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The Impact of "Beca 18" on Secondary Educational Attainment: Experimental Evidence from a Peruvian Scholarship Program

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Abstract

In 2012, the Peruvian Government launched "Beca 18", the largest public scholarship for higher education in its history. I evaluate the impact of this policy on educational attainment, measured by years of schooling and the likelihood of secondary completion. Employing a difference-in-differences methodology, I exploit variations in age cohorts induced by the program's initiation and variations in exposure intensity across departments. I further conduct a triple difference-in-differences by introducing temporal variations. The estimates suggest that the scholarship program significantly increased educational attainment, particularly for females and less poor individuals.

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1 Introduction

The question of which policies increase educational outcomes has emerged as a central focus within the ongoing debate on education, particularly in developing countries. Policies seeking to improve educational attainment typically aim to increase education's immediate benefits or reduce costs (Glewwe & Muralidharan, 2016). One example of such a policy approach is the provision of scholarships. In Latin America, where access to higher education remains low, scholarships have emerged as a viable solution for cost-constrained families to increase their education levels and overcome poverty (Pronabec, 2020). Surprisingly, evidence of the impact of higher education scholarships in Latin America remains scarce. Most research on higher education scholarships focuses on how they affect students' access to tertiary education (e.g., Solis, 2017; Londono-Vélez et al., 2017). However, the positive externalities they may have on schooling attainment remain unstudied. This dissertation contributes to close the research gap by examining the impact of a national scholarship policy for higher education on the secondary educational attainment of students who are eligible to apply for it. I study Beca 18 (B18) since it is Peru's largest public undergraduate scholarship: in terms of number of scholars and budget assigned (Pronabec, 2019). Since its introduction in 2012, it has financially supported the higher education of over 81,000 high-performance scholars from low-income backgrounds (Pronabec, 2022), which represents around 4.5% of the secondary public school-age population.

I formulate the following research questions. First, I assess whether B18 impacted educational attainment, measured by years of education and the likelihood of secondary completion, for its eligible population (public-school students in poverty and extreme poverty). Second, I analyze if there are differential effects by household income and gender. Drawing on Becker's (1962) theory of human capital and empirical evidence concerning the impact of perceived returns to education on schooling decisions, I hypothesize that B18 may increase students' and parents' perceived returns to education. This enhancement could result from the actual increase in the expected benefits of schooling due to the possibility of free higher education but also from B18's potential to update perceptions through the provision of information, peer influence, relatable role models and recognition. These mechanisms could lead to shifts in educational investment choices and potentially elevate levels of schooling attainment.

For the identification strategy, I use the fact that a student's exposure to B18 relies jointly on their age in 2012, when the scholarship started running, and their department of education. I thus conduct a difference-in-differences method exploiting variations in age cohorts and department intensity to estimate the impact of B18 on educational attainment. This quasi-experimental approach has been widely used in the literature to recover causal effects of interest, especially in the educational realm (Duflo, 2001; Brudevold-Newman, 2021; Lucas & Mbiti, 2012). For the age variation, I consider that B18 targeted students in poverty and extreme poverty from public schools. Thus, I define the treated cohort as public-school students in poverty and extreme poverty aged 11-18 in 2012 who were in secondary school after B18 was established. Thus, they were partially or fully exposed to the policy. The untreated cohort is defined as individuals with the same socio-economic characteristics but aged 19-26 in 2012, thus past secondary school. They are unlikely to be affected by B18 since they would have had to return to complete secondary education before turning 22 to be eligible for the scholarship. The primary analysis relies on data from 2019, as it is the latest year unaffected by COVID-19. Thus, in 2019, the treated cohort is analyzed at ages 18 to 25, while the untreated is between 26 to 33 years. For the intensity variation, I consider that B18 was launched nationally, but the number of scholars per 100 students varies across departments. I use this variation to assess the intensity of a student's exposure to the program. I further include individual controls and department-fixed effects.

One limitation of the previous specification is that it does not exploit variation in time. Taking advantage of yearly data availability before and after policy, I introduce temporal variation by conducting a triple difference-in-differences, which provides a more robust approach to identifying the treatment effect. For both specifications, the estimates suggest that the scholarship program significantly increased educational attainment, particularly for females and less poor individuals. Running a robustness check using the same specification but for private schools (out of B18's target) yields insignificant results, thus providing evidence in support of the main findings. To the best of my knowledge, this is the first study to evaluate if the introduction of a scholarship for higher education like B18 improved schooling attainment for the students eligible to apply for it and to provide differential effects by household income and gender. Studying whether educational policies like B18 have positive spillovers can alter the cost-effectiveness of these programs and, therefore, have important policy implications for their continuation and scaling up.

The rest of this paper is organized as follows. Section 2 discusses the background and policy context. Section 3 reviews the conceptual framework and empirical literature regarding the demand for schooling and perceived returns to education. Sections 4, 5 and 6 present the research questions, data, and methodology. Section 7 discusses the findings and heterogeneity analysis from the differences-in-differences. Section 8 shows results from the triple differences-in-differences. Section 9 discusses the limitations, and Section 10 concludes.

2 Background & Policy Context

2.1 Education in Peru

Peru is a middle-income country in Latin America. It has experienced consistent economic growth, accompanied by significant reductions in monetary poverty in the last years. National statistics indicate a decline from 54% to 20% between 2000 and 2019 (INEI, 2021a). However, despite these positive trends, educational attainment remains low and highly unequal. Formal schooling in Peru is comprised of six years of primary education and five years of secondary. Both levels are free of charge in public schools. In 2020, the Peruvian population aged 25 and over obtained 10.1 years of study on average. However, the poorest quintile managed to study for 7.8 years, while those who belong to the wealthiest quintile studied for 12.1 years, resulting in a gap of 4.3 years between income groups (INEI, 2021b). Educational disparities are also substantial by gender. In 2019, a Peruvian women aged 25 and over studied for an average of 9.9 years; while men manage to study 10.4 years, the gap being 0.5 years in favor of men (INEI, 2021b).

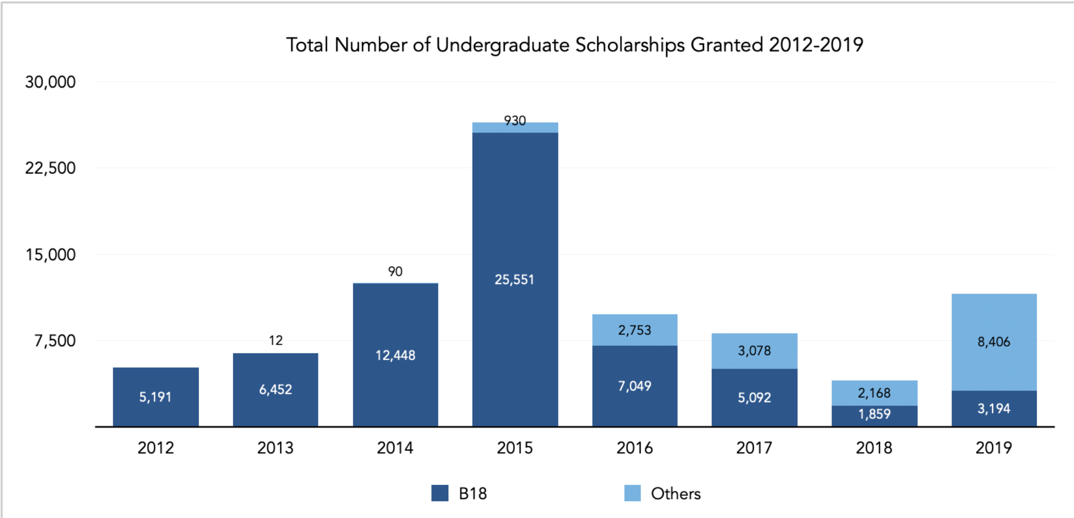
After secondary schooling, education can be continued at universities or technical institutes. Nevertheless, the gross enrollment rate in higher education was 31% as of 2019 (INEI, 2020a), way below the average 54% for the region (UNESCO, 2022a). Young people do not study a career or profession mainly because of the lack of economic resources (INEI, 2011). This is dramatically confirmed by pre-COVID statistics, where in the poorest quintile only 12% of youth between 17-24 were enrolled in tertiary education (INEI 2020a). The probability of being poor in Peru decreases from 35.2% for someone without education to 16.1% if they completed high school and drops to 2.4% for people with completed university (ENAHO, 2019). The effect of protection against poverty translates into a better salary level, greater access to health and lower morbidity and mortality (Yamada & Castro, 2007). Hence, there is a need for a strategy of equity and social inclusion that promotes access to higher education, which is the purpose of B18.

2.2 "Beca 18" Scholarship (B18)

B18 was introduced by the Peruvian government in 2012 to financially support the higher education of high-performer students from disadvantaged backgrounds. The scholarship offers full coverage for tuition fees and expenses. Eligibility for the program requires that candidates (1) hold Peruvian citizenship, (2) have completed their secondary education in a public school, (3) not exceed 22 years of age, (4) certify high academic performance, and (5) belong to a family living in poverty or extreme poverty (Ministerio de Educación, 2013).

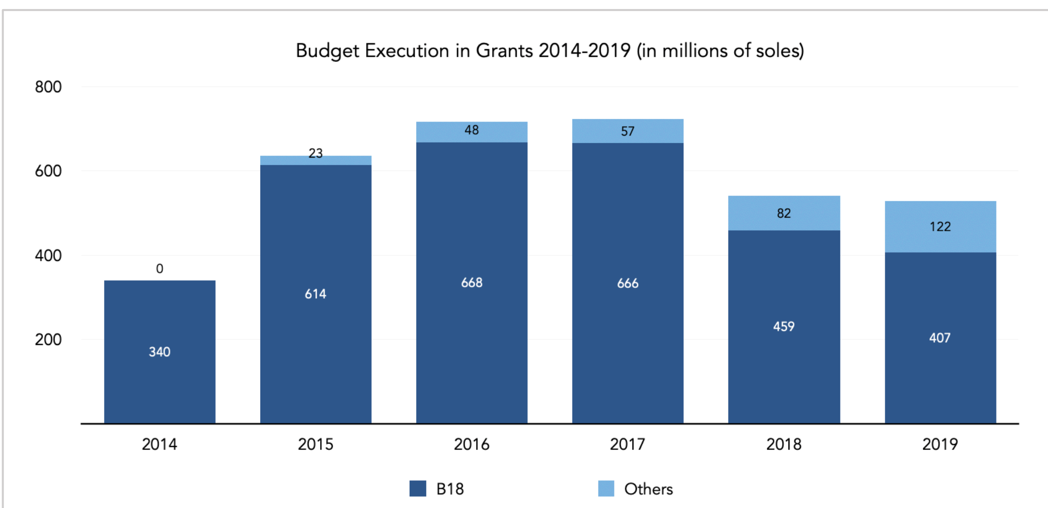
B18 is the largest undergraduate scholarship in Peru: in terms of number of scholars and budget assigned. It has awarded over 66,000 scholarships between 2012-2019 (Figure 1), representing around 2% of the secondary school-age population and 79% of the total number of undergraduate scholarships granted nationally (Pronabec, 2019). Besides, it has executed S/ 3 154 million (Figure 2), representing 90% of the total execution in national grants (Pronabec, 2019). It is also one of the most decentralized scholarships in the country. In 2019, over 80% of the scholarship recipients hailed from departments outside the capital city, demonstrating widespread representation as every department successfully achieved to have scholars (Figure 3) (Pronabec, 2019). Furthermore, it is the most nationally recognized undergraduate scholarship. In 2012, the leading polling companies in the country reported that B18 stood out as one of the main successes of the government (Ministerio de Educación, 2012). I provide further evidence about the program's advertisement strategy in Appendix I.

FIGURE 1
UNDERGRADUATE SCHOLARSHIPS GRANTED PER YEAR



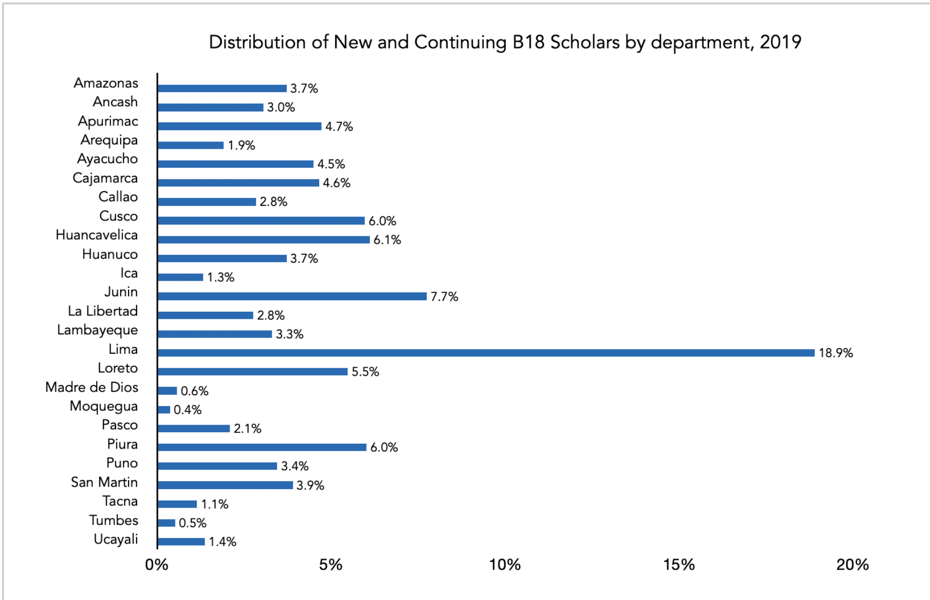
Author's elaboration using data from Pronabec (2019).

FIGURE 2
BUDGET EXECUTION IN GRANDS PER YEAR



Author's elaboration using data from Pronabec (2019).

FIGURE 3
DISTRIBUTION OF B18 SCHOLARS BY DEPARTMENT, 2019



Author's elaboration using data from Pronabec (2019).

3 Literature Review

This section highlights the conceptual framework and empirical literature concerning the impact of scholarships on educational attainment. I introduce Becker's (1962) theory of human capital, where households choose investment in education considering its effects on expected future benefits. I then contrast this perspective with the empirical literature on the returns to education, which posits that schooling decisions are primarily shaped by the perceived returns, especially when limited or imperfect information, as in developing countries. I hypothesize that B18 may increase parents' and students' perceived returns to education by increasing the expected benefits of schooling and updating perceptions through information, peer effects, role models and recognition. This increase could lead to shifts in educational investment choices and potentially elevate levels of schooling attainment. I present empirical findings that provide suggestive evidence for the validity of this hypothesis. I finally examine B18's previous studies and discuss its strengths and limitations, which subsequently inform the development of my identification strategy.

3.1 Conceptual Framework

3.1.1 Demand for schooling and perceived returns to education

Gary Becker's (1962) canonical model, regarded as one of the most influential models of education as a form of human capital, states that individuals take education as an investment decision. They evaluate the costs against the expected future benefits, much like any other investment. As such, parents and students weigh the direct costs (e.g., school fees, uniforms) and indirect costs of education (e.g., potential earnings and professional experience foregone during the study period) against the anticipated lifetime earnings.

Although returns to schooling are higher in developing countries than in developed ones (Psacharopoulos & Patrinos, 2004), educational attainment remains consistently low, especially for poor students (Banerjee & Duflo, 2011). Two main factors help explain this apparent contradiction. Firstly, there are significant challenges in estimating the returns. While economists use extensive datasets and advanced statistical methods, an ordinary student or parent makes educational choices with restricted or imperfect information (Jensen, 2010). Hence, demand for schooling varies depending on the educational investment's *perceived* benefits and associated costs (Manski, 1993). These perceptions might not always be accurate. Evidence has shown that parents and students consistently underestimate the returns to schooling, especially in developing countries (Jensen, 2010; Nguyen, 2008).

Secondly, perceptions of the benefits of education vary due to diverse aptitudes and preferences for schooling (Card, 2001) and can be influenced by factors such as wealth and gender (Duflo, 2021). As such, evidence suggests that lower-income groups in developing countries are more susceptible to underestimating returns to education (Dizon-Ross, 2016; Kaufmann, 2014). This misjudgment contributes to their heightened financial constraints in covering schooling expenses (Navarro, 2011; Attanasio & Kaufmann, 2010), ultimately curtailing their educational attainment. Regarding gender, households may have gender-specific preferences towards their children's capacity to benefit from education (Duflo, 2021). Evidence from rural Peru shows, for example, that parents frequently favour their boys over their girls because of perceived increased educational returns for males (Gertler & Glewwe, 1992).

3.2 Empirical Literature

3.2.1 Scholarships' Impact on Educational Attainment

Extensive literature indicates that scholarships have the potential to raise the educational attainment of low-income backgrounds students in developing countries (Duflo, 2021; Filmer & Schady, 2014). They can influence individuals' perceptions of the future benefits of pursuing schooling and higher education (Sequeira et al., 2016). Moreover, scholarships for secondary education have demonstrated considerable efficacy for girls, as female students with the capacity for success might only pursue senior high school with such support (Duflo, 2021; Filmer & Schady, 2008). Scholarships for tertiary education have also proven successful, especially for low-income, high-ability college students. The empirical evidence shows a significant impact on graduation rates, enrollment probability, and reduced desertion (Cohodes & Goodman, 2014; Andrews et al., 2020; Angrist et al., 2017). Similarly, in Latin America, financial assistance policies aimed at alleviating short-term liquidity constraints, such as scholarships and student loans, have found meaningful results on enrollment and probability of permanence (Londono-Vélez et al., 2017; Rau et al., 2013; Bordón et al., 2015).

Most research on higher education scholarships focuses on how they affect students' access to tertiary education. However, the positive externalities they may have on schooling attainment remain unstudied. Furthermore, only a limited body of literature delves into the mechanisms underlying the observed increases in educational outcomes. The same occurs for the potential differential effects by socioeconomic status and gender. Studying whether these policies have positive spillovers, their underlying mechanisms and differential impact, can enhance the assessment of their cost-effectiveness and, therefore, hold important policy implications for their continuation and scaling up.

3.2.2 Mechanisms of Impact

3.2.2.1 Updating Perceptions through Information

There is a disparity between perceived and actual educational returns due to imperfect information. Households seem to underestimate the earnings associated with increasing levels of education, especially in developing countries where access is exceptionally costly (Jensen, 2010; Kaufmann, 2014). Consequently, they might opt for limited schooling if they believe educational gains are minimal (Foster & Rosenzweig, 1995; Bils & Klenow, 2000). Increasing their perceived returns to education may incentivize them to pursue more schooling, especially for those who initially underestimated its advantages. In this regard, evidence suggests that presenting statistical data on the actual returns helps to update the perception of the average and personal returns to education (Nguyen, 2008; Jensen, 2010). As households revise their perceptions, their educational investments adapt (Nguyen, 2008; Jensen, 2010).

Parents may also interpret the launch of a governmental program as an indicator of the value of education (Damgaard & Nielsen, 2018). For example, Benhassine et al. (2015) found that a Cash Transfer Program (labelled as an education assistance initiative but without conditionality) increased parents' confidence in the value of education as a meaningful investment, which is a probable mechanism for the increased educational outcomes observed. Accordingly, B18 may be acting as a signal of the value of education while also providing information on its returns. As of the first call, B18 has launched comprehensive media campaigns in television, radio, newspapers and social media (Pronabec, 2022), providing information on the importance of pursuing higher education. For low-income students for whom higher education was not possible before, this advertisement could represent their first exposure to the actual returns of education. As the theory predicts, if they were underestimating the actual schooling returns, being exposed to this information might have updated their beliefs upwards.

3.2.2.2 Peer Influence and Role Model Effect

Research shows that social networks produce learning externalities that can influence investment choices (Foster & Rosenzweig, 1995; Bandiera & Rasul, 2006; Conley & Udry, 2010). This pattern extends to the educational realm, where students learn from their peers and adjust their behaviour based on acquired knowledge (Sacerdote, 2001; Kremer & Levy, 2008). In line with these findings, the question is whether peer influence and role models are strong enough to influence others' perceptions of the value of education and thus impact schooling outcomes.

Sequeira et al. (2016) found that information transmitted by fellowship recipients extends to individuals within their networks and that peers increased their likelihood of considering application in the next round. Although they did not find changes in peers' perceived returns to education, they did find greater perceived educational returns in fellows' parents, who expressed heightened expectations for all their offspring, not just the fellowship recipient (Sequeira et al., 2016). Nguyen (2008) found an improvement in educational outcomes among children only when the role model was similar to them. A role model originating from a low-income background enhanced average test scores, whereas the influence of a role model from an affluent background was negligible. These findings are supported by similar studies where the highest level of social learning occurs when information is shared among individuals with similar traits, such as gender, income level, and ethnicity (Ray, 2004; Conley & Udry, 2010), or when facing similar situations (Foster & Rosenzweig, 1995).

Based on the existing literature, B18 scholars can influence their peers through direct exposure (e.g., school friends) or indirect (e.g., advertisements showcasing the success stories of B18 scholars). This influence could increase perceived educational benefits due to the shared resemblance. For many poor students, this was the first time they were looking up to a successful role model within the same age and socio-economic condition. Furthermore, the updated perceived returns of parents may also positively affect their younger children, boosting the educational attainment of further generations.

3.2.2.3 Recognition Effect

Being recognised for academic achievements can shape how students perceive the benefits of education (Sequeira et al., 2016). Recognition involves receiving personalised feedback, which helps students to revise incomplete notions about their capabilities. Thus, top achievers are expected to reevaluate their beliefs upwards, which can influence their educational investments (Bandiera et al., 2015; Azmat & Iriberri, 2010). Acknowledging effort can also change their perception of the value of education and incentivise them to pursue further information regarding the financial returns of their schooling. On this line, when analysing fellowship recipients, Sequeira et al. (2016) found they increased their perceived returns to education by positively updating their beliefs towards future earnings. In Peru, high-achieving students are recognised by their teachers, parents and peers and encouraged to apply to B18. They are offered personalised feedback and, in several cases, further support during their application preparation and process (Pronabec, 2020). This acknowledgement may be updating their perceived returns to education, incentivising them to attain more years of education and complete secondary.

3.2.3 B18 Previous Studies

In the case of Peru, B18 was evaluated by the Ministry of Education and the Ministry of Economy. The latter analyzed the 2013 cohort to calculate the program's impact on access to higher education. They conducted a regression discontinuity by taking advantage of the poverty condition as their threshold and found that university enrollment increased by 24 percentage points (Ministerio de Economía, 2020). They failed, however, to examine whether there were differential impacts by gender and household income. For its part, in the context of the COVID-19, the Ministry of Education measured the impact of the 2019 cohort. The same methodology was followed but using the preselection score as the threshold. They found a higher impact on the probability of enrollment (around 58 percentage points) (Ministerio de Educación, 2022).

They encountered mixed evidence when conducting a heterogeneity analysis by gender. Results indicate that B18 had a greater impact among the preselected men, but a more significant impact was observed for women when real scholarship holders were evaluated. To my surprise, they failed to conduct differential impacts by household income. B18 aims to improve equity in access for youth in poverty and extreme poverty. Hence understanding whether the policy is succeeding in increasing higher education for its poorest target would have been valuable. Furthermore, regression discontinuity, as the methodology chosen for both studies, presents the limitation that provides only the measure of impact around a cutoff point. Hence, these results correspond to the estimators of the local average effect on those treated (LATE) around the threshold. Within quasi-experimental designs, this methodology has a high internal validity since it simulates an experiment at the local level. However, it has reduced external validity since the design relies on the specific context and conditions near the cutoff point.

4 Research questions

As can be seen, the evidence on scholarships for higher education focuses mainly on their impact on access to tertiary instruction. However, their effects on schooling attainment —and their differential effects by socio-economic characteristics and gender — remain unexplored. I chose to study B18 since it is the largest undergraduate scholarship in Peru: in terms of number of scholars and budget assigned (Pronabec, 2019). Previous evaluations have only analyzed whether B18 successfully achieved its intended goal: increasing access to tertiary education for the awarded scholars. This dissertation contributes to close the research gap by examining the effect of this national policy on the secondary educational attainment of students who are eligible to apply for it. To achieve that purpose, I formulate the following research questions:

i. How did B18 impact educational attainment, measured by years of education and the likelihood of secondary completion, for public-school students in poverty and extreme poverty?

ii. To what extent did B18 have a differential impact on educational attainment by household income and gender?

I study whether eligible individuals exposed to B18 attained, on average, more years of schooling and were more likely to complete secondary education than their unexposed peers. In line with the existing literature and empirical evidence, a positive impact would possibly indicate an increase in parents' and students' perceived returns to education due to the policy. However, testing the specific mechanism behind the impact escapes this dissertation's scope.

In the first part of the study, I replicate Duflo (2001) 's identification strategy in which age cohorts and intensity variations are exploited to analyze a policy impact. She employs age cohorts brought by the timing of the program and region variations in the number of schools constructed to examine the impact of Indonesia's school construction program on education and wages. More recently, Brudevold-Newman (2021) and Lucas & Mbiti (2012) used the same approach to analyze schooling policies' effects on educational outcomes. This dissertation resembles these identification strategies by exploiting age and department variations in B18 exposure to estimate the impact of the policy on educational attainment. In the second part, I conduct a triple difference-in-differences in which I add temporal variation with before and after policy years, which allows me to provide a more robust specification.

5 Data

5.1 Dataset

This study uses secondary data from the National Household Survey between 2004-2019, conducted annually by the National Institute of Statistics and Informatics of Peru. The datasets are pooled cross-section, nationally representative and available at the household and individual non-identifiable level. The principal analysis uses data from 2019 since this is the latest year not affected by COVID-19, in which the full impact of B18 can be assessed. Table 1 presents summary statistics for the main variables.

To measure B18 intensity per department, I use B18 scholarships from the National Scholarship and Educational Loan Program and 2017 Census data on schooling attendance, the latest available. I then compose the intensity measure by dividing B18 scholarships by each department's secondary school population. Detailed insight into the dataset and the calculation of the B18 intensity measure is provided in Appendix II.

TABLE 1
SUMMARY STATISTICS FOR THE YEAR 2019

Variables	High-intensity departments					Low-intensity departments				
	Obs	Mean	Stdev	Min	Max	Obs	Mean	Stdev	Min	Max
schoolY	2,151	8.37	2.97	0	11	1,971	8.83	2.81	0	11
secC	2,151	0.45	0.50	0	1	1,971	0.53	0.50	0	1
young	2,151	0.51	0.50	0	1	1,971	0.50	0.50	0	1
age	2,151	25.35	5.00	18	33	1,971	25.45	4.84	18	33
male	2,151	0.46	0.50	0	1	1,971	0.46	0.50	0	1
HHmembers	2,151	5.64	2.23	1	16	1,971	5.63	2.46	1	20
spanish	2,151	0.60	0.49	0	1	1,971	0.82	0.38	0	1
HHincome	2,151	16,775	12,195	779	112,798	1,971	23,972	19,644	478	144,571
urban	2,151	0.36	0.48	0	1	1,971	0.54	0.50	0	1

Note: HHincome is in Peruvian soles. See Appendix III for summary statistics for 2004.

5.2 Outcome Variables and Motivation

Two variables are used to measure educational attainment: the years of schooling education and the likelihood of secondary completion. $SchoolY$ is a discrete variable that quantifies the number of schooling years the individual attains. It can take a minimum value of 0 and a maximum of 11 if total schooling is achieved. $SecC$ is a binary variable that measures whether individuals have completed secondary education. It takes the value of 1 if the individual has completed at least secondary education and 0 otherwise. These two variables allow me to analyze in binary and discrete terms if B18 increased educational attainment for public-school students in poverty and extreme poverty.

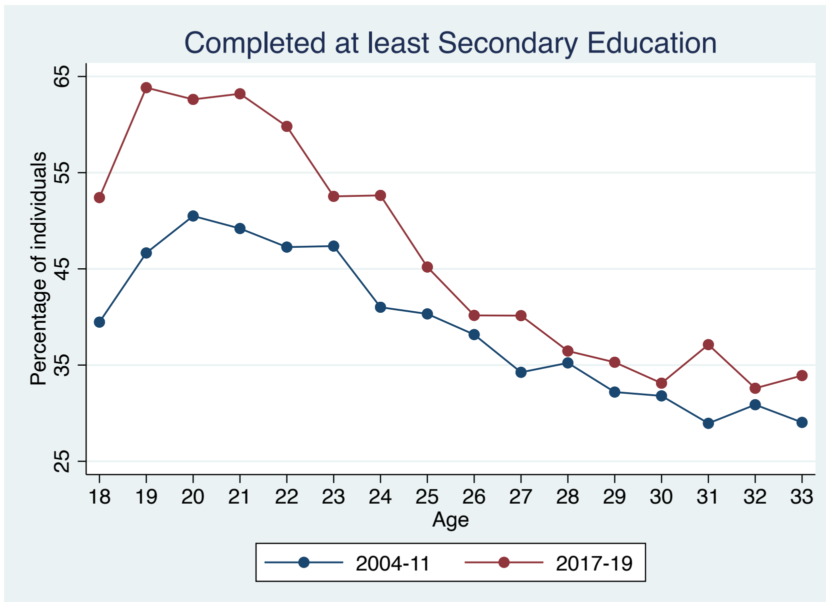
Figure 4 shows the distribution of educational attainment of former public-school students in poverty and extreme poverty between 18 to 33 years. Panel A shows the percentage of individuals who completed at least secondary education per age analyzed before B18 (2004 – 2011) and after the policy (2017 – 2019). I take years between 2017 and 2019 to allow individuals to complete secondary education. The graph shows a higher increase in the likelihood of completing at least secondary education for those aged between 18 to 25 compared to those between 26 to 33. This rise provides suggestive evidence of a policy or shock around 2012 that affected the educational attainment of the 18 – 25 cohort.

Moreover, the differential increase in educational attainment is decreasing with age. This observation is consistent with B18, whose exposure similarly declines as students age. As such, a 19-year-old individual in 2019 was fully exposed to B18 since she started secondary school when the scholarship was introduced. On the contrary, a 25-year-old individual in 2019 may have been only partially exposed since she was likely in her last year of secondary school when B18 began. Moreover, a higher differential increase between those aged 19 – 22 who were the most exposed to the policy would be expected, which is what the graphs show. It is important to note that an 18-year-old individual in 2019 was also fully exposed; however, a lower likelihood of secondary completion is seen because some individuals may still be in school (because of repetition or late entrance, which is especially prominent in low-income backgrounds (INEI, 2020a)). A similar trend is observed in panel B which shows the mean of schooling years per age analyzed before and after the policy. The graph depicts a higher increase in the average years of schooling attained for those between 18 to 25 years, especially pronounced between those between 19 to 22 who were the most exposed to the policy. These patterns motivate the empirical approach adopted in this study.

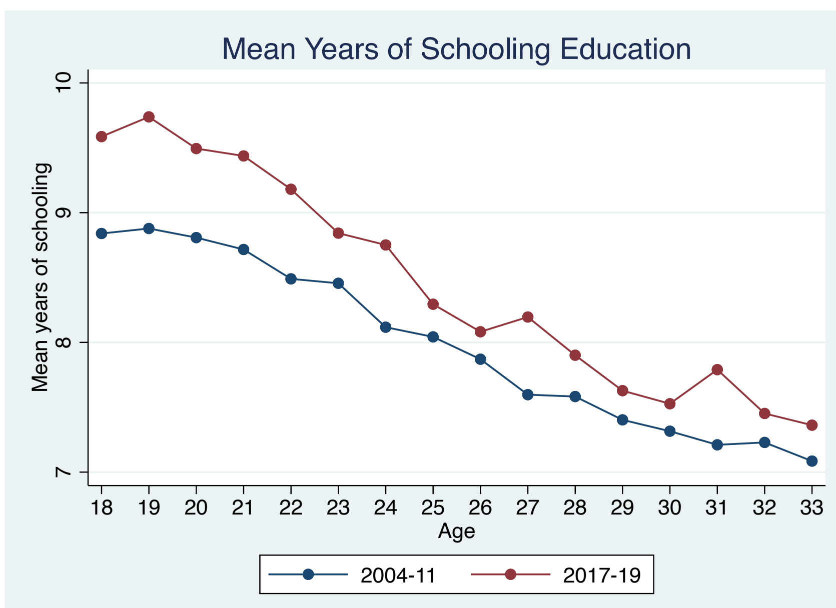
FIGURE 4

EDUCATIONAL ATTAINMENT OF PUBLICLY EDUCATED INDIVIDUALS IN POVERTY AND EXTREME POVERTY BETWEEN 18 TO 33 YEARS

Panel A: Percentage of individuals who completed at least secondary education.



Panel B: Mean of years of schooling education.



Author's elaboration.

5.3 Control Variables and Fixed effects

Control variables with considerable explanatory power decrease the residual variance and, thus, increase the precision of the causal effect of interest. I control for age, household income, household members, gender, urbanity and first language, considering they are the prevalent confounding variables for education, especially in the Peruvian context. Some of these controls were also used by governmental studies when analyzing the impact of B18 on higher educational attainment. Additionally, department fixed effects are included to control for time-invariant differences between departments that may impact secondary completion and years of schooling. I provide an in-depth justification of the chosen controls and department fixed effects in Appendix IV.

6 Methodology

6.1 Identification Strategy

I use a difference-in-differences method exploiting variations in age cohorts and department intensity to estimate the impact of B18 on educational attainment. More specifically, the identification strategy is based on the fact that a student's exposure to B18 relies jointly on their age in 2012 when the scholarship started running and their department of education. This quasi-experimental approach has been widely used in the literature to recover causal effects of interest, especially in the educational realm (Duflo, 2001; Brudevold-Newman, 2021; Lucas & Mbiti, 2012).

For the age cohort variation, I consider that B18 targets students in poverty and extreme poverty from public schools. Thus, I define the treated cohort as public-school students in poverty and extreme poverty aged 11-18 in 2012 who were in secondary school age after B18 was established. I define the untreated cohort as former public-school students in poverty and extreme poverty aged 19-26 in 2012. Peruvian students typically attend secondary school between the ages of 12 and 17. However, I extended the secondary school age range for treated students to 18 to account for grade repetition and late entry, which is especially prominent for low-income students in Peru. According to national statistics, only half of the secondary school population (54%)

attend the year corresponding to them according to age (INEI, 2020a). This figure is only 42% for the poorest stratum, to which the scholarship is aimed (INEI, 2020a). Moreover, according to B18 statistics, 79% of scholarship holders were between 16 and 18 years old on the date of application and applied immediately after finishing secondary (Pronabec, 2019).

The treated cohort was in secondary school after B18 was enacted; thus, they were partially or fully exposed to the policy. For example, an 11-year-old student in 2012 was fully exposed to B18 since she was in secondary age during the whole analysis period. A 17 or 18-year-old student in 2012 was only partially exposed to it since he may have been in his last year of secondary. On the contrary, the untreated cohort was past secondary school age and thus, not affected by B18. The primary analysis relies on data from 2019, as it is the latest year unaffected by COVID-19. Thus, in 2019, the treated cohort is analyzed at ages 18 to 25, while the untreated is between 26 to 33 years.

For the intensity variation, I consider that B18 was launched nationally without determined quotas per department. However, the number of scholars per 100 students varies across departments. I use this variation to assess the intensity of a student's exposure to B18. For example, in Huancavelica, 6 out of 100 secondary students received B18, while in Puno, only 1.5 out of 100 secondary students received it. Both are departments in the Peruvian Andes with one of the highest rates of monetary poverty (INEI, 2020b). However, a student in Huancavelica has a higher chance of being exposed to B18 than a student in Puno. A Huancavelican student may be in closer contact with a former peer who got the scholarship or is preparing for it. They may also be more exposed to B18 publicity by the local media, the Local Educational Management Unit, or their schools. Table A1 in Appendix II shows the intensity per department.

I illustrate the identification strategy in Table 2 and conduct a placebo test employing older cohorts to assess the identification assumption. The means presented in the table are illustrative. Their construction is done through regressions with indicator variables and does not yet include the control variables and fixed effects. In Panel A, I compare years of schooling education and the likelihood of secondary competition of the treated cohort to those of the untreated cohort in high and low-intensity departments. In both cohorts, the average educational attainment measured by the years of schooling and secondary completion is lower in high-intensity departments. In both kinds of departments, average years of schooling and average secondary completion improved over time. Nevertheless, it increased more in high-intensity departments. In high-intensity departments, young individuals achieve, on average, 1.9 more years of schooling than their older peers, while in low-intensity ones, the difference is 1.3 extra years. Among the young cohort, those in high-intensity departments achieved, on average, 0.17 fewer years of schooling than those in low-intensity departments, while among the old cohort, the difference is 0.77 fewer years of schooling. As per the difference-in-differences coefficient, an eligible student exposed to B18 (from the young cohort in a high-intensity department) received, on average, 0.6 more years of schooling and was 6.5% more likely to complete secondary education. The schooling years difference-in-differences coefficient is significant at the 1% level, while the secondary completion coefficient is at the 5%.

The primary assumption underlying this estimate is that in the absence of B18, education in high-intensity and low-intensity departments would have followed the same trend. This assumption would not hold if high-intensity departments caught up to low-intensity ones over time or if concurrent programs affected one type of department differently. I provide evidence for parallel trends by conducting a placebo test leveraging the presence of control groups formed by successive cohorts unaffected by B18, as done by Duflo (2001). Individuals 19 and older in 2012 were unaffected by B18 as they were no longer within the secondary education age range. Therefore, among this age group, the variations in educational outcomes between cohorts should not show any systematic differences across high and low-intensity departments. I consider cohorts aged 19 to 26 in 2012 and 27 to 34 in 2012. Illustrative results are presented in Table 2, Panel B. The calculated difference-in-differences are small and not statistically different from 0. These findings provide suggestive evidence that in the absence of B18, educational outcomes could have followed parallel trends among populations; thus, indicating that this difference-in-differences may be a valid identification strategy.

TABLE 2
MEANS OF EDUCATION BY COHORT AND PROGRAM INTENSITY

	Years of schooling			Secondary completion		
	Program intensity			Program intensity		
	High	Low	Difference	High	Low	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
Aged 11 to 18 in 2012	9.31***	9.48***	-0.17	0.581***	0.628***	-0.047**
	(0.076)	(0.077)	(0.108)	(0.015)	(0.015)	(0.021)
Aged 19 to 26 in 2012	7.41***	8.18***	-0.77***	0.318***	0.430***	-0.112***
	(0.096)	(0.096)	(0.135)	(0.014)	(0.016)	(0.021)
Difference	1.90***	1.30***	0.60***	0.263***	0.200***	0.065**
	(0.122)	(0.123)	(0.173)	(0.021)	(0.022)	(0.030)
Panel B: Control Experiment						
Aged 19 to 26 in 2012	7.41***	8.18***	-0.77***	0.318***	0.430***	-0.112***
	(0.096)	(0.096)	(0.135)	(0.014)	(0.016)	(0.021)
Aged 27 to 34 in 2012	6.60***	7.56***	-0.95***	0.229***	0.365***	-0.137***
	(0.091)	(0.094)	(0.131)	(0.012)	(0.014)	(0.019)
Difference	0.80***	0.62***	0.18	0.089***	0.065***	0.025
	(0.132)	(0.134)	(0.188)	(0.019)	(0.021)	(0.028)

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1)

6.2 Estimating equation

I estimate the following multivariate regression using OLS estimation:

$$(1)\ Outcome_{ij} = \beta_0 + \beta_1 young_i + \beta_2 highI_j + \beta_3 (young_i * highI_j) + \gamma controls_{ij} + \varepsilon_{ij}\ ,$$

where i corresponds to the individual, j to the department and γ is a vector of control variables. I regress the dependent variables of educational attainment: years of schooling $schoolY$ and secondary completion $secC$ against the cohort dummy $young_i$, intensity dummy $highI_j$, the interaction between $young_i * highI_j$, and control variables. $young_i$ equals 1 for individuals in poverty and extreme poverty from public schools aged 18 to 25 in 2019 (11 to 18 in 2012) and 0 for those with the same specifications but aged 26 to 33 in 2019 (19 to 26 in 2012). $highI_j$ equals 1 for departments with more scholars per 100 students and 0 otherwise.

For the years of schooling outcome ($schoolY$), $\widehat{\beta_3}$, the difference-in-differences estimator, measures the average treatment effect of B18, which is the differential impact of B18 on years of schooling between individuals from the young and old cohorts, and between high and low-intensity departments. For the secondary completion outcome ($secC$), the regression represents a linear probability model, where the coefficients reflect the influence on the estimated probability of completing secondary education (Aldrich and Nelson, 1984). The interaction estimator $\widehat{\beta_3}$ measures the average treatment effect of B18, which is the differential probability of completing at least secondary education between the young and old cohorts in high and low-intensity departments.

I conduct regression (1) for both outcomes to calculate the overall effect of B18 on the educational attainment of public-school students in poverty and extreme poverty. I then edit the regression to include department-fixed effects:

$$(2)\ Outcome_{ij} = \beta_0 + \beta_1 young_i + department_j + \beta_2 (young_i * highI_j) + \gamma controls_{ij} + \varepsilon_{ij}\ ,$$

This specification allows me to control for time-invariant characteristics that are different between departments that might be confounding the results. By capturing a weighted average of the 'within' relationship in each department, I run a more rigorous specification to examine if regression (1) results hold. Subsequently, I conduct regression (2) but subsampling by gender and household income. First, subsampling by gender helps to understand whether B18's effects were concentrated among males or women while contrasting the findings with recent empirical evidence where scholarships for girls significantly impacted learning outcomes and tertiary education (Duflo, 2021). Second, subsampling by household income sheds light on whether B18 successfully improves the educational outcomes of their poorest target since they may be facing higher limitations to remain at school.

6.3 Identification Assumptions

The difference-in-differences estimator represent the program's causal effect, mainly assuming parallel trends and no concurrent program. These conditions are convincingly shown to be valid, reducing potential concerns regarding the validity of the identification strategy. Additional assumptions, such as the absence of selection bias, measurement error and concurrent shocks, are considered in Appendix V.

6.3.1 Parallel Trends

The difference-in-differences estimator can be taken as the causal effect of B18, under the assumption that, in the absence of the scholarship, educational attainment in high-intensity and low-intensity departments would have followed the same time trend. For example, we would overestimate the program if educational outcomes from high-intensity departments caught up to low-intensity ones over time independently of B18. To investigate this, I conduct a placebo experiment leveraging the presence of control groups formed by successive cohorts unaffected by B18. Results are presented in Section 7.1. The findings offer suggestive evidence that in the absence of B18, educational outcomes would have followed parallel trends among populations. Thus, indicating that the difference-in-differences may be a valid identification strategy.

6.3.2 No concurrent program

The identification assumption would be violated if other government programs affected low and high-intensity departments distinctly, causing a differential increase in educational outcomes. Although there is no major concurrent governmental program with these characteristics, small educational scholarships were implemented during the analysis period. However, given their relatively reduced importance in number and budget (Figures 1 and 2), these programs are unlikely to affect our estimates significantly.

7 Findings & Discussion

7.1 Difference-in-Differences

Table 3 presents the estimations of equations (1) and (2) for the two outcomes. In Panel A, I compare public-school students in poverty and extreme poverty aged 18 to 25 in 2019 (11 to 18 in 2012) with former public-school students with the same socioeconomic conditions but aged 26 to 33 in 2019 (19 to 26 in 2012). Columns 1 and 4 displays regression (1) findings without any controls. Columns 2 and 5 presents regression (1) results controlling for age, household income, household members, urbanity, gender, and first language. Subsequently, Columns 3 and 6 provides the outcomes from regression (2), where department-fixed effects have been incorporated.

The suggested effect is that a poor or extremely poor individual, publicly educated in a high-intensity department, exposed to B18 during secondary, achieved on average 0.64 more years of schooling and was 7.8% more likely to complete secondary education. Controlling and adding fixed effects increases the estimates' magnitude and significance. Under the condition that the key identification assumptions are satisfied, the coefficients likely indicate the causal effect of B18 on educational attainment, measured by years of schooling and secondary completion.

In Panel B, I present results for the placebo test comparing cohorts aged 19 to 26 and 27 to 34 in 2012. Since both cohorts were past secondary school to benefit from B18, we would expect to see no significant changes in educational outcomes between high and low-intensity regions in this population. As anticipated, the estimators for both outcomes are small and always insignificant, suggesting that education may follow parallel trends among populations in the absence of B18. These results provide suggestive evidence that this difference-in-differences may be a valid identification strategy.

Furthermore, these findings offer support in line with the literature on the impact of scholarships on educational attainment. As such, the 0.64 average increase in years of schooling is similar to Filmer & Schady's (2014) findings. They analysed a comparable scholarship program (also targeted to poor children but for secondary school) in Cambodia, a low-income country with similar school enrollment rates to Peru. Likewise, they found that students offered scholarships achieved 0.6 more years of schooling. Results for secondary completion are, however, lower in magnitude than previous findings. Duflo (2021), for example, found a 27 percentage points increase in secondary completion when examining secondary school scholarships in Ghana. Differences in magnitude are yet, expected since B18's impact is indirect. These papers analyse the effect of scholarships for secondary students on schooling attainment (their intended impact). In contrast, this study analyses whether a scholarship for higher education had positive —probably unintended— spillovers on secondary educational outcomes.

Furthermore, the findings are consistent with the empirical literature on the relation between increased perceived returns to education and additional years of schooling (Sequeira et al., 2016; Nguyen, 2008). B18 may impact educational attainment by changing students' and parents' perceived returns to education. Introducing the possibility to access higher education for free increases the expected benefits of schooling education, potentially incentivising students to attain more years of schooling and complete secondary. This effect may be enhanced by B18's capacity to modify perceptions through information dissemination, offer relatable role models, and foster recognition.

TABLE 3

EFFECT OF B18 ON EDUCATIONAL ATTAINMENT

	Dependent variable					
	Years of schooling			Secondary completion		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
('Young' individuals aged 11 to 18 vs. 'Old' 19 to 26 in 2012)						
HighI	-0.771***	-0.323**		-0.112***	-0.044**	
	(0.135)	(0.129)		(0.021)	(0.021)	
Young	1.303***	-0.0974	-0.269	0.198***	0.043	0.0156
	(0.123)	(0.198)	(0.19)	(0.022)	(0.034)	(0.033)
Young*HighI	0.597***	0.580***	0.643***	0.0649**	0.0665**	0.0777***
	(0.173)	(0.163)	(0.158)	(0.030)	(0.029)	(0.028)
Controls	No	Yes	Yes	No	Yes	Yes
Department fixed effects	No	No	Yes	No	No	Yes
Observations	4,122	4,122	4,122	4,122	4,122	4,122
Panel B: Control Experiment						
('Old' individuals aged 19 to 26 vs. 'Older' 27 to 34 in 2012)						
HighI	-0.953***	-0.385***		-0.137***	-0.061***	
	(0.131)	(0.124)		(0.019)	(0.018)	
Old	0.623***	-0.276	-0.335*	0.0645***	-0.0337	-0.0364
	(0.134)	(0.189)	(0.179)	(0.021)	(0.030)	(0.029)
Old*HighI	0.182	0.242	0.252	0.0248	0.0336	0.0331
	(0.188)	(0.172)	(0.164)	(0.028)	(0.026)	(0.025)
Controls	No	Yes	Yes	No	Yes	Yes
Department fixed effects	No	No	Yes	No	No	Yes
Observations	4,424	4,424	4,424	4,424	4,424	4,424

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Appendix VI shows the full regression table.

7.2 Heterogeneity Analysis

7.2.1 Differential Impact by Household Income

I construct tertiles of poverty based on household income. As can be seen in Table 4, B18's effect is driven by the individuals in the top tertile. A *poor* individual, public educated in a high intensity department, exposed to B18 during secondary, achieved on average 0.69 more years of schooling and was 13.7% more likely to complete secondary education. In contrast, a *poorer* individual achieved, on average, 0.5 more years of schooling (significant at the 10% level), but the impact on its likelihood of secondary completion is not statistically significant from 0. B18 does not seem to significantly affect educational attainment for the *poorest* individuals.

TABLE 4
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT BY HOUSEHOLD INCOME

	Dependent variable					
	Years of schooling			Secondary completion		
	Poor	Poorer	Poorest	Poor	Poorer	Poorest
	(1)	(2)	(3)	(4)	(5)	(6)
Young	0.187	-0.145	-0.737**	0.066	.039	-0.020
	(0.293)	(0.354)	(0.360)	(0.056)	(0.061)	(0.060)
Young*HighI	0.693**	0.500*	0.483	0.137***	0.046	-0.004
	(0.279)	(0.284)	(0.299)	(0.052)	(0.051)	(0.050)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,400	1,361	1,361	1,400	1,361	1,361

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Appendix VII shows the full regression table.

These findings are consistent with existing literature and empirical evidence. First, *the poorest* students face greater credit constraints requiring higher perceived returns to motivate them to pursue tertiary education than their wealthier peers (Kaufmann, 2014). These students also tend to have less educated parents with lower perceived returns to education and, thus, lower willingness to invest in schooling (Lloyd & Blanc, 1996). As such, even if B18 achieved to increase their perceived returns to education, *the poorest* students and their parents may still be unable to afford the forgone benefits of labour earnings.

Secondly, given the positive correlation between household income and grades (Dahl & Lochner, 2015), *the poorest* students have, on average, lower scores, making them less eligible to apply for the program. Since B18 is not a tangible possibility, they may be less driven to change their educational choices. Additionally, their lower grades make the poorest students less likely to be rewarded for their performance. As the theory predicts, when provided individual feedback, accomplished students will adjust their returns to education in an upward direction, whereas students who did not fare as well would likely recalibrate their perceptions downward (Bandiera et al., 2015; Azmat & Iriberri, 2010). These mechanisms may explain the differential impact of B18 by household income.

7.2.2 Differential Impact by Gender

Regarding gender, the intervention showed significant effects in both outcomes only for women (see Table 5). A girl public educated in a high-intensity department, exposed to B18 during secondary, achieved on average 0.84 more years of schooling and was 8.3% more likely to complete secondary education. Conversely, coefficients for males are positive but insignificant. B18 may be particularly increasing the benefits of education for women by providing an opportunity that would be much more difficult to access otherwise compared to their male peers. These results align with recent empirical evidence in which scholarships significantly increase girls' educational attainment. Substantial effects of scholarships for female students (who would otherwise be less likely to attend secondary education) have been found, possibly reflecting unequal gender preferences (Arends-Kuenning & Amin, 2004; Schultz, 2004; Duflo, 2021).

TABLE 5

EFFECT OF B18 ON EDUCATIONAL ATTAINMENT BY GENDER

	Dependent variable			
	Years of schooling		Secondary completion	
	Male	Female	Male	Female
	(1)	(2)	(5)	(6)
Young	-0.159	-0.396	0.053	-0.009
	(0.281)	(0.255)	(0.052)	(0.043)
Young*HighI	0.358	0.840***	0.065	0.083**
	(0.224)	(0.219)	(0.043)	(0.038)
Controls	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes
Observations	1891	2231	1891	2231

Note: Standard errors are in parentheses (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).
Appendix VIII shows the full regression table.

B18 may also increase educational outcomes through role model effects and exposure to information. Of the total scholarship holders served in 2019, 55% were women (Pronabec, 2019). Social learning is maximized when information is shared among individuals of the same gender and income level (Conley & Udry, 2010) or who face comparable situations (Foster & Rosenzweig, 1995). Thus, B18 may be updating perceived returns to education by providing successful female role models to girls. The effect might be significant only for female students since, in contrast to boys, this may be the first time they can look up to a relatable role model from the same gender, age and background.

Moreover, this exposure may be delivering information on the actual returns to education, which are larger for females (average return of 9.8%) than for males (8.7%) in terms of wages (Psacharopoulos & Patrinos, 2004). Girls were likely underestimating the expected benefits of education. Therefore, as the literature predicts, the information may have allowed them to revise their perceptions upward, incentivizing them to pursue more schooling. Similarly, B18 may have acted as a signal of the value of education for parents. Due to unequal gender preferences, parents were likely to underestimate their girls' schooling returns more than their boys. Therefore, educational investments significantly adjusted only for girls when closing the gap between actual and perceived returns. This is consistent with Benhassine et al. (2015)'s findings in which the signal of a cash transfer program notably increased the parental perceived advantages of education for girls.

8 Triple Difference-in-Differences

In section 7.1, I performed a placebo test to assess parallel pre-trends. Although I found suggestive evidence for the assumption's validity, the test was run solely with data from 2019, making it sensitive to the trends of that particular year. Therefore, taking advantage of yearly data availability, I check for robustness by conducting a second placebo: I run regression (2), but for a pre-B18 year, when educational outcomes should remain unaffected due to the policy absence (Appendix IX). The interaction coefficient stands insignificant for years of schooling. However, it is significant at the 10% level for secondary completion, providing evidence of possible differential pre-trends across departments (e.g., since high-intensity departments have, on average, lower educational outcomes, they may have been catching up to low-intensity ones independently of B18).

Including temporal variation allows me to partially control for these differential pre-trends, leading to a more accurate estimation of the impact of B18 on schooling attainment. I thus conduct the following triple differences-in-differences using OLS estimation:

$$(3)\ Outcome_{ijt} = \beta_0 + \beta_1 young_i + department_j + \beta_2 yearT_t + \beta_3 (young_i * highI_j) + \\ + \beta_4 (young_i * yearT_t) + \beta_5 (highI_j * yearT_t) + \beta_6 (young_i * highI_j * yearT_t) + \\ \gamma controls_{ijt} + \varepsilon_{ijt}\ ,$$

where the specification is similar to regression (2) but adding $yearT_t$= 1 for years between 2017-2019 (post-B18) and $yearT_t$= 0 for years between 2004-2011 (pre-B18). I take post-B18 years between 2017-2019 to allow individuals to complete secondary education, and pre-B18 years from 2004 since that is the earliest National Household Survey available using the same methodology. Adding years 2017 and 2018 – which were not included in the difference-in-differences– further allows me to increase the number of observations and, thus, the specification's robustness. I edit variable $young_i$ from regression (2) to account for the variation in cohort exposure due to the inclusion of years 2017 and 2018. Thus, $young_i$ equals 1 for individuals in poverty and extreme poverty from public schools aged 18 to 23 in 2017 - 2019 and 0 for those with the same specifications but aged 26 to 33 in 2019. As can be noticed, I am taking out individuals aged 24-25 in this specification. This is because an individual aged 24-25 analyzed in 2017 was 19-20 years in 2012 and thus was unexposed to B18.

I run regression (3) and present estimates for the two outcomes in Table 6. The suggested effect is that a poor or extremely poor individual, publicly educated in a high-intensity department, exposed to B18 during secondary, achieved, on average, 0.19 more years of schooling and was 4.5% more likely to complete secondary education. The coefficients for both outcomes are smaller in magnitude than those obtained when running regression (2). This suggests we were overestimating the impact of B18 in the previous specification by probably omitting that, regardless of B18, the educational outcomes of young individuals in high-intensity departments were already catching up to those in low-intensity ones. Furthermore, these results align more realistically with the empirical evidence. The difference-in-difference estimate for the years of education outcome was similar in magnitude to previous literature analyzing the impact of a scholarship for secondary school. Since B18 is for higher education, its impact is indirect. Thus, it is reasonable to find lower estimates as in the ones resulting from the triple difference-in-differences.

TABLE 6
TRIPLE DIFFFERENCES-IN-DIFFERENCES
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT

	Dependent variable	
	Years of schooling	Secondary completion
	(1)	(2)
Young	0.087	0.079***
	(0.058)	(0.010)
YearT	0.287***	0.024***
	(0.055)	(0.009)
Young*HighI	0.455***	0.035***
	(0.046)	(0.008)
YearT*HighI	0.133*	0.014
	(0.075)	(0.012)
Young*YearT	0.111	0.045***
	(0.073)	(0.014)
Young*YearT*HighI	0.193*	0.045**
	(0.102)	(0.018)
Controls	Yes	Yes
Department fixed effects	Yes	Yes
Observations	64,888	64,888

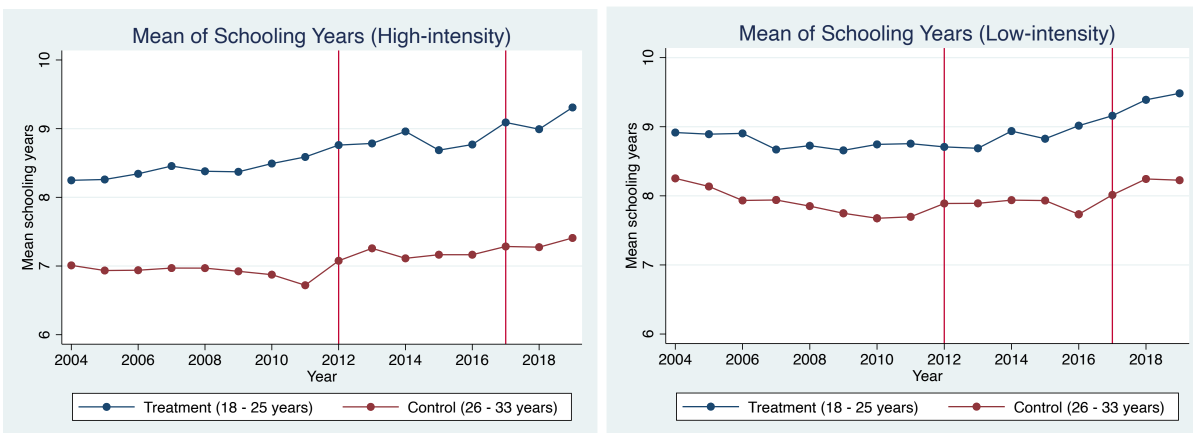
Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Appendix X shows the full regression table.

8.1 Key identification assumption: Parallel Trends

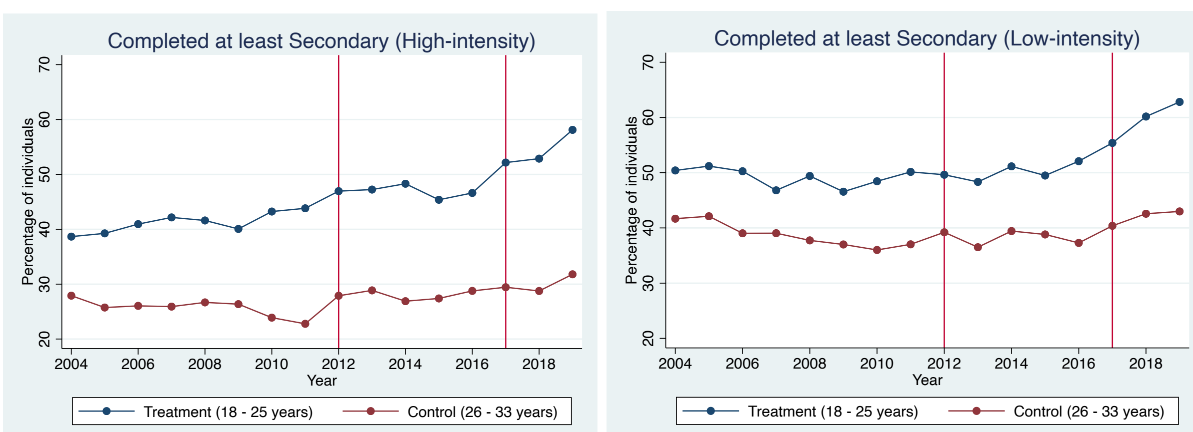
These results highlight the potential positive impact of B18 on educational attainment. However, before claiming causal conclusions, it is crucial to understand whether the parallel trends assumptions hold. Since this is a triple difference-in-differences, we need to check if, in the absence of treatment, the differential in the outcomes of the young and old cohorts in high-intensity departments trends similarly to the differential in the outcomes of the young and old cohorts in low-intensity departments (Olden & Moen, 2022). In Figure 5, I present the trends for both outcomes of interest. Panel A shows trends for the schooling years' outcome, and Panel B for the likelihood of secondary completion. In both cases, the differential in the outcomes between age cohorts in high-intensity departments trends similarly to the differential in the outcomes between age cohorts in low-intensity ones, providing suggestive evidence that the parallel trend assumption holds.

FIGURE 5
PARALLEL TRENDS IN EDUCATIONAL ATTAINMENT

Panel A: Parallel trends for the means of schooling years.



Panel B: Parallel trends for the likelihood of completing at least secondary school.



Author's elaboration.

8.1.1 Placebo test for triple differences' parallel trends

I perform a placebo test to provide further evidence for the parallel trends assumption. In particular, I run the same triple differences-in-differences but compare 2004-2007 and 2008-2011 as the temporal variation. Both are pre-B18 periods unaffected by the policy; thus, we should see no effects during these years. As expected, estimates for both outcomes of interest are insignificant, supporting the parallel trend assumption. Regression results are presented in Appendix XI.

8.2 Heterogeneity analysis

I replicate the heterogeneity analysis conducted in Section 7.2 using the triple differences-in-differences specification. Most estimates are lower in magnitude but remain significant, providing supportive evidence that B18 impacted the educational attainment of the less poor students and females. I discuss results in Appendix XII.

8.3 Robustness check

I run the triple difference-in-differences specification for individuals with the same characteristics as before but from private schools. Since they were ineligible to apply to B18, they should have been unaffected by the policy. Table 7 shows insignificant triple differences-in-differences estimators for both outcomes, providing evidence supporting our findings and key identification assumptions. Thus, we can increase the level of certainty about the absence of an alternative policy affecting high and low-intensity departments or a catching-up trend between them.

TABLE 7
EFFECT OF B18 ON THE EDUCATIONAL ATTAINMENT
OF POOR AND EXTREME POOR STUDENTS FROM PRIVATE SCHOOLS

	Dependent variable	
	Years of schooling	Secondary completion
	(1)	(2)
Young	-0.0398	0.0318
	(0.0961)	(0.0245)
YearT	0.0601	-0.00552
	(0.0721)	(0.0183)
Young*HighI	0.00691	-0.0582**
	(0.112)	(0.0287)
YearT*HighI	0.0575	0.0289
	(0.187)	(0.0394)
Young*YearT	0.0835	0.0432*
	(0.0846)	(0.0246)
Young*YearT*HighI	0.0792	0.0420
	(0.206)	(0.0508)
Controls	Yes	Yes
Department fixed effects	Yes	Yes
Observations	3,964	3,964

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Appendix XIII shows the full regression table.

9 Limitations

9.1 Data availability

B18 targets high-performance students. However, the National Household Survey lacks data on individuals' grades, which poses a limitation in defining the treatment and control groups accurately based on academic achievement. Moreover, having students' grades would have facilitated the execution of a heterogeneity analysis based on academic performance. If B18 affected the high-performers greatly, this would have provided evidence in line with the literature on schooling performance as a determinant of the benefits of education.

9.2 Sample size

The heterogeneity analysis is conducted with a reduced sample size due to subsampling, potentially resulting in less precise estimates and decreased statistical power. Using 2007 Census data would have increased the sample. However, it does not specify whether the individual attended public or private school, preventing the correct identification of the individuals exposed to B18.

9.3 Differential unobservable characteristics

Despite controlling for potential confounders and including temporal variation, there still may be unobservable characteristics influencing the outcomes differently between departments. For example, due to lower initial educational outcomes, there may be a faster increase in perceived returns to education from individuals in high-intensity departments independently of B18. The robustness check conducted, however, offer supportive evidence against this concern.

10 Conclusions

Scholarships play a critical role in human capital development, especially for disadvantaged students in developing countries. Accordingly, recent literature on higher education scholarships focuses on their effects on students' access to tertiary education. However, the positive externalities they may have on schooling attainment remain unstudied. This dissertation contributes to close the research gap by examining the impact of B18, Peru's largest public undergraduate scholarship, on the secondary educational attainment of students who are eligible to apply for it. I based the identification strategy on the fact that a student's exposure to B18 relies jointly on their age in 2012 when the scholarship started running, and their education department. I thus conducted a difference-in-differences exploiting variations in age cohorts and department intensity and a triple difference-in-differences adding temporal variation. Most identification assumptions hold for both specifications, indicating that these difference-in-differences may be valid identification strategies and plausibly determine the causal effect of B18.

The findings highlight that the scholarship program significantly increased educational attainment. Low-income students publicly educated in a high-intensity department, exposed to B18 during secondary, achieved more years of schooling and were more likely to complete secondary education. The results are robust when adding controls, fixed effects, and temporal variation. Moreover, they are consistent with the literature and empirical evidence on the impact of scholarships on educational attainment. Comparable programs denote similar but higher magnitude results. However, these differences are foreseeable due to B18's indirect influence. While the existing literature assesses secondary student scholarships' impact on schooling attainment (their intended impact), this study examines whether a higher education scholarship yielded positive spillovers on secondary educational outcomes.

Results are also consistent with the empirical literature on the relation between increased perceived returns to education and additional years of schooling. B18 might have raised the perceived advantages of schooling for both parents and students, prompting changes in decisions regarding educational investment. This improvement might arise from not only the anticipated growth in the benefits of education due to the possibility of free higher education but also from B18's capacity to modify perceptions through information dissemination, offer relatable role models, and foster recognition.

Regarding differential effects, results show that B18 significantly increased years of schooling and the likelihood of secondary completion for females and less poor individuals. The program may have increased the benefits of education for women by providing an opportunity that was much more difficult to access otherwise compared to their male peers. Due to unequal gender preferences, parents were likely underestimating their girls' schooling returns more than their boys. Therefore, educational investments may have significantly adjusted only for girls when closing the gap between actual and perceived returns. Similarly, B18 may be significant only for less poor individuals, given that *the poorest* face higher credit constraints. Thus, even if B18 achieved to increase their perceived returns to education through information and peer influence, these students and their parents may still be unable to afford the forgone benefits of labour earnings. To my awareness, this is the first study to evaluate whether introducing a scholarship for higher education like B18 improved educational outcomes among eligible secondary school students. The estimates highlight positive externalities on secondary educational achievement, which have the potential to reshape B18's cost-effectiveness holding significant policy implications for its continued sustainability and expansion.

10.1 Further Research

This study does not delve into the exact mechanisms through which educational attainment improves. Parents' and students' perceived returns on secondary education may have increased through several channels: Directly through the opportunity to attend university for free and indirectly through information exposure on the benefits of education, recognition for academic performance, and peer influence and role model effects from former scholars and classmates preparing to apply. Understanding these underlying mechanisms is of significant importance, as it can shed light on the pathways through which B18 influences educational decisions. Hence, using alternative approaches in future research, such as including qualitative methods, offer an opportunity to disentangle the exact mechanisms behind the observed improvement in educational outcomes. Through in-depth surveys, interviews and focus groups, researchers can gain valuable insights into how B18 affects educational decision-making processes, which can be instrumental in eliciting perceived returns of education (e.g., as demonstrated by Sequeira et al., 2016; Jensen, 2010).

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Appendices

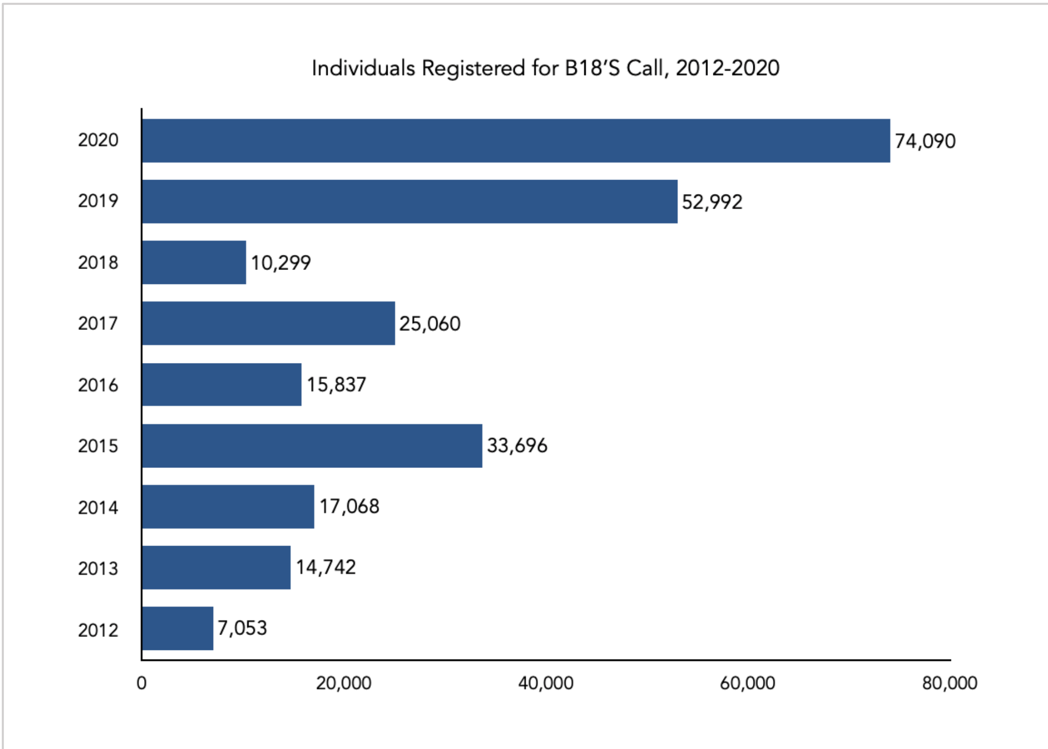
Appendix I: B18 Scholarship, communication campaign and modalities

In 2012, the main polling companies in the country, reported that B18 stood out as one of the main successes of the government (Ministerio de Educación, 2012). As such, the program was addressed in the presidential speech, and it was recorded that 47.6% of the surveyed remembered B18's state advertising. Likewise, Pronabec, the governmental body in charge of B18, has conducted several efforts throughout the years to disseminate it, such as television campaigns, press presence and social media advertisement (Pronabec, 2022) In 2012, their successful development of communication strategies granted them the qualification of Good Practice in Public Management in a recognized competition in the country (Ministerio de Educación, 2012).

Since then, they have kept improving their dissemination strategy. As for the 2019 call, advertising posters were distributed a year in advanced to every public school; while for the 2020 call, personalized letters were delivered to 5th grade students of the target population and informative emails were sent to teachers and directors of secondary schools (Pronabec, 2020). These efforts can be reflected in the increased number of applicants, as seen in Figure A1. For the 2019-2020 calls over 127,000 individuals registered, surpassing the combined number of applicants from the previous six calls (2012-2018).

Furthermore, it is important to note that the program is divided into nine modalities. The ordinary modality, whose target population was described in Section 2.2., has the highest volume of scholarships. It covered 67% of the scholarships attended in 2019. The rest of the modalities also serve youth with similar socio-economic conditions, except the armed forces modality, which targets graduates from Voluntary Military Service (Pronabec, 2019).

FIGURE A1
INDIVIDUALS REGISTERED FOR B18'S CALL, 2012-2020



Author's elaboration using data from Pronabec (2020).

Appendix II: Data

To match the eligibility target of B18, I focus on 4,122 individuals living in poverty and extreme poverty from public schools between 18 and 33 years in 2019 (11 - 26 in 2012). From this sample, the exposed cohort comprises 2,083 individuals who were in secondary school after B18 was enacted (11 - 18 years in 2012) and therefore were affected by the policy. The control cohort is composed of 2,039 individuals who were past secondary school age (19 - 26 years in 2012) and, thus, unlikely to be affected by the policy. Additionally, I include a sample of 2,380 older individuals, former public school students in poverty and extreme poverty, between 27 and 34 years in 2012. This sample is used to run a placebo test between the older cohorts to see if the assumption of parallel trends holds up and if the DiD is a valid estimation strategy.

To calculate a measure of B18 intensity per department, I use the number of B18 scholarships provided by the National Scholarship and Educational Loan Program and the population who effectively attended secondary school using the 2017 National Population and Housing Census, which is the most recent available. I then construct an intensity measure for every department dividing the number of B18 scholarships obtained by its secondary school population. On average, 3.4% of students receive B18 in a high-intensity department, while only 1.4% receive it in a low-intensity one. Hence, on average, there are two more scholars per 100 students in high-intensity departments. Table A1 shows B18 intensity for all departments.

TABLE A1

B18 INTENSITY PER DEPARTMENT

	Percentage of students who received B18	High-Intensity department
Amazonas	3.9%	1
Ancash	1.8%	0
Apurimac	4.8%	1
Arequipa	0.8%	0
Ayacucho	3.4%	1
Cajamarca	1.7%	0
Callao	1.2%	0
Cusco	2.5%	1
Huancavelica	6.1%	1
Huanuco	2.6%	1
Ica	1.0%	0
Junin	2.7%	1
La Libertad	1.0%	0
Lambayeque	1.4%	0
Lima	0.8%	0
Loreto	2.5%	1
Madre de Dios	1.4%	0
Moquegua	1.3%	0
Pasco	3.6%	1
Piura	1.6%	0
Puno	1.5%	0
San Martin	3.0%	1
Tacna	2.1%	1
Tumbes	1.9%	0
Ucayali	1.8%	0

Author's elaboration using data from Pronabec (2019).

Appendix III: Summary Statistics

Summary statistics for 2004 are provided to illustrate statistics from a year before the introduction of B18.

TABLE A2
SUMMARY STATISTICS FOR THE YEAR 2004

Variables	High-intensity departments					Low-intensity departments				
	Obs	Mean	Stdev	Min	Max	Obs	Mean	Stdev	Min	Max
schoolY	4,801	7.71	3.07	0	11	6,159	8.61	2.84	0	11
secC	4,801	0.34	0.47	0	1	6,159	0.47	0.50	0	1
young	4,801	0.56	0.50	0	1	6,159	0.57	0.49	0	1
age	4,801	24.76	4.77	18	33	6,159	24.71	4.74	18	33
male	4,801	0.52	0.50	0	1	6,159	0.50	0.50	0	1
HHmembers	4,801	5.95	2.47	1	17	6,159	6.22	2.47	1	17
spanish	4,801	0.67	0.47	0	1	6,159	0.84	0.37	0	1
HHincome	4,801	8682	7,078	184	71,511	6,159	14,475	13,968	200	255,280
urban	4,801	0.44	0.50	0	1	6,159	0.62	0.48	0	1

Note: HHincome is in Peruvian soles.

Appendix IV: Control variables and fixed effects justification

$HHincome$ (in Peruvian soles) controls for differences in household income since students from wealthier households can afford to stay in school longer and are more likely to complete secondary education (INEI, 2020a). $Urban$ controls for differences in students from urban and rural areas. Students from urban areas are more likely to achieve higher educational attainment than their rural counterparts. In Peru, this is explained by a reduced supply of rural schools, poor teacher quality, low proximity to schools and social indicators of the rural population, to name a few (World Bank, 2002). $HHmembers$ controls for the number of household members since families with more children typically devote less money to each child's education because raising the 'quality' of children through education costs more the more children a family has (Becker & Lewis, 1973).

$Male$ controls for differences in gender since male students tend to achieve more years of education and are more likely to complete secondary (INEI, 2020a). $Spanish$ controls for the fact that students whose first language is Spanish tend to achieve higher educational attainment than their counterparts who speak a native language (INEI, 2020a). Age controls for differences between age groups that could impact educational attainment; for example, differences in job market opportunities, economic conditions, technological advancements, and social norms that may affect differently the educational choices of each age group.

Similarly, $department - fixed\ effects$ are included to control for time-invariant differences between departments that may potentially impact secondary completion and years of schooling, such as access to opportunities (internships, jobs, or educational programs), teaching and school quality, cultural factors, and educational practices. By including fixed effects in the regression model, I am removing between-department variation that might confound the results. As such, I allow each department to have its own intercept term while having a common slope coefficient that captures a weighted average of the 'within' relationship in each department.

Appendix V: Additional identification assumptions

No Selection Bias or Measurement Error

Selection bias would be a problem if individuals could select themselves to a specific age cohort or department to be affected by B18. However, the variation in age cohorts is exogenous since individuals could not choose their age in 2012 when the policy was introduced. Regarding the department intensity variation, endogenous migration could have introduced measurement error if individuals from low-intensity departments had moved to high-intensity ones to take advantage of the policy. This would have caused an underestimation of B18’s impact. However, it is unlikely that families would have migrated to take advantage of the scholarship since this was enacted at the national level without quotas per department.

No Parallel Shock (COVID-19 effects)

It is also important to note that 2019 was chosen as the main year of analysis (or the last one in the case of the triple difference-in-differences) to avoid biasing the results with COVID-19 effects, which were especially relevant in the country. Peru had one of the highest numbers of COVID-related deaths (Johns Hopkins University & Medicine, 2023), and most schools remained closed for almost two years, especially affecting low-income learners (UNESCO, 2022b). If, for example, schools in high-intensity departments remained closed for longer, we could have underestimated B18's impact.

Appendix VI: Effect of B18 on Educational Attainment (Full Regression Table)

TABLE A3
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT

	Dependent variable					
	Years of schooling			Secondary completion		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
('Young' individuals aged 11 to 18 vs. 'Old' 19 to 26 in 2012)						
HighI	-0.771***	-0.323**		-0.112***	-0.044**	
	(0.135)	(0.129)		(0.021)	(0.021)	
Young	1.303***	-0.0974	-0.269	0.198***	0.043	0.0156
	(0.123)	(0.198)	(0.19)	(0.022)	(0.034)	(0.033)
Young*HighI	0.597***	0.580***	0.643***	0.0649**	0.0665**	0.0777***
	(0.173)	(0.163)	(0.158)	(0.030)	(0.029)	(0.028)
Constant	8.180***	12.18***	10.70***	0.430***	0.872***	0.647***
	(0.096)	(0.546)	(0.55)	(0.016)	(0.097)	(0.098)
HHmembers		-0.187***	-0.115***		-0.0333***	-0.0212***
		(0.0215)	(0.0227)		(0.00367)	(0.00396)
Urban		1.186***	0.908***		0.186***	0.138***
		(0.0915)	(0.0935)		(0.0165)	(0.0172)
Male		0.672***	0.710***		0.0866***	0.0919***
		(0.0813)	(0.0782)		(0.0146)	(0.0141)
Spanish		0.279***	1.102***		0.0122	0.133***
		(0.101)	(0.119)		(0.0169)	(0.0200)
HHincome		0.0000231***	0.0000165***		0.00000446***	0.00000345***
		(0.00000363)	(0.00000357)		(0.000000598)	(0.000000606)
Age		-0.156***	-0.160***		-0.0171***	-0.0176***
		(0.0177)	(0.0170)		(0.00315)	(0.00304)
Controls	No	Yes	Yes	No	Yes	Yes
Department fixed effects	No	No	Yes	No	No	Yes
Observations	4,122	4,122	4,122	4,122	4,122	4,122
R-squared	0.086	0.19	0.26	0.061	0.141	0.208

Panel B: Control Experiment

('Old' individuals aged 19 to 26 vs. 'Older' 27 to 34 in 2012)

HighI	-0.953***	-0.385***		-0.137***	-0.061***	
	(0.131)	(0.124)		(0.019)	(0.018)	
Old	0.623***	-0.276	-0.335*	0.0645***	-0.0337	-0.0364
	(0.134)	(0.189)	(0.179)	(0.021)	(0.030)	(0.029)
Old*HighI	0.182	0.242	0.252	0.0248	0.0336	0.0331
	(0.188)	(0.172)	(0.164)	(0.028)	(0.026)	(0.025)
Constant	7.556***	10.47***	9.153***	0.365***	0.678***	0.468***
	(0.094)	(0.712)	(0.697)	(0.014)	(0.109)	(0.107)
HHmembers		-0.226***	-0.172***		-0.036***	-0.0266***
		(0.0242)	(0.0248)		(0.00364)	(0.00381)
Urban		1.753***	1.343***		0.232***	0.173***
		(0.101)	(0.102)		(0.0156)	(0.0162)
Male		1.176***	1.187***		0.133***	0.133***
		(0.0862)	(0.0821)		(0.0132)	(0.0127)
Spanish		0.390***	1.460***		0.0214	0.144***
		(0.0988)	(0.127)		(0.0143)	(0.0189)
HHincome		0.0000317***	0.0000238***		0.00000550***	0.00000417***
		(0.00000447)	(0. 00000429)		(0.000000672)	(0.000000661)
Age		-0.109***	-0.116***		-0.011***	-0.0119***
		(0.0187)	(0.0178)		(0.00284)	(0.00275)
Controls	No	Yes	Yes	No	Yes	Yes
Department fixed effects	No	No	Yes	No	No	Yes
Observations	4,424	4,424	4,424	4,424	4,424	4,424
R-squared	0.031	0.193	0.274	0.024	0.154	0.224

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Coefficients for the 25 department fixed effects are not included for simplicity.

Appendix VII: Effect of B18 on Educational Attainment by Household Income (Full Regression Table)

TABLE A4

EFFECT OF B18 ON EDUCATIONAL ATTAINMENT BY HOUSEHOLD INCOME

	Dependent variable					
	Years of schooling			Secondary completion		
	Poor	Poorer	Poorest	Poor	Poorer	Poorest
	(1)	(2)	(3)	(4)	(5)	(6)
Young	0.187	-0.145	-0.737**	0.066	.039	-0.020
	(0.293)	(0.354)	(0.360)	(0.056)	(0.061)	(0.060)
Young*HighI	0.693**	0.500*	0.483	0.137***	0.046	-0.004
	(0.279)	(0.284)	(0.299)	(0.052)	(0.051)	(0.050)
Constant	8.973***	11.040***	12.780***	0.379**	0.741***	0.877***
	(0.942)	(0.960)	(1.029)	(0.180)	(0.173)	(0.171)
HHmembers	-0.107***	-0.0787*	-0.112**	-0.0217***	-0.00851	-0.0113
	(0.0272)	(0.047)	(0.054)	(0.005)	(0.008)	(0.009)
Urban	0.844***	0.919***	1.095***	0.148***	0.144***	0.152***
	(0.157)	(0.163)	(0.177)	(0.031)	(0.029)	(0.032)
Male	0.380***	0.851***	0.970***	0.0428*	0.107***	0.130***
	(0.124)	(0.139)	(0.145)	(0.025)	(0.024)	(0.024)
Spanish	1.269***	1.210***	0.797***	0.150***	0.137***	0.108***
	(0.233)	(0.185)	(0.227)	(0.041)	(0.031)	(0.035)
Age	-0.0727**	-0.178***	-0.227***	-0.00202	-0.0220***	-0.0272***
	(0.0284)	(0.0292)	(0.031)	(0.005)	(0.005)	(0.005)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,400	1,361	1,361	1,400	1,361	1,361
R-squared	0.2	0.29	0.257	0.141	0.2345	0.2227

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Coefficients for the 25 department fixed effects are not included for simplicity.

Appendix VIII: Effect of B18 on Educational Attainment by Gender (Full Regression Table)

TABLE A5
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT BY GENDER

	Dependent variable			
	Years of schooling		Secondary completion	
	Male	Female	Male	Female
	(1)	(2)	(5)	(6)
Young	-0.159	-0.396	0.053	-0.009
	(0.281)	(0.255)	(0.052)	(0.043)
Young*HighI	0.358	0.840***	0.065	0.083**
	(0.224)	(0.219)	(0.043)	(0.038)
Constant	11.012***	11.319***	0.675***	0.703***
	(0.787)	(0.760)	(0.149)	(0.130)
HHmembers	-0.109***	-0.119***	-0.0241***	-0.0194***
	(0.032)	(0.032)	(0.006)	(0.005)
Urban	0.642***	1.110***	0.107***	0.163***
	(0.133)	(0.13)	(0.026)	(0.023)
Spanish	0.813***	1.345***	0.0972***	0.160***
	(0.18)	(0.158)	(0.031)	(0.026)
HHincome	0.0000108**	0.0000223***	0.00000286***	0.00000420***
	(0.00000530)	(0.00000455)	(0.000000847)	(0.000000874)
Age	-0.124***	-0.200***	-0.0120***	-0.0223***
	(0.025)	(0.024)	(0.005)	(0.004)
Controls	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes
Observations	1891	2231	1891	2231
R-squared	0.194	0.310	0.165	0.247

Note: Standard errors are in parentheses (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).
Coefficients for the 25 department fixed effects are not included for simplicity.

Appendix IX: Placebo test (differences-in-differences)

The following placebo test allows me to check for pre-trends that may differ between high and low-intensity departments. I run the exact differences-in-differences specification as before but for the year 2008. Given that the policy has not been introduced, there should not be effects on the educational outcomes. The interaction coefficient is insignificant for years of schooling but significant at the 10% level for secondary completion, providing evidence of potential pre-trends that are different across departments. These findings motivate the performance of the triple differences-in-differences.

TABLE A6
PLACEBO TEST
DIFFERENCES-IN-DIFFERENCES
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT, 2008

	Dependent variable	
	Years of schooling	Secondary completion
	(1)	(2)
Young	0.0716***	0.0258
	(0.0246)	(0.149)
Young*HighI	0.00311	0.238*
	(0.0208)	(0.127)
Constant	0.0800	7.744***
	(0.0739)	(0.448)
Controls	Yes	Yes
Department fixed effects	Yes	Yes
Observations	7,317	7,317
R-squared	0.176	0.239

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Coefficients for the 25 department fixed effects are not included for simplicity.

Appendix X: Triple Diffferences-in-Differences Effect of B18 on Educational Attainment (Full Regresion Table)

TABLE A7
TRIPLE DIFFFERENCES-IN-DIFFERENCES
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT

	Dependent variable	
	Years of schooling	Secondary completion
	(1)	(2)
Young	0.087	0.079***
	(0.058)	(0.010)
YearT	0.287***	0.024***
	(0.055)	(0.009)
Young*HighI	0.455***	0.035***
	(0.046)	(0.008)
YearT*HighI	0.133*	0.014
	(0.075)	(0.012)
Young*YearT	0.111	0.045***
	(0.073)	(0.014)
Young*YearT*HighI	0.193*	0.045**
	(0.102)	(0.018)
Constant	7.606***	0.037
	(0.164)	(0.027)
HHmembers	-0.0360***	-0.00666***
	(0.005)	(0.001)
Urban	1.409***	0.211***
	(0.025)	(0.004)
Male	0.879***	0.0899***
	(0.021)	(0.004)
Spanish	1.008***	0.146***
	(0.031)	(0.005)
HHincome	0.0000242***	0.00000422***
	(0.00000140)	(0.000000230)
Age	-0.0985***	-0.00319***
	(0.005)	(0.001)
Controls	Yes	Yes
Department fixed effects	Yes	Yes
Observations	64,888	64,888
R-squared	0.248	0.18

Note: Standard errors are in parentheses (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).
Coefficients for the 25 department fixed effects are not included for simplicity.

Appendix XI: Placebo test for triple differences' parallel trends

Coefficient estimates for the years of schooling and secondary completion outcomes are not statistically different from 0, providing evidence that the parallel trend assumption holds for the triple differences-in-differences identification strategy.

TABLE A8
PLACEBO TEST
TRIPLE DIFFERENCES-IN-DIFFERENCES
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT
2004-2007 vs. 2008-2011

	Dependent variable	
	Years of schooling	Secondary completion
	(1)	(2)
Young	0.0440	0.0751***
	(0.0671)	(0.0114)
YearT	-0.241***	-0.0294***
	(0.0496)	(0.00805)
Young*HighI	0.454***	0.0297***
	(0.0592)	(0.0101)
YearT*HighI	0.194***	0.0186*
	(0.0690)	(0.0106)
Young*YearT	0.180***	0.0196*
	(0.0670)	(0.0117)
Young*YearT*HighI	-0.0244	0.00914
	(0.0932)	(0.0157)
Constant	7.295***	-0.000952
	(0.184)	(0.0301)
Controls	Yes	Yes
Department fixed effects	Yes	Yes
Observations	52,754	52,754
R-squared	0.249	0.177

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Coefficients for the 25 department fixed effects are not included for simplicity.

Appendix XII: Heterogeneity analysis

I conduct a heterogeneity analysis based on the triple differences-in-differences specification. Findings show that a *poor* individual, publicly educated in a high-intensity department and exposed to B18, achieved an average of 0.34 more years of schooling and was 7.4% more likely to complete secondary education (Table A2). Similarly, girls were 5.3% more likely to complete secondary. However, the estimate for years of schooling is positive but insignificant (Table A3). Compared to the heterogeneity analysis run with the differences-in-differences specification, most estimates are lower in magnitude but remain significant, providing supportive evidence that B18 impacted the educational attainment of the less poor students and females.

TABLE A9
TRIPLE DIFFFERENCES-IN-DIFFERENCES
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT BY HOUSEHOLD INCOME

	Dependent variable					
	Years of schooling			Secondary completion		
	Poor	Poorer	Poorest	Poor	Poorer	Poorest
	(1)	(2)	(3)	(4)	(5)	(4)
Young	-0.133	0.119	0.325***	0.0362**	0.109***	0.0941***
	(0.0886)	(0.104)	(0.109)	(0.0171)	(0.0177)	(0.0160)
YearT	-0.143*	0.447***	0.814***	-0.0227*	0.0531***	0.0835***
	(0.0735)	(0.101)	(0.135)	(0.0133)	(0.0163)	(0.0197)
Young*HighI	0.313***	0.386***	0.307***	0.0227	0.0337**	0.0158
	(0.0827)	(0.0801)	(0.0829)	(0.0156)	(0.0137)	(0.0126)
YearT*HighI	-0.170	-0.234*	0.0371	-0.0349*	-0.0269	0.00186
	(0.118)	(0.132)	(0.178)	(0.0202)	(0.0204)	(0.0255)
Young*YearT	0.448***	0.0618	0.239	0.0741***	0.0518**	0.0681**
	(0.0938)	(0.144)	(0.201)	(0.0185)	(0.0256)	(0.0339)
Young*YearT*HighI	0.335**	0.355*	-0.130	0.0742***	0.0391	-0.0317
	(0.148)	(0.187)	(0.266)	(0.0279)	(0.0329)	(0.0449)
Constant	8.835***	8.736***	6.883***	0.276***	0.0916*	-0.0205
	(0.269)	(0.291)	(0.292)	(0.0513)	(0.0484)	(0.0419)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,377	21,439	21,072	22,377	21,439	21,072
R-squared	0.168	0.219	0.219	0.118	0.155	0.146

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Coefficients for the 25 department fixed effects are not included for simplicity.

TABLE A10
TRIPLE DIFFFERENCES-IN-DIFFERENCES
EFFECT OF B18 ON EDUCATIONAL ATTAINMENT BY GENDER

	Dependent variable			
	Years of schooling		Secondary completion	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
Young	0.0179	0.121	0.0818***	0.0728***
	(0.0794)	(0.0829)	(0.0145)	(0.0131)
YearT	0.210***	0.369***	0.0235*	0.0262**
	(0.0771)	(0.0770)	(0.0140)	(0.0119)
Young*HighI	0.470***	0.474***	0.0592***	0.0141
	(0.0624)	(0.0661)	(0.0113)	(0.0105)
YearT*HighI	0.0912	0.212**	0.00505	0.0227
	(0.107)	(0.104)	(0.0186)	(0.0152)
Young*YearT	0.152	0.0639	0.0521***	0.0349*
	(0.101)	(0.104)	(0.0200)	(0.0181)
Young*YearT*HighI	0.0985	0.229	0.0363	0.0534**
	(0.143)	(0.144)	(0.0274)	(0.0246)
Constant	8.494***	7.637***	0.0517	0.115***
	(0.225)	(0.234)	(0.0403)	(0.0359)
Controls	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes
Observations	32,250	32,638	32,250	32,638
R-squared	0.183	0.291	0.141	0.221

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Coefficients for the 25 department fixed effects are not included for simplicity.

Appendix XIII: Effect of B18 on the Educational Attainment of Poor and Extreme Poor Students from Private Schools (Full Regresion Table)

TABLE A11
EFFECT OF B18 ON THE EDUCATIONAL ATTAINMENT
OF POOR AND EXTREME POOR STUDENTS FROM PRIVATE SCHOOLS

	Dependent variable	
	Years of schooling	Secondary completion
	(1)	(2)
Young	-0.0398	0.0318
	(0.0961)	(0.0245)
YearT	0.0601	-0.00552
	(0.0721)	(0.0183)
Young*HighI	0.00691	-0.0582**
	(0.112)	(0.0287)
YearT*HighI	0.0575	0.0289
	(0.187)	(0.0394)
Young*YearT	0.0835	0.0432*
	(0.0846)	(0.0246)
Young*YearT*HighI	0.0792	0.0420
	(0.206)	(0.0508)
Constant	10.33***	0.487***
	(0.326)	(0.0924)
HHmembers	-0.00654	-0.00315
	(0.010)	(0.003)
Urban	0.394***	0.118***
	(0.062)	(0.017)
Male	-0.00415	-0.0103
	(0.039)	(0.011)
Spanish	0.0973	0.0313
	(0.067)	(0.020)
HHincome	0.00000620***	0.00000214***
	(0.00000126)	(0.000000327)
Age	-0.00657	0.00763***
	(0.010)	(0.003)
Controls	Yes	Yes
Department fixed effects	Yes	Yes
Observations	3,964	3,964
R-squared	0.060	0.096

Note: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Coefficients for the 25 department fixed effects are not included for simplicity.