

Schooling and Intergenerational Mobility: Consequences of Expanding Higher Education Institutions *

Noemí Katzkowicz[†] Victor Lavy[‡] Martina Querejeta[§] Tatiana Rosá[¶]

December 1, 2023

Abstract

Poor post-secondary education infrastructure and opportunities partly explain the low higher education rates in developing countries. This paper estimates the effect of a program that improved post-secondary education infrastructure by building many university campuses across Uruguay. Leveraging temporal and geographic variation in program implementation, we use a two-way fixed effect design and comprehensive administrative records to assess the program's causal impact. By lowering the distance to a university campus, the program successfully increased university enrollment, particularly of less privileged students who are the first in their families to attend a university. The program impacted students from localities up to 30 kilometers from the new campus, reducing spatial inequality. Importantly, this expansion did not lower university completion rates. Furthermore, the program increased high school attendance and completion rates and the proportion of educated workers in the affected localities.

JEL Classification: D63, J16, I23, I28

Keywords: Spatial inequality, Education, Public policies, Intergenerational mobility, Developing countries.

*This research project was carried out with financial and scientific support from CAF. We thank Lucila Berniell and all participants in the Academic Workshop "RED 2022: Intergenerational mobility in Latin America" for excellent technical suggestions and comments. We gratefully acknowledge the central offices at Universidad de la República for providing us with the data and valuable information on the program, comments, and suggestions. We also thank participants at seminars in Instituto de Economía at Universidad de la República in Uruguay, Pontificia Universidad Católica de Chile, EAFIT University in Colombia, Center for Human Development Studies of the Universidad de San Andrés in Argentina, WIDER development annual conference in Colombia, and the Annual Meeting of the Chilean Economic Society (SECHI) in Chile for useful comments and suggestions.

[†]Instituto de Economía, FCEA, Universidad de la República. Email: noma.katzkowicz@fcea.edu.uy

[‡]Department of Economics, Warwick University and the Hebrew University of Jerusalem. Email: vic-tor.lavy@mail.huji.ac.il

[§]Departamento de Economía, FCEA, Universidad de la República. Email: martinaquerejeta@gmail.com

[¶]Pontificia Universidad Católica de Chile. Email: tatiana.rosa@uc.cl

1 Introduction

The rate of return on higher education in developing countries is high and increasing (Montenegro and Patrinos, 2014). Yet, post-secondary schooling attainment remains low. There are diverse explanations for this low human capital investment (Delavande and Zafar, 2019; Caucutt and Lochner, 2020) related to family background and city or locality characteristics (Carneiro and Heckman, 2002). These are related to the so-called “birth lottery” (Chetty et al., 2014) and further lead to low intergenerational mobility (Black et al., 2005; Black and Devereux, 2011; Björklund and Salvanes, 2011) and spatial inequalities in opportunities (Chetty et al., 2014).

This study investigates the impact of a public program in Uruguay aimed at improving access to post-secondary schooling, focused on the population residing outside the capital city. Uruguay has high per capita income and relatively low-income inequality compared to most Latin American countries. However, despite free public education, including university, completion rates for secondary schooling and university enrollment remain low. Moreover, Uruguay lags in upward mobility in secondary education (Berniell et al., 2021). Therefore, Uruguay is an interesting context to assess the effects of investments in higher education infrastructure.

The program aimed at building new university campuses nationwide started in 2008. Budget constraints forced the program’s gradual implementation, which we leverage for identification. Based on time and geographic variation in the program’s timing, we estimate a two-way fixed effect model to provide causal evidence of the program’s effects. Our analysis relies on novel administrative records of students’ enrollment in Uruguay’s main university –*Universidad de la República*. The data covers 2002 to 2020, encompassing over 86% of Uruguay’s total tertiary students.¹

The university expansion effectively reduced the average distance to a university campus by 300 kilometers (km), which accounts for more than half of the longest distance one can traverse within Uruguay (571 km). To assess its impact, we first estimate the effects of this change on university enrollment and intergenerational mobility. We use the share of students who are the first generation in their families to enroll in a university as a measure of intergenerational mobility.² According to the National Household Survey (NHS) in 2020, around 80% of individuals did not pursue tertiary education. Therefore, we focus on the top 20% of the educational ladder.

Our findings reveal that opening a new campus in a given locality resulted in an average increase of 0.36 percentage points (pp) in the share of enrolled students among the population under 30 during the first six years after the campus opening.³ This effect size is sizable, with a 37 percent increase

¹The remaining 14% attend vocational training, teacher training programs or private universities (Udelar, 2020).

²Our analysis only focuses on the university population. Therefore, we abstract from the bottleneck that might exist in completing high school.

³This equates to around 100 new enrolled students from that locality.

relative to a pre-treatment mean of 1.1%. Additionally, we find an average increase of 0.32 pp in the share of first-generation students in total enrollment during the same period. The implied effect size is a 40% increase relative to the pre-treatment share (0.8%). Notably, the program had a more pronounced effect among students from less advantaged backgrounds as measured based on their parents' educational attainment. The effects on total enrollment extend to localities up to 30 km from the new campus, and the impact declines as we move further away. Furthermore, our results show that the program did not significantly lower the university degree completion rate, in contrast to arguments in the policy debate that there is a potential trade-off between universalizing access to higher education and maintaining degree attainment rates.⁴

We also examine the program's effects on other educational outcomes and its labor market consequences. We find that the program increased high school attendance rates in localities where new campuses were built, which may increase further tertiary education in the longer term. Additionally, the expansion of university campuses increased the share of highly educated workers in the labor market in treated localities, which led to a decline in the wages of these workers in the short term.

Moreover, results hold when relying on an alternative identification strategy that leverages the exposure to treatment, measured by the age of individuals when the campus was opened and the staggered geographic implementation. We follow [Duflo \(2001\)](#) to this aim and estimate a Canonical Difference-in-Differences (DiD) model with controls. This alternative analysis is conducted using data from the NHS, which, unlike university administrative records, allows us to observe individuals who did not choose to attend university and older cohorts. This enables us to exploit variation in the exposure to treatment. Although this identification approach relies on micro-level data and properly identifies the program's causal effect, the NHS data lacks information on individuals' parents, making it impossible to analyze intergenerational mobility outcomes. Furthermore, due to the survey nature of the data and its representativeness, we had to restrict our sample to only municipal capitals. These limitations make this alternative strategy an important robustness check for our main analysis but limit its use as our primary identification strategy.

This paper contributes to two strands of the literature. First, to the literature on the role of public policies in educational attainments. There is evidence of the effects of changes in compulsory schooling laws ([Angrist and Krueger, 1991](#); [Meghir and Palme, 2005](#); [Oreopoulos, 2006](#); [Alzúa et al., 2015](#)), school infrastructure investments ([Duflo, 2001](#); [Mazumder and Triyana, 2019](#); [Akresh et al., 2022](#)), and college proximity ([Card, 1993](#)). Literature shows that such public policies have significant short and long-term effects on years of education, labor market outcomes, crime, and teenage preg-

⁴A potential concern arising from increased access to high-level education is that it may create congestion effects, which could diminish the overall quality of education that students receive ([Duraisamy et al., 1997](#); [Guerra and Lastra-Anadón, 2019](#); [Buckner and Zhang, 2021](#)).

nancy. More related to our research, previous studies show that geographical distance to university is an important determinant in youth's decision to enroll in tertiary education (Alm and Winters, 2009; Spiess and Wrohlich, 2010) and that it has significant labor market consequences (Carneiro et al., 2023). Opening new universities significantly affects local enrollment and has even stronger effects among lower-income students (Frenette, 2009; Lapid, 2018). However, most of this literature focuses on developed countries, and there is little evidence for developing countries' contexts (Duflo, 2001; Alzúa et al., 2015; Alzúa and Velázquez, 2017; Mazumder and Triyana, 2019; Akresh et al., 2022). Related studies show that public investment improves education outcomes and intergenerational mobility. For example, investment in transport infrastructure (Meneses et al., 2021), school construction (Hertz and Jayasundera, 2007), and expenditure on primary schools (Behrman et al., 2020). Consistent with this literature, we add the findings that the geographical expansion of university campuses increased human capital and intergenerational mobility and reduced spatial educational inequality.⁵

Our work is also closely related to two papers that study a higher education policy change in Chile. Espinoza et al. (2022) find that the introduction of gratuity in higher education has only marginal effects on the first-generation enrollment rate, questioning the hypothesis of the importance of financial constraints. On the other hand, Rau et al. (2012) show that access to credit reduces dropout from higher education, and the effect is even stronger among low-skill individuals from low-income families. Nevertheless, they also find negative effects on earnings, possibly mediated by a reduction in the quality of education. We provide robust evidence consistent with the latter study, showing that university expansion increases enrollment and the share of first-generation university graduates.

Second, this paper also relates to the literature on the intergenerational transmission of education in developing countries. Despite the vast empirical literature on income mobility in developed countries (Black and Devereux, 2011; Chetty et al., 2014; Jäntti and Jenkins, 2015; Chetty et al., 2020) and the growing evidence for developing countries (Cuesta et al., 2011; Ferreira et al., 2013), fewer studies focused on the role of education (Black et al., 2005; Björklund and Salvanes, 2011; Daude and Robano, 2015; Fleury and Gilles, 2018; Björklund and Jäntti, 2020), especially in developing countries (Torche, 2019; Bautista et al., 2023). One relevant study finds that intergenerational educational mobility is rising in Latin America, mainly at the lower part of the distribution (Neidhöfer et al., 2018). We add to this literature by focusing on mobility at the upper part of the educational distribution and addressing spatial inequality.⁶

⁵Méndez (2020) analyzes the same program and finds that university enrollment increases are mostly explained by students from high-educational backgrounds. However, the evidence is not causal and does not address the intergenerational mobility dimension.

⁶Previous literature on intergenerational mobility in Uruguay provides evidence on income mobility (Araya, 2019; Leites et al., 2020), years of education (Sanroman, 2010; Gandelman and Robano, 2012; Soto, 2020), and occupations (Urraburu, 2020), but there is no evidence focusing on the top of the educational distribution. Also, this literature does not analyze the spatial dimension of mobility.

The rest of the paper proceeds as follows. Section 2 describes the relevant institutional context, including details on the university's geographic expansion program. Section 3 describes the data, sample selection, and the diverse outcome variables used in the study. The primary identification strategy is presented in Section 4. Section 5 shows the main results of evaluating the university program, including heterogeneity analysis and spatial spillovers. Section 6 shows the effects on other educational and labor market outcomes, and Section 7 provides evidence relying on an alternative identification strategy. Finally, Section 8 concludes.

2 Institutional Context

Uruguay's population is 3.5 million inhabitants. It ranks fourth in GDP per capita (World Bank Statistics⁷) and third in the Human Development Index (UNDP, 2022) among Latin American countries. It has a strong welfare system, providing free access to public education at all levels, including university schooling. Moreover, public universities have no admission exams. Yet, secondary school completion rates and tertiary education enrollment are low.

Educational coverage at mandatory levels in Uruguay has increased substantially in the last decade.⁸ Nevertheless, while coverage is almost universal until 14 years of age, educational attainment significantly drops in upper secondary school (ages 15 to 17). Moreover, strong socioeconomic patterns still prevail, and individuals from low SES face lower educational attainment at almost all levels (INEEd, 2018).⁹ Therefore, while educational attainment up to the lower secondary school is high relative to other Latin American countries, the secondary school completion rate is still relatively low: 40 percent versus 62 percent in Latin American average (INEEd, 2020). Consequently, tertiary education attainment is low, as only 26 percent of the population completes some university schooling with a strong socioeconomic gradient (Udelar, 2020).

Geographically, Uruguay is divided into 19 divisions called departments, and each department is subdivided into several locations. Half the country's population resides in the capital city (Montevideo). Uruguay's primary university, Universidad de la República, was historically located in Montevideo, its enrollment covering 86 percent of all tertiary students.¹⁰ In 2008, the university authorities started a program of building campuses in many other parts of the country. By 2020, six of the nineteen departments had university campuses. Figure 1 shows the timing of university expansion.¹¹

⁷See <https://data.worldbank.org/indicator/NY.GDP.PCAP.PP.CD?locations=ZJ>

⁸Particularly, secondary completion rates have increased by almost 10 pp since 2006, as shown in panel (b) of Figure 4.

⁹See Panel (a) of Figure 4.

¹⁰The remaining 14% attend vocational training, teacher training programs, or private universities (Udelar, 2020).

¹¹Prior to the program starting, three localities already had some university infrastructure: Paysandú, Salto, and Rivera. However, it was not until 2010, 2011, and 2013 that the expansion was consolidated with the corresponding allocation of resources and the increase in the supply of majors.

Before this program, Uruguay had a high spatial inequality in university enrollment rates and in education mobility. Panel a in Figure 2 shows the average number of students enrolled in university per 1,000 inhabitants by geographic region between 2002 and 2007. In Montevideo, 28 students (per 1,000 inhabitants) enroll in university yearly, while in other departments, it ranges from 7 to 16. Moreover, the orange points in Figure 2 suggest that the new campuses opened in or near locations with a low average number of enrolled students.

Similarly, panel (b) shows the average proportion of students who are the first in their families to enroll in university. The distribution of these rates in Figure 2 also shows the same pattern: high spatial inequality in educational opportunities.

An increase in enrollment of students living outside Montevideo followed the geographical expansion. Panel (a) in Figure 3 shows the evolution of the number of entry students by the geographical region where they lived before entering university. In 2002, students from outside the capital represented 44.5% of total enrollment, while in 2020, this rate increased to 58 percent. Moreover, the decentralization program effectively reduced the migration of university students from their home locality to the capital city. Panel (b) in Figure 3 shows the share of entry students by the geographical region where they enrolled in their first year. The percentage of students enrolled at a university campus outside the capital city increased from around 0.2 to 15 percent.

3 Data

We use administrative records of students enrolled at Uruguay's public university from 2002 to 2020.¹² Our primary dataset consists of a combination of administrative records for entry students and census data applied to all students during the entry year on a mandatory basis. In addition, information on university completion is available for a shorter period from 2006 to 2020. The data includes students' demographic and socioeconomic information and education data such as their secondary school, years to completion, and the maximum educational level attained by their parents. We also use information on students' geographic location before commencing university schooling.¹³ Finally, we also use data from the National Household Survey (NHS) collected by the National Institute of Statistics in Uruguay for the years 2002 to 2019.¹⁴ We use this information to study the program's effect on other educational and labor market outcomes on a nationally-representative basis, as well as for the alternative identification approach.

¹²For 2020, we include only enrollment before the COVID pandemic started in March.

¹³The dataset provides information on the center where the student completed her last year of secondary education. We extracted or computed the geographical information based on that and recovered this information for 93% of the original sample.

¹⁴Nationally representative cross-section survey. Data and all related documents are available through this [link](#).

Empirical measures. The treatment variable is a dummy indicator of the opening of a new campus in the locality based on the timeline presented in Figure 1. The primary outcome variables are students' enrollment and degree completion. At the individual level, the enrollment indicator takes value 1 for all individuals in our sample (all enrolled at Universidad de la República), and the completion indicator takes value 1 if the student completed a university degree within five years since enrollment and 0 otherwise.¹⁵ These variables are then collapsed to the means at the locality level, based on the locality in which a student completed high school. We estimate the effect of the expansion of the university campuses on locality-level means of general enrollment and enrollment of first-generation students (FGS), defined as those who are the first in their families to enroll in a university.¹⁶

To measure the program's impact on enrollment, we use the share of enrolled students over the total population under 30 years old. To avoid potential endogeneity of the contemporaneous number of inhabitants under 30 in a given locality, we use the population under 30 in 1996, before the start of the program.¹⁷ We compute the share of FGS in the total population under 30 and denote this measure *FGS over population*. The share in total university enrollment is denoted as *FGS over enrollment*. The first measure allows us to assess the change in the number of students who moved up the educational ladder relative to their parents. We view the share of FGS in total enrollment as a *proxy* for intergenerational mobility.¹⁸

Estimation sample. To estimate the effect of the university campuses expansion program, we restrict the sample to individuals aged 30 or less upon starting university schooling. Additional sample restrictions are the following: including only those who appear in the enrollment administrative records and have completed the Census sample in their first university enrollment year; only students whose location before enrolling in university is known; and students from other than localities in Montevideo and Canelones. The former is where the main campus was located for many years, and the latter is its geographically close neighbor. The population in these areas is also very different in socioeconomic characteristics and transport infrastructure. The estimation sample consists of 64,820 students from 140 different localities, yielding 1,226 locality-year observations.

Descriptives. Table 1 presents summary statistics for the main variables at the individual and locality levels. Sixty-three percent of the sample are female, and the average age at enrollment is

¹⁵We limit this analysis to five-year time intervals, as representing the typical duration required to earn a degree in most university careers.

¹⁶We account for all students from households where any parent ever enrolled in university. We considered parents' enrollment in university irrespective of completion.

¹⁷1996 is the last Population Census before the program.

¹⁸This measure will not capture downward mobility and will signal upward mobility in the right tail of the human capital distribution. Also, the proposed measure can be considered part of the family of those measuring absolute intergenerational mobility.

19. Twelve percent of the students have parents with primary education as the maximum level, 51 percent secondary, 18 have tertiary vocational education, and 18% have university schooling.¹⁹ That is, 82% of the students are the first in their families to enroll in a university. The proportion of students ever completing a university degree is 36% among students that entered university between 2002 and 2015.²⁰

The average number of enrolled students at the locality level is 129, and the average share of enrolled students in the population under 30 years old is 2%. Of the total enrolled students, 102 are FGS, an average share of 80%. Finally, the locality-average probability of completing a university degree within five years of enrollment is 9%, the same mean for FGS.

4 Empirical Strategy

As a starting point, we provide evidence that distance affects enrollment. Estimates of a Two-Way Fixed Effects model show that an increase of 100 km to the campus reduces the share of enrolled students by 0.09 p.p, implying a decrease of around 8% of the pre-treatment mean (Online Appendix Table A.1). We find similar results for FGS, indicating that increasing the distance to a university decreases the enrollment share of FGS in total enrollment. Consistent with Card (1993), we provide evidence that distance to the campus significantly explains variations in university enrollment and intergenerational mobility. This enhances the importance of investigating the effects of the Uruguayan program aimed at reducing the distance to university campuses.

To estimate the program's causal effect, we use a staggered DiD strategy that leverages the variation in treatment time across localities.²¹ We then exploit this variation and estimate a static two-way fixed effects (TWFE) model and a dynamic one following the event study approach in Borusyak and Jaravel (2017). The author's imputation estimation only estimates the counterfactual values on untreated observations and extrapolates it to the treated observations. We run our analysis at the locality level and estimate the average program's effect on university enrollment, intergenerational mobility, and degree completion.²²

In our empirical strategy, each locality l at time t belongs to one of the following three groups: (i) untreated, if no new campuses ever opened in that locality; (ii) localities to be treated, namely no

¹⁹Four percent of students have missing information on the father's education and 0.5% on the mother's. For those, the maximum level of the non-missing parent was considered for computing the household's educational background. No students have missing education data for both parents.

²⁰Entry students from 2016 onward are not considered as we only have information on completion until 2020.

²¹See Goodman-Bacon (2021) for a review of difference-in-differences with variation in treatment timing.

²²Running our analysis at the individual level is possible for some of our main outcomes, such as the probability of obtaining a degree conditional on enrollment. Still, given that we have administrative records of only the students who enrolled in university, we cannot compute the probability of an individual enrolling in university. Online Appendix Table A.5 and Online Appendix Figure A.3 provide estimates at the individual level.

campus opened yet but it will eventually; and (iii) treated localities where new campuses have already opened. Individuals are assigned to localities where they lived before enrolling in the university. The control group includes the first two groups, and the treated group includes students from localities where new campuses opened according to the timeline presented in Figure 1. Our identification assumption is that conditional on locality and calendar year fixed effects, campus openings and timing are not correlated with potential outcomes.

To assess the program’s effect on the outcomes of interest, we estimate the following models at a locality(l)-year(y) level:

$$Outcome_{l,t} = \alpha_0 + \mu_l + \mu_t + \gamma E_{l,t} + \epsilon_{l,t}^s \quad (1)$$

$$Outcome_{l,t} = \alpha_0 + \mu_l + \mu_t + \sum_{h=-a}^{h=b-1} \gamma_h 1[K_{lt} = h] + \gamma_b^i [K_{lt} = b] + \epsilon_{l,t}^d \quad (2)$$

where μ_l and μ_t are the locality and year (two-way) fixed effects. In equation 1, $E_{l,t}$ takes value 1 if there is a campus in locality l at year t and 0 otherwise. In equation 2, $a \geq 0$ and $b \geq 0$ are the numbers of included “leads” and “lags” of the event indicator, respectively. $K_{l,t} = t - E_l$ is the number of periods since the event date $E_{l,t}$, namely the relative time variable in an event study design. We also allow that some units are never treated, denoted by $E_{l,t} = \text{inf}$. The coefficients on the leads are interpreted as pre-trend measures and the hypotheses that $\gamma_{-a} = \gamma_{-a+1} = \dots = \gamma_{-2} = 0$ is tested visually and statistically. Conditionally on this test passing, the coefficients on the leads are interpreted as a dynamic path of causal effects. $\epsilon_{l,t}^s$ and $\epsilon_{l,t}^d$ are the error terms in the static and dynamic model, respectively

As is often common, there is variability in students’ adherence to the reform within treated localities. In our empirical analysis, individuals are exposed to the program if they lived where a new campus opened before enrolling at a university. Therefore, we are identifying an intention to treat effect that can be viewed as a lower bound of the treatment effect.

5 The Effect of University Expansion

5.1 University Enrollment and Intergenerational Mobility

Figure 5 and Online Appendix Table A.3 show the estimates of Equation 2 for the share of enrolled students relative to the population under 30 during the first six years of the program (Panel a), the share of enrolled students in the population under 30 during the first ten years of the program (Panel b), the share of FGS in the population under 30 (Panel c), and the share of FGS in the total enrollment

(Panel d). The static TWFE estimates are shown at the bottom of each event study graph and reported in Table 2.

The share of enrolled students in treated localities during the six years following the campus opening increased by 0.36 p.p. relative to the share in untreated localities. Though the effect seems to decrease in years 5 and 6 after campus opening, this trend does not persist, as shown in Panel b, implying that the program had a positive and significant effect on enrollment in the long run (0.34 p.p for ten years).²³ This effect is sizable, especially considering the pre-treatment mean enrollment share of 1.1%, accounting for nearly 37% of the pre-treatment mean. This estimate is not statistically different from the respective estimate obtained using the static model.

The effect on the share of enrolled FGS is also statistically significant and positive. According to the event study and the static model estimates, opening a campus in a given locality increased the share of FGS in the population under 30 by 0.32 p.p. Regarding total enrollment, the effect seems to decrease in years 5 and 6 after campus opening. Still, this trend does not persist in time. This implies that in localities where the campus opened, the share of individuals surpassing their parents' educational level increased significantly with the program, providing evidence of the program's effect on educational intergenerational mobility. Finally, comparing the program's effect on FGS enrollment in total enrollment, we find that it increased 6.1 p.p. Both the static and the dynamic approaches yielded the same estimate. This suggests that the program also affected the enrollment composition, increasing the share of first-generation students among the total number of students.²⁴

The preceding results underscore the program's impact on disadvantaged students. Our findings provide compelling evidence that reducing the distance to the university can help overcome various barriers to university enrollment, such as financial constraints. This results in increased enrollment and positively enhances intergenerational mobility in educational attainment at the top of the educational distribution.

5.2 Heterogeneous Effects

We explore the heterogeneity of the results presented above by parental education. Online Appendix Figure 6 and Online Appendix Table A.4 show the estimates of the dynamic model by parental educational level. The first group includes students with parents with some primary education (Panel a), the second those with parents with some secondary education (Panel b), and the third those with parents with some tertiary education (Panel c). Table 3 shows the static model estimates. Overall, expanding university campuses had a larger effect on students from less educated families. In all cases,

²³Estimates using a ten-year window for the after-program period are less precise given that our panel is balanced only for the first six years after the program implementation (the last university campus opened in 2013).

²⁴Results on the total number of enrolled students and total FGS are reported in Online Appendix Figure A.1

the results of the static and dynamic models are consistent.

Panel (a) and Column 1 in Table 3 show that during the first six years after the expansion, the share of students from families with parental primary education enrolled in university increased by 0.067 p.p. This effect size is large considering this group's pre-treatment mean enrollment share of 0.10%. The effect size accounts for 67% of the pre-treatment mean. Panel (b) and column 2 in Table 3 show the same pattern for students whose parents reached secondary education. After the program's implementation, the proportion of these students increased by 0.22 p.p on average relative to a pre-treatment mean of 0.51%. Finally, Panel (c) and Column 3 in Table 3 show a smaller effect of 0.034 pp on the enrollment share of students with parents with tertiary education. Though the effect is not statistically different from zero at a 10% significance level, it accounts for nearly 20% of the pre-treatment mean (0.18%).

Overall, our results suggest heterogeneous effects of the program depending on students' parental backgrounds. While the larger estimate is among students whose parents have secondary education, the higher effect relative to the pre-treatment mean is observed among students whose parents only achieved primary education. These results are consistent with the program having a greater impact on students from more disadvantaged backgrounds. While we do not have information about the channels through which the program operated, this heterogeneity could be attributed to the fact that less educated parents may be less able to financially support their children's while in college or may not transmit preferences for higher education.

5.3 Spatial Spillovers Effects

While campuses open in a given location, the program can also affect students from nearby localities. To assess such potential spillover effects, we also considered as treated localities in a radius of 30 or 50 km of the new campus. Thus, we estimate an alternative model specification considering the distance to the new campus. We use geo-referenced data to calculate the geodetic distance between each locality in a given department, year, and the closest new campus.²⁵ We then define two buffers centered at the locality where the new campus opened with a 30 and 50 kilometers radius. Figure 1 shows the treated localities under the new definition.

Table 4 shows the static TWFE estimates for this alternative treatment definition. The variable *Buffer 30 km* takes the value of 1 if the locality is within 30 kilometers (or less) of where the campus opened, excluding the localities where the campus opened (distance 0) and 0 otherwise. Similarly, *Buffer 50 km* takes the value of 1 if the locality is more than 30 kilometers but less than 50 kilometers

²⁵Geodetic distance is the length of the shortest curve between two points along the surface of a mathematical model of the earth. We use the *geodist* stata command and latitude and longitude information on each locality and campus to compute it.

away from where the campus opened and 0 otherwise. The omitted category includes the non-treated localities. Column 1 shows the program's effect on the share of total enrollment relative to the population under 30, column 2 on the share of FGS relative to the population under 30, and column 3 on the share of FGS in total enrollment.

Overall, the positive program effects on the share of enrolled students and the share of FGS hold up to 30 km. Furthermore, the Wald test result suggests that the program's effect on treated localities is similar that on localities 30 km away.²⁶ Nevertheless, the program's effect seems to vanish in localities 50 km from the campus, implying that these localities show the same outcomes as non-treated localities.

Results point in a different direction when examining the estimates presented in column 3. The program's impact on the share of FGS in total enrollment disappears when considering localities other than those where the campus opened. This implies that while the program increased enrollment for FGS relative to second-generation students, this effect is localized. One possible explanation is that FGS may be more distance-sensitive due to potential financial constraints. Even though 30 kilometers may not seem a significant distance, it could act as a barrier to enrollment for more disadvantaged students. However, the program was still effective in attracting FGS from areas located 30 kilometers away from the campus; the difference between it and the effect on second-generation student enrollment is no longer statistically significant.

As a robustness check of our analysis, we estimate the spatial spillovers using a continuous distance measure (treatment intensity). Columns 1, 2, and 3 in Table A.2 show how the program's effect varies with the distance to the campus. The estimated program's effect when the distance is modeled linearly is positive for all three outcomes, and it declines as the distance from campus increases.

5.4 University Completion Rate

Given that there is neither tuition nor an entry exam in the Uruguayan public university, a potential adverse program's effect could be universalizing access at the cost of higher dropout rates. To explore this possibility, we analyze the program's impact on the probability of a student obtaining a degree five years after enrolling. To do so, we restrict the sample to only those students who enrolled in university between 2002 and 2015, ensuring at least a five-year gap between enrollment and degree attainment. Online Appendix Figure 7 and Online Appendix Table A.6 show the results for the share of students who completed a degree within five years of enrollment (Panel a) and the share of FGS who completed a degree within five years among all FGS enrolled at the university (Panel b). The

²⁶We run a Wald test on $\beta_{treated} = \beta_{30km}$ and find a p-value of 0.7260, rejecting the null hypothesis of the effects being different

analysis is run based on the enrollment year of each student. That is, year 0 shows the share of students enrolled in university the year the campus was opened and completed a degree within five years. Results show that the proportion of students achieving a university degree did not change significantly with the program. Our results provide evidence of no drop-out cost of the university expansion program.

As this analysis relies on a smaller sample, we provide evidence of the absence of sample selection in the university enrollment results estimated using this restricted sample. Online Appendix Figure [A.2](#) shows that the enrollment and intergenerational mobility results hold when considering this ‘completion sample’.

6 Program’s Spillovers Effects

6.1 High School Outcomes

So far, we have presented evidence that university expansion has positively impacted university enrollment, especially for FGS, without any associated decline in completion rates. We may expect that the program will also influence lower-level educational attainment by enhancing the benefits of completing high school education, thanks to the new opportunities for further formal studies (see for example [Lavy \(1996\)](#), [Lincove \(2009\)](#), and [Mukhopadhyay and Sahoo \(2016\)](#)).

We use data from a nationally representative National Household Survey conducted from 2002 to 2019 to examine such spillover effects. We use the methodology described in Section [4](#) to estimate the program’s effect on high school outcomes. To estimate the effect of high school attendance and high school completion, we restrict the sample to individuals aged 15 to 18 and 18 to 30 respectively.

Figure [8](#) and Online Appendix Table [A.8](#) show the effect of university expansion on high school attendance for individuals aged 15 to 18 (Panel a) and high school completion for individuals aged 18 to 30 (Panel b).²⁷ The estimated effects indicate increased high school attendance in localities where new campuses opened. Over one thousand (982) additional high school students were enrolled in high school during the first six years after the program’s implementation. This change implies an increase of around 25% relative to the pre-treatment mean. There is also a significant rise in the average number of individuals completing high school education: 1,086 additional students during the first six years after the program implementation. That is, the program successfully generated incentives to continue formal schooling. Furthermore, we observe an anticipation effect consistent with the notion that potentially attending a nearby university motivates young students to pursue formal education.

²⁷The selection of these age groups is based on the fact that ages 15 to 18 represent the majority of high school attendance, while individuals aged 18 to 30 are expected to have already completed high school.

6.2 Labor Market Outcomes

This subsection presents estimates of the program’s effect on labor market outcomes. Obviously, opening a campus will expand employment in a locality, but we are interested in the program’s effect beyond this ‘mechanical’ impact. We use data from National Household Surveys of 2002 to 2019 and the same methodology described in Section 4, but here we use individual-level data. To estimate the effect of labor market outcomes, we restrict the sample to individuals aged 21 to 30. Table A.7 shows the main sample statistics.

Panel (a) of Figure 9 presents the impact of the university campuses expansion on the share at the locality level of employed individuals aged between 21 and 30 with more than 12 years of education (i.e., highly educated workers) over total employed individuals. Panel (b) presents the effects on highly educated workers’ monthly earnings based on estimates of regressions at the individual level. In both cases, we redefine treatment year to 4 years after campus openings to allow for some time gap between enrollment and expected effects. These results combine the direct effect that enrolling in a university may have on subsequent labor outcomes and the indirect spillover effects on individuals who did not enroll at a university but live in a treated locality. Any potential direct program’s effect will come with some years of delay, and any contemporary effect might be interpreted as a mechanical effect on the local labor force.

The results show a decline in the share of highly educated workers during the program’s first years (period -3 to 0 in panel a of the figure). We view this as an indication that some students finishing secondary school do not enter the labor market and instead enroll in a university. After completing five years of university schooling (period one onward in the figure), there is a significant 2.1 pp increase in the share of highly educated workers in treated localities. After eight years, the increase is 5 pp (Panel a). On the other hand, there is suggestive evidence of a decrease in the returns to education among highly educated workers in treated localities (Panel b). Although the static TWFE estimates are not precise, they show a pattern of a reduction in the earnings of highly educated workers. The mechanism is a local increase in the supply of college graduates, which lead to a negative but small short-term impact on their earnings.

Additionally, Online Appendix Table A.9 shows no treatment effect on the probability of employment among the population nor among highly educated workers. Given that there is no selection due to an effect on employment, we estimate ‘Mincerian’ equations on the sample of the employed to explore treatment effects on labor earnings. To evaluate the program’s effect on the labor market outcomes of highly, middle, and low educated workers, we define three dummy variables: *HighEducated* takes value 1 if the individual has at least 12 years of education and 0 otherwise, *MidEducated* takes value 1 if the individual has between 7 and 12 years of education and 0 other-

wise, and finally, *LowEducated* takes value 1 if the individual has 6 or less years of education and 0 otherwise. The estimates presented in Online Appendix Table A.11 show a negative effect on the rate of return to education for highly educated workers, although it is not statistically significant.

7 Evidence Based on an Alternative Identification Strategy

7.1 Effects on Educational and Labor Outcomes

Since the program only affects students old enough to enroll in university, we leverage the variation in exposure to treatment and the staggered geographic implementation to construct an alternative identification strategy. In Uruguay, students typically decide to enroll in university after completing their high school education, at around 17 or 18 years old. However, grade repetition and delayed school entry can postpone the university enrollment decisions for some students in our sample. As a result, older cohorts at the time of the campus opening in their localities may also benefit from the program.

In this alternative identification strategy, we use individual-level information on educational outcomes from the National Household Survey. We focus on the number of years of education and the probabilities of being high, middle, or low educated as the educational outcomes of interest. Online Appendix Table A.10 shows the descriptive statistics for this estimation sample. Compared to the NHS sample used for the staggered analysis of the program (Online Appendix Table A.7), this sample has older individuals with a higher probability of being highly educated, married, or employed.

The program is expected to impact the mid and long-term labor market outcomes. Ideally, we would like to measure the program impact 20 years later, as in Duflo (2001). Nevertheless, we do not have such a long-term horizon in our context. Therefore, we estimate the program impact on labor market outcomes the further in time possible, restricting our sample to 2018 and 2019.

Before showing evidence of difference-in-differences estimates, we first estimate a dynamic model to assess the heterogeneous program's effect for different age cohorts. We run the following regression model, using data at an individual(*i*)-locality(*l*)-year(*y*) level:

$$Outcome_{i,l,t} = \beta_0 + \sum_{c=10}^{c=30} \gamma_c (Campus_{l,t} \times Dc_i) + \beta_1 X_i + \mu_l + \mu_{a(i)} + \mu_t + \epsilon_{i,l,t}^d \quad (3)$$

where Dc_i is a dummy variable that takes value 1 if individual i is aged c when the campus opened and 0 otherwise, for $c = 10 \dots 31$. Cohorts aged 31 are the omitted category in this model. Since there is no opening date for non-treated localities, as a conservative approach, we consider an individual's age in 2008 for non-treated localities (the year of the first campus opening). The variable $Campus$ takes the

value 1 if there is a campus in locality l in year t and 0 otherwise. X_i is a set of variables accounting for individual characteristics, including gender, marital status, and number of kids under 12 years old. Then, the set of parameters γ_c shows the program effect for the different cohorts. Finally, μ_l , $\mu_{a(i)}$, and μ_t stand for a locality, individual age when the campus opened, and calendar year fixed effects, and ϵ^d represents the idiosyncratic shock of the model.

Figure 10 shows the results. The treatment effects for most cohorts older than 20 at the time of campus opening fluctuate around zero. Some are positive but small and insignificant. The zero effect or lack of precision in the estimates for these older age cohorts is likely because many individuals in this age group have already made their schooling decisions. On the other hand, the estimates for the 16-20 cohorts are positive and statistically significant, as shown by the 95 percent confidence intervals. Also, the effect sizes are large. For example, for those 19 years old, when a campus was opened, years of schooling increased by 0.8.

Results are similar when we use the indicator of high-level education as an outcome. While the effect is positive and significant for cohorts aged 15 to 20, it is smaller and insignificant for any of the older ages when the campus was opened. For ages 16-19, the likelihood of having a high education level is higher by almost 10 percent.

We use these results to guide building treatment and control groups for estimating Difference-in-Differences (DiD) models with controls following a similar approach to Duflo (2001). To this end, we consider the localities where campuses opened as treated (after their respective opening dates) and those where campuses never opened as non-treated.²⁸ Therefore, we consider cohorts aged between 14 and 18 when the campus opened as exposed to the program and those aged between 30 and 35 as non-exposed cohorts. We will also show the evidence when varying the ages of treated and control groups. Finally, we rely on the double difference between exposed and non-exposed cohorts and between treated and non-treated localities to estimate the program's causal effect.

Online Appendix Table A.12 presents the balancing tests for mean differences in pre-treatment characteristics. Treated and non-treated localities did not differ significantly regarding average years of education, the share of highly educated individuals, employment rates, and the proportion of women. However, treated localities did exhibit, on average, a slightly higher share of low-educated and married individuals, along with higher labor earnings among those employed. In contrast, non-treated localities had a greater share of individuals with middle education and a higher average population age.

Online Appendix Tables A.13 and A.14 compare the means of some of the main outcomes and

²⁸In this alternative identification approach, we use as controls only individuals living in municipal capitals, apart from Montevideo and Canelones, to keep treatment and control groups as similar as possible in terms of observable characteristics

characteristics. These tests are based on a comparison between exposed and non-exposed cohorts in localities where campus opened and where they did not. This comparison already suggests that the programs affected educational outcomes of exposed cohorts in treated localities but had no effect on labor market outcomes and other demographics.

Then, to assess the causal program’s effect on the outcomes of interest, we estimate the following model at an individual(i)-locality(l)-year(t) level:

$$Outcome_{i,l,t} = \beta_0 + \gamma(Exposed_i * Campus_{l,t}) + \beta_1 X_i + \mu_l + \mu_{a(i)} + \mu_t + \epsilon_{i,l,t}^d \quad (4)$$

where $Exposed * Campus$ is the interaction of the two dummy variables, $Exposed$ that takes value 1 if the individual i is aged between 14 and 18 the year the campus opened or in 2008 if a campus never opened in locality l , and 0 if aged between 30 and 34 at that time, and $Campus$ that takes value 1 if there is a campus in locality l in year t and 0 otherwise. X_i is a set of variables accounting for individual characteristics, including gender, marital status, number of kids under 12 years old, and years of experience. Then μ_l , $\mu_{a(i)}$, and μ_t stand for a locality, individual age when the campus opened, and calendar year fixed effects. Finally, ϵ^d represents the idiosyncratic shock of the model.

We define three dummy variables for individuals with high, middle, and low levels of education, as explained in Section 6.2.²⁹ As for the labor market outcomes, we focus on employment, the log earnings, and the log earnings conditional on employment. We follow the definition of the same variables detailed in Section 6.2.

Table 5 shows estimates of the program’s effects on educational outcomes. The estimate in column 1 shows the effect on completed years of schooling. This estimate is positive and statistically significant, about half a year of schooling. It implies a large effect size, nearly 5% of the non-exposed cohorts’ average years of education (9 years). Column 2 presents the program’s effect on the probability of individuals having high-level education. This estimate shows a significant increase of 3.2 p.p in the probability of exposed cohorts having high-level education in the localities where the campus opened. This effect is sizable, accounting for nearly 18% of the non-exposed cohorts’ mean. Both results are consistent with the program affecting university enrollment. Column 3 shows no program effect on the likelihood of individuals having middle-level education. This result aligns with an intensive margin effect; people who otherwise would have terminated their schooling in high school, following the program they continue into post-secondary schooling (which is shown empirically in out regression as a decrease in the probability of having middle-level education), and

²⁹In Section 6.2, we define *HighEducated* as a dummy variable that takes the value 1 if the individual has at least 12 years of education, and 0 otherwise; *MidEducated* as a dummy variable that takes the value 1 if the individual has between 7 and 12 years of education, and 0 otherwise; and *LowEducated* as a dummy variable that takes the value 1 if the individual has 6 or fewer years of education, and 0 otherwise.

more individuals enrolling in high school. Column 4 shows that the policy effectively reduced the share of low-educated individuals in treated localities. This is also an intensive margin effect that aligns with the increased high school completion rate that we documented in Section 6 above.

Table 6 shows estimates for labor market outcomes. Consistent with the evidence we obtained based on findings using the staggered diff-in-diff strategy, there is no significant effect on employment and earnings.

Finally, online Appendix Table A.17 shows the results when we designate the 19-24 cohorts as treated. The difference-in-differences results show no program effect on these older cohorts. This result reflects that these individuals made their university enrollment decisions before the new campuses came by and were not changing them when a new campus was built nearby. Even if some students decide to enroll in university at those ages, the results show that the program did not affect their educational or labor market outcomes compared to individuals not exposed to the program (aged 30-35).

7.2 Evidence from a Controlled Experiment

To support the above findings and their interpretation, we estimate a control experiment (“placebo exercise”) using the cohorts aged between 30 and 35 as exposed to treatment and those aged 36 to 40 as a control group. We expect to find no significant program’s effect for these cohorts since the individuals in these samples are already old enough to have made their decisions about university schooling. Online Appendix Table A.15 shows small positive and not statistically significant program’s effect. However, there is a statistically significant negative effect on the likelihood of being low-educated. This change can be viewed as additional evidence of the potential spillover program’s effect on older cohorts. Overall, these placebo estimates support the validity of differences-in-differences estimates shown above as causal evidence of the program’s effect. Below, we offer additional controlled experimental evidence that enhances this conclusion.

8 Conclusions

This paper evaluated an unusual supply expansion of higher education campuses meant to increase educational opportunities for students living outside the capital. Our findings shed light on the multifaceted effects of the campus expansion program. We find that the program significantly and positively impacted various educational outcomes. Firstly, it led to a substantial increase in overall enrollment, which indicates its success in expanding access to higher education. Moreover, the program had an especially favorable effect on FGS, increasing intergenerational mobility. Importantly, it did not result in increased drop-out rates, evidence suggesting sustainability. The spatial spillovers of

the program extended up to 30 kilometers from the campuses, underscoring its regional reach. Additionally, we observed positive educational spillovers, with increased high school attendance and completion rates. The estimated effects on labor market outcomes are mixed, boosting the share of highly educated workers in the locality where the campus was opened and decreasing their short-term earnings. The lack of data on longer-term labor market outcomes limits more conclusive evidence.

These results are significant because they show a promising program option to extend human capital formation, especially among the disadvantaged. Building over previous evidence showing that low-level educational outcomes can be improved by building primary schooling infrastructure (Duflo, 2001), this paper provides evidence on the effects of the higher education sector. At the same time, the program enhanced middle-level education and contributed to increasing intergenerational mobility, which can ultimately help reduce the educational gap in developing countries.

References

- Akresh, R., D. Halim, and M. Kleemans (2022). Long-Term and Intergenerational Effects of Education: Evidence from School Construction in Indonesia. *The Economic Journal* 133(650), 582–612.
- Alm, J. and J. V. Winters (2009). Distance and Intrastate College Student Migration. *Economics of Education Review* 28(6), 728–738.
- Alzúa, M. L. and C. Velázquez (2017). The Effect of Education on Teenage Fertility: Causal Evidence for Argentina. *IZA Journal of Development and Migration* 7(7), 1–28.
- Alzúa, M. L., L. Gasparini, and F. Haimovich (2015). Education Reform and Labor Market Outcomes: The Case of Argentina's Ley Federal De Educación. *Journal of Applied Economics* 18(1), 21–43.
- Angrist, J. D. and A. Krueger (1991). Does Compulsory School Attendance Affect Schooling and Earnings? *The Quarterly Journal of Economics* 106(4), 979–1014.
- Araya, F. (2019). Evidencia Sobre la Movilidad Intergeneracional de Ingresos Laborales para un País en Desarrollo: El Caso de Uruguay. *El Trimestre Económico* 86(342), 265–305.
- Bautista, M. A., F. González, L. R. Martínez, P. Muñoz, and M. Prem (2023). The Intergenerational Transmission of Higher Education: Evidence from The 1973 Coup in Chile. *Explorations in Economic History* 90, 101540.
- Behrman, J., N. Birdsall, and M. Székely (2020). Intergenerational Mobility in Latin America: Deeper Markets and Better Schools Make a Difference. In N. Birdsall and C. Graham (Eds.), *New Markets, New Opportunities?: Economic and Social Mobility in a Changing World*. The Brookings Institution and Carnegie Endowment for International Peace.
- Berniell, L., C. Bonavida, D. de la Mata, and E. Schargrotsky (2021). La Movilidad Educativa Intergeneracional en el Siglo XX en América Latina y el Caribe. *CAF Working Paper Series* (24).
- Björklund, A. and M. Jäntti (2020). Intergenerational Mobility, Intergenerational Effects, Sibling Correlations, and Equality of Opportunity: A Comparison of Four Approaches. *Research in Social Stratification and Mobility* 70, 100455.
- Björklund, A. and K. G. Salvanes (2011). Education and Family Background: Mechanisms and Policies. Volume 3 of *Handbook of the Economics of Education*, Chapter 3, pp. 201–247. Elsevier.

- Black, S. E. and P. J. Devereux (2011). Recent Developments in Intergenerational Mobility. In O. Ashenfelter and D. Card (Eds.), *Handbook of Labor Economics*, Volume 4 of *Handbook of Labor Economics*, Chapter 16, pp. 1487–1541. Elsevier.
- Black, S. E., P. J. Devereux, and K. G. Salvanes (2005). Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital. *American economic review* 95(1), 437–449.
- Borusyak, K. and X. Jaravel (2017). Revisiting Event Study Designs. *Available at SSRN 2826228*.
- Buckner, E. and Y. Zhang (2021). The Quantity-Quality Tradeoff: A Cross-national, Longitudinal Analysis of National Student-Faculty Ratios in Higher Education. *Higher Education* 82, 39–60.
- Card, D. (1993). Using Geographic Variation in College Proximity to Estimate the Return to Schooling. NBER Working Papers 4483, National Bureau of Economic Research.
- Carneiro, P. and J. J. Heckman (2002). The Evidence on Credit Constraints in Post-Secondary Schooling. *The Economic Journal* 112(482), 705–734.
- Carneiro, P., K. Liu, and K. G. Salvanes (2023). The Supply of Skill and Endogenous Technical Change: Evidence from a College Expansion Reform. *Journal of the European Economic Association* 21(1), 48–92.
- Caucutt, E. M. and L. Lochner (2020). Early and Late Human Capital Investments, Borrowing Constraints, and the Family. *Journal of Political Economy* 128(3), 1065–1147.
- Chetty, R., J. N. Friedman, E. Saez, N. Turner, and D. Yagan (2020). Income Segregation and Intergenerational Mobility Across Colleges in the United States. *The Quarterly Journal of Economics* 135(3), 1567–1633.
- Chetty, R., N. Hendren, P. Kline, and E. Saez (2014). Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States. *The Quarterly Journal of Economics* 129(4), 1553–1623.
- Chetty, R., N. Hendren, P. Kline, E. Saez, and N. Turner (2014). Is the United States Still a Land of Opportunity? Recent Trends in Intergenerational Mobility. *American Economic Review* 104(5), 141–47.
- Cuesta, J., H. Ñopo, and G. Pizzolotto (2011). Using Pseudo-Panels to Measure Income Mobility in Latin America. *Review of Income and Wealth* 57(2), 224–246.

- Daude, C. and V. Robano (2015). On Intergenerational (im)mobility in Latin America. *Latin American Economic Review* 24, 115–135.
- Delavande, A. and B. Zafar (2019). University Choice: The Role of Expected Earnings, Nonpecuniary Outcomes, and Financial Constraints. *Journal of Political Economy* 127(5), 2343–2393.
- Duflo, E. (2001). Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment. *The American Economic Review* 91(4), 795–813.
- Duraisamy, P., others, P. Duraisamy, et al. (1997). *Is There a Quantity-Quality Trade-Off as Enrollments Increase?: Evidence from Tamil Nadu, India*, Volume 1768. World Bank Publications.
- Espinoza, O., L. E. González, L. Sandoval, N. McGinn, and B. Corradi (2022). Reducing Inequality in Access to University in Chile: The Relative Contribution of Cultural Capital and Financial Aid. *Higher Education* 83(6), 1355–1370.
- Ferreira, F. H., J. Messina, J. Rigolini, L.-F. López-Calva, M. A. Lugo, and R. Vakis (2013). *La Movilidad Económica y el Crecimiento de la clase Media en América Latina*. World Bank Latin American and Caribbean Studies.
- Fleury, N. and F. Gilles (2018). The Intergenerational Transmission of Education. A Meta-Regression Analysis. *Education Economics* 26(6), 557–573.
- Frenette, M. (2009). Do Universities Benefit Local Youth? Evidence from the Creation of New Universities. *Economics of Education Review* 28(3), 318–328.
- Gandelman, N. and V. Robano (2012). Intergenerational Mobility, Middle Sectors and Entrepreneurship in Uruguay.
- Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of Econometrics* 225(2), 254–277.
- Guerra, S. C. and C. X. Lastra-Anadón (2019). The Quality-Access Tradeoff in Decentralizing Public Services: Evidence from Education in the OECD and Spain. *Journal of Comparative Economics* 47(2), 295–316.
- Hertz, T. and T. Jayasundera (2007). School Construction and Intergenerational Mobility in Indonesia.
- INEEd (2018). Reporte del Mirador Educativo 1. Desigualdades en el Acceso a la Educación Obligatoria. Technical report, INEE, Montevideo.

- INEEd (2020). Reporte del Mirador Educativo 6. 40 años de Egreso de la Educación Media en Uruguay. Technical report, INEEEd, Montevideo.
- Jäntti, M. and S. P. Jenkins (2015). Income Mobility. In *Handbook of income distribution*, Volume 2, pp. 807–935. Elsevier.
- Lapid, P. A. (2018). Expanding College Access: The Impact of New Universities on Local Enrollment.
- Lavy, V. (1996). School Supply Constraints and Children’s Educational Outcomes in Rural Ghana. *Journal of Development Economics* 51(2), 291–314.
- Leites, M., E. Sena, and J. Vila (2020). *Movilidad Intergeneracional de Ingresos en Uruguay: Una Mirada Basada en Registros Administrativos*. Cuaderno sobre Desarrollo Humano 12, PNUD.
- Lincove, J. A. (2009). Determinants of Schooling for Boys and Girls in Nigeria Under a Policy of Free Primary Education. *Economics of Education Review* 28(4), 474–484.
- Mazumder, Bhashkar, M. R.-R. and M. Triyana (2019). Intergenerational Human Capital Spillovers: Indonesia’s School Construction and its Effects on the Next Generation. *AEA Papers and Proceedings* (109), 243–49.
- Meghir, C. and M. Palme (2005). Educational Reform, Ability, and Family Background. *The American Economic Review* 95(1), 414–424.
- Meneses, F. et al. (2021). Intergenerational Mobility After Expanding Educational Opportunities: A Quasi Experiment.
- Montenegro, C. E. and H. A. Patrinos (2014). Comparable Estimates of Returns to Schooling Around the World. *World Bank policy research working paper* (7020).
- Mukhopadhyay, A. and S. Sahoo (2016). Does Access to Secondary Education Affect Primary Schooling? Evidence from India. *Economics of Education Review* 54, 124–142.
- Méndez, L. (2020). University Supply Expansion and Inequality of Opportunity of Access: The Case of Uruguay. *Education Economics* 28(2), 115–135.
- Neidhöfer, G., J. Serrano, and L. Gasparini (2018). Educational Inequality and Intergenerational Mobility in Latin America: A new Database. *Journal of Development Economics* 134, 329 – 349.
- Oreopoulos, P. (2006). Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter. *American Economic Review* 96(1), 152–175.

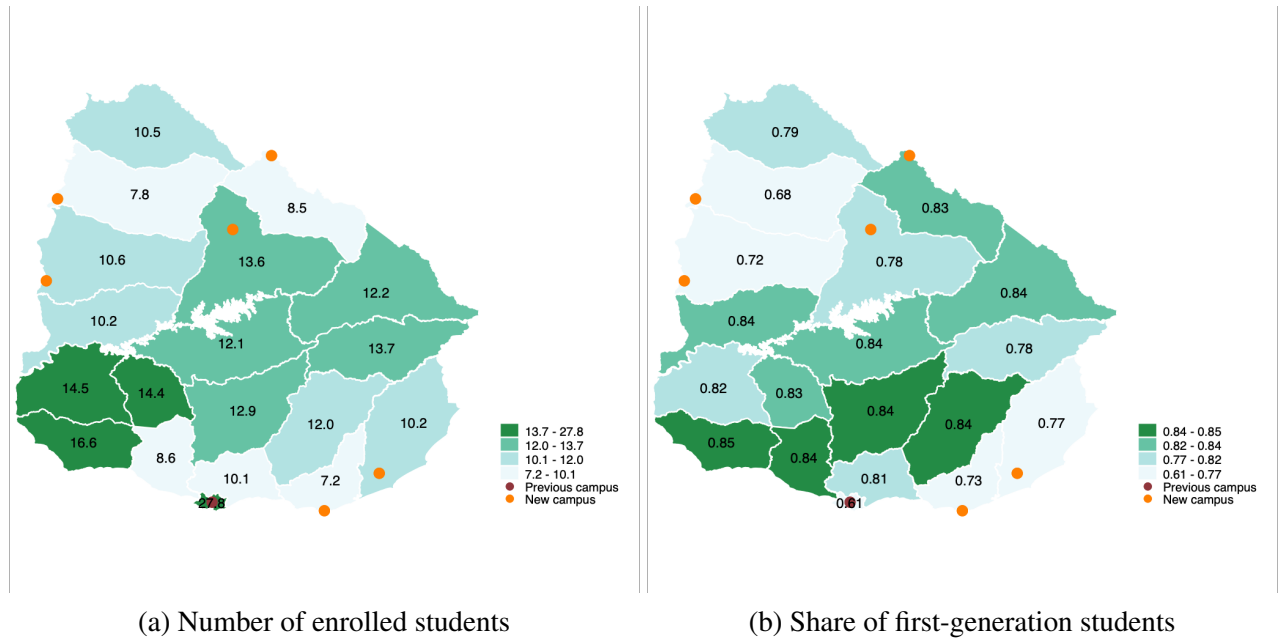
- Rau, T., E. Rojas, and S. Urzúa (2012). Higher Education Dropouts, Access to Credit, and Labor Market Outcomes: Evidence from Chile. In *F. Buera & N. Fuchs-Schündeln (Chair), SED Annual Meeting*.
- Sanroman, G. (2010). Intergenerational Educational Mobility: Evidence from Three Approaches for Brazil, Chile, Uruguay and the USA (1995-2006). *Working paper 01/2010, DECON, Udelar*.
- Soto, S. (2020). La Influencia del Contexto en la Transmisión Educativa en Uruguay: Tres Aproximaciones Empíricas.
- Spiess, C. K. and K. Wrohlich (2010). Does Distance Determine Who Attends a University in Germany? *Economics of Education Review* 29(3), 470–479.
- Torche, F. (2019). Educational Mobility in Developing Countries. Technical report, WIDER Working Paper 2019/88 Helsinki: UNU-WIDER.
- Udelar (2020). *Propuesta al País 2020-2024. Plan Estratégico de Desarrollo de la Universidad de la República*.
- UNDP (2022). *Human Development Report 2021-22: Uncertain Times, Unsettled Lives: Shaping our Future in a Transforming World*.
- Urraburu, J. (2020). Movilidad Educativa y Ocupacional Intergeneracional en Uruguay.

Figure 1: Timeline of University Expansion Program, Including Localities 30 and 50 km Away

2008	2010	2011	2012	2013 onward
Maldonado	Maldonado	Maldonado	Maldonado	Maldonado
Rocha	Rocha	Rocha	Rocha	Rocha
La Paloma*	Paysandú	Paysandú	Paysandú	Paysandú
Pan de Azucar*	La Paloma*	Salto	Salto	Salto
Piriapolis*	Pan de Azucar*	La Paloma*	Tacuarembó	Tacuarembó
Punta del Este*	Piriapolis*	Pan de Azucar*	La Paloma*	Rivera
San Carlos*	Punta del Este*	Piriapolis*	Pan de Azucar*	La Paloma*
Aigua**	San Carlos*	Punta del Este*	Piriapolis*	Pan de Azucar*
	Aigua**	San Carlos*	Punta del Este*	Piriapolis*
	Quebracho**	Aigua**	San Carlos*	Punta del Este*
	San Javier**	Quebracho**	Aigua**	San Carlos*
		San Javier**	Quebracho**	Aigua**
			San Javier**	Quebracho**
			Tranqueras**	San Javier**
				Tranqueras**

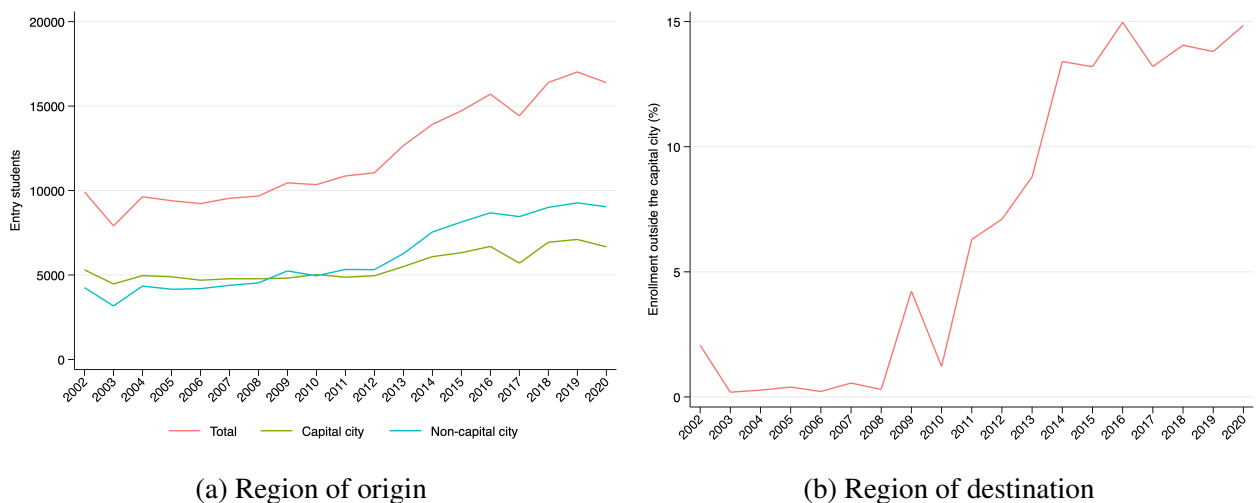
Notes: The information is taken from the university official documents. *Localities included in the 30 km buffer, **Localities included in the 50 km buffer. Unless specified, campuses are located in the department's capital city.

Figure 2: Enrollment and Educational Mobility in Uruguay Before the University Expansion, 2002-2007



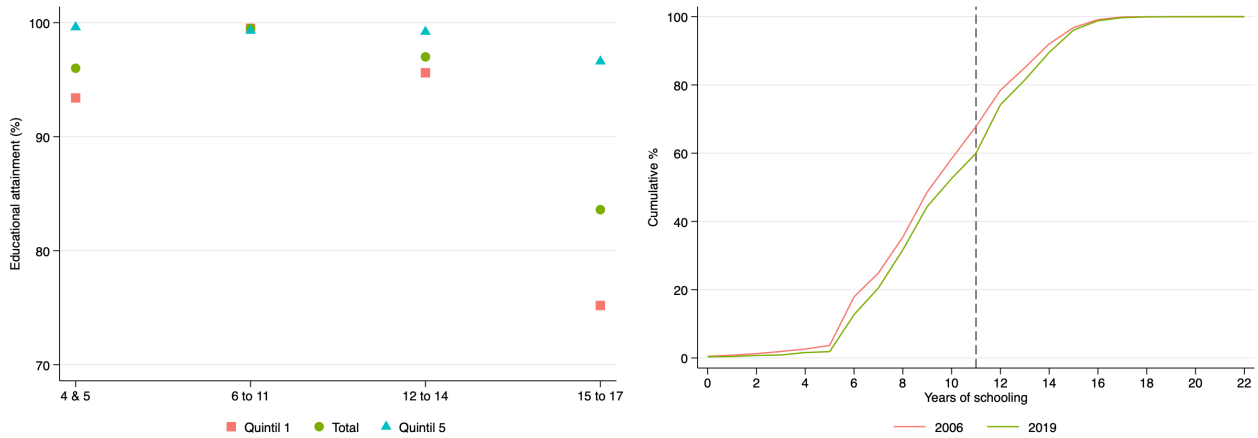
Notes: The figures show (Panel a) the number of enrolled students per 1,000 inhabitants and (Panel b) the percentage of first-generation in university among enrolled students by geographical region. Points represent exact geographic information on where University campuses are located. Previous campuses that opened in 2006 are shown in maroon, and new campuses that opened from 2008 onward are shown in orange. All new entry students with information in administrative records and the Census in the enrollment year were considered. The sample was drawn from the University administrative records and Census of the enrollment year.

Figure 3: First Year Enrollment Students by Geographical Region of Origin and Destination, Years 2002-2020



Notes: The Figure in Panel (a) shows the number of entry students by region where they lived before entering university. The Figure in Panel (b) shows the share of entry students enrolled in a university campus outside the capital city. All new entry students with information both in administrative records and the Census in the enrollment year were included. The sample was drawn from the University administrative records and Census at enrollment year.

Figure 4: Educational Context: Attainment and Years of Schooling in Uruguay

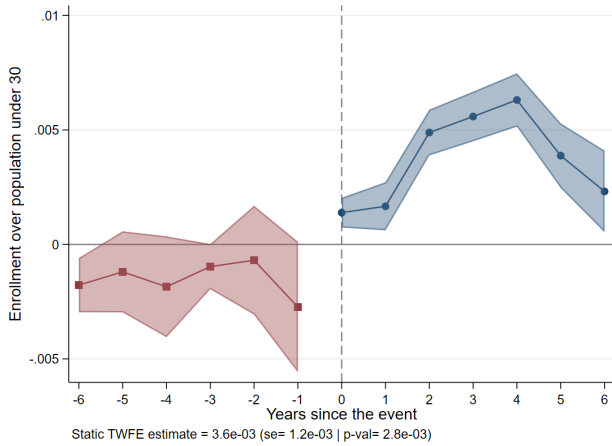


(a) Attainment by socioeconomic level

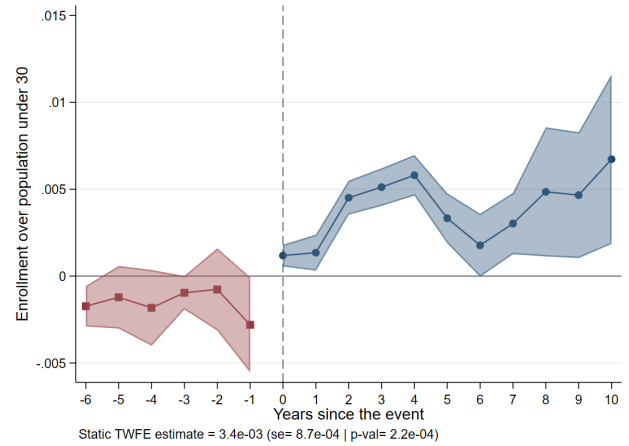
(b) Years of schooling (ages 20-24)

Notes: The Figure in Panel (a) shows the percentage of people attending school by income quintile for different age branches according to educational levels. Quintiles computed at the household level considering per capita income. Data from [INEEd \(2018\)](#). The Figure in Panel (b) shows the cumulative distribution of years of schooling for people aged between 20 and 24. Own calculations based on NHS of the years 2006 and 2019.

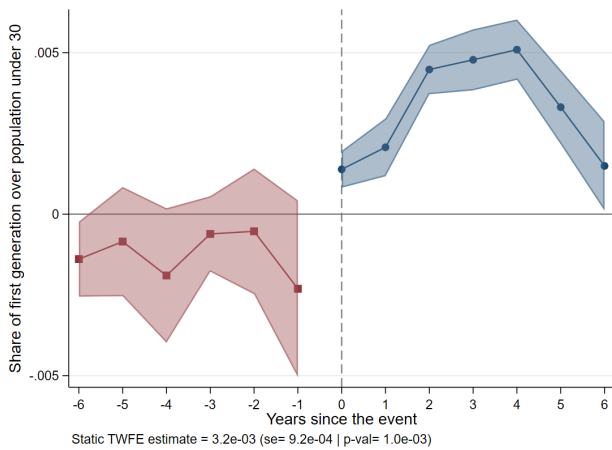
Figure 5: Event Study Estimated Effects on the Share of Enrolled Students and Educational Intergenerational Mobility



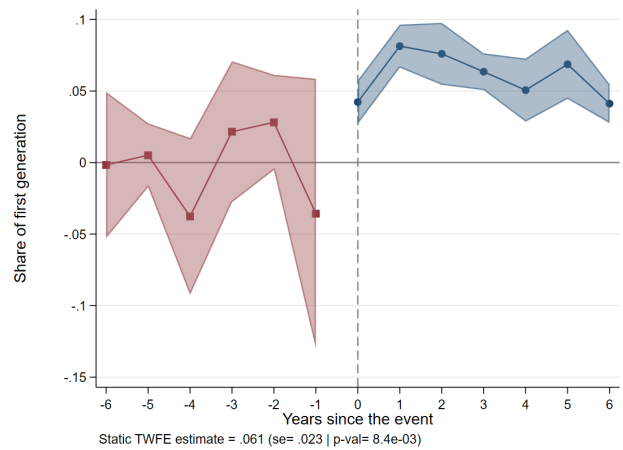
(a) Share of enrollment over population under 30



(b) Share of enrollment over population under 30



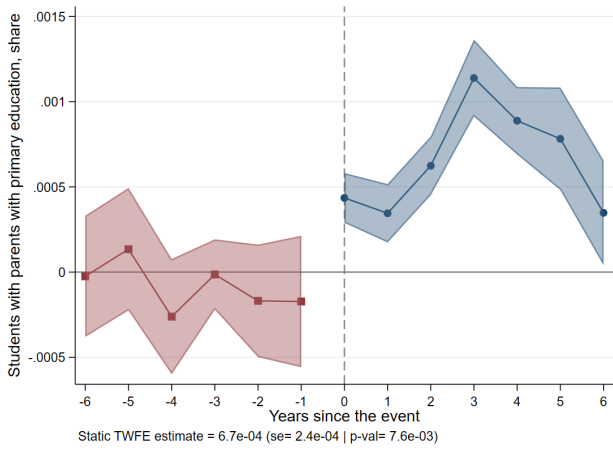
(c) FGS in population under 30



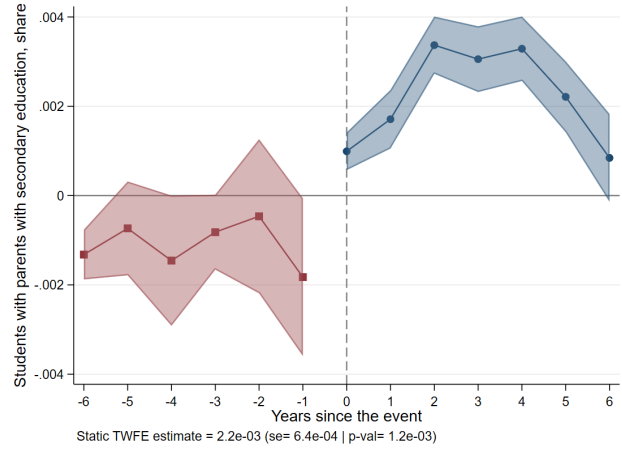
(d) FGS in total enrollment

Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

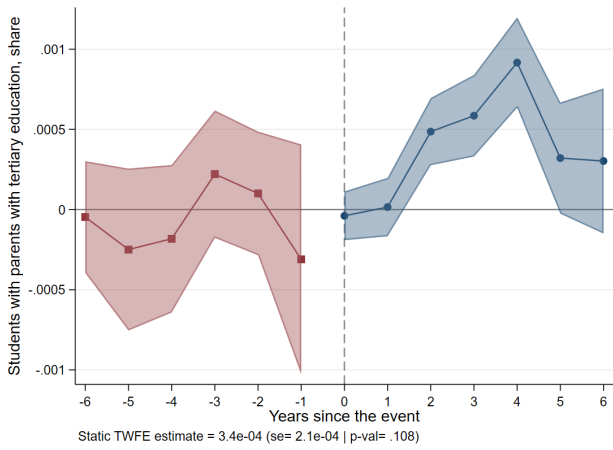
Figure 6: Event Study Estimated Effects on the Share of FGS by Parental Background



(a) Parents with Primary Education



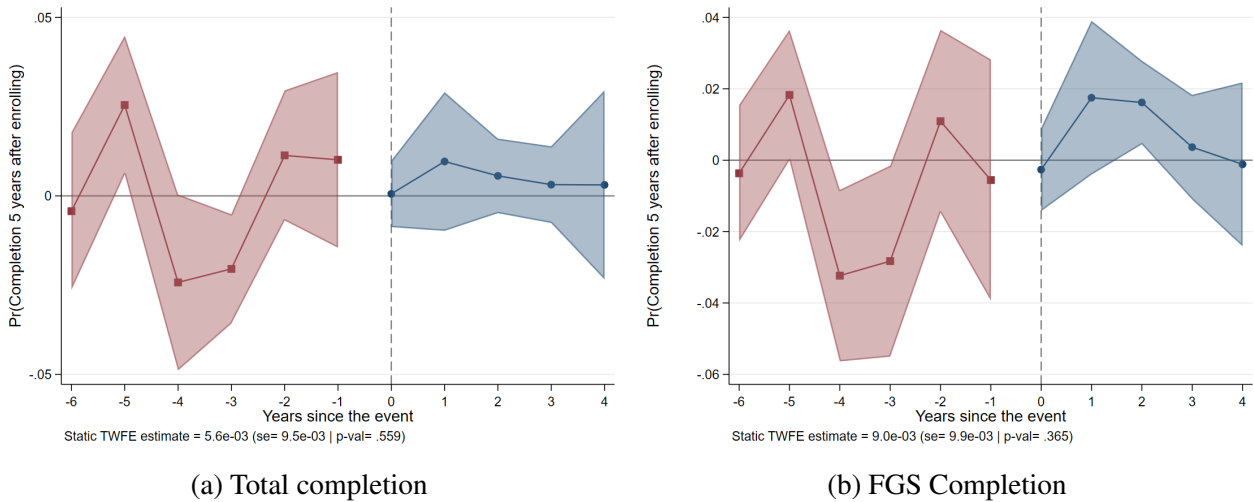
(b) Parents with Secondary Education



(c) Parents with Tertiary Education

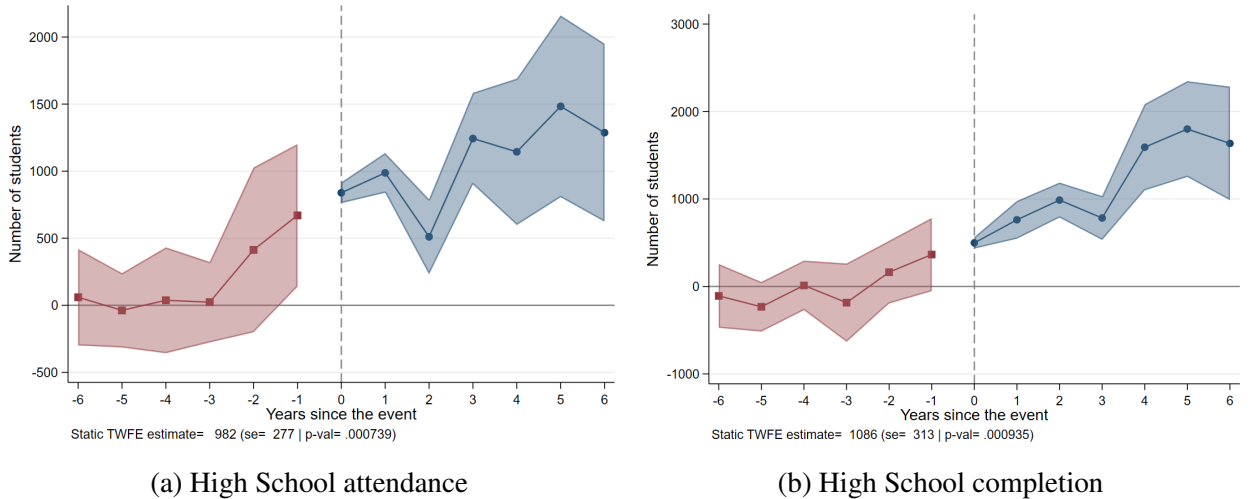
Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Figure 7: Event Study Estimated Effects on Completion of University Degree: All and First-Generation Students



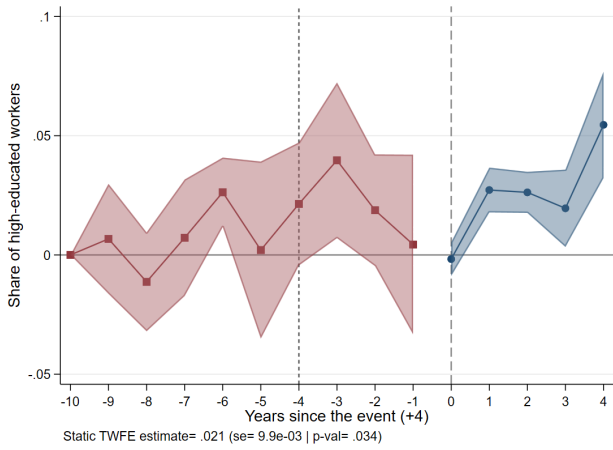
Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. For this analysis, the sample was restricted to students who enrolled in university between 2002 and 2015. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Figure 8: Event Study Estimated Effects on High School Outcomes

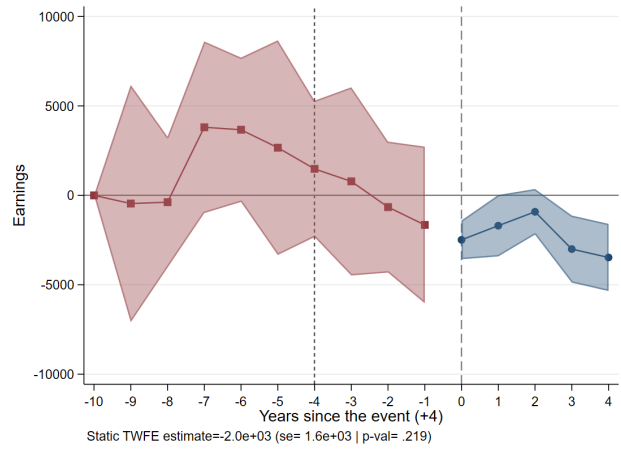


Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. We use in the estimations survey weights calculated by the National Institute of Statistics. Localities were weighted using population under 30 years old by locality from a pre-reform period (2002 to 2007).

Figure 9: Event Study Estimated Effects on Labor Outcomes



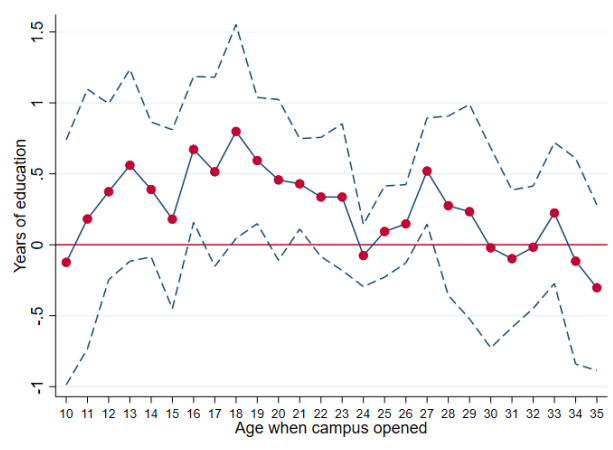
(a) Share of high-educated workers



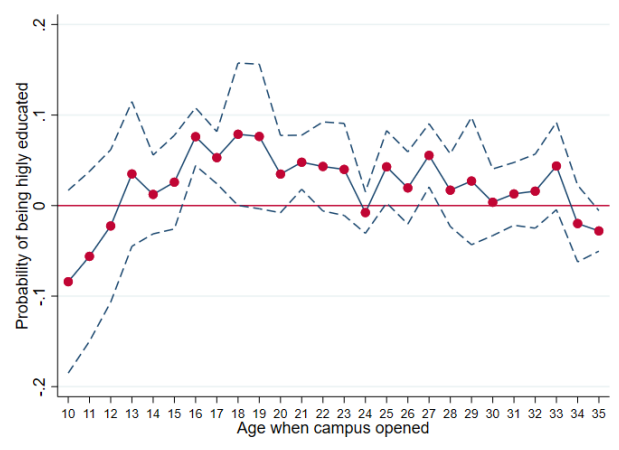
(b) Earnings of high-educated workers

Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. Panel (a) results from a regression at the locality level weighted using the population under 30 years old by locality from a pre-reform period (2002 to 2007). Panel (b) results from a regression at the individual level, including sex, marital status, presence of children under 12 in the household, and experience as control variables. Income in Uruguayan pesos indexed to the CPI of January 2011; 1 USD = 44.5 Uruguayan pesos.

Figure 10: Program's Effect by Age Cohort



(a) Years of Education



(b) Probability of being highly educated

Notes: The Figures show the average causal effects of the program by age when the campus opened. The omitted category is cohort age 36. The dashed area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year, age cohort, and locality fixed effects. The estimation uses survey weights calculated by the National Institute of Statistics.

Table 1: Descriptive Statistics

Panel A: Individual Level			
	Mean	SD	Obs.
Student's Characteristics			
Female	0.63	0.48	64,820
Age	19.02	2.31	64,820
Parents' Education			
Illiterate	0.00	0.05	64,820
Primary	0.12	0.33	64,820
Secondary	0.51	0.50	64,820
Tertiary	0.18	0.39	64,820
University	0.18	0.39	64,820
Outcomes			
First generation university	0.82	0.39	64,820
Completion	0.36	0.48	34,390
Panel B: Locality Level			
	Mean	SD	Obs.
Outcomes			
Enrollment	129.23	122.98	1,425
Share enrollment	0.02	0.01	1,425
FG enrollment	102.44	97.91	1,425
Share FG	0.80	0.10	1,246
Completion	0.09	0.07	878
FG completion	0.09	0.08	872

Notes: The Table presents the means, standard deviations, and valid observations of the main characteristics of the estimation sample. For completion, the sample was restricted to students who enrolled in university between 2002 and 2015. Data at the locality level is weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Table 2: Static Two Way Fixed Effects Estimates

	Share Enrollment	Share FG (pop)	Share FG (enroll)
Treated Locality	0.004*** (0.00)	0.003*** (0.00)	0.061*** (0.02)
Pre-treatment Mean	0.011	0.008	0.728
Obs.	1224	1224	1224

Notes: The Table shows the static TWFE estimate. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Table 3: Static Estimates on Enrollment Shares by Parental Background

	Primary	Secondary	Tertiary
Treated Locality	0.00067*** (0.00)	0.00215*** (0.00)	0.00034 (0.00)
Pre-treatment Mean	0.0010	0.0051	0.0017
Obs.	1224	1224	1224

Notes: The Table presents the static TWFE estimate. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Table 4: Estimates of the Spatial Spillovers of the Program

	Share Enrollment	Share FG (pop)	Share FG (enroll)
Treated locality	0.0038*** (0.00)	0.0033*** (0.00)	0.0620*** (0.02)
Buffer 30km	0.0048* (0.00)	0.0031** (0.00)	0.0082 (0.04)
Buffer 50km	0.0005 (0.00)	0.0021 (0.00)	0.0281 (0.04)
Obs.	1224	1224	1224
R-squared	0.800	0.791	0.526

Notes: The Table presents the static TWFE estimates. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Table 5: Diff-in Diff Estimates of the Program Effect on Educational Outcomes

	Years of Education	High-Education	Middle-Education	Low-Education
Exposed x Campus	0.443** (0.21)	0.032* (0.02)	0.013 (0.03)	-0.045* (0.03)
Obs.	8184	8184	8184	8184
R-squared	0.049	0.038	0.018	0.038

Notes: The Table presents the average causal program's effect on the probability of being high, middle, and low educated. High Education is a dummy variable that takes value 1 if the individual has more than 12 years of education and 0 otherwise, Middle Education is a dummy variable that takes value 1 if the individual has between 7 and 12 years of education and 0 otherwise, and Low Education is a dummy variable that takes value 1 if the individual has 6 or less years of education. All specifications include as controls calendar year, locality, cohort fixed effects, gender, marital status, and number of children under 12 in the household. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. We use the National Institute of Statistics survey weights.

Table 6: Diff-in Diff Estimates of the Program Effect on Labor Market Outcomes

	Employment	Earnings(log)	Cond. Earnings(log)
Exposed x Campus	-0.008 (0.02)	0.162 (0.23)	-0.041 (0.06)
Obs.	8184	8184	6258
R-squared	0.171	0.224	0.140

Notes: The Table presents the average causal program's effect on the probability of being employed, the log earnings, and the log earnings conditionally on being employed. All specifications include as controls calendar year, locality, cohort fixed effects, experience and its square, gender, marital status, and number of children under 12. Standard errors clustered at the locality level in parentheses.***significant at the 1 % level, **5 % level, *10 % level. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. We use the National Institute of Statistics survey weights.

Online Appendix

A.1 Distance and Enrollment

As a starting point of our analysis, we show that distance to campus matters regarding enrollment. To that end, we estimate the following equation,

$$Outcome_{l,t} = \alpha_0 + \mu_l + \mu_t + \beta Distance_{l,t} + \epsilon_{l,t}^d \quad (A.1)$$

where μ_l and μ_t represent the locality and year (two-way) fixed effects, respectively. $Distance_{l,t}$ denotes the distance in hundreds of kilometers between locality l and the nearest campus at time t , and $\epsilon_{l,t}^d$ represents the idiosyncratic error terms in the model. The outcomes of interest are the enrollment share, the FG share over population, and the FG share in total enrollment, as defined in Section 3. Table A.1 displays the estimates from Equation A.1. Overall, distance has a negative and significant effect on university enrollment.

Table A.1: Regression Estimates of Distance and Enrollment

	Share Enrollment	Share FG (pop)	Share FG (enroll)
Distance to closest campus	-0.0009*** (0.00)	-0.0009*** (0.00)	-0.0159*** (0.00)
Pre-treatment Mean	0.017	0.014	0.808
Obs.	1118	1118	1118

Notes: The Table presents the marginal effect of distance (in hundred km) on enrollment and intergenerational mobility. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

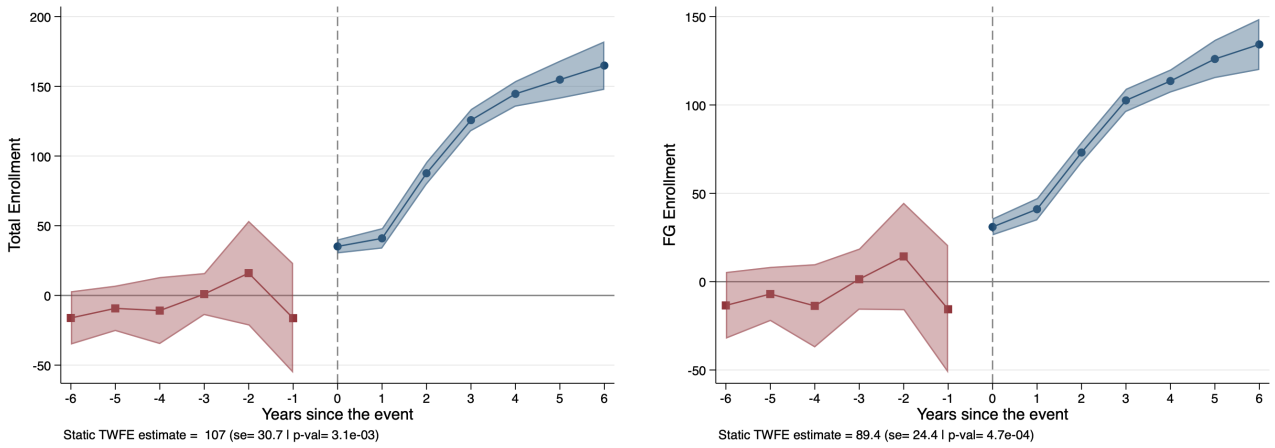
Table A.2: Regression Estimates of Distance to the Closest Campus and Enrollment

	Share Enrollment	Share FG (pop)	Share FG (enroll)
Treated locality	0.0020* (0.00)	0.0019* (0.00)	0.0407*** (0.01)
Distance x Treated	-0.0000** (0.00)	-0.0000** (0.00)	-0.0001** (0.00)
Pre-treatment Mean	0.017	0.014	0.808
Obs.	1118	1118	1118

Notes: The Table presents the static TWFE estimates. Distance is measured in hundred kilometers. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

A.2 Main results

Figure A.1: Effect on Total Enrollment and FGS

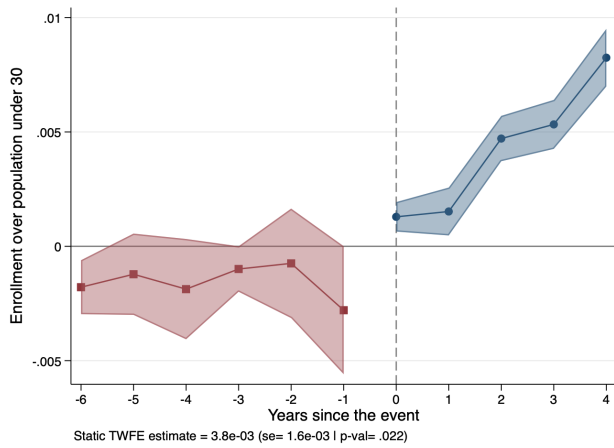


(a) Total enrollment

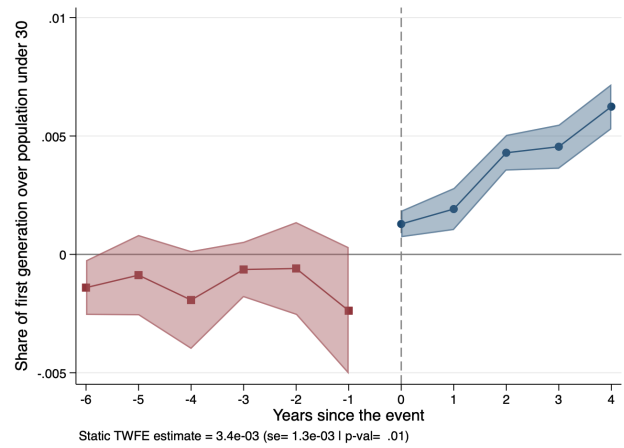
(b) FGS enrollment

Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

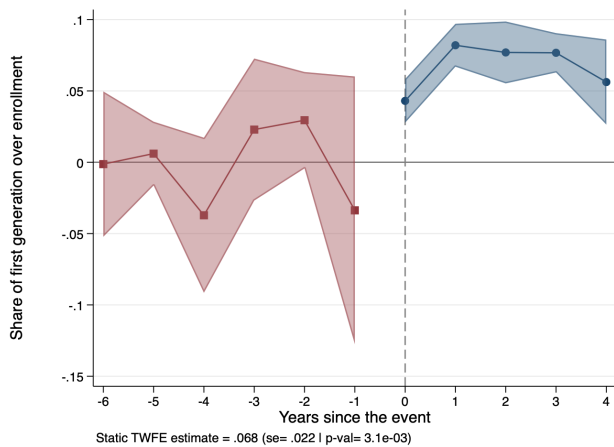
Figure A.2: Effect on the Share of Enrolled Students and Educational Intergenerational Mobility Using the Completion Sample



(a) Enrollment share



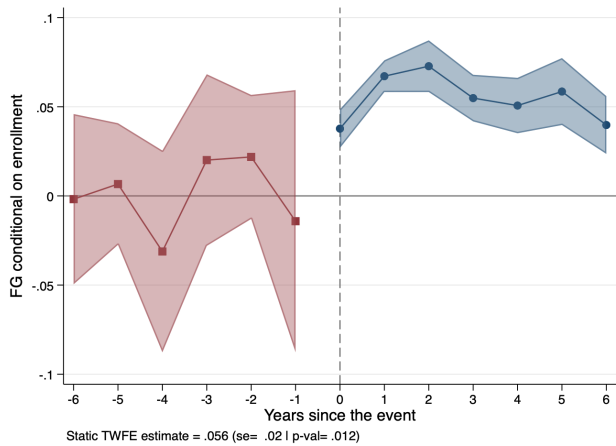
(b) FGS share in the population under 30



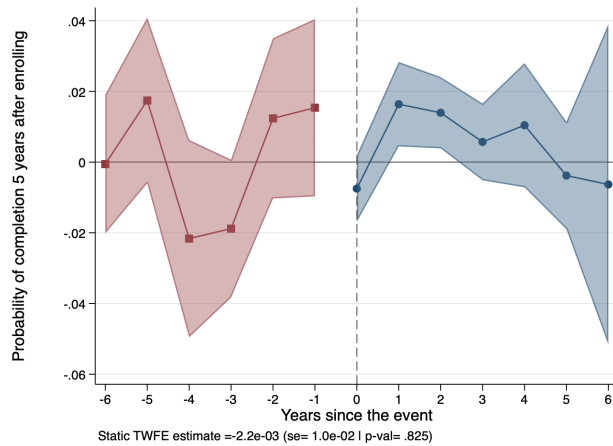
(c) FGS share in total enrollment

Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. For this analysis, the sample was restricted to students who enrolled in university between 2002 and 2015. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Figure A.3: Effect on the Probability of Being First Generation Students and Degree Completion After 5 Years of Enrolling - Individual Level Data



(a) Probability FG



(b) Probability of completion after 5 years of enrolling

Notes: The Figures show the average causal effects of the program. The colored area represents 95% confidence intervals. Standard errors are clustered at the locality level. All specifications include calendar year and locality fixed effects. For completion analysis, the sample was restricted to students who enrolled in university between 2002 to 2015.

Table A.3: Effect on the Share of Enrolled Students, and Educational Intergenerational Mobility

	Share Enrollment	Share Enrollment	Share FG (pop)	Share FG (enroll)
pre1	-0.003** (0.00)	-0.003* (0.00)	-0.002* (0.00)	-0.036 (0.05)
pre2	-0.001 (0.00)	-0.001 (0.00)	-0.001 (0.00)	0.028* (0.02)
pre3	-0.001* (0.00)	-0.001* (0.00)	-0.001 (0.00)	0.022 (0.03)
pre4	-0.002 (0.00)	-0.002* (0.00)	-0.002* (0.00)	-0.038 (0.03)
pre5	-0.001 (0.00)	-0.001 (0.00)	-0.001 (0.00)	0.005 (0.01)
pre6	-0.002*** (0.00)	-0.002*** (0.00)	-0.001** (0.00)	-0.002 (0.03)
tau0	0.001*** (0.00)	0.001*** (0.00)	0.001*** (0.00)	0.042*** (0.01)
tau1	0.001** (0.00)	0.002*** (0.00)	0.002*** (0.00)	0.081*** (0.01)
tau2	0.005*** (0.00)	0.005*** (0.00)	0.004*** (0.00)	0.076*** (0.01)
tau3	0.005*** (0.00)	0.006*** (0.00)	0.005*** (0.00)	0.064*** (0.01)
tau4	0.006*** (0.00)	0.006*** (0.00)	0.005*** (0.00)	0.051*** (0.01)
tau5	0.003*** (0.00)	0.004*** (0.00)	0.003*** (0.00)	0.069*** (0.01)
tau6	0.002* (0.00)	0.002** (0.00)	0.001** (0.00)	0.041*** (0.01)
tau7	0.003*** (0.00)			
tau8	0.005** (0.00)			
tau9	0.005** (0.00)			
tau10	0.007*** (0.00)			
Pre-treatment Mean	0.011	0.011	0.008	0.728
Obs.	1421	1224	1224	1224

Notes: The Table presents the average causal effects of the program. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Table A.4: Effect on First Generation Students, by Parental Background

	Primary	Secondary	Tertiary
pre1	-0.000 (0.00)	-0.002** (0.00)	-0.000 (0.00)
pre2	-0.000 (0.00)	-0.000 (0.00)	0.000 (0.00)
pre3	-0.000 (0.00)	-0.001* (0.00)	0.000 (0.00)
pre4	-0.000 (0.00)	-0.001* (0.00)	-0.000 (0.00)
pre5	0.000 (0.00)	-0.001 (0.00)	-0.000 (0.00)
pre6	-0.000 (0.00)	-0.001*** (0.00)	-0.000 (0.00)
tau0	0.000*** (0.00)	0.001*** (0.00)	-0.000 (0.00)
tau1	0.000*** (0.00)	0.002*** (0.00)	0.000 (0.00)
tau2	0.001*** (0.00)	0.003*** (0.00)	0.000*** (0.00)
tau3	0.001*** (0.00)	0.003*** (0.00)	0.001*** (0.00)
tau4	0.001*** (0.00)	0.003*** (0.00)	0.001*** (0.00)
tau5	0.001*** (0.00)	0.002*** (0.00)	0.000* (0.00)
tau6	0.000** (0.00)	0.001* (0.00)	0.000 (0.00)
Pre-treatment Mean	0.001	0.005	0.002
Obs.	1224	1224	1224

Notes: The Table presents the average causal effects of the program. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Table A.5: Effect on the Probability of Being First Generation Students and Completion After 5 Years of Enrolling - Individual Level Data

	Share FG (enroll)	Completion
pre1	-0.014 (0.04)	0.015 (0.01)
pre2	0.022 (0.02)	0.012 (0.01)
pre3	0.020 (0.02)	-0.019* (0.01)
pre4	-0.031 (0.03)	-0.022 (0.01)
pre5	0.007 (0.02)	0.017 (0.01)
pre6	-0.002 (0.02)	-0.001 (0.01)
tau0	0.038*** (0.01)	-0.007 (0.00)
tau1	0.067*** (0.00)	0.016*** (0.01)
tau2	0.073*** (0.01)	0.014*** (0.01)
tau3	0.055*** (0.01)	0.006 (0.01)
tau4	0.051*** (0.01)	0.010 (0.01)
tau5	0.059*** (0.01)	-0.004 (0.01)
tau6	0.040*** (0.01)	-0.006 (0.02)
Pre-treatment Mean	0.745	0.450
Obs.	57281	34190

Notes: The Table presents the average causal effects of the program. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. For completion analysis, the sample was restricted to students who enrolled in university between 2002 to 2015.

Table A.6: Effects on Degree Completion for Total and First-Generation Students

	Completion	First Generation
pre1	0.010 (0.01)	-0.006 (0.02)
pre2	0.011 (0.01)	0.011 (0.01)
pre3	-0.020*** (0.01)	-0.028** (0.01)
pre4	-0.024* (0.01)	-0.032*** (0.01)
pre5	0.025** (0.01)	0.018** (0.01)
pre6	-0.004 (0.01)	-0.004 (0.01)
tau0	0.001 (0.00)	-0.003 (0.01)
tau1	0.010 (0.01)	0.018 (0.01)
tau2	0.006 (0.01)	0.016*** (0.01)
tau3	0.003 (0.01)	0.004 (0.01)
tau4	0.003 (0.01)	-0.001 (0.01)
Pre-treatment Mean	0.073	0.065
Obs.	865	865

Notes: The Table presents the average causal effects of the program. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. For this analysis, the sample was restricted to students who enrolled in university between 2002 and 2015. Localities were weighted using population under 30 years old by locality from a pre-reform period (Census 1996).

Table A.7: Descriptive Statistics of the Labor Market Analysis Sample

	Mean	SD	N
Years of Education	9.28	3.163	111,545
High-Education	0.15	0.354	111,545
Middle-Education	0.63	0.483	111,545
Low-Education	0.22	0.415	111,545
Cond. High-Education	0.13	0.331	77,254
Employment rate	0.69	0.462	111,545
log_labour_income_tot	6.86	4.575	111,545
log_labour_income_tot_cond	9.62	1.762	77,254
Women	0.51	0.500	111,545
Age	25.35	2.916	111,545
Married	0.48	0.500	111,545
Children	2.91	2.891	111,545

Notes: The Table presents summary statistics of the estimation sample used in the labor market outcome analysis. The variable 'Conditional High Educated' is the average probability of being high educated among the employed, 'Conditional Earnings' measures the average earnings (in logs) among the employed. The sample used in this table is restricted to individuals aged 21-30. We use the National Institute of Statistics' survey weights.

Table A.8: Estimated Effects on Other Educational Outcomes

	HS attendance	HS completion
pre1	670.483** (270.98)	364.839* (213.35)
pre2	413.225 (313.24)	163.581 (182.42)
pre3	23.185 (152.96)	-184.960 (228.10)
pre4	37.249 (201.06)	12.486 (144.48)
pre5	-37.661 (141.30)	-232.357 (144.65)
pre6	60.027 (183.22)	-107.574 (185.84)
tau0	838.943*** (39.43)	498.517*** (33.47)
tau1	987.333*** (75.52)	761.967*** (110.12)
tau2	510.635*** (142.52)	987.593*** (101.99)
tau3	1243.155*** (173.94)	782.693*** (127.80)
tau4	1144.152*** (278.32)	1591.945*** (251.73)
tau5	1483.403*** (345.67)	1799.586*** (279.26)
tau6	1286.953*** (338.49)	1635.388*** (331.30)
Pre-treatment Mean	4412	2762
Obs.	1025	1025

Notes: The Table presents the average causal effects of the program. All specifications include calendar year and locality fixed effects. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population under 30 years old by locality from a pre-reform period (2002 to 2007).

Table A.9: Employment Effects

	All	High-educated
Treated	-0.003 (0.012)	0.006 (0.020)
Obs.	111,583	16,144
R-squared	0.130	0.218

Notes: The Table presents the estimates of regressing employment status on the treatment variable. All specifications include as controls calendar year and locality fixed effects, experience and its square, gender, marital status, and children under 12. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. The sample includes individuals between 21 and 30. We use the National Institute of Statistics' survey weights.

Table A.10: Descriptive Statistics: Sample of the Alternative Identification Strategy

	Mean	SD	N
Years of Education	9.75	3.481	14,210
High-Education	0.19	0.391	14,210
Middle-Education	0.62	0.487	14,210
Low-Education	0.20	0.398	14,210
Women	0.52	0.500	14,210
Married	0.63	0.482	14,210
Age	32.83	6.326	14,210
Employment	0.78	0.415	14,210
Earnings(log)	8.01	4.223	14,210
Cond. Earnings(log)	10.06	1.363	11,088

Notes: The Table presents summary statistics for the outcomes in the sample used in the alternative identification strategy. We use the National Institute of Statistics' survey weights.

Table A.11: Mincer Wage Equations

	Labor Income	Log(Labor Income)
Mid Educated	6,717.87*** (285.37)	0.32*** (0.02)
High Educated	22,598.00*** (618.00)	0.92*** (0.03)
Treated	-669.94 (698.79)	-0.06 (0.04)
Treated x Mid	-217.32 (849.94)	0.02 (0.05)
Treated x High	-766.48 (1981.79)	-0.07 (0.09)
Obs.	74,974	74,974
R-squared	0.193	0.208

Notes: The Table presents the estimates of Mincer equations for the education variables and the interaction term with the treatment variable. All specifications include as controls calendar year and locality fixed effects, experience and its square, gender, marital status, and children under 12. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. The sample includes individuals between 21 and 30. We use the National Institute of Statistics' survey weights.

Table A.12: Balancing Tests of Pre-Treatment Characteristics (2002-2007)

	Control	Treatment	(C-T)
Years of Education	6.67	6.70	0.023
High-Education	0.07	0.07	0.000
Middle-Education	0.40	0.39	0.010***
Low-Education	0.53	0.54	-0.010***
Cond. Earnings(log)	9.30	9.51	-0.129***
Employment	0.39	0.38	0.000
Women	0.53	0.53	0.002
Married	0.54	0.55	-0.007**
Age	35.95	34.59	1.170***

Notes: The Table presents the mean and mean difference test of the main variables before the first campus opened, by treatment status, depending on the locality. Sample is restricted to individuals in municipal capitals, except Montevideo and Canelones.***significant at the 1% level, **5% level, *10% level. We use the National Institute of Statistics' survey weights.

Table A.13: Descriptive Statistics by Treatment Status of Locality: All Cohorts

	Non-Treated Localities		Treated Localities	
	Mean	SD	Mean	SD
Years of Education	8.84	3.456	9.04	3.586
High-Education	0.13	0.331	0.14	0.346
Middle-Education	0.58	0.493	0.57	0.495
Low-Education	0.29	0.454	0.29	0.453
Employment	0.55	0.498	0.55	0.497
Cond. Earnings(log)	9.97	1.558	10.00	1.522
Women	0.52	0.499	0.52	0.499
Married	0.53	0.499	0.53	0.499
Age	43.65	19.971	42.66	19.485
Observations	22562		25301	

Notes: The Table presents the mean and standard deviation of the main variables by treatment status depending on the locality. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. We use the National Institute of Statistics' survey weights.

Table A.14: Descriptive Statistics by Treatment Status of Locality and Cohort Exposition to the Program

	Non-treated & Non-exposed		Non-treated & Exposed		Treated & Non-exposed		Treated & Exposed	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Years of Education	9.47	3.486	9.58	3.226	9.53	3.569	10.06	3.175
High-Education	0.17	0.371	0.17	0.375	0.16	0.371	0.21	0.405
Middle-Education	0.61	0.488	0.65	0.476	0.59	0.492	0.64	0.480
Low-Education	0.23	0.419	0.18	0.382	0.24	0.430	0.15	0.361
Women	0.51	0.500	0.51	0.500	0.52	0.500	0.50	0.500
Married	0.72	0.449	0.50	0.500	0.74	0.440	0.38	0.484
Age	42.95	1.793	26.43	1.511	40.80	2.502	23.89	2.383
Employment	0.87	0.339	0.72	0.447	0.84	0.369	0.61	0.488
Earnings(log)	8.97	3.526	7.29	4.519	8.64	3.873	6.22	4.797
Cond. Earnings(log)	10.22	1.249	9.83	1.637	10.23	1.246	9.70	1.533
Observations	2165		1473		2475		2071	

Notes: The Table presents the mean and standard deviation of the main variables by treatment status. Exposed cohorts are those aged between 14 and 18 when the campus opened, and non-exposed cohorts are those aged 30 to 35. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. We use the National Institute of Statistics' survey weights.

Table A.15: Diff-in Diff Estimates of the Program Effect: Control Experiment

	Years of Education	High-Education	Middle-Education	Low-Education	Employment	Cond. Earnings(log)
Exposed x Campus	0.201 (0.12)	0.010 (0.01)	0.028 (0.03)	-0.039** (0.02)	-0.000 (0.02)	-0.036 (0.05)
Obs.	259434	259434	259434	259434	259434	216762
R-squared	0.038	0.023	0.016	0.031	0.107	0.144

Notes: The Table presents the average causal program's effect on the average years of education, the probability of being high, middle, or low educated, the probability of being employed, and the log earnings conditional on employment. Exposed cohorts are between 30-35 when a campus opens. Non-exposed are cohorts aged 36 to 40. All specifications include calendar year, locality, and cohort fixed effects. In the educational outcomes regressions we include controls

for gender, marital status, and number of children under 12 in the household. We add controls for experience and its square in the labor market outcomes regressions. Standard errors clustered at the locality level in parentheses. ***significant at the 1 % level, **5 % level, *10 % level. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. We use the National Institute of Statistics' survey weights.

Table A.16: Diff-in Diff Estimates: Alternative Non-Exposed Cohorts Definition

	Years of Education	High-Educ.	Middle-Educ.	Low-Educ.	Employment	Earnings(log)	Cond. Earnings(log)
Exposed x Campus	0.290 (0.22)	0.024 (0.02)	-0.003 (0.03)	-0.020 (0.02)	0.004 (0.02)	0.245 (0.19)	0.005 (0.07)
Obs.	264218	264218	264218	264218	264218	264218	201659
R-squared	0.052	0.039	0.014	0.038	0.171	0.222	0.134

Notes: The Table presents the average causal program's effect on the average years of education, probability of being high, middle, or low educated, probability of being employed, log earnings, and log earnings conditional on employment. Exposed cohorts are 14-18 when a campus opened. Non-exposed are cohorts aged 28 to 33. All specifications include calendar year, locality, and cohort fixed effects. In the educational outcomes regressions we include controls for gender, marital status, and number of children under 12 in the household. In the labor market outcomes regressions we add controls for experience and its square. Standard errors clustered at the locality level in parentheses. ***significant at the 1 % level, **5 % level, *10 % level. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. We use the National Institute of Statistics' survey weights.

Table A.17: Diff-in Diff Estimates: Alternative Exposed Cohorts Definition

	Years of Education	High-Educ.	Middle-Educ.	Low-Educ.	Employment	Earnings(log)	Cond. Earnings(log)
Exposed x Campus	0.019 (0.20)	0.005 (0.02)	0.015 (0.02)	-0.021 (0.02)	-0.011 (0.01)	0.022 (0.13)	-0.047 (0.05)
Obs.	270352	270352	270352	270352	270352	270352	222360
R-squared	0.060	0.042	0.014	0.039	0.119	0.177	0.140

Notes: The Table presents the average causal program's effect on the average years of education, the probability of being high, middle, or low educated, the probability of being employed, the log earnings, and the log earnings conditional on employment. Exposed cohorts are those aged between 19 and 24 when the campus opened, and non-exposed cohorts are those aged 30 to 35. All regressions include calendar year, locality, and cohort fixed effects as controls. In the educational outcomes regressions we include controls for gender, marital status, and number of children under 12 in the household. We also control for experience and its square in the labor market outcomes regressions. Standard errors are clustered at the locality level in parentheses. ***significant at the 1 % level, **5 % level, *10 % level. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. We use the National Institute of Statistics' survey weights.