University at Your Doorstep: Local Human Capital Effects of Higher Education Expansion*

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Abstract

University systems in many countries expanded by establishing new institutions in areas previously lacking higher education. We study Italy's postwar university expansion, which opened the first faculties in provinces that had never hosted one. Exploiting variation in the timing of these openings across provinces and birth cohorts in an event-study design, we find that local access increased graduation rates by 3.2 percentage points on average across treated cohorts (approximately 26 percent relative to the pre-treatment mean), with a sharp discontinuity of 1.2 percentage points at the enrollment-age threshold. The effect is significant across all urbanization levels and increasing in more urbanized provinces, consistent with complementarities between university access and local labour market conditions. Women benefited disproportionately, though gender gaps in labour market outcomes narrowed by less than those in education. At the province level, these first openings narrowed educational disparities between provinces that gained a university and those that remained unserved.

Keywords: Tertiary education, Higher education expansion, Gender gap, spatial inequality.

JEL codes: I23, J16, J21, J24, H55, R12.

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1 Introduction

Access to tertiary education is a key determinant of individuals' educational trajectories and long run labour market outcomes. By shaping human capital accumulation early in life, investments in higher education can have persistent effects on labour market attachment well beyond the schooling period. Understanding how changes in access to universities affect educational attainment and subsequent labour market participation is therefore central to the design of effective education and labour market policies.

A growing literature emphasizes the crucial role of geographic accessibility in tertiary education choices. The financial and opportunity costs of university attendance increase with distance, making local availability of higher education institutions a major determinant of enrollment and completion decisions (Carneiro et al., 2011; Nybom, 2017). Expansions of university supply, particularly through the establishment of universities in previously unserved areas, can substantially lower these barriers and broaden access to higher education (Ichino et al., 2025), though the effects depend on the specific barriers addressed, whether geographic, financial, or ability-related. While a robust body of evidence documents positive effects of such expansions on educational attainment, findings on their longer run labour market consequences remain mixed, and little is known about the spatial mechanisms through which these effects operate.[1]

A smaller but growing strand of this literature focuses specifically on the spatial dimension of university access. Falch et al. (2013) show that geographic distance significantly reduces educational attainment in Norway, with effects concentrated among students from low-income families. Andersson et al. (2004) study Sweden's university decentralization policy and find positive effects on local human capital, while Kobus et al. (2015) document that commuting time to university reduces academic achievement in the Netherlands. In a closely related setting, Caner et al. (2024) exploit Turkey's large-scale university expansion to study effects on attainment and gender equality, finding that new universities increased enrollment but did not close the gender gap—a result that contrasts with the Italian experience documented in our paper. These studies share the insight that geographic barriers to higher education are not merely a friction but a binding constraint that shapes the distribution of human capital across space. Our contribution is to combine this spatial perspective with a rich institutional setting, an explicit gender dimension, and

[1]See Currie and Moretti (2003) for an early contribution on the role of geographic proximity in college enrollment. Fack and Grenet (2015) and Oppedisano (2011) document enrollment effects of university proximity in France and Italy, respectively.

a comprehensive analysis of the mechanisms through which proximity operates.

This paper studies the expansion of tertiary education in Italy by focusing on the opening of the first university faculty in provinces that previously lacked any higher education institution. These openings—concentrated in the 1960s and 1970s—represent a sharp and localized increase in access to tertiary education and provide a source of quasi-exogenous variation across birth cohorts within the same province. Such openings predominantly occurred in relatively peripheral, less urbanized areas that had historically limited access to university education. We exploit this variation to examine how local university openings affect university graduation, employment, and inactivity later in life, with an explicit focus on gender differences and on the geographic heterogeneity of the effect.[2]

We make three main contributions to the literature. First, we provide causal evidence on the effects of local university openings on university graduation, and ITT evidence on their association with subsequent labour market outcomes, focusing on both employment and inactivity. By exploiting the staggered timing of the first university opening at the province level, we isolate the extensive margin of tertiary education expansion and trace its long-term consequences across cohorts exposed to improved local access. Our event-study estimate of 1.2 percentage points at the enrollment-age threshold is confirmed by the de Chaisemartin and D'Haultfœuille (2020) estimator designed for staggered treatment adoption. Aggregating across all treated cohorts, the difference-in-differences estimate yields an average treatment effect of 3.2 percentage points (approximately 26 percent relative to the pre-treatment mean), reflecting the stronger response of cohorts who gained access at younger ages. Both estimates survive a comprehensive battery of placebo tests, leave-one-out exercises, and alternative specifications.

Second, we uncover a pronounced geographic gradient in the treatment effect. The effect is positive and significant across all urbanization terciles, and increasing in more urbanized provinces. This pattern is consistent with a hypothesis of labour market complementarities between university access and local labour market conditions: in more urbanized areas, deeper and more diversified labour markets may amplify the returns to obtaining a degree once a local institution becomes available. The sharp attenuation of the effect for cohorts above typical enrollment age provides further support for a causal

[2] In this paper, we use the terms "college," "tertiary," and "university" education interchangeably. We focus on tertiary education broadly defined, including undergraduate and postgraduate university education; tertiary non-university education is very limited in Italy. For identification, we primarily leverage local proximity to university premises following the opening of the first university in a province.

interpretation of the results.[3]

Third, we contribute to the literature on gender differences in higher education and labour market outcomes. Women benefited disproportionately from local university openings, consistent with the hypothesis that geographic barriers to higher education weigh more heavily on women due to social norms and family constraints governing female mobility (Boelman, 2021; Braccioli et al., 2023). Yet the translation of educational gains into labour market outcomes is asymmetric: while exposure to local universities is associated with higher employment and lower inactivity at older ages, the effects are smaller and less precisely estimated than for educational attainment, and gender differences in labour market responses persist.[4]

Beyond the individual-level analysis, we document that the university expansion contributed to a partial convergence in human capital across Italian provinces. Late-expansion provinces substantially narrowed their graduation gap relative to historical university towns, while provinces that never received a university fell further behind. No spillover effects are detected in neighboring provinces, confirming that the benefits operate through strictly local access. Because our data record province of current residence rather than province of birth, we show that residential mobility introduces attenuation bias and calibrate bounds on the true effect using 2001 Census mobility data, under random misclassification, the corrected estimate rises to 4.3 percentage points; graduate-weighted correction yields 4.6 percentage points.

Our empirical results contribute to the broader literature on the spatial allocation of public investment in education and its consequences for regional inequality (Moretti, 2004). The findings are particularly relevant for policy debates in countries with geographically concentrated higher education systems and persistent territorial disparities in human capital.

The remainder of the paper is organized as follows. Section 2 provides institutional background on the Italian university expansion. Section 3 describes the data and presents descriptive evidence. Section 4 introduces the empirical strategy. Section 5 presents the main results, including heterogeneity by gender and urbanization. Section 6 discusses robustness checks. Section 7 examines spatial spillovers and convergence in human capital

[3]We find a large and precisely estimated effect for cohorts aged 18 or younger at the time of university opening, which drops to zero at age 19—consistent with the fact that university enrollment decisions in Italy are made at age 18–19.

[4]See also Elsayed and Shirshikova (2023) for evidence on gender-differentiated effects of university access in a developing-country context.

across provinces. Section 8 concludes.

2 Institutional Background

Italy's higher education system has undergone a dramatic geographic transformation over the second half of the twentieth century. Until 1950, university education was concentrated in a small number of historical seats—major cities such as Rome, Milan, Naples, Bologna, Florence, Turin, Padua, and Palermo—leaving vast portions of the country without local access to tertiary education. For individuals residing in provinces without a university, attending higher education required relocating to a distant city, entailing substantial financial, logistic, and personal costs.

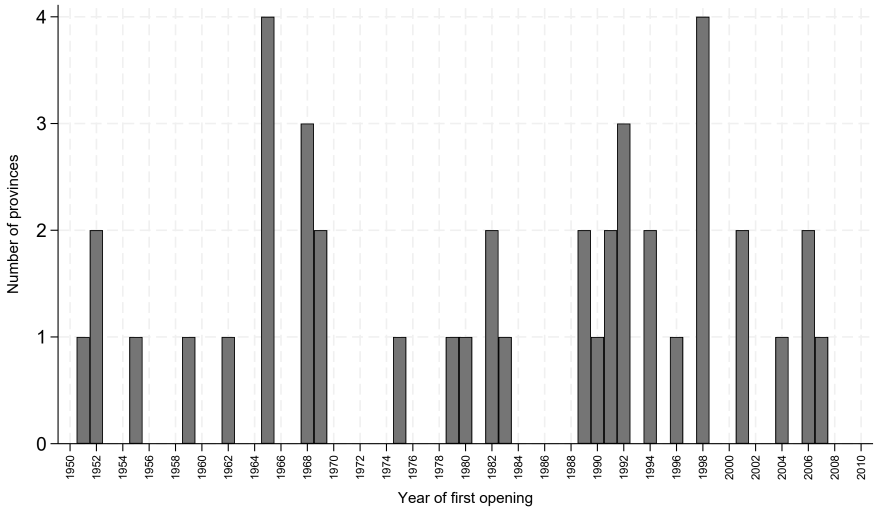
Beginning in the 1950s, new university branches and satellite campuses began to open in medium-sized provincial capitals. This process was driven by a sequence of post-war reforms that progressively expanded and restructured the Italian higher education system. Law 910/1969 liberalized university access for students holding a five-year secondary diploma from technical schools, leading to a sharp increase in demand. Law 766/1973 promoted the opening of new universities and the hiring of academic staff. Law 382/1980 reformed university governance and academic careers. Laws 392/1989 and 245–341/1990 modified funding allocation rules, while Law 59/1997 granted universities greater financial and teaching autonomy. As a result, the creation of new faculties accelerated markedly from the 1970s onward, and by the early 2000s the number of provinces hosting at least one university faculty had more than tripled relative to 1950.

Figure 1 reports the timing of first university openings across provinces. The pattern closely mirrors the sequence of higher education reforms, with clusters of new openings following each major legislative change.

The decision to open a new university branch was not part of a centralized national plan. Rather, it was typically driven by a combination of local political pressure, regional development objectives, and initiatives from existing universities seeking to expand their catchment area. The timing of openings varied considerably across provinces for reasons largely unrelated to local educational outcomes. This variation in timing, after conditioning on province and cohort fixed effects, is plausibly exogenous to individual-level educational outcomes, and forms the basis of our empirical strategy.

Figure 2 displays the geographic distribution of university openings, classifying provinces

Figure 1: Number of provinces where the first university opened, by year of opening



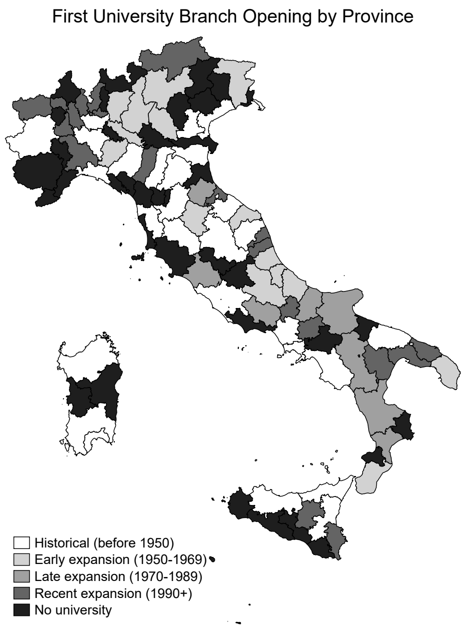
Source: Own calculations using the HIU dataset.

by the period of first opening. The map reveals that the expansion process does not follow a simple North-to-South pattern. Historical seats are distributed across the entire peninsula, while subsequent waves of expansion occurred both in northern provinces (e.g., Brescia, Varese, Bolzano, Vercelli) and in southern ones (e.g., Cosenza, Potenza, Campobasso, Foggia). A substantial number of provinces, predominantly small and inland, never received a university branch during our sample period. This geographic dispersion of treatment timing ensures that the variation we exploit is not confounded by single regional trends.

The expansion of university access occurred against the backdrop of a profound transformation in women's educational attainment in Italy. In the 1950s and 1960s, female university enrollment was extremely low, particularly in southern and rural provinces where social norms discouraged women from leaving home to study in distant cities (Braccioli et al., 2023). The cost of geographic mobility for university education was thus disproportionately borne by women, for whom distance represented not only a financial barrier but also a social and cultural barrier. The opening of local university branches potentially reduced these gender-specific barriers to access, a hypothesis we test directly in Section 5.

Over the same period, tertiary education enrollment and attainment increased steadily in Italy. Census data document that the share of individuals aged six and above holding a university degree rose from 1.8 percent in 1971 to 11.2 percent in 2011, representing

Figure 2: Year of first university opening by province



Source: Own elaborations based on the HIU dataset (Cottini et al., 2026). Provinces are classified by the year of first faculty opening. White: historical university seats (before 1950); light grey: early expansion (1950–1969); medium grey: late expansion (1970–1989); dark grey: recent expansion (1990 and later); black: no university by 2010.

more than a sixfold increase over four decades. Figure A-1, in the Appendix, provides descriptive evidence on how graduation rates evolved across cohorts for different types of provinces. All province groups exhibit rising graduation rates, reflecting the nationwide trend. However, provinces that received a new university show a steeper increase for younger cohorts, converging toward historical seats, while provinces that never received a university lag behind. This descriptive pattern is consistent with a positive effect of local university presence on graduation rates, though our causal identification strategy—described in Section 4—relies on within-province variation across cohorts rather than on these between-province comparisons.

3 Data and descriptive evidence

University expansion data. We combine two main data sources. The first is the History of Italian Universities (HIU), assembled by Cottini et al. (2026). This database provides a comprehensive registry of university openings in Italy, recording the dates of creation of each faculty within each university from Italian unification in 1861 up to 2010.[5] Using the HIU, we construct a province-level panel dataset which, for each year since 1950, records the number of faculties operating in each province and tracks the timing of every new faculty opening.

LFS data on education and labour outcomes. The second data source is the Italian Labour Force Survey (LFS, *Rilevazione trimestrale sulle forze di lavoro*) administered by ISTAT for the period 2009–2019. The LFS provides detailed information on individuals' labour market status, educational attainment, and geographic location (province and region of residence at the time of the survey).

We select individuals aged 35 to 65 observed between 2009 and 2019.[6] Given this age restriction, the youngest cohort observed in the data is that born in 1975. To ensure that individuals are observed for a sufficient number of years during the later stages of their labour market career, we also restrict the sample to cohorts born from 1950 onwards. Under these selection criteria, the baseline dataset is a repeated cross-section comprising approximately 2,151,000 observations, of which 13 percent are university graduates and about 52 percent are women. The average age in the sample is 50.5 years. In terms of labour market status, 64 percent of individuals are employed, while 31 percent are inactive.

A limitation of the LFS is that we observe individuals' province of *current residence* rather than province of birth. If individuals who benefited from a local university subsequently moved to a different province, treatment assignment is measured with error. Using microdata from the 2001 Population Census, we document that 76 percent of individuals aged 25–54 reside in their province of birth, with this share rising to 87 percent in the South. Since this type of measurement error attenuates the estimated treatment effect toward zero, our estimates should be interpreted as a lower bound of the true effect. We

[5]The terms university/college and faculty/school may have different meanings across countries and education systems. Throughout the paper, we use college and university interchangeably to indicate a single administrative unit offering degree programs in multiple fields through different faculties or schools (e.g. economics, humanities, engineering). We also use faculty and school interchangeably to denote individual teaching units within a university. Typically, each faculty offers one or more degree programs in a specific field. We focus on institutions offering undergraduate education. Institutions specialized exclusively in postgraduate education or targeting foreign students are excluded, as proximity to a university is likely to matter primarily for undergraduate students, who are less geographically mobile.

[6]We exclude individuals aged 25–34 to focus on the long-term implications of tertiary education for labour market outcomes. In Italy, the average age at university graduation was close to 28 until recently, implying that early labour market experiences are often unstable and less representative of the core working career, especially for graduates.

provide a detailed analysis of this issue, including formal bounds following Manski (1990), in Section 6. Moreover, the most plausible form of selective migration, graduates from peripheral treated provinces relocating to larger cities in search of better employment opportunities, would itself generate attenuation bias, since these individuals are reclassified as controls in the destination province. This reinforces the lower-bound interpretation of our estimates rather than threatening their validity.

The combined LFS–HIU dataset. We merge LFS microdata with the HIU panel using the individual's province of residence. This allows us to reconstruct the local history of university faculty openings, including the number of faculties operating in a province in a given year, the year of the first university opening, and the individual's age at first exposure to tertiary education.

Figure A-2 in the Appendix shows the average number of faculties available in the province at age 18 by birth cohort. Individuals born in 1950 had on average 3.5 faculties available; this rises to 4 for the 1960 cohort and 5 for the 1970 cohort.

For each individual, we observe whether a faculty was already present by their 19th birthday, the typical age of tertiary education choice in Italy. Cohorts within the same province differ in their age at the time of the first university opening, generating variation in exposure to local tertiary education. How this variation is exploited for causal identification is discussed in Section 4.

Our analysis focuses on provinces that initially lacked a university and uses the timing of the first faculty opening to define the treatment. We restrict attention to individuals aged 8–28 at the time of the first opening. Individuals younger than 19 at the opening are classified as treated, those 19 or older as controls, resulting in a balanced 8–28 age window across cohorts.[7] The relatively wide window captures the gradual effects of establishing a university in previously unserved provinces.

We focus on the extensive margin of university expansion: first faculty openings. Additional faculties in provinces that already hosted a university are excluded, as are provinces that never experience a first opening. This ensures clean identification driven by the timing of initial exposure across cohorts within the same province. Overall, 33 provinces (about 30 percent of the total) contribute to the analysis, yielding a final sample

[7]For example, the 1970 cohort contributes to the event-study from age 8 (1978) to 28 (1998), with treated ages corresponding to <19. Cohorts 1950–1975 are fully observed over this window, ensuring comparable exposure probabilities.

of about 283,000 observations.[8]

Descriptive evidence. Table 1 shows summary statistics for the main variables, overall and by gender. The sample is broadly similar to the LFS population. The balanced 8–28 age window ensures equal representation of treated (51 percent) and control individuals.

Table 1: Summary statistics by gender

	All		Men		Women	
	mean	sd	mean	sd	mean	sd
Age	49.81	7.84	49.72	7.83	49.90	7.84
Female	0.52	0.50	0.00	0.00	1.00	0.00
Graduate	0.12	0.33	0.11	0.32	0.13	0.34
Employed	0.62	0.48	0.75	0.43	0.50	0.50
Inactive	0.33	0.47	0.20	0.40	0.46	0.50
Treated	0.51	0.50	0.51	0.50	0.51	0.50
Obs.	282,729		136,350		146,379	

Source: own elaborations on the LFS-HIU dataset. The variable Treated is missing for 12,102 observations (individuals aged 19 at the opening, used as the normalization group in the event-study, see Section 4).

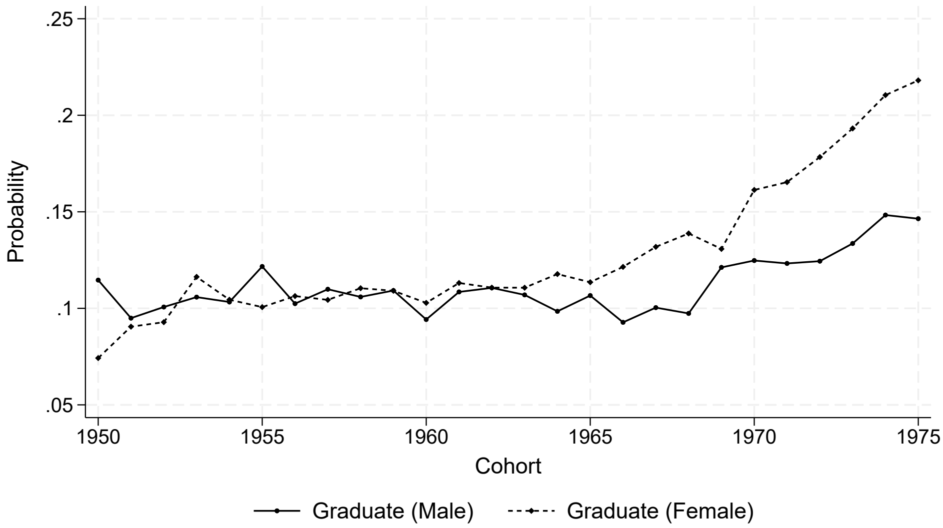
Figure 3 illustrates the graduation probability by cohort and gender. In the peripheral provinces considered, college attainment was initially low (about 10 percent) and similar for men and women. From cohorts born in the mid-1960s, graduation rates rose rapidly, especially among women, who surpassed men by about 2 percentage points.

Table 2 reports means by treatment status and gender. Graduation differences are small. Employment declines with age for all groups and is systematically lower for treated individuals, especially women, while inactivity shows the opposite pattern.

Figure A-1 in the Appendix shows complementary evidence on employment and inactivity rates by educational attainment and gender. In particular, the employment gender gap behaves differently depending on the level of education as individuals age: it tends to decrease with age for individuals with a high-school degree or lower and reaches its lowest point for the 60–65 age group. Conversely, the gender gap increases with age for college-educated individuals, reaching its maximum in the 60–65 cohort. These patterns reflect heterogeneous labour market behaviors at the end of working life across education groups

[8] They are: Alessandria, Ancona, Aosta, Arezzo, Ascoli Piceno, Benevento, Bergamo, Bolzano, Brescia, Campobasso, Catanzaro, Chieti, Como, Cosenza, Foggia, Forlì-Cesena, Frosinone, Isernia, Novara, Pescara, Potenza, Reggio Di Calabria, Reggio Nell'Emilia, Rimini, Siracusa, Taranto, Teramo, Trento, Udine, Varese, Vercelli, Verona, Viterbo.

Figure 3: Graduation probability, by cohort and gender (all cohorts).



Source: own elaborations on the LFS-HIU dataset.

Table 2: Descriptive statistics: Treated vs Control by age group and gender

	All		Men		Women	
	Treated	Control	Treated	Control	Treated	Control
Graduate (all ages)	0.12	0.13	0.11	0.12	0.13	0.14
Employed (all ages)	0.56	0.68	0.70	0.80	0.43	0.57
Age 35–44	0.67	0.78	0.82	0.88	0.53	0.67
Age 45–54	0.65	0.70	0.81	0.83	0.49	0.59
Age 55–59	0.55	0.57	0.67	0.70	0.44	0.45
Age 60–65	0.31	0.40	0.40	0.50	0.24	0.31
Inactive (all ages)	0.39	0.27	0.25	0.14	0.53	0.39
Age 35–44	0.26	0.17	0.11	0.06	0.41	0.28
Age 45–54	0.30	0.24	0.13	0.11	0.46	0.37
Age 55–59	0.42	0.40	0.29	0.25	0.55	0.53
Age 60–65	0.68	0.58	0.59	0.47	0.75	0.68

Source: own elaborations on the LFS-HIU dataset. Means are reported for all variables.

and are consistent with findings that women—even those with college degrees—may exit the labour market earlier than men due to care responsibilities or other constraints (Brilli and Bassoli, 2025; Ciani, 2016; Maura and Profeta, 2025).

A deeper understanding of these descriptive patterns motivates the econometric analysis discussed in the next section.

4 Empirical strategy

To analyse the effect of higher education expansion on individuals' educational and labour market outcomes, we estimate an event-study model of the form:

$$Y_{icat} = \alpha_a + \eta_c + \mu_{ac} + \beta_t + \beta_q + \sum_{\substack{s=8 \\ s \neq 19}}^{28} \gamma_s \cdot \mathbf{1}(s = \text{Year opening}_a - \text{Year birth}_i) + \boldsymbol{X}_{it}\delta + \epsilon_{icat} \quad (1)$$

Here, Y_{icat} denotes the outcome of individual i at time t, from cohort c, residing in province a, and can be either a binary indicator for university graduation or for employment/inactivity. The model includes province fixed effects α_a, while year fixed effects β_t capture shocks common to all provinces in a given year. Since the LFS is collected quarterly, quarter fixed effects β_q absorb within-year seasonality in the rotating sample design. Cohort fixed effects η_c, defined over five-year birth intervals, control for cohort-specific shocks. μ_{ac} is a full set of cohort $\times$ province indicator variables (cohort $\times$ province fixed effects), absorbing all cohort-specific differences within each province. $\boldsymbol{X}_{it}$ is a vector of individual characteristics, including gender, age, and age squared, and ϵ_{icat} is the error term; standard errors are clustered at the province level.[9]

The coefficients of interest, γ_s, measure the effect of exposure to the first university opening at age s, normalized to $\gamma_{19} = 0$, corresponding to the typical age of tertiary education choice. The time window spans ages 8 to 28 relative to the opening, including 11 treated cohorts (ages 8–18 at opening), 9 control cohorts (ages 20–28 at opening), plus the normalization cohort at age 19 (excluded from the sum, $\gamma_{19} \equiv 0$). This structure allows us to examine pre-trends and trace dynamic effects of treatment across the life course. Figure A-3 in the Appendix shows that observations are reasonably balanced across this age window.

[9] age and age squared enters only in the employment and inactivity specifications.

In other words, the empirical strategy exploits the fact that university openings occurred at different points in time, generating variation in age at exposure across cohorts within the same province, while we identify effects exclusively from differences in age at exposure to the first university opening within province and birth cohort.[10] The inclusion of cohort × province fixed effects absorbs all cross-cohort and cross-province heterogeneity, so that the coefficients γ_s are identified by comparing individuals who belong to the same cohort and province but differ only in their age relative to the opening year. As a result, the identifying assumption is that, absent the first faculty opening, outcomes would have evolved smoothly with age within a given province–cohort cell. Under the parallel trends assumption, any divergence in outcomes after exposure is attributable to the causal effect of local university expansion, net of cohort-specific provincial characteristics. To assess the plausibility of this assumption, we examine the evolution of outcomes in the pre-treatment period for treated and control cohorts, allowing for a visual check of parallel pre-trends.

When Y_{icat} is the employment or inactivity indicator, equation (1) estimates intention-to-treat (ITT) effects, capturing the impact of university expansion on labour market outcomes. In this respect, tertiary education can be interpreted as a natural mediator.

To complement the event-study analysis, we also estimate a difference-in-differences (DiD) specification that aggregates the age-specific treatment effects into a single binary treatment indicator, equal to one for individuals younger than 19 at the time of the first faculty opening. While more restrictive, this specification provides a compact summary of the average treatment effect and facilitates comparison across subgroups. We assess the robustness of our results to alternative estimation windows, different control specifications, and potential bias from staggered treatment adoption using the estimator of de Chaisemartin and D'Haultfœuille (2020). These checks are presented in Section 6.

5 Results

In this section, we present causal estimates of the effects of tertiary education expansion on university graduation, and ITT estimates of its association with labour market outcomes. We first assess the impact on tertiary education completion, then examine heterogeneity

[10]While university openings are staggered over time across provinces, identification does not rely on cross-province comparisons. Instead, differences in the timing of university openings generate variation in age at exposure across cohorts within the same province, and identification relies exclusively on comparing individuals of the same birth cohort within a province who differ in their age at first exposure.

by gender and urbanization, and finally turn to labour market outcomes.

5.1 University graduation

Figure 4 presents the estimated event-study coefficients for the probability of obtaining a university degree, by age at which individuals were exposed to the first faculty opening in their province of residence. The pre-treatment coefficients provide a visual check of the parallel trends assumption, while the post-treatment coefficients trace the dynamic effects of exposure across ages. Coefficients prior to exposure are small and statistically indistinguishable from zero, providing no evidence of differential pre-trends and supporting the validity of the identification strategy.

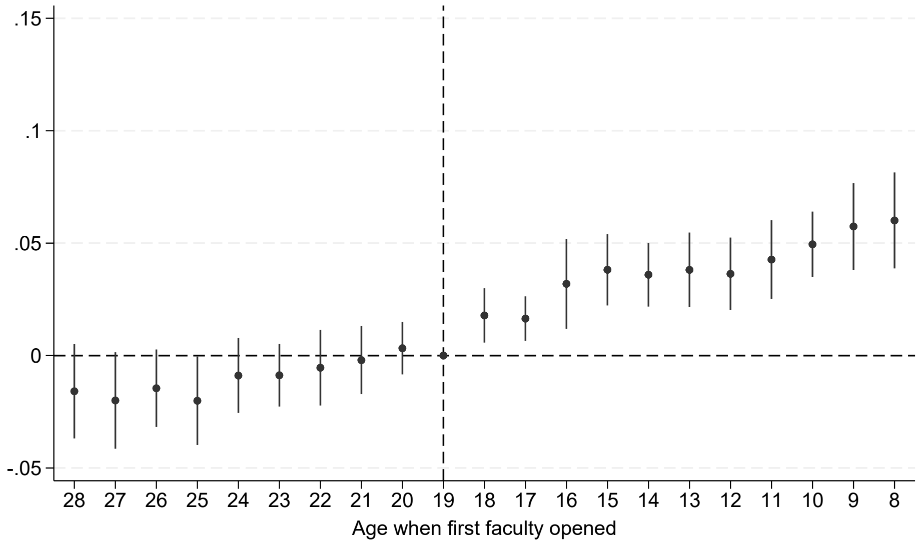
Following exposure, results for the full sample in Panel (a) suggest that the probability of completing tertiary education increases substantially, with statistically significant effects emerging around the typical age of university enrollment. The largest effects—an increase of about 5–6 percentage points in graduation probability—are observed when the first faculty opened while individuals were in the lower part of the age window (8 to 10 years old). This pattern is consistent with the idea that newly opened faculties take time to become fully operational and salient to prospective students, gradually reducing informational and access barriers to higher education. Overall, these results are consistent with the idea that the geographic expansion of universities substantially lowered barriers to tertiary education and translated into higher rates of university graduation for exposed cohorts.

5.2 Heterogeneity by gender

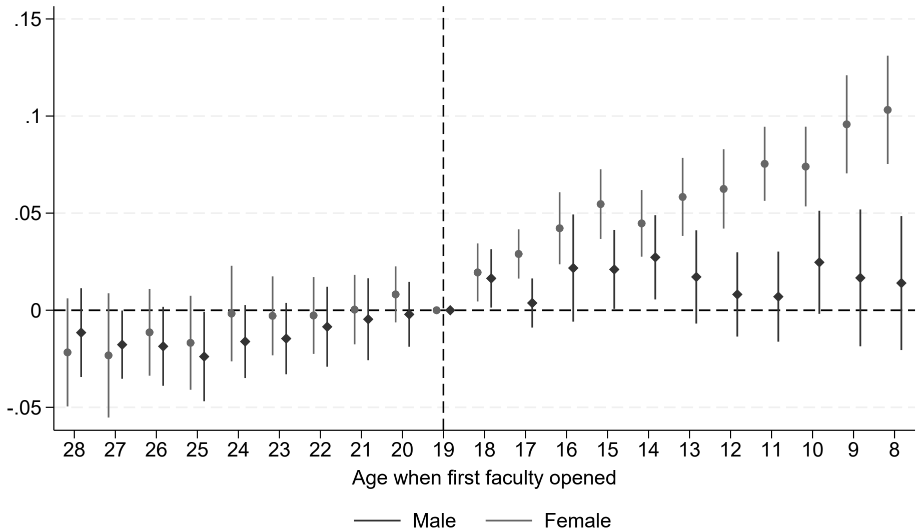
Figure 4, Panel (b) presents the estimated event-study coefficients separately by gender. The increase in university graduation following the opening of new faculties is markedly stronger for women than for men. For women, the effect rises steeply with exposure, with the largest age-specific coefficients approaching 10 percentage points for the youngest female cohorts. For men, the effect is positive but substantially smaller and less precisely estimated throughout the post-treatment window.

This asymmetry is consistent with the hypothesis that geographic barriers to higher education disproportionately affected women. In the Italian context of the 1960s and 1970s, attending a university in a distant city required not only financial resources but also

Figure 4: Event-study: Probability of graduating by age of first faculty opened in the province (full sample and separately by gender)



(a) Full sample.



(b) Women and men separately.

Source: Estimated γ_s coefficients (dots) and their 95% confidence intervals (bars) from equation (1), when Y_{icat} is a dummy for graduating. OLS regression, standard errors clustered at the province level.

a degree of geographic independence that social norms and family expectations made less accessible to young women than to young men. The opening of a local university branch potentially reduced this gender-specific barrier, consistent with the hypothesis that women who otherwise would not have relocated were able to enroll. Boelman (2021) documents a similar pattern in the context of Germany's university expansion, finding that women's lower geographic mobility is a key driver of gender differences in the response to local university availability. The observed gender gradient is consistent with both geographic mobility constraints rooted in social norms and with higher returns to education for marginal female students in this context; our data do not allow us to distinguish between these two channels.

The DiD estimates in Appendix Table A-2 confirm the gender gap quantitatively: the treatment effect on graduation is 0.037 for women ($p < 0.01$) and 0.027 for men ($p < 0.05$), implying that women's gains were roughly 40 percent larger than men's. Overall, these results indicate that the geographic expansion of university supply played an important role in closing—and in some cases reversing—the gender gap in higher education observed across cohorts born between 1950 and 1975.

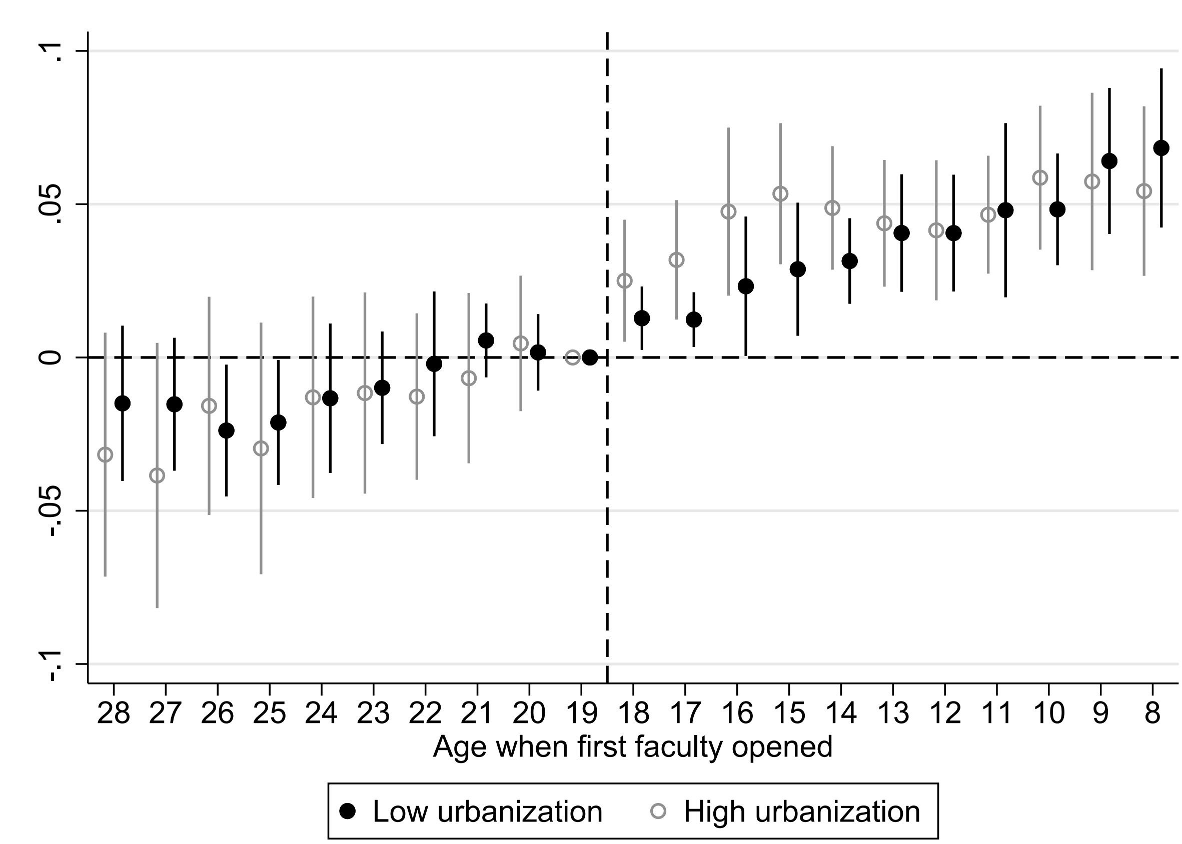
5.3 Heterogeneity by urbanization

A natural question is whether the effect of university expansion varies with local characteristics. We investigate this by estimating the baseline specification separately by tercile of provincial urbanization.[11] Figure 5 shows the event-study coefficients by urbanization level; Appendix Table **??** reports the corresponding DiD estimates. for two cohort windows.

The DiD estimates (Appendix Table A-3) show positive and significant effects across all urbanization terciles, with point estimates rising from 0.018 ($p < 0.05$) in low-urbanization provinces to 0.041 ($p < 0.10$) and 0.045 ($p < 0.05$) in more urbanized areas. The gradient is consistent with the idea that broader and more diversified local labour markets amplify the returns to higher education once a local university becomes available, though the upper-tercile estimates carry wider confidence intervals and the underlying mechanism

[11] Province-level population density is measured as inhabitants per km^2, averaged over the LFS sample period 2009–2019 (source: ISTAT). Low-urbanization provinces: Aosta, Isernia, Potenza, Bolzano, Campobasso, Vercelli, Trento, Foggia, Viterbo, Arezzo, Cosenza. Medium-urbanization provinces: Udine, Alessandria, Benevento, Chieti, Catanzaro, Frosinone, Teramo, Forlì-Cesena, Ascoli Piceno, Reggio di Calabria, Siracusa. High-urbanization provinces: Reggio nell'Emilia, Taranto, Ancona, Pescara, Brescia, Novara, Verona, Rimini, Bergamo, Como, Varese.

Figure 5: Event-study by urbanization level



Source: Estimated γ_s coefficients from equation (1), cohorts born 1950–1975, estimated separately by urbanization tercile (low, medium, high). 95% confidence intervals shown. Standard errors clustered at the province level.

warrants further investigation.

Appendix Table A-4 examines whether the urbanization gradient differs by gender. Two patterns emerge. First, in low-urbanization provinces, only women show a significant effect (0.023, $p < 0.01$), while the male coefficient is small and insignificant (0.013). This is consistent with a cost-of-access mechanism operating primarily through female mobility constraints: in peripheral areas, where geographic barriers were most binding, local university openings disproportionately relaxed the constraints faced by women. Second, in high-urbanization provinces, women again drive the effect (0.062, $p < 0.05$), while men show a smaller and less precisely estimated gain (0.029, $p < 0.10$). This suggests that labour market complementarities also amplify female gains more than male gains, possibly because urban labour markets offer a broader range of graduate-level occupations that are accessible to women once the educational barrier is removed. Taken together, these results indicate that the two mechanisms underlying the urbanization gradient—geographic access and labour market complementarities—operate primarily through the female margin, though the data do not allow us to distinguish between them.

5.4 Labour market outcomes

We then turn to labour outcomes. Full sample event-study results (ITT effects) for employment and inactivity probability are in Figure A-4 in the Appendix. Overall, coefficient estimates are relatively imprecise, but suggest that exposure to a university opening is associated with a higher probability of employment later in life: increased access to tertiary education appears to have positive and persistent effects on labour market attachment, the DiD estimate indicates an average employment gain of 2.1 percentage points; though event-study coefficients are imprecisely estimated. These associations may reflect improved labour market outcomes for graduates, but could also capture general equilibrium effects of the university opening.

Figure 6 Panel (a) reports ITT coefficients for the employment equation; Panel (b) does the same for the probability of inactivity. Gender differences in labour market outcomes tell a more nuanced story than for educational attainment. For employment, the effect is positive and precisely estimated for men but smaller and not statistically significant for women, suggesting that educational gains do not automatically translate into labour market gains for female graduates. The results for inactivity are more precisely estimated for both genders: university expansion significantly reduced inactivity rates, with the effect larger for women (-0.036, $p < 0.01$) than for men (-0.025, $p < 0.01$). This asymmetry is consistent with the view that, despite benefiting more in terms of education, women might face additional barriers in converting their human capital into sustained employment.

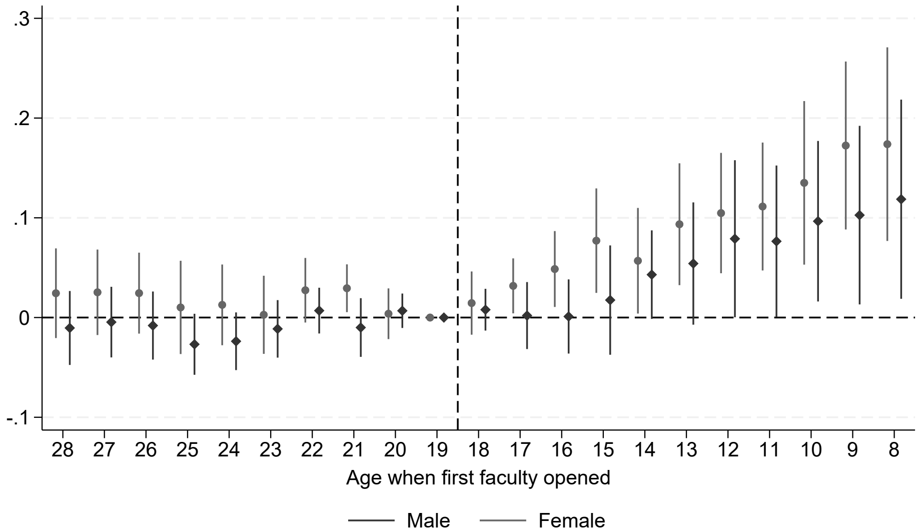
To complement the event-study analysis and provide a more synthetic summary of the estimated effects, we also estimate a standard difference-in-differences (DiD) specification as a robustness exercise. It aggregates the age-specific treatment effects into a single treatment indicator. While this model is more restrictive and does not capture dynamic or age-specific responses to the opening, it provides a useful benchmark.[12] The corresponding estimates are reported in Appendix Table A-2.

This DiD specification uses the same sample and fixed effects structure as the main analysis (cohorts 1950–1975, age at opening [8,,28], cohort-bin $\times$ province fixed effects, $N = 270{,}627$). For the graduation equation, the estimated ATT is 0.032 for the full sample, 0.027 for men ($p < 0.05$), and 0.037 for women ($p < 0.01$), confirming that women's educational gains were roughly 40 percent larger than men's. For the labour market equations, the treatment effect on employment is 0.021 for the full sample ($p < 0.05$), driven primarily by men (0.029, $p < 0.01$) rather than women (0.017, $p > 0.10$). Inactivity declines significantly for both genders (-0.025 for men, -0.036 for women, both $p < 0.01$). Overall, DiD estimates reinforce the main message: university expansion raised educational attainment substantially, especially for women, but the translation into labour market outcomes is incomplete and gender-asymmetric.

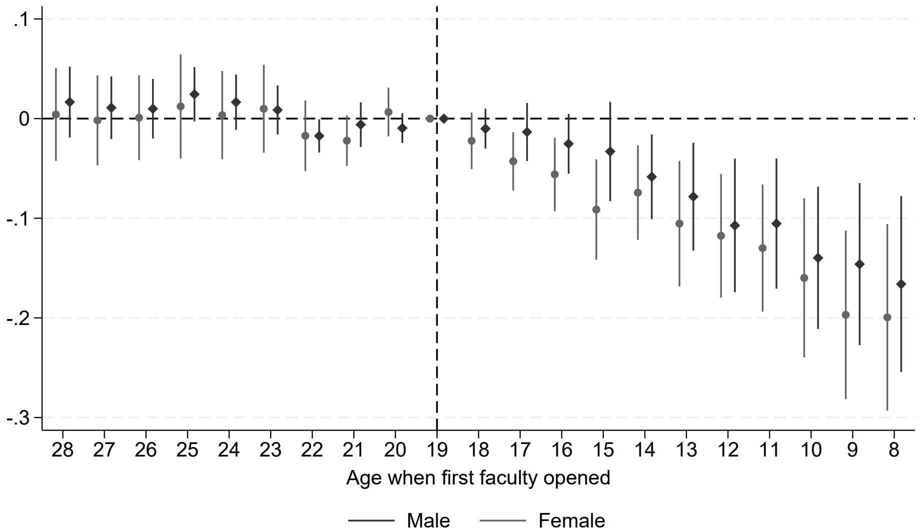
This result is consistent with the descriptive evidence presented in Section 3, showing that highly educated women are more likely than men to exit employment in the years approaching retirement. Institutional features of the labour market, care responsibilities, and retirement incentives may therefore limit the extent to which educational gains translate into sustained employment for women later in life.

[12] In this model, the treatment indicator discussed in Section 3 collapses the age-specific effects captured by the γ_s coefficients in equation (1) into a single binary variable, equal to one for individuals younger than 19 at the time of the first faculty opening and zero for those older than 19, excluding the normalization cohort. Relative to the event-study framework, this specification imposes a more restrictive structure by assuming a homogeneous average treatment effect across treated cohorts.

Figure 6: Event-study: Probability of being employed and inactive by age of first faculty opened in the province (separately for women and men).



(a) Employment.



(b) Inactivity.

Source: Estimated γ_s coefficients (dots) and their 95% confidence intervals (bars) from equation (1), when Y_{icat} is a dummy for being employed or inactive. OLS regression, standard errors clustered at the province level.

6 Robustness

We subject our main results to an extensive battery of robustness checks, addressing concerns related to staggered treatment adoption, pre-trends, specification sensitivity, and the influence of individual provinces.

Staggered treatment bias. A potential concern with two-way fixed effects estimation under staggered treatment adoption is that already-treated units may serve as controls for later-treated ones, leading to biased estimates when treatment effects are heterogeneous over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). We address this concern using two heterogeneity-robust estimators: Callaway and Sant'Anna (2021) (C&S) and de Chaisemartin and D'Haultfœuille (2020) (dCH). Both estimators use only not-yet-treated provinces as controls and are robust to heterogeneous and dynamic treatment effects. The C&S simple ATT is 0.013 (SE = 0.006) and the dCH average cumulative effect is 0.012 (SE = 0.004), both positive and statistically significant, consistent with our TWFE baseline estimate in sign and significance (Table A-5). Pre-treatment placebo tests for both estimators show no evidence of differential pre-trends: the C&S average pre-treatment effect is 0.002 ($p = 0.36$) and the joint test of the dCH placebo coefficients yields $p = 0.096$, supporting the parallel trends assumption.

Randomization inference. With only 33 treated provinces, asymptotic inference may be unreliable even with clustered standard errors. We complement the wild-cluster bootstrap with a randomization inference test: we randomly permute the treatment timing among the 33 treated provinces, re-estimate the baseline specification on each permutation, and build the null distribution of ATTs under the assumption that treatment assignment is arbitrary. Across 500 permutations, zero produce an effect as large as the actual estimate in absolute value, yielding a two-sided is p-value$<$0.002. Appendix Figure A-5 displays the null distribution alongside the actual ATT. This result confirms that the estimated effect is highly unlikely to arise from chance variation in the assignment of university opening years across provinces.

Leave-one-out. To verify that our results are not driven by any single province, we re-estimate the baseline specification dropping one province at a time. Appendix Figure A-6 reports the 33 estimated coefficients, ranked from smallest to largest, together with their 95% confidence intervals. The estimates range from 0.026 to 0.036, with the full-sample estimate of 0.032 lying near the centre of the distribution. All leave-one-out estimates remain positive and statistically significant. Appendix Table A-8 provides summary statistics for the leave-one-out distribution.

Specification sensitivity. We assess the sensitivity of our results to several features of the empirical specification. First, we vary the estimation window from the baseline [8, 28] to narrower windows ([10, 26], [12, 24], [14, 22]). As shown in Appendix Table A-9, the estimated treatment effect is virtually unchanged across all windows (0.0323–0.0324), indicating that the result is not driven by observations at the tails of the age distribution. Second, we consider alternative specifications of cohort × province controls. Appendix Table A-10 shows that replacing cohort × province fixed effects with region-by-cohort fixed effects yields a coefficient of 0.019 ($p < 0.05$), while removing cohort × province controls entirely gives 0.019. These results confirm that our findings are robust to the particular granularity of the fixed effects structure, though the most demanding specification (cohort × province) provides the most precise estimates.

Inference with few clusters. With 33 provinces contributing to identification, asymptotic cluster-robust standard errors may be less reliable (Cameron et al., 2008). We address this concern by computing wild-cluster bootstrap p-values with Rademacher weights and 999 replications (Roodman et al., 2019). The bootstrap p-value for the full-sample baseline estimate is 0.009, well below the 1% level (95% bootstrap CI: [0.012, 0.056]). For the gender subgroups, bootstrap p-values are 0.011 for women and 0.025 for men. The wild-cluster bootstrap therefore corroborates asymptotic inference and allays concerns about few-cluster bias in both the full-sample and subgroup estimates.

Age cutoff sensitivity. Our baseline specification defines treatment as having a university available by age 18. Appendix Figure A-7 and Table A-12 examine the sensitivity of the DiD coefficient to this choice by varying the age cutoff from 16 to 21. The coefficient is large and statistically significant for cutoffs at age 18 or below, ranging from 0.025 to 0.032. At age 19—one year past the typical university enrollment age in Italy—the coefficient drops sharply to 0.012 and becomes statistically insignificant. This discontinuity at exactly the enrollment-relevant age provides strong support for the causal interpretation: the effect operates through university enrollment at the appropriate age rather than through confounding trends or selective migration patterns that would not exhibit such a sharp age-specific threshold.

Treatment intensity. If the effect operates through local access, provinces where more faculties opened should exhibit stronger effects. We test this by replacing the binary treatment indicator with the number of faculties available in the province when the individual turned 19, set to zero for control groups and untreated cohorts. Appendix Table A-7 reports the results by gender. Panel B shows that each additional faculty is associated with a 1.1 percentage point increase in graduation probability for the full sample, with the effect nearly twice as large for women (1.5 pp) as for men (0.7 pp). Panel A (dose bins) reveals a monotonically increasing pattern: across all bins, women's coefficients are consistently larger than men's, ranging from 2.7 pp (one faculty) to 5.7 pp (three or more faculties) for women, compared to 2.7 pp and 3.3 pp for men. While pairwise differences across bins are not individually significant—reflecting the limited number of treated provinces—the gender gradient in the dose-response is consistent with the interpretation that geographic access barriers weigh more heavily on women: as more programs become available locally, the gains from removing distance constraints are amplified for those who face stronger mobility constraints.

Cohort window sensitivity. Appendix Table A-6 examines how the main results change as the cohort window used for the analysis is progressively extended from considering individuals born until 1965 to 1975. Panel A shows that the ATT is remarkably stable at approximately 0.018 for windows 1950–1965 and 1950–1970, then jumps to 0.032 when cohorts born 1970–1975 are included. These cohorts are the youngest in the treated sample: on the one hand, they were exposed for a longer period as treated to the openings that also affected older cohorts (likely in primary or middle school when their province's university opened, meaning the institution was available throughout their entire secondary schooling trajectory); on the other hand, they experienced more openings as compared to individuals born, say, in 1965 or 1070. This maximum-exposure profile is consistent with stronger responsiveness among those who could plan their educational investments from an early age with full knowledge of local university availability and where this availability is higher.

Residence misclassification. A potential concern is that our data record province of current residence rather than province of birth, introducing measurement error in the treatment assignment. If individuals who benefited from a local university opening subsequently moved to a different province, the observed effect would understate the true causal impact. We address this using 2001 Census data on inter-provincial mobility for

Italian citizens born between 1947 and 1976.[13] Table A-11 reports the results. Panel A shows that 76% of the full sample and 87% of Southern residents still live in their birth province. Graduates are more mobile than non-graduates (71% vs. 77% residing in their birth province), which is the relevant margin for attenuation bias. Panel B presents three sets of bounds. Under random misclassification, the corrected estimate is $\hat{\beta}/\pi = 0.043$; using the graduate-specific mobility rate yields $\hat{\beta}/\pi_g = 0.046$. These are modestly larger than the observed effect of 0.032, suggesting attenuation of approximately 30–40%.[14] The worst-case Manski bounds without additional assumptions are wide (Panel B), but imposing the monotonicity restriction that university openings weakly increase graduation—supported by our event-study evidence—tightens the lower bound to zero (Panel C). Taken together, these results indicate that our baseline estimates are, if anything, conservative, and the true effect likely lies in the range $[0.032, 0.046]$.

7 Spatial Analysis

The individual-level analysis presented in Section 5 establishes that university openings increased graduation rates in the provinces where new institutions were established. Two natural extensions follow: whether these effects spilled over to neighboring provinces, and whether the expansion contributed to a convergence in human capital across Italian provinces. We address both questions using province-level data, collapsing individual graduation outcomes into province-by-cohort-group cells.[15]

Spillover effects. If proximity to a university reduces the cost of attendance, provinces adjacent to those receiving new institutions may also benefit. We exploit a province adjacency matrix to distinguish three groups: provinces that received their own university post-1968 ("got own"), never provinces whose neighbor received one ("neighbor got"), and

[13]The Census records both municipality of residence and municipality of birth, allowing us to compute the share of individuals residing in their birth province by cohort and education level.However, the Census records province of birth as a relative indicator with respect to current residence, and birth year is available only in five-year age classes rather than exact years. These limitations preclude computing the age-at-opening variable that underlies our identification strategy. We therefore use the Census to calibrate the attenuation correction rather than to estimate the treatment effect directly.

[14]This correction follows Manski (1990). The key insight is that measurement error in a binary treatment variable attenuates the OLS coefficient toward zero, so the observed effect is a lower bound under the assumption that misclassification is non-differential.

[15]We define five cohort groups of approximately equal size: 1947–1952, 1953–1958, 1959–1964, 1965–1970, and 1971–1976. The first three are "pre-treatment" (born before the expansion could have affected university enrollment decisions), while the last two are "post-treatment."

never provinces with no treated neighbors.[16] Table A-13 estimates direct and spillover effects simultaneously. In the preferred specification with province and cohort fixed effects (column 3), the direct effect is positive and marginally significant (0.013, $p < 0.10$), while the spillover coefficient is close to zero and statistically insignificant (-0.004). Figure A-8 illustrates this pattern: provinces that received their own university exhibit a clear upward break in graduation rates for post-treatment cohorts, while never provinces—regardless of whether their neighbors were treated—follow virtually identical trajectories.[17] This null result supports the interpretation that the effect of university openings operates through local access rather than broader regional diffusion.

Convergence in human capital. Italy provides a particularly informative setting for studying educational convergence, given its well-documented geographical divide in human capital accumulation and local labour market outcomes (Paci and Pigliaru, 1997; Dalmazzo and De Blasio, 2007). We compare graduation trends in late-expansion provinces (first university after 1968) with never provinces using a difference-in-differences framework. Table A-14 reports the results. The interaction between late-expansion status and post-1968 cohorts yields a coefficient of 0.012, significant at the 5% level with province and cohort fixed effects (column 3). This estimate is remarkably close to the individual-level baseline, providing province-level corroboration of the micro-econometric results.[18] In the South, the point estimate is slightly larger (0.014) but loses statistical significance due to the smaller number of provinces (columns 5–6). Figure A-9 visualizes these dynamics by plotting the graduation gap of each province type relative to historical university provinces (those with a university before 1945). Early-expansion provinces nearly close the gap; late-expansion provinces narrow it substantially; never provinces fall further behind.[19] These findings are suggestive but indicate that the university expansion contributed to a partial convergence in educational attainment, with the largest gains in previously underserved areas.

[16] The adjacency matrix is based on shared provincial borders as defined by ISTAT administrative boundaries. A province is classified as having a "treated neighbor" if at least one contiguous province received its first university after 1968.

[17] The absence of spillovers is also relevant for the SUTVA assumption: if the treatment in one province does not affect outcomes in neighboring provinces, interference across units is unlikely to bias our individual-level estimates.

[18] The concordance is reassuring, as the two approaches differ in unit of analysis, fixed effect structure, and identifying variation. The province-level approach compares only late-expansion versus never provinces, while the individual-level event-study exploits the full staggered timing.

[19] Population weights (column 4) yield a virtually identical coefficient (0.012), ruling out compositional effects from differential population growth across province types.

8 Conclusion

We investigate the impact of Italy's university expansion, specifically the opening of a first faculty in provinces that had never hosted a university, on educational attainment and labour market outcomes. Exploiting variation in the timing of first university openings across provinces in an event-study framework, we provide causal evidence that a first local university substantially raised the probability of obtaining a university degree. The event-study estimate at the enrollment-age threshold is 1.2 percentage points; aggregating across all treated cohorts, the difference-in-differences ATT is 3.2 percentage points (approximately 26% relative to the pre-treatment mean). Both are robust to a comprehensive battery of specification checks, alternative estimators, and randomization inference. Randomly permuting the treatment timing among the 25 treated provinces yields an ATT as large as the observed one in zero out of 500 draws (two-sided $p = 0.002$), providing strong evidence that the estimated effect is not attributable to chance variation in the timing of university openings.

Four sets of findings merit emphasis. First, the effect exhibits a significant geographic gradient: it is positive and significant across all urbanization terciles and increasing in more urbanized provinces, consistent with complementarities between university access and local labour market conditions. Dose-response analysis is consistent with both the access and labor market complementarity channels: replacing the binary treatment indicator with the number of faculties available by age 18 yields a monotonically increasing pattern—the effect rises from 2.7 percentage points in provinces with one faculty to 4.5 percentage points in provinces with three or more. The sharp drop in the effect for cohorts above typical enrollment age (19 and older) provides further support for the causal interpretation. Second, heterogeneity by gender reveals that women benefited disproportionately from local university openings, consistent with the hypothesis that geographic barriers weigh more heavily on women due to social norms and family constraints governing female mobility. The dose-response gradient is also stronger for women: each additional faculty is associated with a 1.5 percentage point increase in female graduation probability, compared to 0.7 for men, consistent with the interpretation that mobility constraints are more binding for women not only at the extensive margin of access but also along the intensive margin of breadth of supply.

Yet, despite narrowing the gender gap in education, increased access did not fully translate into comparable labour market gains. Third, the treatment effect varies systematically across cohort windows. For cohorts born before 1971 the effect is stable at approximately 1.8 percentage points, with the impact concentrated in less urbanized provinces, consistent with a cost-of-access mechanism. When the youngest cohorts (born 1973–1975) are included, the overall ATT rises to 3.2 percentage points and the urbanization gradient reverses, reflecting the stronger response of cohorts who had full exposure to local universities throughout their secondary schooling. Fourth, at the province level, the expansion contributed to a partial convergence in human capital across Italian regions. Late-expansion provinces substantially narrowed their graduation gap relative to historical university towns, while provinces that never received a university fell further behind. No spillover effects are detected in neighboring provinces, indicating that the benefits of university openings are strictly local.

These findings contribute to an active policy debate on the role of geographic access in shaping educational attainment and its long-run labour market consequences. The causal evidence that establishing a first university in previously unserved provinces raised graduation rates meaningfully — with effects larger for women, consistent with geographic barriers being more binding where mobility constraints were strongest — suggests that place-based investment in higher education can be an effective tool for expanding access in underserved areas. The weaker translation of women's educational gains into labour market outcomes signals that access expansion alone is insufficient: complementary policies addressing occupational segregation and care responsibilities remain important.

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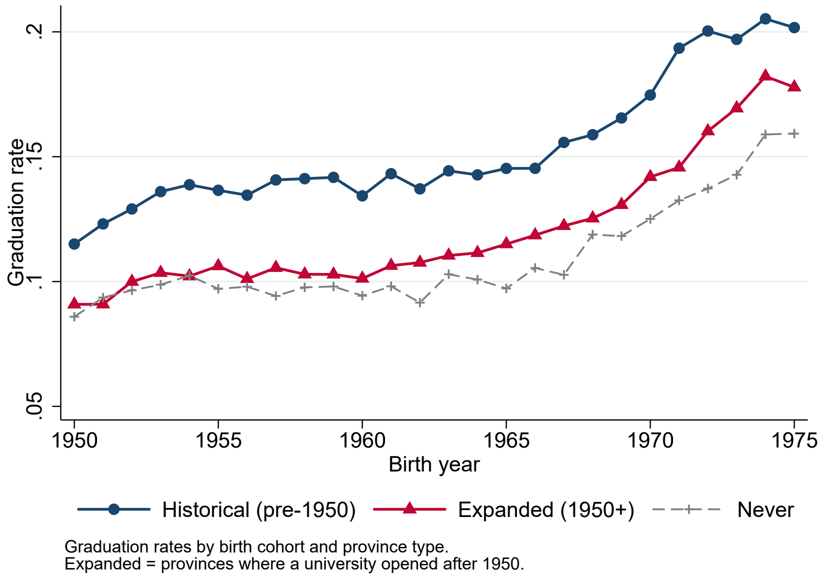
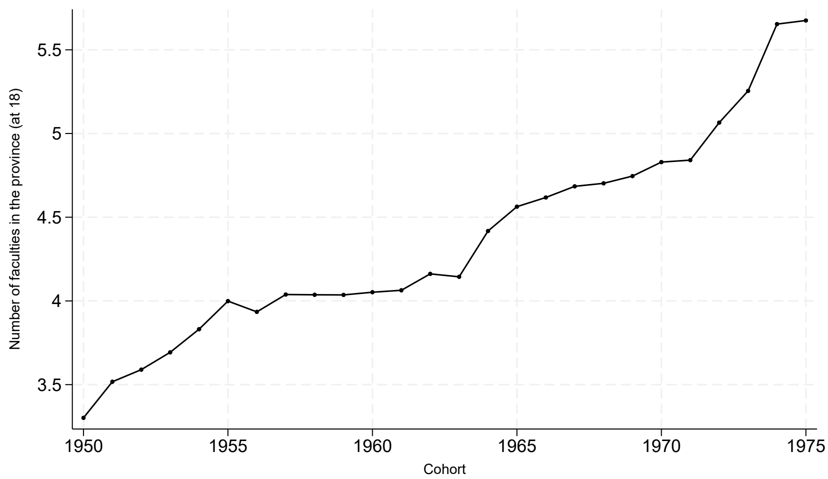
Appendix

Figure A-1: Graduation rates by birth cohort and province type



Source: Own elaborations on the LFS-HIU dataset. Historical: provinces with a university before 1950. Expanded: provinces where the first university opened after 1950. Never: provinces that never received a university by 2010. Graduation rates are computed for five-year birth cohorts.

Figure A-2: Number of faculties by province and cohort



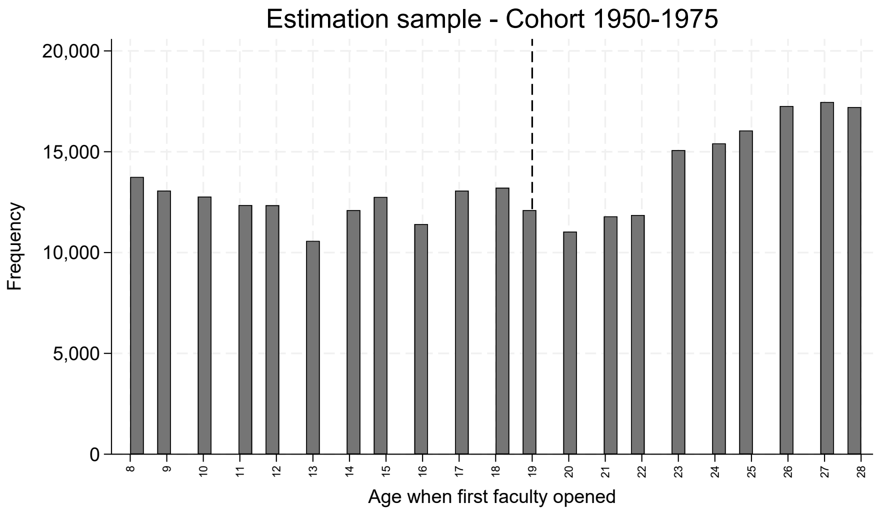
Source: Own elaborations based on the combined LFS-HIU dataset.

Table A-1: Descriptive statistics: Graduated vs Not graduated by age group and gender

	All		Men		Women	
	Graduate	No grad.	Graduate	No grad.	Graduate	No grad.
Employed (all ages)	0.85	0.59	0.91	0.73	0.81	0.45
Age 35–44	0.87	0.70	0.93	0.85	0.83	0.56
Age 45–54	0.90	0.65	0.95	0.81	0.85	0.50
Age 55–59	0.88	0.52	0.93	0.66	0.84	0.40
Age 60–65	0.66	0.30	0.75	0.38	0.56	0.22
Inactive (all ages)	0.12	0.36	0.07	0.21	0.17	0.50
Age 35–44	0.09	0.23	0.04	0.09	0.13	0.38
Age 45–54	0.08	0.29	0.03	0.13	0.12	0.45
Age 55–59	0.11	0.45	0.06	0.30	0.15	0.59
Age 60–65	0.34	0.69	0.24	0.59	0.43	0.77

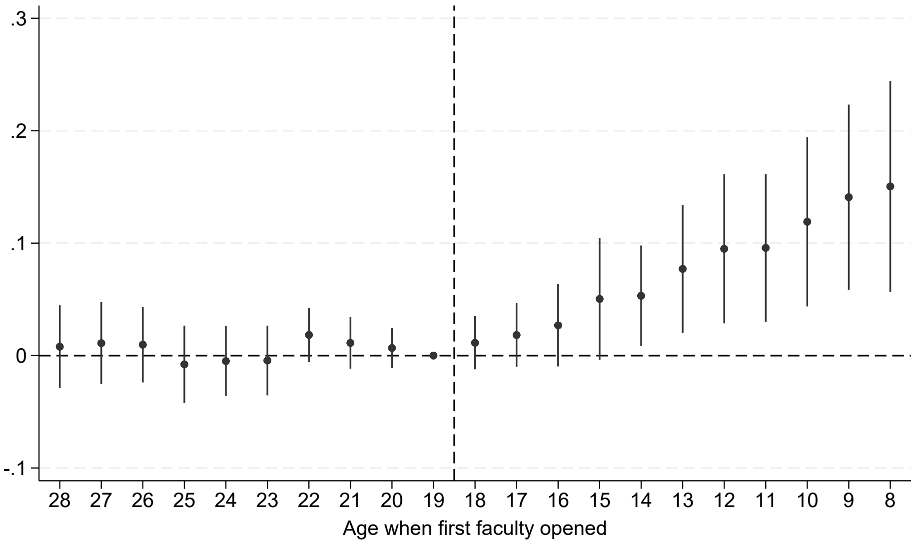
Source: own elaborations on the LFS-HIU dataset. The values are the means of the corresponding variables.

Figure A-3: Distribution of observations by age at which the first faculty opened in the province.

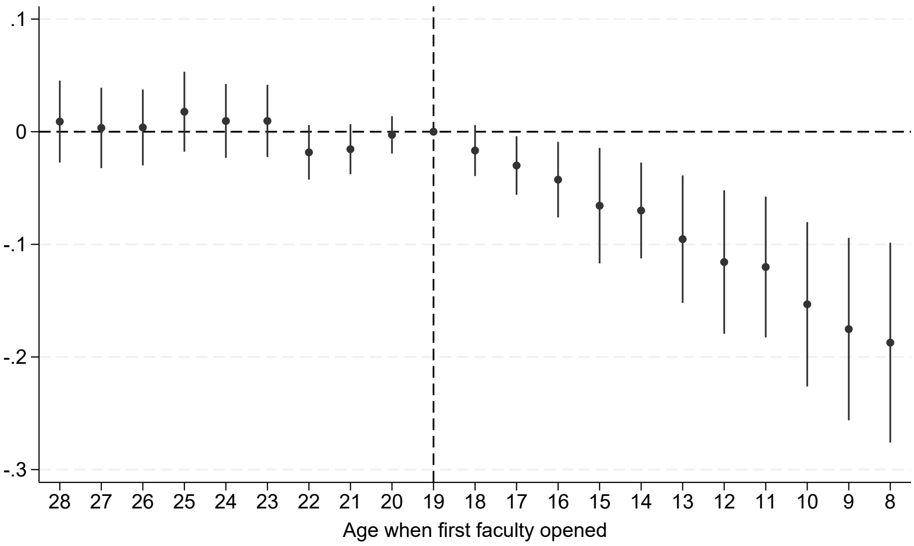


Source: Own calculations using the LFS-HIU dataset. The observations aged 8-18 are treated; from 19 to 28 are the controls. Cohorts born between 1950 and 1970 (included).

Figure A-4: Event-study: Probability of being employed and inactive by age of first faculty opened in the province (full sample)



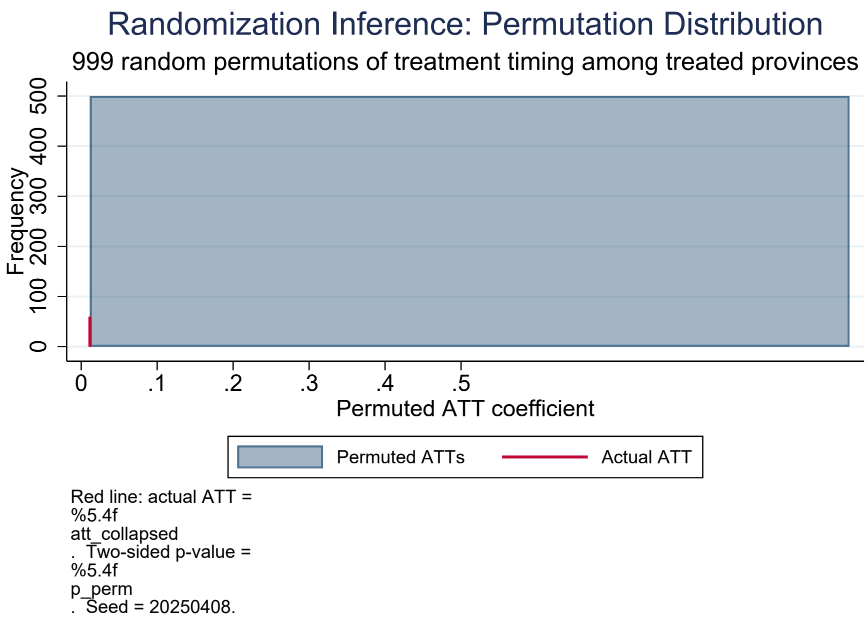
(a) Employment.



(b) Inactivity.

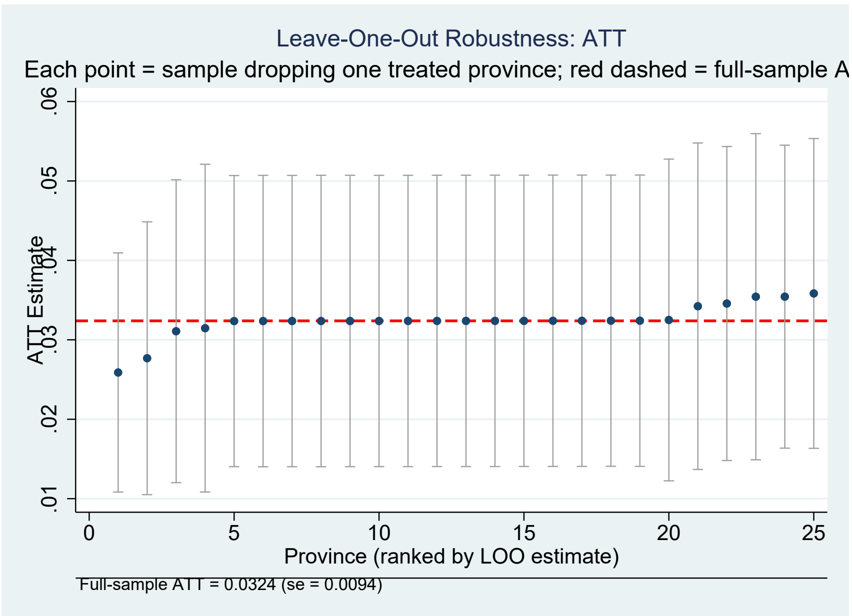
Source: Estimated γ_s coefficients (dots) and their 95% confidence intervals (bars) from equation (1), when Y_{icat} is a dummy for being employed or inactive. OLS regression, standard errors clustered at the province level.

Figure A-5: Randomization inference: permutation distribution of ATT



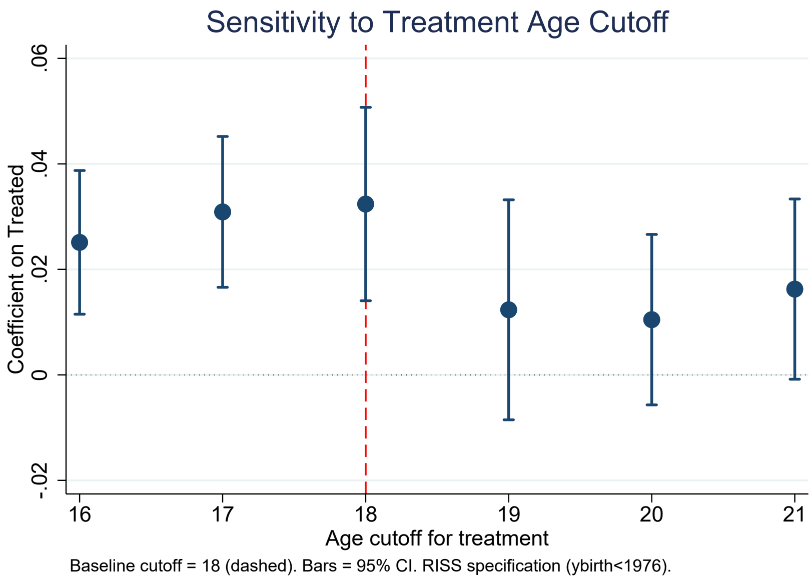
Notes: Distribution of ATT estimates from 500 random permutations of treatment timing among the 33 treated provinces. Red vertical line: actual ATT (province-cohort panel). Zero permutations yield an effect as large as the actual estimate; two-sided p-value = 0.002.

Figure A-6: Leave-one-out: treated coefficient



Source: Each point represents the coefficient on Treated from the baseline DiD specification estimated excluding one province. Dashed red line: full sample estimate. Vertical bars: 95% confidence intervals. Standard errors clustered at the province level.

Figure A-7: DiD coefficient by age cutoff defining treatment



Source: Each point is the coefficient on Treated from the baseline DiD, varying the age cutoff from 16 to 21. Vertical bars: 95% confidence intervals. SE clustered at the province level.

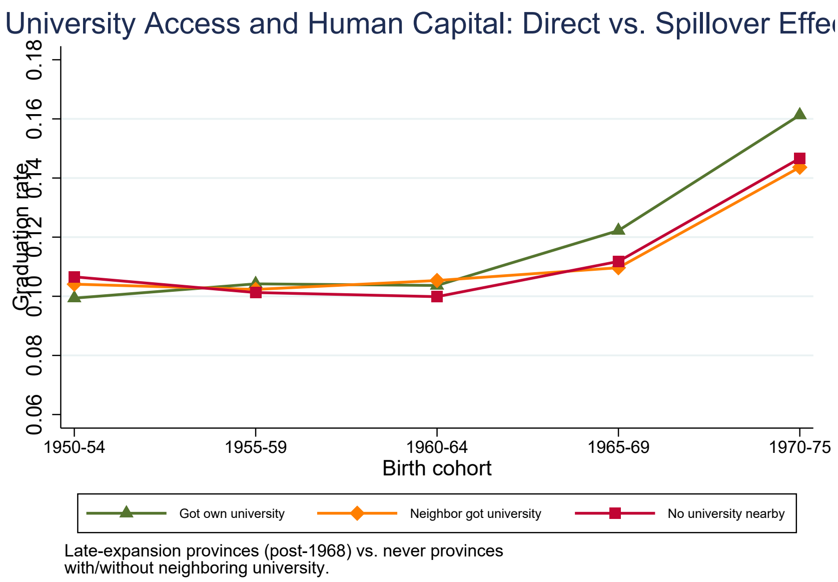


Figure A-8: Graduation rates by province type and birth cohort. "Got own university": late-expansion provinces (post-1968). "Neighbor got university": never provinces with at least one treated neighbor. "No university nearby": never provinces with no treated neighbor. The three groups exhibit similar graduation rates in pre-treatment cohorts; only provinces that received their own university show a differential increase after 1965.

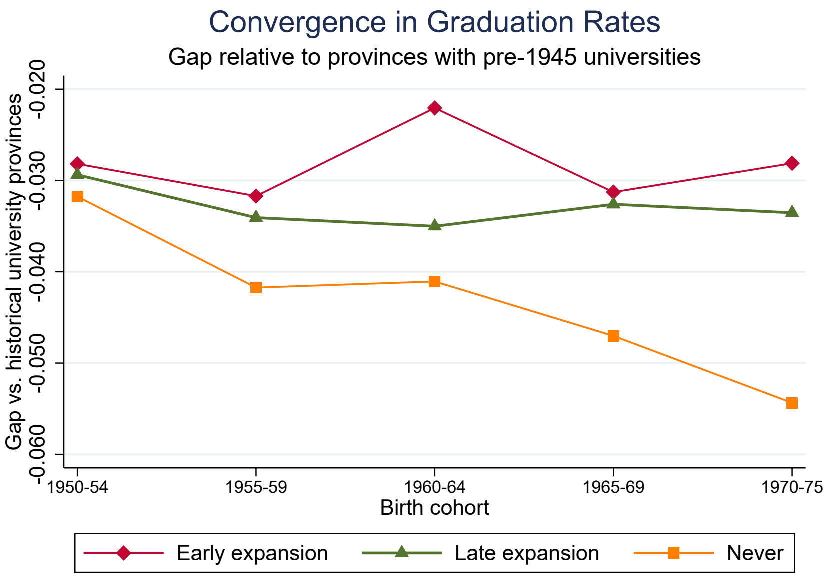


Figure A-9: Graduation rate gap relative to historical university provinces (with a university before 1945), by province type and birth cohort. Early-expansion provinces nearly close the gap by the final cohort group; late-expansion provinces narrow it substantially; never provinces fall further behind. The divergence between late-expansion and never provinces for post-1965 cohorts is consistent with the causal effect documented in Table A-14.

Table A-2: DiD model, treatment effects for graduate, employment, and inactivity equations

	All	Men	Women
Treated (Graduate)	0.0324***	0.0272**	0.0372***
	(0.0094)	(0.0113)	(0.0113)
Treated (Employed)	0.0211**	0.0288***	0.0169
	(0.0086)	(0.0072)	(0.0139)
Treated (Inactive)	−0.0294***	−0.0250***	−0.0362***
	(0.0080)	(0.0064)	(0.0113)
Observations	270,627	130,454	140,173
Cohort × Prov FE	Yes	Yes	Yes
Mean dep. var. (Graduate)	0.125	0.114	0.135
Mean dep. var. (Employed)	0.622	0.751	0.501
Mean dep. var. (Inactive)	0.333	0.198	0.458

Notes: Treated = 1 if individual was aged ≤ 18 when the first faculty opened in their province; age 19 at opening (normalization cohort) excluded. Birth cohorts 1950–1975, age at opening in [8, 28]. All specifications include province, cohort-bin, year, and quarter fixed effects, cohort-bin × province fixed effects, and gender (All column). Standard errors clustered at the province level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A-3: DiD estimates by urbanization tercile

	(1) Low	(2) Medium	(3) High
Treated	0.018**	0.041*	0.045**
	(0.006)	(0.020)	(0.019)
Observations	134,132	77,375	59,120
Cohort × Prov FE	Yes	Yes	Yes
Mean dep. var.	0.126	0.121	0.125

Notes: Urbanization terciles defined at province level. All specifications include province, cohort-bin, year, and quarter FE, cohort-bin × province FE, and gender. SE clustered at the province level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A-4: DiD estimates by gender and urbanization tercile

	Urbanization tercile		
	Low	Medium	High
Panel A: Men			
Treated	0.0127	0.0491	0.0288*
	(0.0127)	(0.0299)	(0.0156)
Observations	65,108	36,789	28,557
Mean dep. var.	0.117	0.107	0.116
Panel B: Women			
Treated	0.0234***	0.0340	0.0615**
	(0.0025)	(0.0212)	(0.0272)
Observations	69,024	40,586	30,563
Mean dep. var.	0.136	0.133	0.134
Cohort × Prov FE	Yes	Yes	Yes

Notes: Treated = 1 if aged ≤ 18 at university opening in the province; age 19 excluded. Birth cohorts 1950–1975, age at opening $[8, 28]$. All specifications include province, cohort-bin, year, and quarter FE, cohort-bin × province FE. Urbanization terciles defined at province level. SE clustered at the province level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A-5: Robustness to Staggered Treatment: Heterogeneity-Robust Estimators

	Callaway & Sant'Anna (2021)	de Chaisemartin & D'H. (2020)
ATT (graduate)	0.013**	0.012***
	(0.006)	(0.004)
Pre-trend test (p-value)	0.360	0.096
Provinces	33	33
Cells	377	377

Notes: Both estimators use not-yet-treated provinces as the control group. Callaway & Sant'Anna (2021): simple ATT aggregated across groups and periods (`csdid`, `notyet` option). De Chaisemartin &D'Haultfoeuille (2020): average cumulative effect per treatment unit(`did_multiplegt_dyn`), weighted by cell size, 8 post-treatment periods. Pre-trend test: C&S average pre-treatment ATT (p-value from t-test); dCH joint test of 4 placebo coefficients. Outcome: graduate degree. Standard errors clustered at province level. TWFE baseline estimate: 0.032 (SE = 0.007).

Table A-6: Sensitivity to cohort window: ATT, distance gradient, and urbanization gradient

	Cohort window		
	(1) yb≤1965	(2) yb≤1970	(3) yb≤1975
Treated (ATT)	0.0179**	0.0178**	0.0324***
	(0.0066)	(0.0066)	(0.0094)
Observations	143,421	203,096	270,627
Cohort × Prov FE	Yes	Yes	Yes
Mean dep. var.	0.106	0.111	0.125

Each column restricts to the indicated cohort window.
Panels B and C restrict to treated provinces.
All specifications include province FE, cohort×province FE, year FE, quarter FE, and gender dummy.
SE clustered by province. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-7: Dose-response: treatment intensity by number of faculties, by gender

Panel A: Dose bins (reference = 0 faculties by age 18)

	(1) All	(2) Women	(3) Men
1 faculty	0.0270**	0.0271**	0.0270
	(0.0121)	(0.0114)	(0.0182)
2 faculties	0.0313***	0.0406**	0.0214**
	(0.0114)	(0.0190)	(0.0100)
3+ faculties	0.0453***	0.0565***	0.0327**
	(0.0103)	(0.0161)	(0.0140)
Observations	270,627	140,173	130,454
Cohort × Prov FE	Yes	Yes	Yes
Mean dep. var.	0.125	0.135	0.114

Panel B: Continuous treatment intensity (number of faculties)

	(1) All	(2) Women	(3) Men
N faculties (count)	0.0114***	0.0154***	0.0072*
	(0.0038)	(0.0046)	(0.0038)
Observations	282,729	146,379	136,350
Cohort × Prov FE	Yes	Yes	Yes
Mean dep. var.	0.124	0.134	0.114

Outcome: probability of university graduation.
Treatment intensity = number of faculties in province when individual turned 18, set to zero for control groups and untreated cohorts (age_fac_open > 18).
All specifications include province, cohort-bin, year, quarter FE, cohort-bin × province FE.
SE clustered by province. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-8: Leave-one-out robustness: summary statistics

	Coefficient on Treated
Full sample	0.0324
	(0.0094)
Leave-one-out statistics	
Mean	0.0319
Minimum	0.0259
Maximum	0.0358
N provinces	25

Each LOO estimate drops one treated province and re-estimates the baseline.
SE clustered by province.

Table A-9: Robustness to estimation window

	(1) [8,28]	(2) [10,26]	(3) [12,24]	(4) [14,22]
Treated (age_fac_open≤18)	0.0324***	0.0324***	0.0323***	0.0324***
	(0.0094)	(0.0094)	(0.0093)	(0.0094)
Observations	270,627	209,123	150,672	97,250
Cohort x Province FE	Yes	Yes	Yes	Yes
Window	[8,28]	[10,26]	[12,24]	[14,22]
Mean dep. var.	0.125	0.123	0.123	0.124

Treat = university open by age 18. Different age windows. SE clustered by province.

Table A-10: Robustness to alternative control specifications

	(1) Baseline	(2) Region x Coh	(3) Minimal
Treated (age_fac_open≤18)	0.0324***	0.0191**	0.0193**
	(0.0094)	(0.0077)	(0.0072)
Observations	270,627	270,627	270,627
Prov-cohort controls	Cohort x Prov	Region x Cohort	None
Mean dep. var.	0.125	0.125	0.125

SE clustered by province. All include province, cohort, year, quarter FE and gender.

Table A-11: Bounds on the Treatment Effect Under Province Misclassification

	Observed effect (lower bound)	Random misclass. $\hat{\beta}/\pi$	Graduate-weighted $\hat{\beta}/\pi_g$	Manski (worst case)
Panel A: Mobility rates (Census 2001, % in birth province)				
Full sample	76.0%	77.4% (non-grad)	71.1% (grad)	
South	86.8%	87.5% (non-grad)	80.3% (grad)	
Panel B: Bounds on treatment effect				
Full sample	0.0324	0.0426	0.0456	$[-0.273, 0.358]$
South	0.0287	0.0331	0.0357	$[-0.119, 0.185]$
Panel C: With monotonicity ($\beta_{true} \geq 0$)				
Full sample	0.0324	0.0426	0.0456	$[0.000, 0.358]$
South	0.0287	0.0331	0.0357	$[0.000, 0.185]$

Notes: Bounds on the true treatment effect under province misclassification (province of residence observed; province of birth required). Mobility rates from the 2001 Census (Italian citizens, cohorts 1947–1976). *Random misclassification*: assumes classical (non-differential) measurement error in the binary treatment; the observed coefficient is attenuated by π, so $\hat{\beta}_{\text{true}} = \hat{\beta}_{\text{obs}}/\pi$, where π is the share of individuals residing in their birth province. *Graduate-weighted*: uses π_g (the graduate-specific retention rate) as the relevant attenuation factor, since misclassification is more likely among graduates. *Manski worst-case bounds*: following Manski (1990), since $Y \in \{0, 1\}$ (binary outcome), the worst-case bounds are $\left[(\hat{\beta}_{\text{obs}} - (1 - \pi))/\pi,\ (\hat{\beta}_{\text{obs}} + (1 - \pi))/\pi\right]$; for the full sample ($\hat{\beta}_{\text{obs}} = 0.0324$, $\pi = 0.760$) this yields $[-0.273, 0.358]$. *Monotonicity*: imposes $\beta_{\text{true}} \geq 0$ (university openings weakly increase graduation), truncating the lower bound at zero.

Table A-12: DiD estimates by age cutoff defining treatment

	(1) Age 16	(2) Age 17	(3) Age 18	(4) Age 19	(5) Age 20	(6) Age 21
treat_cut	0.0251***	0.0309***	0.0324***	0.0123	0.0105	0.0163*
	(0.0069)	(0.0073)	(0.0094)	(0.0106)	(0.0082)	(0.0087)
Observations	269,661	269,517	270,627	271,692	270,931	270,867
Cohort x Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.124	0.124	0.125	0.124	0.124	0.125

Treat = university open by age X (omits age X+1). SE clustered by province.

Table A-13: Direct and Spillover Effects of University Openings

	(1) OLS	(2) South	(3) FE	(4) W-FE	(5) South FE
Got own university	-0.0002	0.0022			
	(0.0041)	(0.0040)			
Neighbor got university	0.0013	0.0002			
	(0.0046)	(0.0046)			
Post (1965+)	0.0267***	0.0267***			
	(0.0074)	(0.0070)			
Own uni × Post	0.0127	0.0127	0.0127*	0.0127	0.0226
	(0.0089)	(0.0085)	(0.0073)	(0.0081)	(0.0142)
Neighbor uni × Post	-0.0039	-0.0039	-0.0039	-0.0016	-0.0058
	(0.0099)	(0.0095)	(0.0084)	(0.0087)	(0.0188)
South		-0.0124***			
		(0.0031)			
Province FE	No	No	Yes	Yes	Yes
Cohort FE	No	No	Yes	Yes	Yes
Observations	290	290	290	290	125
R-squared	0.278	0.315	0.803	0.822	0.731

Notes: Unit of observation: province × cohort group. Reference category: provinces with no university nearby (neither own nor neighbor). Own uni: province received a university post-1968. Neighbor uni: at least one neighboring province received a university post-1968. Post: birth cohorts 1965 and later. Column 5 restricted to Southern provinces. Standard errors in parentheses, clustered by province in columns 3–5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-14: University Openings and Human Capital Convergence

	(1) OLS	(2) South	(3) FE	(4) W-FE	(5) FE South	(6) W-FE South
Late expansion	0.0054*	0.0064**				
	(0.0029)	(0.0028)				
Post (cohorts 1965+)	0.0271***	0.0271***				
	(0.0041)	(0.0038)				
Late expansion × Post	0.0123*	0.0123**	0.0123**	0.0116**	0.0136	0.0113
	(0.0064)	(0.0062)	(0.0049)	(0.0049)	(0.0081)	(0.0086)
South		-0.0163***				
		(0.0027)				
Province FE	No	No	Yes	Yes	Yes	Yes
Cohort FE	No	No	Yes	Yes	Yes	Yes
Observations	350	350	350	350	180	180
R-squared	0.289	0.357	0.808	0.828	0.736	0.760

Notes: Unit of observation: province × cohort group. Sample restricted to late-expansion and never provinces. Late expansion: first university opened after 1968. Post: birth cohorts 1965 and later. Even columns weighted by cell population. Columns 5–6 restricted to Southern provinces (ISTAT code > 59). Standard errors in parentheses, clustered by province in columns 3–6. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.