.210	0.015
Very good	0.252	0.287	0.307
Good	0.317	0.400	0.022
Sufficient	0.139	0.103	0.149
No Remedial exam	0.536	0.527	0.804
No Failure	0.772	0.813	0.178
Aims to enroll in University	0.502	0.507	0.911
Observations	427	289	716

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 700) = 1.09, Prob > F. = 0.358.

Table B2. Balancing test for students from the academic track.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.577	0.612	0.522
ISEE	12030.817	12889.906	0.272
Parental occupational class			
Service and white collar	0.481	0.432	0.370
Self-employed	0.115	0.108	0.830
Working class	0.404	0.460	0.298
Parental education			
Up to lower secondary degree	0.327	0.374	0.367
Upper secondary degree	0.476	0.482	0.912
Tertiary degree	0.197	0.144	0.203
Migration background			
Native	0.870	0.885	0.685
Mixed parents	0.058	0.029	0.209
Two migrant parents	0.072	0.086	0.629
Household size (>5)	0.087	0.115	0.382
Low. Sec. Grade			
Excellent	0.337	0.223	0.023
Very good	0.308	0.338	0.553
Good	0.298	0.388	0.081
Sufficient	0.058	0.050	0.769
No Remedial exam	0.481	0.432	0.370
No Failure	0.837	0.842	0.898
Aims to enroll in University	0.591	0.597	0.915
Observations	208	139	347

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 331) = 1.02, Prob > F. = 0.435.

Table B3. Balancing test for students from the technical track.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.438	0.500	0.329
ISEE	6480.115	7054.765	0.564
Parental occupational class			
Service and white collar	0.307	0.308	0.993
Self-employed	0.150	0.135	0.726
Working class	0.542	0.558	0.811
Parental education			
Up to lower secondary degree	0.431	0.452	0.746
Upper secondary degree	0.471	0.471	0.993
Tertiary degree	0.098	0.077	0.562
Migration background			
Native	0.725	0.740	0.792
Mixed parents	0.072	0.029	0.137
Two migrant parents	0.203	0.231	0.591
Household size (>5)	0.118	0.067	0.183
Low. Sec. Grade			
Excellent	0.275	0.221	0.336
Very good	0.248	0.279	0.587
Good	0.320	0.375	0.366
Sufficient	0.157	0.125	0.477
No Remedial exam	0.582	0.558	0.704
No Failure	0.752	0.769	0.747
Aims to enroll in University	0.412	0.442	0.628
Observations	153	104	257

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 241) = 0.53, Prob > F. = 0.924.

Table B4. Balancing test for students from the vocational track.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.697	0.739	0.631
ISEE	8873.015	7534.326	0.304
Parental occupational class			
Service and white collar	0.212	0.174	0.620
Self-employed	0.152	0.261	0.155
Working class	0.636	0.565	0.453
Parental education			
Up to lower secondary degree	0.530	0.630	0.296
Upper secondary degree	0.409	0.261	0.107
Tertiary degree	0.061	0.109	0.362
Migration background			
Native	0.727	0.630	0.281
Mixed parents	0.045	0.109	0.205
Two migrant parents	0.227	0.261	0.686
Household size (>5)	0.152	0.130	0.756
Low. Sec. Grade			
Excellent	0.152	0.174	0.754
Very good	0.106	0.130	0.695
Good	0.394	0.478	0.380
Sufficient	0.348	0.217	0.137
No Remedial exam	0.561	0.804	0.007
No Failure	0.606	0.848	0.005
Aims to enroll in University	0.455	0.348	0.263
Observations	66	46	112

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 96) = 1.33, Prob > F. = 0.198.

Table B5. Balancing test for students from grade 12.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.503	0.639	0.037
ISEE	10735.662	11235.598	0.573
Parental occupational class			
Service and white collar	0.393	0.381	0.856
Self-employed	0.090	0.093	0.934
Working class	0.517	0.526	0.897
Parental education			
Up to lower secondary degree	0.434	0.454	0.770
Upper secondary degree	0.428	0.412	0.815
Tertiary degree	0.138	0.134	0.931
Migration background			
Native	0.745	0.773	0.616
Mixed parents	0.055	0.072	0.593
Two migrant parents	0.200	0.155	0.372
Household size (>5)	0.110	0.082	0.479
Low. Sec. Grade			
Excellent	0.228	0.155	0.164
Very good	0.255	0.258	0.965
Good	0.359	0.464	0.102
Sufficient	0.159	0.124	0.451
No Remedial exam	0.441	0.505	0.332
No Failure	0.710	0.794	0.146
Aims to enroll in University	0.379	0.515	0.036
Observations	145	97	242

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 226) = 1.06, Prob > F. = 0.395.

Table B6. Balancing test for students from grade 13.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.567	0.568	0.994
ISEE	8946.155	9281.867	0.662
Parental occupational class			
Service and white collar	0.369	0.328	0.364
Self-employed	0.156	0.167	0.757
Working class	0.475	0.505	0.522
Parental education			
Up to lower secondary degree	0.376	0.438	0.180
Upper secondary degree	0.482	0.458	0.609
Tertiary degree	0.142	0.104	0.227
Migration background			
Native	0.823	0.802	0.572
Mixed parents	0.064	0.026	0.060
Two migrant parents	0.113	0.172	0.070
Household size (>5)	0.106	0.109	0.918
Low. Sec. Grade			
Excellent	0.316	0.245	0.095
Very good	0.255	0.297	0.319
Good	0.301	0.365	0.151
Sufficient	0.128	0.094	0.255
No Remedial exam	0.574	0.552	0.630
No Failure	0.801	0.828	0.466
Aims to enroll in University	0.571	0.495	0.103
Observations	282	192	474

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 458) = 1.27, Prob > F. = 0.214.

Table B7. Balancing test for students after the first follow-up survey.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.532	0.599	0.082
ISEE	9680.296	9867.873	0.758
Parental occupational class			
Service and white collar	0.386	0.340	0.222
Self-employed	0.131	0.145	0.595
Working class	0.483	0.514	0.417
Parental education			
Up to lower secondary degree	0.386	0.440	0.161
Upper secondary degree	0.468	0.447	0.587
Tertiary degree	0.146	0.113	0.217
Migration background			
Native	0.800	0.791	0.781
Mixed parents	0.057	0.043	0.401
Two migrant parents	0.144	0.167	0.409
Household size (>5)	0.101	0.103	0.954
Low. *Sec*. Grade			
Excellent	0.287	0.213	0.028
Very good	0.262	0.280	0.607
Good	0.312	0.401	0.016
Sufficient	0.139	0.106	0.210
No Remedial exam	0.530	0.532	0.955
No Failure	0.777	0.816	0.223
Aims to enroll in University	0.510	0.504	0.870
Observations	404	282	686

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 670) = 1.13, Prob > F. = 0.328.

Table B8. Balancing test for students after the second follow-up survey.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.532	0.585	0.175
ISEE	9665.341	9786.508	0.845
Parental occupational class			
Service and white collar	0.384	0.333	0.181
Self-employed	0.135	0.137	0.936
Working class	0.481	0.530	0.218
Parental education			
Up to lower secondary degree	0.389	0.444	0.157
Upper secondary degree	0.471	0.441	0.447
Tertiary degree	0.140	0.115	0.345
Migration background			
Native	0.804	0.800	0.897
Mixed parents	0.053	0.037	0.327
Two migrant parents	0.142	0.163	0.470
Household size (>5)	0.104	0.107	0.899
Low. Sec. Grade			
Excellent	0.285	0.215	0.042
Very good	0.262	0.270	0.813
Good	0.313	0.404	0.016
Sufficient	0.140	0.111	0.276
No Remedial exam	0.529	0.522	0.859
No Failure	0.774	0.811	0.245
Aims to enroll in University	0.514	0.493	0.589
Observations	393	270	663

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 647) = 1.00, Prob > F. = 0.454.

Table B9. Balancing test for students after the third follow-up survey.

	Control Group Mean	Treatment Group Mean	P-value t-test
Female	0.540	0.625	0.221
ISEE	6288.082	6540.237	0.823
Parental occupational class			
Service and white collar	0.331	0.318	0.850
Self-employed	0.194	0.170	0.671
Working class	0.476	0.511	0.612
Parental education			
Up to lower secondary degree	0.435	0.432	0.958
Upper secondary degree	0.435	0.477	0.549
Tertiary degree	0.129	0.091	0.390
Migration background			
Native	0.831	0.807	0.658
Mixed parents	0.048	0.034	0.613
Two migrant parents	0.121	0.159	0.428
Household size (>5)	0.121	0.057	0.116
Low. Sec. Grade			
Excellent	0.194	0.125	0.187
Very good	0.306	0.352	0.485
Good	0.315	0.398	0.212
Sufficient	0.185	0.125	0.239
No Remedial exam	0.452	0.432	0.776
No Failure	0.774	0.864	0.102
Aims to enroll in University	0.532	0.443	0.203
Observations	124	88	212

Notes: F-test of joint significance from a regression of all characteristics on the probability to be assigned to the treatment group. F-test (15, 196) = 0.78, Prob > F. = 0.701.

APPENDIX C

Table C1. ACHAB's data collection's plan.

Experimental Cohort	Grade	Baseline (Application Form)	Follow-up I	Follow-up II	Follow-up III
1 – 2014/2015	13	Fall 2014	Spring 2016	Fall 2016	Fall 2017
1 – 2014/2015	12	Fall 2014	Spring 2017	Fall 2017	
2 – 2015/2016	13	Fall 2015	Spring 2017	Fall 2017	

Table C2. Follow-up survey participation and response rates.

	Cohort 1				Cohort 2	
	Grade 13		Grade 12		Grade 13	
	Treated	Controls	Treated	Controls	Treated	Controls
Baseline (application form)	103	153	97	145	89	129
First follow-up						
Respondents	101	147	95	135	86	122
Response rate	*98.1%*	*96.1%*	*97.9%*	*93.1%*	*96.6%*	*94.6%*
Second follow-up						
Respondents	96	142	90	130	84	121
Response rate	*93.2%*	*92.8%*	*92.8%*	*89.7%*	*94.4%*	*93.8%*
Third follow-up						
Respondents	88	124	-	-	-	-
Response rate	*85.4%*	*81.0%*				

Notes: Follow-up surveys were administered to study participants depending on eligibility: i.e., students who dropped out from school or university were no longer interviewed as the program was intended to sustain continued education participation and those exiting education were excluded from the program.

APPENDIX D

Table D1. Regression adjusted ITT estimates according to different model specifications. Robust standard errors are in parentheses.

	Model 1	Model 2	Model 3	Model 4
Enrollment	0.085**	0.087***	0.087***	0.086***
	(0.035)	(0.033)	(0.032)	(0.032)
Persistence to the second year	0.085**	0.089**	0.089**	0.088**
	(0.038)	(0.036)	(0.035)	(0.035)
Persistence to the third year	0.135*	0.127*	0.113*	0.118*
	(0.069)	(0.067)	(0.066)	(0.065)
Blocking variables	No	Yes	Yes	Yes
Selected controls	No	No	Yes	Yes
Extended controls	No	No	No	Yes

Notes: Linear probability models. Blocking variables are: experimental cohort, grade, and school track. Selected controls are: sex, ISEE, and school career (failure and remedial courses in earlier grades). Extended controls are all variables presented in Table B1. *p<0.10; **p<0.05; ***p<0.01.

Table D2. Regression adjusted ITT estimates according to different model specifications. Robust standard errors are in parentheses.

	Model 1	Model 2	Model 3	Model 4
Academic track:				
Enrollment	0.084**	0.085**	0.091**	0.092**
	(0.042)	(0.041)	(0.040)	(0.041)
Persistence to the second year	0.056	0.060	0.071	0.074
	(0.048)	(0.046)	(0.045)	(0.045)
Technical track:				
Enrollment	0.042	0.040	0.047	0.040
	(0.062)	(0.062)	(0.059)	(0.059)
Persistence to the second year	0.050	0.046	0.051	0.041
	(0.066)	(0.065)	(0.063)	(0.064)
Vocational track				
Enrollment	0.204**	0.205**	0.205**	0.187*
	(0.097)	(0.095)	(0.102)	(0.107)
Persistence to the second year	0.281***	0.279***	0.274***	0.274**
	(0.097)	(0.095)	(0.103)	(0.108)
Blocking variables	No	Yes	Yes	Yes
Selected controls	No	No	Yes	Yes
Extended controls	No	No	No	Yes

Notes: Linear probability models. Blocking variables are: experimental cohort and grade. Selected controls are: sex, ISEE, and school career (failure and remedial courses in earlier grades). Extended controls are all variables presented in Table B1. *p<0.10; **p<0.05; ***p<0.01.

Table D3. Bounds on treatment effects for enrollment and persistence to the second and the third year. Standard errors are in parentheses.

	Lower bound	Upper bound
Enrollment	0.077**	0.108***
	(0.035)	(0.037)
Persistence to the second year	0.080**	0.095**
	(0.039)	(0.041)
Persistence to the third year	0.115	0.164
	(0.086)	(0.102)

Notes: Lee (2009) bounds' estimates based on the most conservative modeling specification (i.e., Model 1 in Table D1): i.e., covariates are not included. These results confirm that the program's impact estimates presented in Table D1 and Figure 3 are not biased by differential attrition. The only difference concerns persistence to the third year, on which the lower bound effect is not significant, while the main impact estimate was marginally significant. Statistical insignificance could be due to the small sample size on this particular outcome. *p<0.10; **p<0.05; ***p<0.01.

Table D4. Bounds on treatment effects for enrollment and persistence to the second year, by high school track. Standard errors are in parentheses.

	Lower bound	Upper bound
Academic track:		
Enrollment	0.079*	0.112**
	(0.043)	(0.047)
Persistence to the second year	0.055	0.061
	(0.049)	(0.054)
Technical track:		
Enrollment	0.040	0.044
	(0.062)	(0.064)
Persistence to the second year	0.048	0.051
	(0.068)	(0.068)
Vocational track		
Enrollment	0.170*	0.265**
	(0.103)	(0.107)
Persistence to the second year	0.244**	0.333***
	(0.104)	(0.107)

Notes: Lee (2009) bounds' estimates based on the most conservative modeling specification (i.e., Model 1 in Table D2): i.e., covariates are not included. These results confirm that the program's impact estimates presented in Table D2 and in Figure 4 are not biased by differential attrition. *p<0.10; **p<0.05; ***p<0.01.

APPENDIX E

Field of study is recoded in a four-category variable, partially following Ballarino and Bratti (2009): 1) scientific field (math, physic, biology, chemistry, medicine, pharmacy); 2) technical field (engineering, ICT, economics & statistics, agriculture); 3) law; 4) humanities and social sciences. To estimate the effect of the treatment on the choice of the field of study, we run a multinomial logit regression and report the results in the form of average marginal effects (Table E1). In line with the overall approach applied in the paper, we run an unconditional analysis, i.e., we do not condition on enrollment but instead include an additional category for those students who do not enroll ("0 not enrolled").

Table E1. Multinomial logit regression, ITT estimates through average marginal effects (AME).

Field	AME	S.E.
Not enrolled	-0.086***	0.032
Scientific field	0.052*	0.029
Technical field	0.015	0.031
Law	-0.014	0.011
Humanities and social sciences	0.033	0.033
N	686	
Pseudo-R^2	0.012	

Note: *p<0.10; **p<0.05; ***p<0.01.

APPENDIX F

Table F1. Heterogeneity of impacts, by predicted probability of enrollment.

Predicted probability	Enrollment				Persistence to the second year			
	Control	ITT	S.E.	N	Control	ITT	S.E.	N
First tertile	46.2	0.104	0.068	225	37.2	0.119*	0.068	223
Second tertile	65.7	0.114*	0.060	229	55.2	0.094	0.066	218
Third tertile	88.8	0.054	0.038	232	85.4	0.041	0.046	222

Notes: Linear probability models controlling for blocking variables, sex, ISEE, and school career (failure and remedial courses in earlier grades). Robust standard errors are calculated. *p<0.10; **p<0.05; ***p<0.01.

APPENDIX G

One of the risks of financial aid is to increase college enrollment among students who have a high risk of dropping out. Hence, beyond college enrollment, also persistence in college and graduation from college are policy-relevant outcomes. Unfortunately, data on graduation were not available for this study. Observing graduation for study participants is also made difficult by the fact that university programs have different duration: some last three years, while others last four to six years. Hence, a long observation time is needed to measure graduation properly. To have an external reference estimate of college graduation, we use the ISTAT (Italian National Institute of Statistics) survey on high school leavers (*Percorsi di Studio e di Lavoro dei Diplomati*). The survey collects information on the school and work choices of a cohort of Italian high school qualified. The data used refer to students who completed high school in 2011 and were interviewed in 2015, between Summer and Fall. Since the academic year ends officially in March of the following year, this survey measures graduation after approximately 3.5 academic years after enrollment (i.e., 117 percent of regular graduation time for students in three-year programs). Even if this timing is not optimal, this is the only available national survey with which to study university careers in Italy.

We compute an outcome variable, which takes the value of one if students have either completed college or are still regularly enrolled at the university after four years and value zero if students have dropped out. We estimate the average of this "new" outcome variable for the group of students enrolled in three-year and longer programs who were comparable to those included in our study sample: (a) who lived in Northwest Italy (restricting to the specific region—Piemonte—would have implied too small a sample); (b) who were regularly enrolled in year 2 (the latest outcome we could measure on our entire sample); and (c) who were below the same predicted probability of college enrollment (calculated with nearly the same individual characteristics

employed for the targeting model used in the experiment, and definitely the most relevant ones, such as type of high school attended, past school outcomes, family background information). Since in ACHAB there were two experimental cohorts with two distinct predicted probabilities (0.675 for the first cohort and 0.826 for the second one), we calculate two conditional probabilities of persistence to the fourth year (or completion). These two probabilities are respectively equal to 0.912 and 0.924, whose average is 0.918. We assume that this value is the same for treated and controls.

The first part of Table G1 shows the ACHAB experimental results, while the last row reports the simulated scenario using ISTAT data. The latter is obtained by multiplying the predicted probability of 0.918 by the percentage of treated and controls that persist at the end of the second year. We interpret the difference between the two probabilities just calculated for treated and controls as the impact of the program on persistence to the fourth year or completion. Looking at the ACHAB data (which suggest that the program's impact increases in time), we interpret this as a lower-bound estimate.

Table G1. Real and simulated effects of the program.

	Controls (%)	Treated (%)	ITT
Observed from ACHAB data			
Enrollment	67.1	75.5	8.4
Persistence to the second year	59.3	67.8	8.5
Persistence to the third year	46.8	60.2	13.4
Simulated with ISTAT data			
Persistence to the fourth year or completion	43.0	55.3	12.3

Note: Here, the ITT is calculated as the simple difference between treated and controls and differs from the estimates obtained via OLS presented in the main text.

APPENDIX H

We calculate the cost-effectiveness of 68 programs aimed at enhancing college participation (Herbaut & Geven, 2020). The last column of Table H1 reports the estimated cost-effectiveness. Programs showing a better (i.e., lower) cost-effectiveness index than *Percorsi* are reported in bold, while programs that had negative effects are reported in italics. Programs with a better cost-effectiveness index are 22 (32.4 percent). If we exclude programs with negative impacts, the cost-effectiveness index value of *Percorsi* (7.7) is well below the median value observed in this sample of studies (11.51). If the analysis is restricted to financial aid programs only (i.e., if we exclude outreach and information programs), *Percorsi*'s relative performance improves, as the median cost-effectiveness for the other financial aid programs considered is 17.2. Unfortunately, cost estimates are not available for all programs, thus not enabling us to have a benchmark for our second cost-effectiveness indicator.

Table H1. Estimated cost-effectiveness from recent studies aimed at evaluating the effect of programs aimed at enhancing higher education participation.

Author	Program	Type of program	Outcome	Reference population	Deadweight	Estimated effect	Cost-effectiveness
Domina (2009)	College outreach programs (United States)	Outreach (any type)	Enrollment (any)	Disadvantaged high school students	0.739	0.055	13.44
Abbiati et al. (2017)	Information intervention (Italy)	"information" outreach programs	Enrollment (any)	Senior high school students with low educated parents	0.393	-0.032	*-12.28*
Abbiati et al. (2017)	Information intervention (Italy)	"information" outreach programs	Enrollment (any)	Senior high school students from the working class	0.432	-0.006	*-72.00*
Bettinger et al. (2012)	H&R Block Fafsa Experiment (United States)	"information" outreach programs	Enrollment (any)	Low-income 17-year-olds whose parents/families received treatment	0.342	-0.004	*-85.50*
Bettinger et al. (2012)	H&R Block Fafsa Experiment (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income 17-year-olds whose parents/families received treatment	0.342	0.081	**4.22**
Bettinger et al. (2012)	H&R Block Fafsa Experiment (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income young adults, with no prior college	0.095	0.015	**6.33**
Bird et al. (2017)	Information-only financial aid nudge campaign (United States)	"information" outreach programs	Enrollment (any)	First-generation college-intending high school seniors	0.817	0.008	102.13
Bird et al. (2017)	Information-only financial aid nudge campaign (United States)	"Information and guidance" outreach programs	Enrollment (any)	First-generation college-intending high school seniors	0.817	0.017	48.06
Bonilla, Bottan, & Ham (2017)	Information presentation (Colombia)	"information" outreach programs	Enrollment (any)	Low-income high school seniors in public schools	0.448	0.006	74.67
Hastings, Neilson & Zimmerman (2015)	Disclosure of information on costs and returns (Chile)	"information" outreach programs	Enrollment (any)	Low-SES High school graduates applying to federal student loan	0.770	0.000	----
Loyalka et al. (2013)	information campaign	"information" outreach programs	Enrollment (any)	High school seniors in the poorest counties	0.530	0.080	**6.63**
Rosinger (2015)	Information in financial aid award notifications (United States)	"information" outreach programs	Enrollment (any)	Pell-eligible students admitted to the university	0.480	-0.041	*-11.71*
Avery (2010)	Individualized college counseling	"Information and guidance" outreach programs	Enrollment in most competitive institutions	High-Achieving, Low Income high school seniors	0.420	0.079	**5.32**
Barr & Castleman (2017)	Bottom Line college advising model (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income, first generation junior or senior high school students with minimum GPA of 2.5	0.827	0.070	11.81
Bos et al. (2012)	Student Outreach for College Enrollment (SOURCE) program (United States)	"Information and guidance" outreach programs	Enrollment (4-year institution)	Junior high school students whose primary language is Spanish	0.404	0.106	**3.81**
Bos et al. (2012)	Student Outreach for College Enrollment (SOURCE) program (United States)	"Information and guidance" outreach programs	Enrollment (4-year institution)	Junior high school students whose parents did not attend college	0.493	0.061	8.08
Carell & Sacerdote (2013)	Mentoring program with financial incentives (United States)	"Information and guidance" outreach programs	Enrollment (any)	Non-white high school seniors	0.518	0.171	**3.03**
Carell & Sacerdote (2013)	Mentoring program with financial incentives (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income high school seniors	0.518	0.202	**2.56**
Castleman & Page (2015)	Outreach during summer after high school graduation (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income college intending high school graduates	0.696	0.019	36.63
Castleman & Page (2015)	Outreach during summer after high school graduation (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income college intending high school graduates	0.676	0.023	29.39
Castleman & Page (2017)	Outreach during summer after high school graduation (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income college intending high school graduates	0.664	0.057	11.65
Castleman & Page (2017)	Outreach during summer after high school graduation (United States)	"Information and guidance" outreach programs	Enrollment (any)	First-generation college-intending high school graduates	0.638	0.045	14.18

Castleman, Arnold & Wartman (2012)	Summer individualized counseling (United States)	"Information and guidance" outreach programs	Enrollment (4-year institution)	All graduates from high schools with predominantly non-white and low-income students	0.260	0.140	**1.86**
Castleman, Owen & Page (2015)	Summer college matriculation support (United States)	"Information and guidance" outreach programs	Enrollment (any)	Hispanic high school graduates admitted to university-Males	0.840	0.095	8.84
Castleman, Owen & Page (2015)	Summer college matriculation support (United States)	"Information and guidance" outreach programs	Enrollment (any)	Hispanic high school graduates admitted to university-Females	0.930	-0.011	*-84.55*
Castleman, Page & Schooley (2014)	Summer counseling intervention (United States)	"Information and guidance" outreach programs	Enrollment (any)	Lowest-income college-intending high school graduates	0.763	0.123	**6.20**
Castleman, Page & Schooley (2014)	Summer counseling intervention (United States)	"Information and guidance" outreach programs	Enrollment (any)	Lowest-income college-intending high school graduates	0.634	0.085	**7.46**
Ford et al. (2012)	Explore Your Horizons program (Canada)	"Information and guidance" outreach programs	Enrollment (any)	Low-income and first-generation high school students (from 10th grade)	0.537	0.094	**5.71**
Ford et al. (2014)	Explore Your Horizons program (Canada)	"Information and guidance" outreach programs	Enrollment (any)	Low-income and first-generation high school students (from 10th grade)	0.385	0.101	**3.81**
Hoxby & Turner (2013)	ECO Comprehensive Intervention (United States)	"Information and guidance" outreach programs	Enrollment in a "peer college"	High-performing low-income high school seniors	0.286	0.053	**5.40**
Castleman & Goodman (2014)	"Bottom Line" (United States)	"Information and guidance" outreach programs	Enrollment (4-year institution)	Low-income college ready students in senior year of high school	0.500	0.173	**2.89**
Constantine et al. (2006)	Talent search program (United States, Texas)	"Information and guidance" outreach programs	Enrollment (any public institution)	Primarily targeting low-income, potentially first-generation students in high school (from 9th grade)	0.400	0.180	**2.22**
Constantine et al. (2006)	Talent search program (United States, Indiana)	"Information and guidance" outreach programs	Enrollment (any)	Primarily targeting low-income, potentially first-generation students in high school (from 9th grade)	0.520	0.040	13.00
Constantine et al. (2006)	Talent search program (United States, Florida)	"Information and guidance" outreach programs	Enrollment (any public institution)	Primarily targeting low-income, potentially first-generation students in high school (from 9th grade)	0.360	0.150	**2.40**
Cunha, Miller & Weisburst (2018)	GO Center Project (United States)	"Information and guidance" outreach programs	Enrollment (any)	Low-income high school students in selected schools	0.670	0.035	19.14
Stephan & Rosenbaum (2013)	College coach program (United States)	"Information and guidance" outreach programs	Enrollment (any)	Disadvantaged High school seniors (primarily African American, Latino and low-income)	0.530	0.030	17.67
Avery (2013)	College Possible Program (United States)	"Information, guidance and academic tutoring" outreach programs	Enrollment (any)	High school students mostly of color with below median family income and GPA > 2.0 (from 11th grade)	0.638	0.017	37.53
Myers et al. (2004)	Upward Bound program (United States)	"Information, guidance and academic tutoring" outreach programs	Enrollment (any)	Low-income or first-generation high school students (from 9th or 10th grade)	0.710	0.030	23.67
Seftor, Mamun & Schirm (2009)	Upward Bound program (United States)	"Information, guidance and academic tutoring" outreach programs	Enrollment (any)	Low-income or first-generation high school students (from 9th or 10th grade)	0.791	0.015	52.73
Denning (2017)	Community College Tuition Reductions, Texas (United States)	Universal financial aid	Enrollment (4-year institution)	Economically disadvantaged high school graduates	0.250	-0.031	*-8.06*
Denning (2017)	Community College Tuition Reductions, Texas (United States)	Universal financial aid	Enrollment (4-year institution)	Black high school graduates	0.250	-0.034	*-7.35*
Ford et al. (2014)	New Brunswick Learning Accounts (Canada)	Need-based financial aid	Enrollment (any)	Low-income and first-generation high school students-from 10th grade	0.386	0.107	**3.61**
Richburg Hayes et al. (2015)	California Cash for College (CFC) (United States)	Need-based financial aid	Enrollment (any)	Low-income high school seniors	0.844	0.035	24.11
Baumgartner & Steiner (2006)	BaFöG (Germany)	Need-based financial aid	Enrollment (university)	Low-income high school graduates	0.640	0.015	42.67

Castleman & Long (2013)	Florida Student Access Grant	Need-based financial aid	Enrollment (any)	Low-income high school graduates	0.610	0.032	19.06
Dearden, Fitzsimmons, Wyness (2014)	Maintenance grants (United Kingdom)	Need-based financial aid	Enrollment (any)	Low-income 18- & 19-year-olds	0.155	0.038	**4.08**
Denning, Marx & Turner (2017)	Maximum Pell grants (United States)	Need-based financial aid	Enrollment (4-year institution)	Lowest-income university entrants (EFC=0)	0.760	0.040	19.00
Dynarski (2003)	Social Security Student Benefit Program (United States)	Need-based financial aid	Enrollment (any, by age 23)	High school seniors with father deceased during childhood (more likely to be low-income and/or black)	0.350	0.219	**1.60**
Fack & Grenet (2015)	Bourses sur Critères Sociaux (France), fee waiver only	Need-based financial aid	Enrollment (any)	Low-income grant applicants	0.773	0.003	257.67
Fack & Grenet (2015)	Bourses sur Critères Sociaux (France)	Need-based financial aid	Enrollment (any)	Low-income grant applicants	0.786	0.027	29.11
Linsenmeier et al. (2006)	Institutional grant, replacing loan (United States)	Need-based financial aid	Institutional enrollment (yield rate)	Admitted low income students	0.519	0.020	25.95
Linsenmeier et al. (2006)	Institutional grant, replacing loan (United States)	Need-based financial aid	Institutional enrollment (yield rate)	Admitted minority low income students	0.471	0.089	**5.29**
Lovenheim & Owens (2014)	Ineligibility of federal financial aid (United States)	Need-based financial aid	Enrollment (any)	Convicted drug offenders (majority of disadvantaged males)	0.401	-0.080	*-5.01*
Bruce & Carruthers (2014)	HOPE scholarship, Tennessee (United States)	Merit-based financial aid	Enrollment (any)	Pell-grant eligible high school graduates	0.859	0.000	----
Bruce & Carruthers (2014)	HOPE scholarship, Tennessee (United States)	Merit-based financial aid	Enrollment (any)	Non-white high school graduates	0.859	-0.026	*-33.04*
Cohodes & Goodman (2014)	Adams Scholarship, Massachusetts (United States)	Merit-based financial aid	Enrollment (4-year institution)	Non-white high school seniors	0.716	0.063	11.37
Cohodes & Goodman (2014)	Adams Scholarship, Massachusetts (United States)	Merit-based financial aid	Enrollment (4-year institution)	Low-income high school seniors	0.716	0.037	19.35
Dynarski (2000)	HOPE scholarship, Georgia (United States)	Merit-based financial aid	Enrollment (any)	Low-income 18- & 19-year-olds	0.300	-0.014	*-21.43*
Dynarski (2000)	HOPE scholarship, Georgia (United States)	Merit-based financial aid	Enrollment (any)	Black 18- & 19-year-olds	0.300	-0.027	*-11.11*
Kane (2003)	Cal Grant, California (United States)	Merit-based financial aid	Enrollment (any)	17- to 20-year-old grant low-income applicants	0.870	0.042	20.71
Sjoquist & Winters (2015)	State-wide merit aid programs, (United States)	Merit-based financial aid	Enrollment (any)	Non-White or Hispanic men	0.635	-0.020	*-31.91*
Sjoquist & Winters (2015)	State-wide merit aid programs, (United States)	Merit-based financial aid	Enrollment (any)	Non-White or Hispanic Women	0.635	-0.010	*-65.46*
Vergolini, Zanini & Bazoli (2014)	Trento 5B grant (Italy)	Need- and merit-based financial aid	Enrollment (low-income)		0.700	0.065	10.77
Barrow et al. (2014)	Opening Doors Louisiana (United States)	Performance-based	Enrollment (2-year institution)	Low-income parents accepted in community colleges	0.767	0.053	14.47
Binder et al. (2015)	VISTA at University of New Mexico (United States)	Performance-based	Enrollment (4-year institution)	Low-income incoming freshmen	0.994	-0.013	*-76.46*
Richburg Hayes, et al. (2015)	California CFCPBS (United States)	Performance-based	Enrollment (any)	Low-income high school seniors	0.844	0.049	17.22
Solis (2013)	National loan programs (Chile)	Loan	Enrollment		0.133	0.200	**0.67**

Gurgand, Lorenceau & Melonio	Eduloan (South Africa)	Loan	Enrollment	0.443	0.419	**1.06**

Note: See Herbaut and Geven (2020) for the full list of references.