NBER WORKING PAPER SERIES

SCHOOLING AND INTERGENERATIONAL MOBILITY: CONSEQUENCES OF EXPANDING HIGHER EDUCATION INSTITUTIONS

Noemí Katzkowicz
Victor Lavy
Martina Querejeta
Tatiana Rosá

Working Paper 31906
http://www.nber.org/papers/w31906

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2023, Revised July 2024

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, the Annual Meeting of the Chilean Economic Society (SECHI) in Chile, and the Bristol 2024 Conference on Higher Education for useful comments and suggestions.

Schooling and Intergenerational Mobility: Consequences of Expanding Higher Education Institutions
Noemí Katzkowicz, Victor Lavy, Martina Querejeta, and Tatiana Rosá
NBER Working Paper No. 31906
November 2023, Revised July 2024
JEL No. D63,I23,I28,J16

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.

Noemí Katzkowicz
Instituto de Economía, FCEA,
Universidad de la República
noma.katzkowicz@fcea.edu.uy

Victor Lavy
Department of Economics
University of Warwick
Coventry, CV4 7AL
United Kingdom
and Hebrew University of Jerusalem
and also NBER
v.lavy@warwick.ac.uk

Martina Querejeta
Instituto de Economía, FCEA,
Universidad de la República
martinaquerejeta@gmail.com

Tatiana Rosá
Pontificia Universidad Católica de Chile
tatiana.rosa@uc.cl

1 Introduction

Returns to higher education in developing countries are high and increasing (Montenegro and Patrinos, 2014). Yet, post-secondary schooling attainment remains low. Various explanations exist for this low human capital investment, including financial constraints (Delavande and Zafar, 2019; Caucutt and Lochner, 2020), family background, and city or locality characteristics (Carneiro and Heckman, 2002). These latter factors are related to the so-called "birth lottery" (Chetty et al., 2014), which leads 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 focuses on the effect of the supply of higher education institutions. We exploit a unique context and policy in Uruguay, where the government aimed to improve access to university education for the population residing outside the capital city, Montevideo. This objective was to be achieved by building new university campuses nationwide. The program started in 2008, with gradual implementation due to budget constraints. The timing of the opening of new campuses across the country was unrelated to potential outcomes, allowing us to leverage this staggered supply expansion to identify its causal effect. We estimate a two-way fixed effect model of the program's effects based on time and geographic variation in the program's placement. We use novel administrative data on student enrollment in Uruguay's main university, Universidad de la República. The data covers the period from 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.[2] 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. 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. This equates to around 100 new enrolled students from that locality. The effect size is substantial, representing a 37 percent increase relative to a pre-treatment mean of 1.1%. Additionally, we find an average increase of 0.32 percentage points (pp) in the share of first-generation students over the total population under 30 during the same period.

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

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

The implied effect size is a 40% increase relative to the pre-treatment share of 0.8%.

Notably, the program had a more pronounced effect among students from less advantaged backgrounds, as measured by their parents' educational attainment. The effects on total enrollment extend to localities up to 30 km from the new campus, with the impact declining as the distance increases. Furthermore, our results show that the program did not significantly lower the university degree completion rate, countering arguments in the policy debate that there is a potential trade-off between universalizing access to higher education and maintaining degree attainment rates.[3]

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 further increase 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, leading to a short-term decline in the wages of these workers.

Moreover, these results hold when using 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. Following Duflo (2001), we estimate a Canonical Difference-in-Differences (DiD) model with controls. In this part of the study, we use 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 exposure to treatment.

Although this identification approach relies on micro-level data and also 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. Because of these limitations, we use this alternative strategy only as a robustness check for our main results.

This paper contributes to two strands of the literature. First, we add credible evidence 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; Currie and Moretti, 2003; Mazumder and Triyana, 2019; Akresh et al., 2022), and college proximity (Card, 1993).The literature shows that such public policies have significant short- and long-term effects on years of education, labor market outcomes, crime, and teenage pregnancy. More related to our research, previous studies demonstrate that geographical distance to university is an important determinant in youths' decision to enroll in tertiary education (Alm and Winters, 2009; Spiess and Wrohlich, 2010)

[3]A 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).

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). We build on this literature by showing that the geographical expansion of university campuses increased human capital and intergenerational mobility and reduced spatial educational inequality.[4]

Our work is also closely related to two papers that study a change in higher education policy 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 importance of financial constraints. On the other hand, Rau et al. (2012) show that access to credit reduces dropout rates from higher education, with even stronger effects among low-skill individuals from low-income families. However, 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 have 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.[5]

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 4 details the data, sample

[4] Méndez (2020) analyzes the same program and finds that students from high-educational backgrounds mostly explain university enrollment increases. However, this evidence is only correlational and cannot be viewed as causal. This study does not address the intergenerational mobility dimension.

[5] Previous 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, 2022), 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.

selection, and the various outcome variables used in the study. The primary identification strategy is presented in Section 5. Section 6 presents the main results of the evaluation of the university program, including heterogeneity analysis and spatial spillovers. Section 7 discusses the effects on other educational and labor market outcomes, and Section 8 provides evidence based on an alternative identification strategy. Finally, Section 9 concludes.

2 Institutional Context

Uruguay's population is 3.5 million inhabitants. It ranks fourth in GDP per capita among Latin American countries (World Bank Statistics[6]). Uruguay ranks third in the Human Development Index (UNDP, 2022). Its strong welfare system provides free access to public education at all levels, including university schooling. Moreover, public universities have no admission exams. Despite this, secondary school completion rates and tertiary education enrollment remain low.

Educational coverage at mandatory levels in Uruguay has increased substantially in the last decade.[7] 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 backgrounds face lower educational attainment at almost all levels (INEEd, 2018) (Panel A of Figure A.1). Therefore, while educational attainment up to lower secondary school is high relative to other Latin American countries, the secondary school completion rate is still relatively low: 40 percent compared to the Latin American average of 62 percent (INEEd, 2020). Consequently, tertiary education attainment is low, with only 26 percent of the population completing some university schooling, showing a strong socioeconomic gradient (Udelar, 2020).

Geographically, Uruguay is divided into 19 departments, each subdivided into several locations. Half the country's population resides in the capital city, Montevideo. Uruguay's main university, Universidad de la República, was historically located in Montevideo and covers 86 percent of all tertiary students.[8] In 2008, the university authorities initiated a program to build campuses in various parts of the country. By 2020, six of the nineteen departments had university campuses. Figure 1 shows the timing of university expansion.[9]

Before this program, Uruguay had high spatial inequality in university enrollment rates and educational mobility. Panel (a) in Figure 2 shows the average number of students enrolled in university

[6]Information available through this link.

[7]Secondary completion rates have increased by almost 10 pp since 2006 (INEEd, 2018).

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

[9]Prior to the program starting, three localities already had some university infrastructure: Paysandu, 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.

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 17. Moreover, the orange points suggest that the new campuses opened in or near locations with a low average number of enrolled students. Similarly, panel (b) in Figure 2 shows that the average proportion of students who are the first in their families to enroll in university exhibits 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 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 in 2002 to 15 percent in 2020.

3 Program implementation and Implications for Internal Validity

In 2006, a new University President was appointed after strongly advocating for the University expansion program.[10] A commission was established to oversee the program's implementation, and by the beginning of 2007, they decided to open the first campus in Maldonado. Later that year, University authorities approved the establishment of the remaining campuses in Rocha, Salto, Paysandú, Rivera, and Tacuarembó, although specific dates for their realization were not yet specified.

Before the initiation of the expansion program, certain localities had University houses, which are administrative units that link specific colleges' activities occurring in localities outside Montevideo and the colleges' headquarters in Montevideo. While some university activities took place outside of Montevideo before the program, they were financed and managed independently by colleges. The 2008 program expansion involved the opening of new proper campuses, coordinated centrally by the University authorities rather than the individual colleges. This entailed a significant allocation of resources to these new campuses.

According to the authorities at the time, the University's goal was to launch the program as soon as possible. When determining which localities would host the new campuses, they considered the presence of existing institutional links or infrastructure in those places as an advantage. However,

[10]Some of the material presented in this section is based on information from an interview we held recently with one of the university principal authorities at the time of the program's implementation. This person was involved in the decision-making regarding the expansion of university campuses and therefore is very familiar with factors leading to the placement and timing of new university campuses.

they encountered difficulties moving the program forward due to internal disputes and rivalries between localities, particularly those already hosting university activities. Consequently, the authorities decided to begin with the Maldonado campus, which had no previous university activity. In line with their strategy to minimize potential internal conflicts that could hinder the program's implementation and, taking into account the available university infrastructure, they proceeded with the roll-out as outlined in Figure 1 in the paper.

The validity of the empirical strategy relies on quasi-exogenous factors determining the location and timing of the campus openings. In this regard, authorities made campus opening decisions considering preexisting University activities while seeking to minimize the mentioned internal conflicts and rivalries between localities that could impede or excessively delay the program's start.

One could argue that campuses tend to open in municipalities where residents' education is already increasing or is expected to rise, and therefore, are not a cause but an effect of increasing education. Nevertheless, as shown in the results section, there is no anticipation in university enrollment and no significant differences between treated and non-treated localities before treatment. However, we observed fewer students enrolled in universities from the treated localities before the program's beginning compared to the non-treated localities, though the differences were not significant. One possible explanation for this disparity could be that treated localities are, on average, situated farther away from the capital city (308 km) than the non-treated ones (224 km), resulting in higher enrollment costs for students from those areas. This should not affect the internal validity of our identification strategy but should be considered when analyzing the external validity of our results.

Apart from educational attainment, one might wonder if the localities where campuses opened differed from untreated ones in other observed and unobserved dimensions. Table 1 provides evidence that treated and untreated localities were not different regarding labor market and educational outcomes. Although we cannot test for pre-program differences in all observables or any unobservable characteristics, the selection process of the localities where the campuses would open suggests quasi-random variation.

Finally, a crucial aspect of the validity of the empirical strategy relies on quasi-exogenous factors that determine the timing of campus openings. As discussed above, the decision of where and when to open a new campus is not solely based on efficiencies but also on political considerations. Once the locations for the new campuses were defined, the roll-out was primarily motivated by the University's objective of promptly implementing the program. Subsequently, as the program commenced and the initial two campuses became operational, the University opted to leverage preexisting infrastructure, even if it meant navigating conflicts or rivalries between localities. Additionally, to the best of our knowledge, the exact timing was also influenced by the personal characteristics of the local authorities and their rapport with university officials, which expedited the campus openings.

4 Data

We use administrative records of students enrolled at Uruguay's public university from 2002 to 2020.[11] Our primary dataset consists of administrative records for entry students and census data applied to all students during the entry year on a mandatory basis. Additionally, university completion information is available from 2006 to 2020. The data includes students' demographic and socioeconomic information, such as their secondary school and the maximum educational level attained by their parents. We also use the information on students' geographic location before commencing university schooling.[12] 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.[13] 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.

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). The completion indicator takes the value 1 if the student completed a university degree within five years since enrollment and 0 otherwise.[14] 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 total enrollment and enrollment of first-generation students (FGS), defined as those who are the first in their families to enroll in a university. FGS indicates students from households where any parent ever enrolled in university (irrespective of completion).[15]

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. This corresponds to the last Population Census before the program started. We compute the share of FGS in the total population under 30 and denote this measure of *FGS over population*. The share in total university enrollment is denoted as *FGS over enrollment*. The first measure allows

[11] We include only enrollment before the COVID pandemic started in March 2020.

[12] The dataset provides information on the institution where the student completed the 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.

[13] Nationally representative cross-section survey. Data and all related documents are available through this link.

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

[15] It is important to distinguish between tertiary and university education. If parents have tertiary non-university education, the student will be categorized as first generation.

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.[16]

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 also has very different 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 2 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 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.[17] 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.[18]

The average number of enrolled students at the locality level is 129, and the average share of enrolled students in the population between 18 and 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.

5 Empirical Strategy

5.1 Distance and Enrollment

As a starting point, we provide evidence that distance affects enrollment. To that end, we estimate the following equation:

$$Outcome_{l,t} = \alpha_0 + \mu_l + \mu_t + \beta Distance_{l,t} + \epsilon^d_{l,t} \tag{1}$$

[16]This 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.

[17]Four 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.

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

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 $\epsilon^d_{l,t}$ 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 4.

Table 3 shows 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 (Table 3).We find similar results for FGS, indicating that increasing the distance to a university by 100 km decreases the enrollment share of FGS in total enrollment by 0.016 p.p. Consistent with Card (1993), our findings show that distance to the campus significantly explains variations in university enrollment and intergenerational mobility. This enhances the importance of estimating the effects of the Uruguayan program aimed at reducing the distance to university campuses.

5.2 Identification strategy

To assess the program's effect on the outcomes of interest, we apply a staggered DiD strategy that leverages the variation in treatment time across localities.[19] 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 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.

We exploit this variation and estimate a static two way fixed effect (TWFE) model at the locality(l)-year(t) level:

$$Outcome_{l,t} = \alpha_0 + \mu_l + \mu_t + \gamma E_{l,t} + \epsilon^s_{l,t} \tag{2}$$

And a dynamic TWFE model (Borusyak and Jaravel, 2017)[20]:

$$Outcome_{l,t} = \alpha_0 + \mu_l + \mu_t + \sum_{h=-a,}^{h=b-1} \gamma_h 1[K_{lt} = h] + \gamma^i_b [K_{lt} = b] + \epsilon^d_{l,t} \tag{3}$$

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

[20]The author's imputation estimation only estimates the counterfactual values on untreated observations and extrapolates it to the treated observations.

where μ_l and μ_t are the locality and year (two-way) fixed effects. In equation 2, $E_{l,t}$ takes value 1 if there is a campus in locality l at year t and 0 otherwise. In equation 3, $a \geq 0$ and $b \geq 0$ are the numbers of included leads and lags of the event indicator, respectively. $K_{l,t} = t - E_I$ 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 $E_{l,t} = \inf$. The coefficients on the leads are interpreted as pre-trend measures and the hypotheses that $\gamma_{-a} = \gamma_{-a+1} = \ldots = \gamma_{-2} = 0$ is tested visually and statistically. Conditionally on this test passing, the coefficients on the leads are interpreted as a dynamic path of causal effects. $\epsilon^s_{l,t}$ and $\epsilon^d_{l,t}$ are the error terms in the static and dynamic model, respectively

We run our analysis at the locality level and estimate the average program's effect on university enrollment, intergenerational mobility, and degree completion.[21]

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.

6 The Effect of University Campuses Expansion

6.1 University Enrollment and Intergenerational Mobility

Figure 4 and Table A.1 in the Appendix show the estimates of Equation 3 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 5.

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. However, the effect seems to decrease in years 5 and 6 after campus opening, Figure 4. Panel (b) shows that this decline is reversed from year 7. This result implies that the program had a positive and significant effect on enrollment in the long run (0.34 p.p for ten years).[22] This is a sizable effect, especially relative to

[21]Running 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 only for enrolled students, we cannot compute the probability of an individual enrolling in university. Table A.3 and Figure A.3 in the Appendix provide estimates at the individual level.

[22]Estimates 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).

the pre-treatment mean enrollment share of 1.1%. Therefore, the effect size is 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. Based on 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. Similarly to the pattern of the event study estimates in panel (a), in panel (c) as well we see a decline in treatment effect in years 5 and 6 after campus opening, which is reversed in later years.

These results imply that in localities where the campus opened, the share of individuals surpass¬ing their parents' educational level increased significantly due to the program. Thus, this is evidence that the supply expansion program enhanced educational intergenerational mobility.

Finally, the program increased by 6.1 p.p, the share of FGS over total enrollment. 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.

Moreover, Figure 6 shows the program's effect on the total number of enrolled students and total FGS. Results indicate an average increase of over 100 enrolled students in treated localities, of which around 90 are the first generation in their families to enter university.

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 increases enrollment and positively enhances intergenerational mobility in educational attainment at the top of the educational distribution.

6.2 Heterogeneous Effects

We explore the heterogeneity of the results presented above by parental education. Figure 7 and Table A.2 in the Appendix 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). Overall, expanding university campuses had a larger effect on students from less educated families.

Panel (a) shows that during the first six years after the supply expansion, the share of students from families with parental primary education enrolled in university increased by 0.067 pp This effect size is large considering this group's pre-treatment mean enrollment share of 0.10% (Table A.2). It is

67% of the pre-treatment mean.

Panel (b) shows the same pattern for students whose parents reached secondary education. After the program´s implementation, the proportion of these students increased by 0.22 pp on average relative to a pre-treatment mean of 0.51%.

Finally, Panel (c) shows a smaller effect of 0.034 pp on the enrollment share of students with parents with tertiary non-university education. Though the effect is not statistically different from zero at a 10% significance level for most years after the program, it accounts for nearly 20% of the pre-treatment mean (0.18%).

Our results suggest that the program has heterogeneous effects 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's 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 while in college (an income effect mechanism) or may not have their children's higher education as a high priority (change in preferences mechanism).

6.3 Spatial Spillovers Effects

While campuses open in a given location, the program could also have affected 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 where we define the treatment exposure by 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.[23] We then define two buffers centered at the locality where the new campus opened with a 30 and 50 kilometer radius. Figure 1 lists the treated localities by their distance from the new campus.

Table 6 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 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 popu-

[23] Geodetic 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, as well as latitude and longitude information on each locality and campus, to compute the distance.

lation 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 program positively affected the share of enrolled students and the share of FGS over population under 30 within a 30 km radius. Furthermore, the program's effect on treated localities is statistically similar to that on localities farther away, up to 30 km.[24] But, the program's effect seems to vanish in localities 330 to 50 km from the campus, implying that in these localities, the mean of outcomes is the same as in non-treated localities.

Results point in a different direction when exploring the spatial spillovers on the share of FGS in total enrollment. The effect disappears when considering localities other than those where the campus opened. This implies that while the program increased the share of FGS in total enrollment, 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.

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 4 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.

6.4 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 lower completion 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.

Figure 8 and Table A.4 in the Appendix 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 analysis is 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 who completed a degree within five years. Results show that the proportion of students achieving a university degree did not change significantly with the program. All the event study estimates in both panels of Figure 8 are small and not statistically different from zero. This

[24]The Wald test on $\beta_{treated} = \beta_{30km}$ results in a p-value of 0.7260, rejecting the null hypothesis of the effects being different.

result is evidence that the university expansion program does not have the unintended consequence (cost) of lowering the university schooling completion rates.

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. Figure A.2 in the Appendix shows that the enrollment and intergenerational mobility results hold when considering this reduced sample.

7 Program's Spillover Effects

7.1 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 continuing 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 and apply the same TWFE methodology described in Section 5 to estimate the program's effect on high school outcomes. Figure 9 and Table A.6 in the Appendix 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).[25]

The estimated effects indicate increased high school attendance in localities where new campuses opened. Around one thousand (982) additional high school students enrolled in high school during the first six years after the program's implementation. This 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.

7.2 Labor Market Outcomes

This subsection presents estimates of the program's effect on labor market outcomes. We use data from National Household Surveys from 2002 to 2019 and the methodology described in Section 5.

[25]The selection of these age groups is based on the fact that ages 15 to 18 represent the majority of upper high school students. In contrast individuals aged 18 to 30 are expected to have already completed high school.

We restrict the sample to individuals aged 21 to 30 to estimate the effect of labor market outcomes. Table A.5 shows the main sample statistics.

Panel (a) of Figure 10 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 degree completion 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). We interpret 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. A mechanism consistent with our findings is a local increase in the supply of college graduates, which leads to a negative but small short-term impact on their earnings.

Additionally, Table A.7 in the Appendix shows no treatment effect on the probability of employment among the population of 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 return to highly, middle, and low-educated workers, we define three dummy variables: $HighEducated$ takes the value 1 if the individual has at least 12 years of education and 0 otherwise, $MidEducated$ takes the value 1 if the individual has between 7 and 12 years of education and 0 otherwise, and finally, $LowEducated$ takes value 1 if the individual has 6 or fewer years of education and 0 otherwise. The estimates presented in Table A.8 in the Appendix show a negative effect on the rate of return to education for highly educated workers, although it is not statistically significant.

8 Evidence Based on an Alternative Identification Strategy

8.1 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 apply an alternative identification strategy. In Uruguay, students typically decide to enroll in university after completing high school, at around 17 or 18 years old.

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. Table A.9 in the Appendix shows the descriptive statistics for this estimation sample.

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.

We estimate 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.[26] 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 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.

Table 1 in the Appendix 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.

Tables A.10 and A.11 in the Appendix compare the means of some of the main outcomes and 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

[26] In 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

programs affected the educational outcomes of exposed cohorts in treated localities but did not affect labor market outcomes and other demographics.

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:

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

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 children 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.

Table 7 shows estimates of the program's effects on educational outcomes. We define three dummy variables for individuals with high, middle, and low levels of education, as explained in Section 7.2.[27] 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 continue into post-secondary schooling (which is shown empirically in our regression as a decrease in the probability of having middle-level education), and 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 7 above.

Table 8 shows estimates for labor market outcomes. We focus on employment, the log earnings,

[27]In Section 7.2, 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.

and the log earnings conditional on employment following the definition of the variables in Section 7.2. 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, Table A.14 in the Appendix 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).

To support the above findings and their interpretation, we estimate a control experiment ("placebo exercise") using the cohorts aged between 30 and 35 exposed to treatment and those aged 36 to 40 as a control group. We expect to find no significant program effect for these cohorts since the individuals in these samples are already old enough to have made their decisions about university schooling. Table A.12 in the Appendix shows small positive and not statistically significant program 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.

9 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 decreased completion 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 on previous evidence showing that low-level educational outcomes can be improved by building primary schooling infrastructure, this paper provides evidence on the effects of higher education. 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. Schargrodsky (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.

Currie, J. and E. Moretti (2003). Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings. *The Quarterly journal of economics 118*(4), 1495–1532.

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, INEEd, Montevideo.

INEEd (2020). Reporte del Mirador Educativo 6. 40 años de Egreso de la Educación Media en Uruguay. Technical report, INEEd, 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. (2022). La Influencia del Contexto en la Transmisión Intergenerational Educativa en un País en Desarrollo: Tres Aproximaciones Empíricas para Uruguay. *Desarrollo y Sociedad* (92), 93–139.

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.

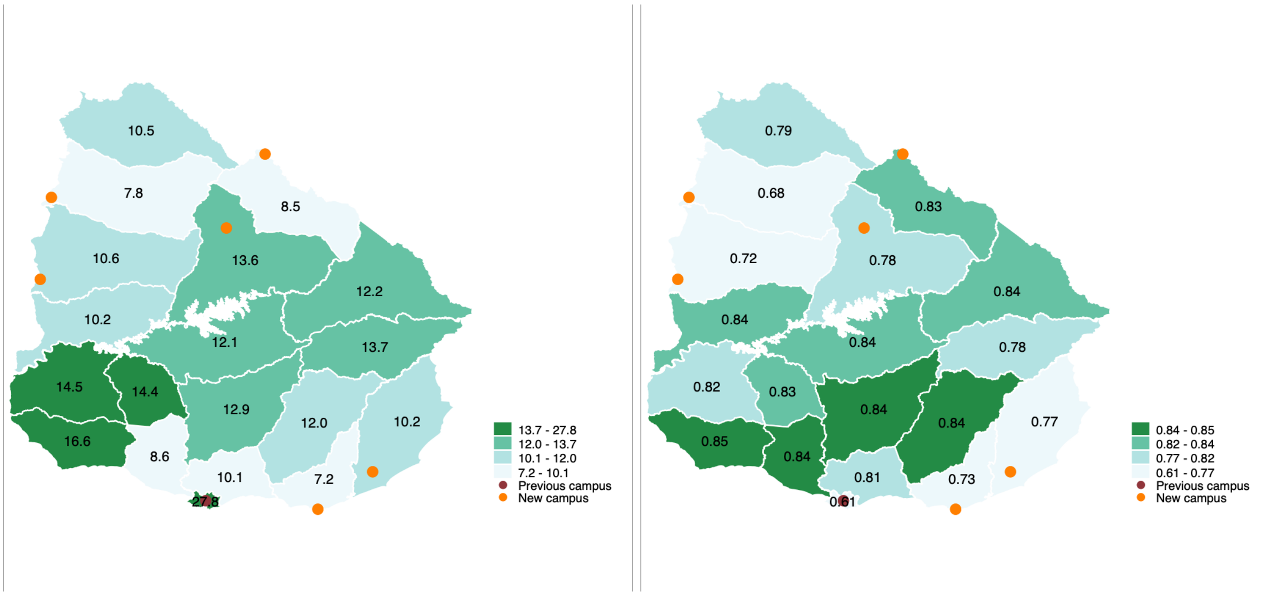
10 Figures and Tables

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

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*	Tacuarembo	Tacuarembo
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´s 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. Back to Institutional Context 2, Back to Data 4, Back to Identification Strategy 5.2, Back to Spatial Spillovers 6.3.

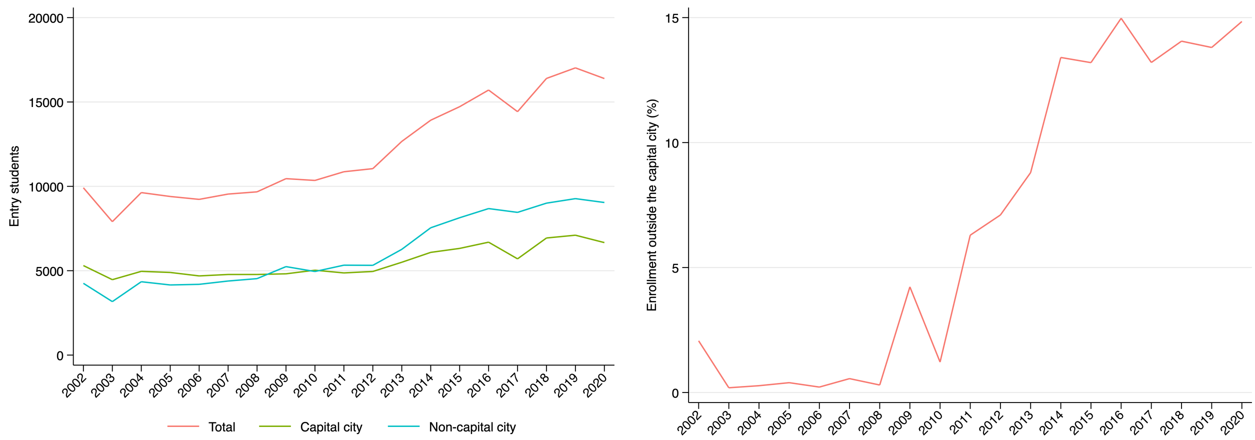
Figure 2: Enrollment and Educational Mobility in Uruguay Before the University Expansion, 2002-2007



(a) Number of Enrolled students (b) Share of First-Generation Students

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 University campuses' locations. Previous campuses are shown in maroon, and new campuses 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 the enrollment year census. Back to Institutional Context 2.

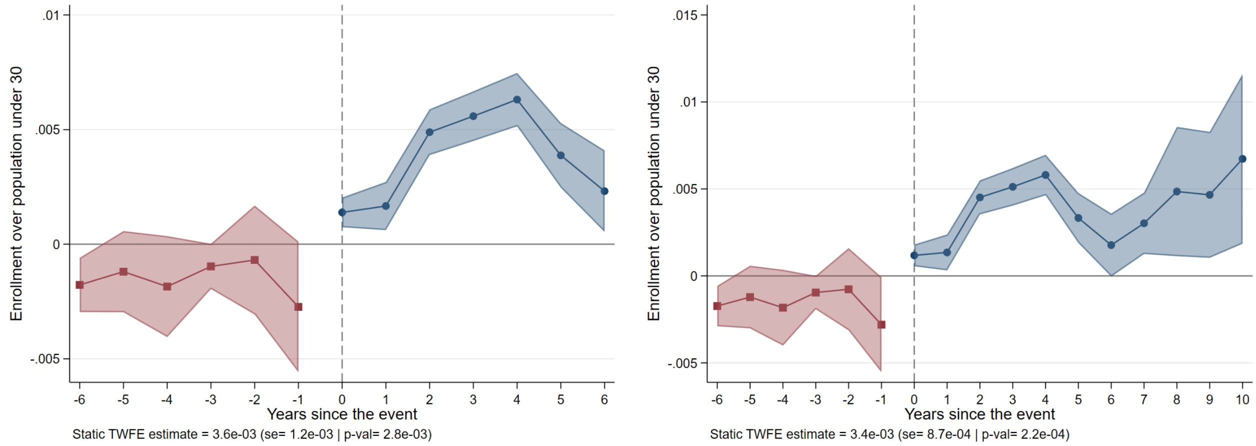
Figure 3: Fresher Students by Geographical Region of Origin and Destination, Years 2002-2020



(a) Region of Origin (b) Region of Destination

Notes: The Figures show (Panel a) the number of entry students by region where they lived before entering university and (Panel b) the share of entry students enrolled in a university campus outside the capital city. 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 for the enrollment year. Back to Institutional Context 2.

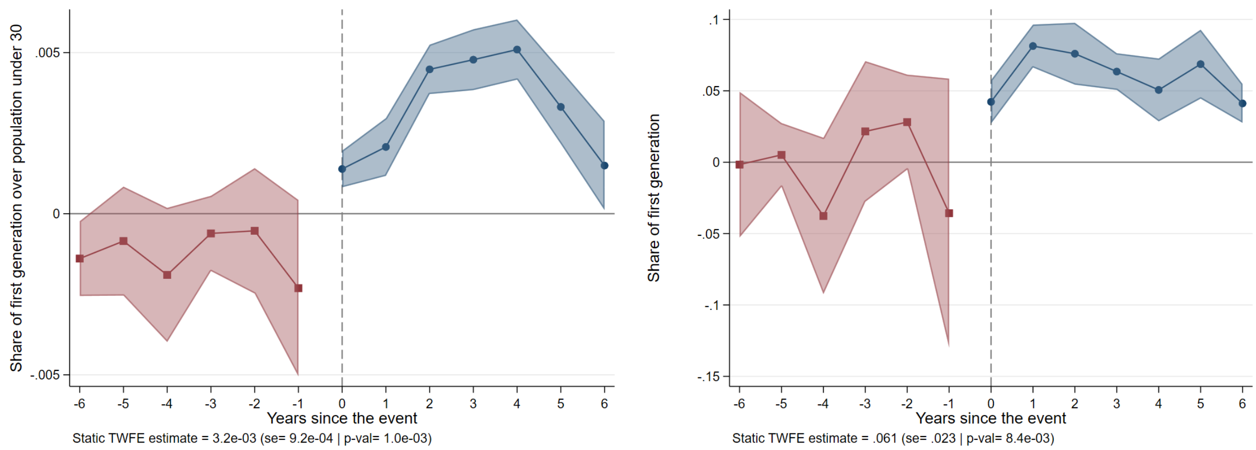
Figure 4: Event Study Estimated Effects on the Share of Enrolled Students



(a) Share of enrollment over population 18-30 (b) Share of enrollment over population 18-30

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 between 18 and 30 years old by locality from a pre-reform period (Census 1996). The sample was drawn from the university administrative records and census and the enrollment year. Back to Results 6.

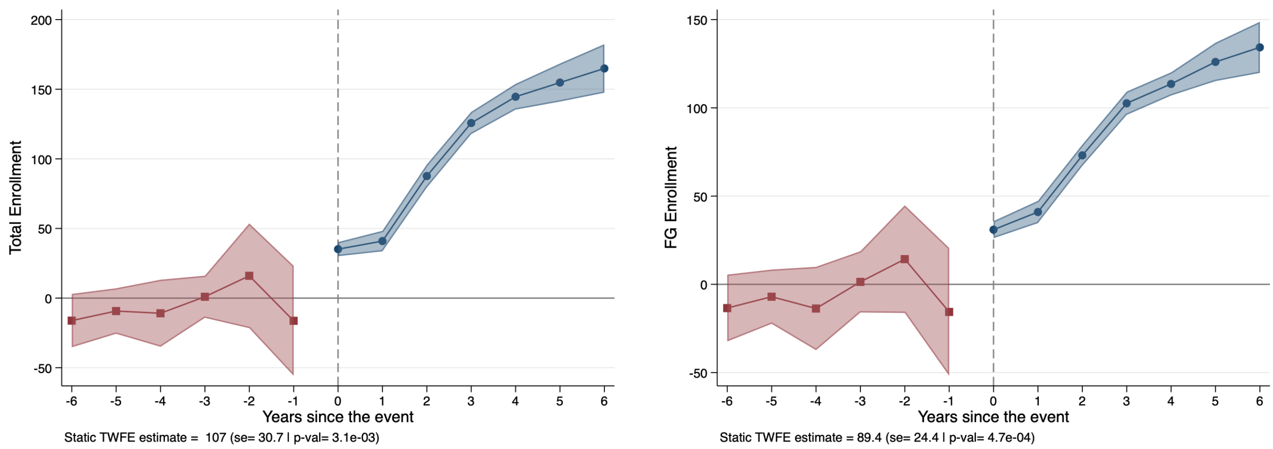
Figure 5: Event Study Estimated Effects on the Educational Intergenerational Mobility



(a) FGS in Population 18-30 (b) 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 between 18 and 30 years old by locality from a pre-reform period (Census 1996). The sample was drawn from the university administrative records and census and the enrollment year. Back to Results 6.

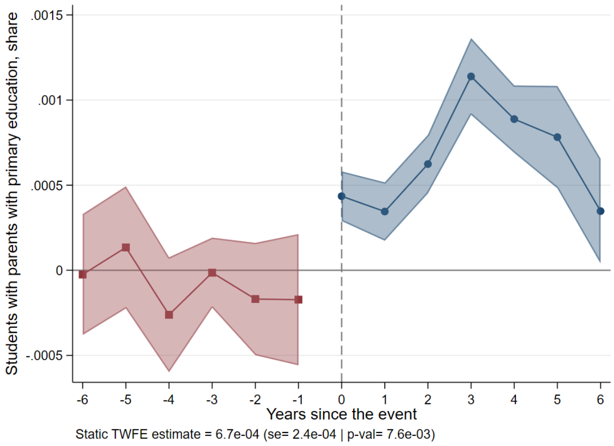
Figure 6: Event Study Estimated Effects on Total Enrollment and FGS



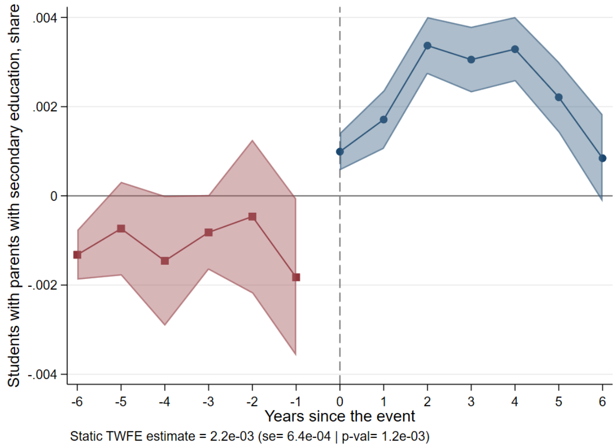
(a) Total Enrollment (b) FGS Enrollment

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 between 18 and 30 years oldbased on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Results 6.

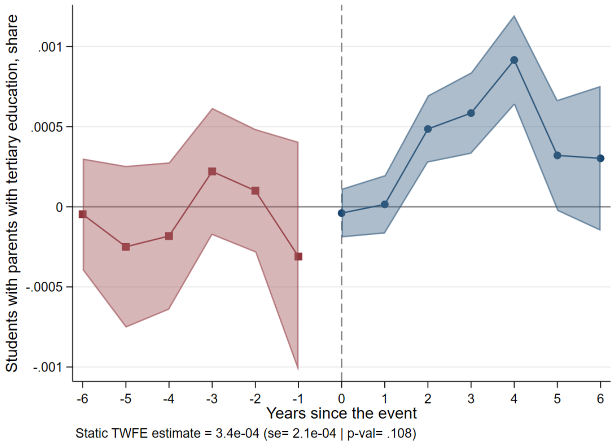
Figure 7: Event Study Estimated Effects on the Share of FGS by Parental Background



(a) Parents with Primary Education



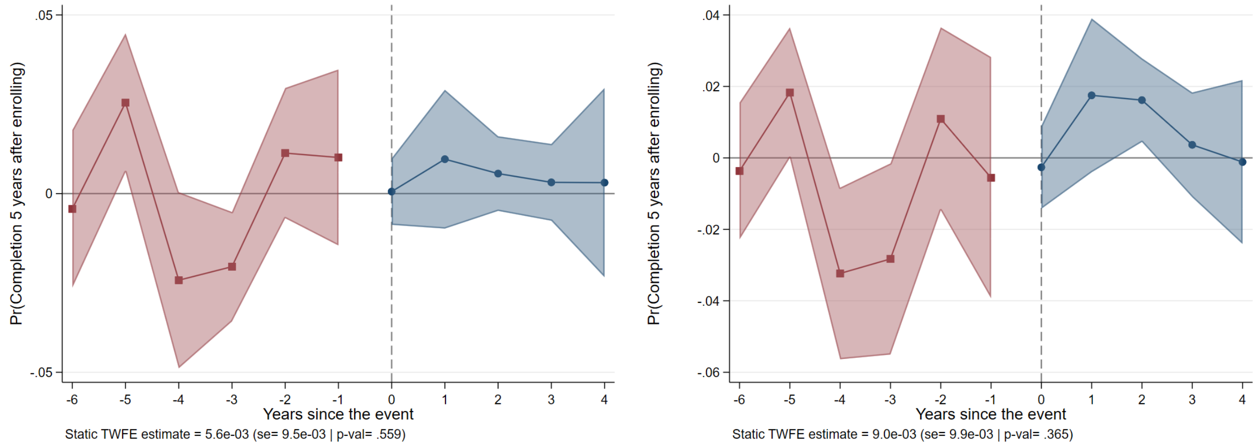
(b) Parents with Secondary Education



(c) Parents with Tertiary (non-Univ) 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 between 18 and 30 years old by locality from a pre-reform period (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Subsection 6.2.

Figure 8: Event Study Estimated Effects on Completion of University Degree: All and First-Generation Students

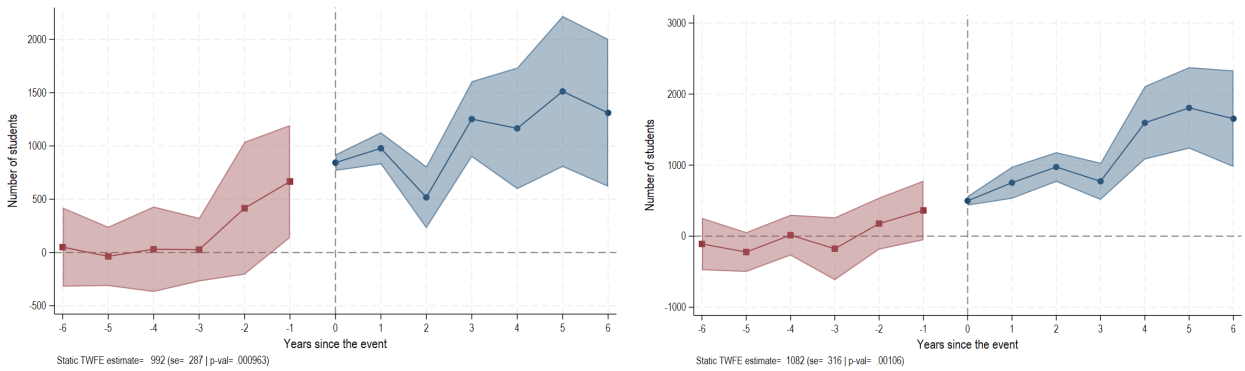


(a) All Students University Degree Completion Rate

(b) FGS University Degree Completion

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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. For this analysis, the sample was restricted to students who enrolled in university between 2002 and 2015. Back to Subsection 6.4.

Figure 9: Event Study Estimated Effects on High School Outcomes

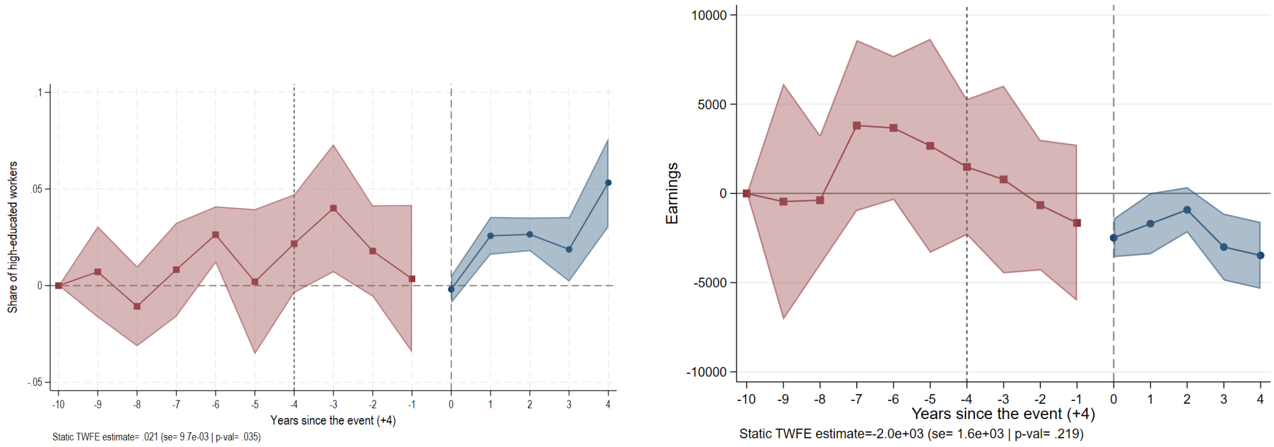


(a) High School Attendance

(b) High School Completion

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 between 18 and 30 years old by locality from a pre-reform period (2002 to 2007). The sample was drawn from the NHS years 2002 to 2019 using survey weights computed by the National Institute of Statistics. Back to Subsection 7.1.

Figure 10: 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 between 18 and 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. The sample was drawn from the NHS years 2002 to 2019 using survey weights computed by the National Institute of Statistics. Back to Subsection 7.2.

Table 1: Balancing Tests of Pre-Treatment Characteristics (2002-2006)

Variable	Control	Treatment	(C-T)
Female	0.523	0.533	0.010***
Age	40.297	39.853	-0.444***
Employment	0.646	0.640	-0.006
Formality	0.583	0.569	-0.013
Earnings (cond. on employment)	6,158.052	6,071.764	-86.288
Years of education	9.030	9.350	0.321***
Primary education	0.215	0.205	-0.010
Secondary education	0.648	0.643	-0.006
Tertiary education	0.132	0.147	0.014*

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. ***significant at the 1% level, **5% level, *10% level. Localities were weighted using population between 18 and 30 years old by locality from a pre-reform period (2002 to 2007). The sample was drawn from the NHS years 2002 to 2006 using survey weights computed by the National Institute of Statistics. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones.

Table 2: 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 for the main characteristics of the estimation sample. The sample was drawn from the university administrative records and the enrollment year census. 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 between 18 and 30 years old based on pre-reform data (Census 1996). Back to Section 4.

Table 3: Regression Estimates of Distance and Enrollment

	Share Enrollment	Share FG (pop)	Share FG (enroll)
Distance to closest campus	-0.0009***	-0.0009***	-0.0159***
	(0.00)	(0.00)	(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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Subsection 5.1.

Table 4: Regression Estimates of Distance to the Closest Campus and Enrollment

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

Notes: The Table presents the static TWFE estimates of distance (in hundred km). 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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Subsection 6.3.

Table 5: Static Two Way Fixed Effects Estimates

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

Notes: The Table shows 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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Results 6.

Table 6: Estimates of the Spatial Spillovers of the Program

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

Notes: The Table shows 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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Results 6.3.

Table 7: 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**	0.032*	0.013	-0.045*
	(0.21)	(0.02)	(0.03)	(0.03)
Obs.	8184	8184	8184	8184
R-squared	0.049	0.038	0.018	0.038

Notes: The Table shows the average causal effects of the program. 'High Education' is a dummy variable taking value 1 if the individual has more than 12 years of education, 'Middle Education' takes value 1 if the individual has between 7 and 12 years of education, and 'Low Education' 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 was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. Sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. Back to Section 8.

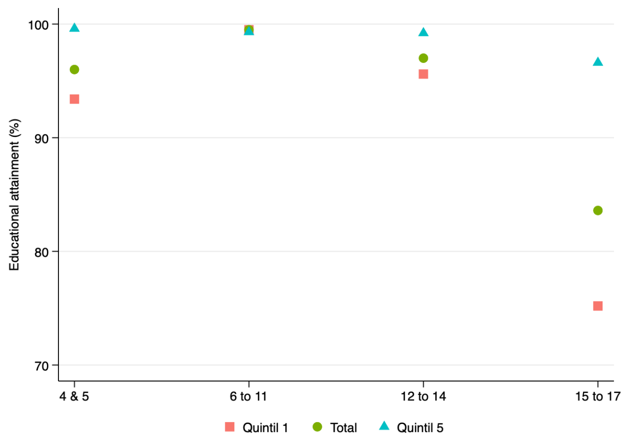
Table 8: Diff-in Diff Estimates of the Program Effect on Labor Market Outcomes

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

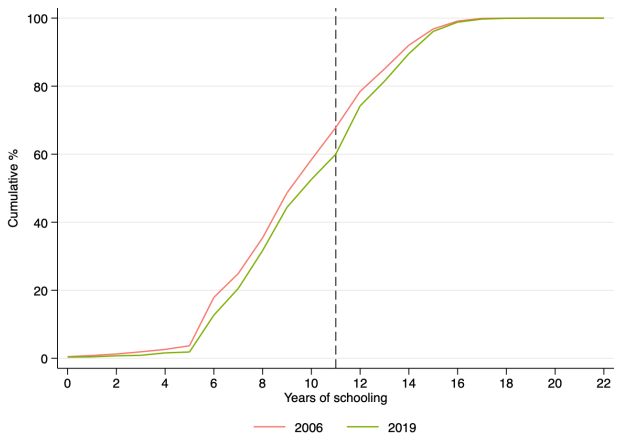
Notes: The Table shows the average causal effects of the program. All specifications include as controls calendar year, locality, cohort fixed effects, experience and its square, gender, marital status, and number of children under 12. Standard errors clustered at the locality level in parentheses.***significant at the 1 % level, **5 % level, *10 % level. The sample was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. Sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. Back to Section 8.

Online Appendix: Not For Publication

Figure A.1: Educational Context: Attainment and Years of Schooling in Uruguay



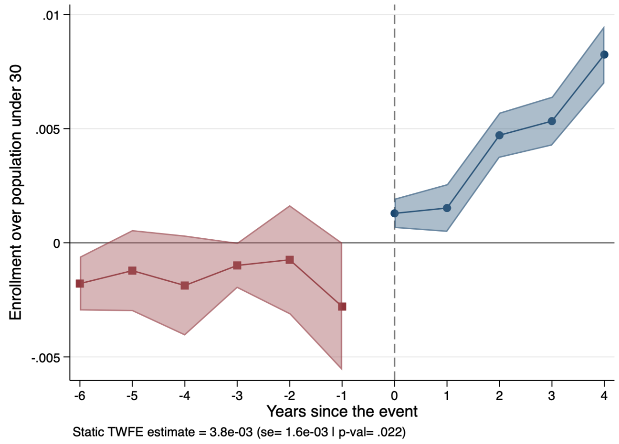
(a) Attainment by socioeconomic level



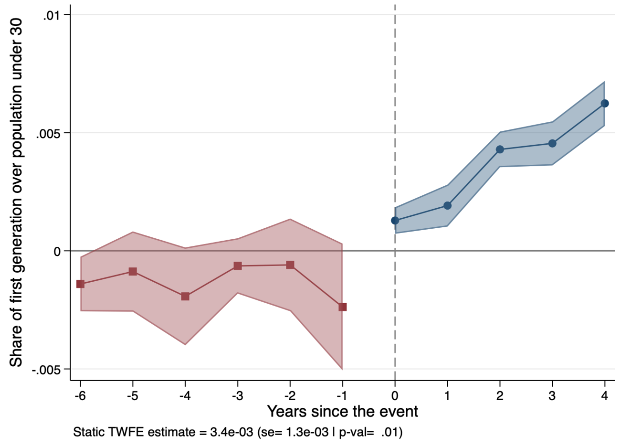
(b) Years of schooling (ages 20-24)

Notes: The Figure shows (Panel a) 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). And (Panel b) the cumulative distribution of years of schooling for people aged between 20 and 24. Own calculations based on NHS years 2006 and 2019 using survey weights computed by the National Institute of Statistics. Back to Section 2.

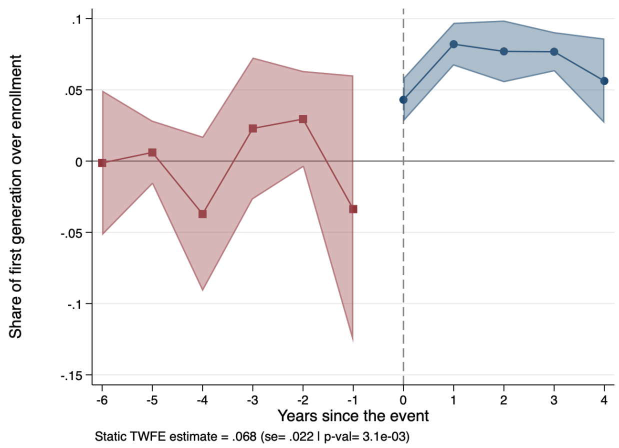
Figure A.2: Event Study Estimated Effects on the Share of Enrolled Students and Educational Intergenerational Mobility - Completion Sample



(a) Enrollment share



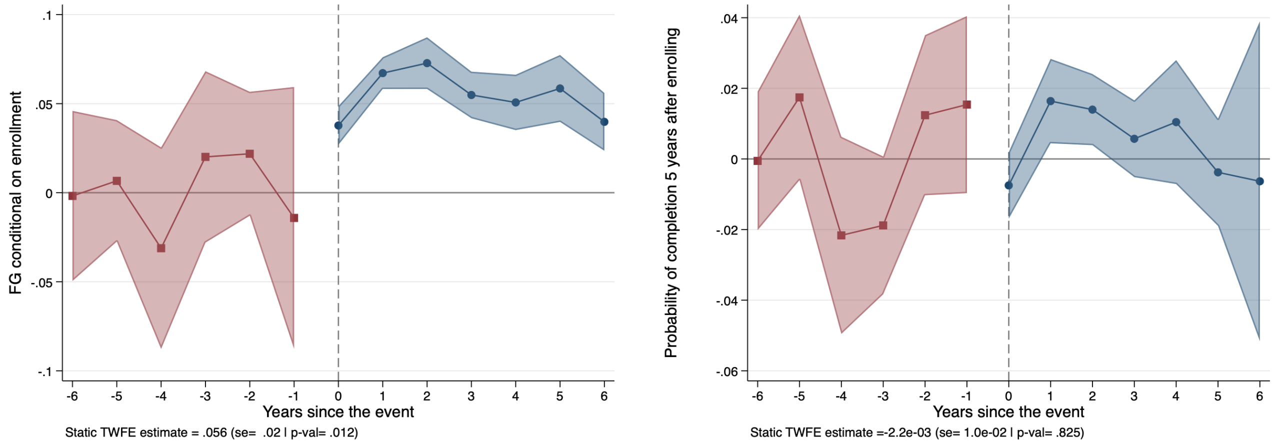
(b) FGS share in the population under 30



(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. Localities were weighted using population between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. For this analysis, the sample was restricted to students who enrolled in university between 2002 and 2015. Back to Section 6.4.

Figure A.3: Event Study Estimated Effects 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. The sample was drawn from the university administrative records and the enrollment year census. For completion, the sample was restricted to students who enrolled in university between 2002 to 2015. Back to Section 5.2.

Table A.1: Effect on the Share of Enrolled Students, and Educational Intergenerational Mobility

	Share Enrollment	Share Enrollment	Share FG (pop)	Share FG (enroll)
pre1	-0.003**	-0.003*	-0.002*	-0.036
	(0.00)	(0.00)	(0.00)	(0.05)
pre2	-0.001	-0.001	-0.001	0.028*
	(0.00)	(0.00)	(0.00)	(0.02)
pre3	-0.001*	-0.001*	-0.001	0.022
	(0.00)	(0.00)	(0.00)	(0.03)
pre4	-0.002	-0.002*	-0.002*	-0.038
	(0.00)	(0.00)	(0.00)	(0.03)
pre5	-0.001	-0.001	-0.001	0.005
	(0.00)	(0.00)	(0.00)	(0.01)
pre6	-0.002***	-0.002***	-0.001**	-0.002
	(0.00)	(0.00)	(0.00)	(0.03)
tau0	0.001***	0.001***	0.001***	0.042***
	(0.00)	(0.00)	(0.00)	(0.01)
tau1	0.001**	0.002***	0.002***	0.081***
	(0.00)	(0.00)	(0.00)	(0.01)
tau2	0.005***	0.005***	0.004***	0.076***
	(0.00)	(0.00)	(0.00)	(0.01)
tau3	0.005***	0.006***	0.005***	0.064***
	(0.00)	(0.00)	(0.00)	(0.01)
tau4	0.006***	0.006***	0.005***	0.051***
	(0.00)	(0.00)	(0.00)	(0.01)
tau5	0.003***	0.004***	0.003***	0.069***
	(0.00)	(0.00)	(0.00)	(0.01)
tau6	0.002*	0.002**	0.001**	0.041***
	(0.00)	(0.00)	(0.00)	(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 shows 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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Section 6.

Table A.2: Effect on First Generation Students, by Parental Background

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

Notes: The Table shows 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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. Back to Section 6.2.

Table A.3: 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	0.015
	(0.04)	(0.01)
pre2	0.022	0.012
	(0.02)	(0.01)
pre3	0.020	-0.019*
	(0.02)	(0.01)
pre4	-0.031	-0.022
	(0.03)	(0.01)
pre5	0.007	0.017
	(0.02)	(0.01)
pre6	-0.002	-0.001
	(0.02)	(0.01)
tau0	0.038***	-0.007
	(0.01)	(0.00)
tau1	0.067***	0.016***
	(0.00)	(0.01)
tau2	0.073***	0.014***
	(0.01)	(0.01)
tau3	0.055***	0.006
	(0.01)	(0.01)
tau4	0.051***	0.010
	(0.01)	(0.01)
tau5	0.059***	-0.004
	(0.01)	(0.01)
tau6	0.040***	-0.006
	(0.01)	(0.02)
Pre-treatment Mean	0.745	0.450
Obs.	57281	34190

Notes: The Table shows 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. The sample was drawn from the university administrative records and the enrollment year census. For completion analysis, the sample was restricted to students who enrolled in university between 2002 to 2015. Back to Section 5.2.

Table A.4: Effects on Degree Completion for Total and First-Generation Students

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

Notes: The Table shows 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 between 18 and 30 years old based on pre-reform data (Census 1996). The sample was drawn from the university administrative records and the enrollment year census. For this analysis, the sample was restricted to students who enrolled in university between 2002 and 2015. Back to Section 6.4.

Table A.5: 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 shows 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. Analysis restricted to individuals aged 21-30.

The sample was drawn from the NHS years 2002 to 2019 using survey weights computed by the National Institute of Statistics. Back to Section 7.2.

Table A.6: Estimated Effects on Other Educational Outcomes

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

Notes: The Table shows 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 between 18 and 30 years old based on pre-reform data (2002 to 2007). The sample was drawn from the NHS years 2002 to 2019 using survey weights computed by the National Institute of Statistics. Back to Section 7.1.

Table A.7: Employment Effects

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

Notes: The Table shows 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 in the household. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Analysis restricted to individuals aged 21-30. The sample was drawn from the NHS years 2002 to 2019 using survey weights computed by the National Institute of Statistics. Back to Section 7.2.

Table A.8: Mincer Wage Equations

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

Notes: The Table shows 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 in the household. Standard errors clustered at the locality level in parentheses. ***significant at the 1% level, **5% level, *10% level. Analysis restricted to individuals aged 21-30. The sample was drawn from the NHS years 2002 to 2019 using survey weights computed by the National Institute of Statistics. Back to Section 7.2.

Table A.9: 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 shows summary statistics for the outcomes variables. Analysis restricted to individuals aged 14-33 when the campus opened. The sample was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. Back to Section 8.

Table A.10: 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 shows the mean and standard deviation of the main variables by treatment status depending on the locality. The sample was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. Back to Section 8.

Table A.11: 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 shows 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 was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. Back to Section 8.

Table A.12: 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	0.010	0.028	-0.039**	-0.000	-0.036
	(0.12)	(0.01)	(0.03)	(0.02)	(0.02)	(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 shows the average causal effects of the program 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 was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. Back to Section 8.

Table A.13: 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	0.024	-0.003	-0.020	0.004	0.245	0.005
	(0.22)	(0.02)	(0.03)	(0.02)	(0.02)	(0.19)	(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 shows the average causal effects of the program 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 was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. Back to Section 8.

Table A.14: 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	0.005	0.015	-0.021	-0.011	0.022	-0.047
	(0.20)	(0.02)	(0.02)	(0.02)	(0.01)	(0.13)	(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 shows the average causal effects of the program 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 was drawn from the NHS years 2018 to 2019 using survey weights computed by the National Institute of Statistics. The sample is restricted to individuals in municipal capitals, except Montevideo and Canelones. Back to Section 8.