The Effects of Educational Expansions: Evidence from a Large Enrollment Increase in STEM Majors

Nicola Bianchi*

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Abstract

Increasing access to education may have consequences that go beyond the effects on marginal students induced to enroll. It may change school quality, peer effects, and returns to skill. This paper analyzes these effects by examining a 1961 Italian reform that increased university enrollment in STEM fields by 216 percent. Newly collected data on 27,236 students suggest that the reform decreased learning in STEM fields due to overcrowded universities and negative peer effects. Some students who would have otherwise chosen STEM enrolled in other programs. There is also evidence that the income of STEM students decreased after the enrollment expansion.

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1 Introduction

Many policymakers are considering large-scale reforms to increase the number and diversity of university students. The goals of these policies are to rapidly increase the share of workers with a university education, to promote inclusion in the university, and to raise incomes. One recent example of reforms that aim at increasing access to postsecondary education is the "America's College Promise Act", which promises to grant all US students access to free community college. Another example is the 1999 Chinese reform that more than doubled university enrollment by increasing the size and number of public universities. Sweeping education reforms, however, can have substantial unintended consequences. Reforms that expand university enrollment, for instance, can lead to overcrowded universities, straining resources and reducing the quality of education. When these reforms induce less-well-prepared students to enroll, they can disrupt pedagogical practices and generate negative peer effects in the classroom. In addition, enrollment expansions can drive down the wage of workers with postsecondary degrees by increasing the supply of skilled labor. These negative effects can induce talented students who would otherwise have enrolled in the expanded fields to shift toward other programs, depriving the target area of some of its best learners.

This paper analyzes the role of each of these channels in understanding the effects of an Italian reform that led to a 216 percent increase in enrollment in university STEM (Science, Technology, Engineering, and Mathematics) programs over 8 years. The results suggest that some of the best students who would have otherwise pursued a STEM degree decided to enroll in other programs. Educational expansion, in fact, led to lower human-capital accumulation in STEM majors due to severe congestion of university resources and to higher class heterogeneity. Finally, there is evidence suggesting that the reform decreased the long-run income of students with a STEM education.

Like many recent policies, this reform was intended to increase postsecondary education among students with low rates of university participation, and to increase the share of workers with STEM skills. Compared to other educational expansions, however, the 1961 Italian reform has several advantages for the empirical analysis. First, in any study of a large-scale policy that affects all individuals simultaneously, econometric identification poses the main obstacle. Italy's reform, although massive, made only a specific type of high school graduates eligible for university studies and allowed them to enroll in STEM majors only—not in the "restricted" majors, which included law, medicine, architecture, social sciences (excluding business economics), and the humanities. Thanks to these features, this paper employs a variety of empirical strategies that exploit cross-sectional as well as cross-cohort variation. Second, detailed data from university transcripts make it possible to dig into the education production function of university students and to tease out the effects of resource congestion and class heterogeneity on human-capital accumulation. Third, thanks to the historical context, this paper can analyze the long-run effects of the reform.

Until 1960, a student who graduated from a university-prep high school (hereafter "type A") could enroll in a university in any major. A student who graduated from a high school for industry-sector professionals (type B) could enroll in only a few majors and most often did not enroll in a university at all. In 1961, the Italian government allowed type B graduates to enroll in STEM majors at universities for the first time. Although temporary caps somewhat limited the growth of type B enrollment through 1964, freshman enrollment in STEM programs had increased by 216 percent by 1968. After this, enrollment stabilized substantially. Since type B students were, on average, less prepared for the university and from less advantaged backgrounds, they changed not just the size but also the composition of the student bodies. Throughout the period, the restricted majors remained inaccessible to type B students, a fact that is important for my empirical strategies. Moreover, there is no evidence that students changed how they chose high schools after 1961.

To analyze the reform, I collected and digitized administrative data for the population of 27,236 students who completed high school in Milan—the Italian city with the thickest market for university-type jobs— between 1958 and 1968. Data from high school registries include the grades students receive on the exit exam. University transcripts contain detailed information on each course students enroll in, each exam they take, and the degrees they attain.

To guide the empirical analysis, I lay out a simple model in which enrollment expansion affects human capital and returns through three main channels. First, it potentially decreases the educational resources per student, a proxy for the quality of education. This may occur if resources do not adjust elastically so that higher enrollment induces crowding. The result may be lower human-capital accumulation per unit of education attained. Second, enrollment expansion can also increase the degree of heterogeneity of university students, especially when the incremental students are less well prepared. In the resulting classrooms, learning will be difficult if teaching is less effective with heterogeneous students or if the less-well-prepared students exert negative peer effects. Third, enrollment expansion may depress skill prices by increasing the supply of human capital. The model also predicts that some type A students who would otherwise enroll in STEM majors would be induced to enroll in other programs in order to avoid the negative indirect effects from resource crowding, peers, or skill prices.

Testing the last prediction, the paper shows that the probability of STEM enrollment among type A students decreased by 0.9 percentage points from 1961 to 1964 and by 8.5

percentage points from 1965 to 1968.[1] On the contrary, their probability of enrolling in restricted majors increased by 5.9 percentage points from 1961 to 1964 and by 17.2 percentage points from 1965 to 1968. Moreover, the best prepared type A students were the most likely to switch from STEM to restricted programs, suggesting that their greater aptitude made them able to do well in a wider array of fields (or that potentially they suffered the most from the deterioration of the signal attached to a university STEM degree).

The analysis then focuses on how the reform affected human-capital accumulation in STEM majors. Here, the unit of observation is a student by university course by academic year combination. The university transcripts, in fact, make it possible to measure changes in human capital—the variable of interest—at a very disaggregated level by using the grade students received in each university course.[2] Overall, there is evidence that the educational expansion lowered learning in STEM majors by congesting university resources and by increasing class heterogeneity.

Initially, the paper shows that educational expansion severely crowded resources per student, a measure of quality of education. To assess its effect on human capital, the analysis exploits within-major between-course variation in the congestion of university inputs generated by increased enrollment. In Italian universities, in fact, courses were assigned resources based on the tenure status of the professors, rather than the courses' demand for resources. As a result, two similar courses could have different endowments (specifically, number of teaching assistants). We might then expect that the educational expansion, combined with fixed total resources after the reform, led to more congestion (and, consequently, lower quality of education) in courses that had fewer resources in 1961. The paper estimates how grades changed after 1961 in compulsory STEM courses with high preexisting student–faculty ratios (fewer resources and larger quality decrease), relative to courses in the same major with low student–faculty ratios (more resources and smaller quality decrease). Due to a lower quality of education, grades decreased by 0.09σ from 1961 to 1964 and by 0.08σ from 1965 to 1968 in the average STEM course.[3] These results should be interpreted as a lower bound, if educational expansion caused crowding on dimensions other than the student–faculty ratio.

In addition to crowding university resources, educational expansion dramatically changed

[1] A cohort is a group of students who completed high school in the same year. In this context, the phrase "from 1961 to 1964" refers to the 1961 through 1964 cohorts of type A high school graduates.

[2] It is important to note that, unlike in the US, university tests in Italy were not graded on a curve. Most exams, in fact, were administered orally and students were graded sequentially.

[3] These results could be driven by the inability of junior professors (more likely to teach courses with more crowding) to deal with larger classes after 1961. In Italy, however, tenure was linked to a course. In the same academic year, some professors were assigned "with tenure" to one course and "without tenure" to another course. The estimated effect of crowding on human capital is robust to the inclusion of professor fixed effects.

the composition of students in university STEM programs. To estimate the effect of higher class heterogeneity, the analysis exploits within-major between-course variation in the preparedness of type B students. Some scientific disciplines, in fact, were taught in type B schools (mainly the applied fields), while others were not (math, physics, and the formal sciences). University STEM courses, however, were tailored specifically to type A students, and their syllabus did not change after 1961. The entry of type B students, then, was more likely to generate negative peer effects and disrupt teaching practices in the STEM courses that were not taught in type B schools. For example, engineering students had to take "Technical Drawing" (taught in type B schools) and "Math I" (not taught in type B schools) during their freshman year. Within each STEM major, the paper estimates how grades of type A students changed after 1961 in courses like "Math I," relative to courses like "Technical Drawing." Grades of type A students decreased by 0.07σ from 1961 to 1964 and by 0.09σ from 1965 to 1968 in courses not included in the precollegiate curriculum of type B students.

Finally, the education data are merged with the (former) students' 2005 income information from their tax returns. Since the individuals were between 56 and 67 years old in 2005, the observed income is a long-run outcome. The first set of results analyzes the impact of the reform on the long-run income—adjusted to eliminate age effects—of type B students. Despite a marked increase in the number of university degrees awarded to type B students after 1961 (72 percent increase from 1961 to 1964 and 183 percent from 1965 to 1968), there is little evidence that these students earned positive returns to a university education. This finding is robust to a variety of specifications tests including difference-in-differences, which do not rely solely on cross-cohort variation. The second set of results analyzes the effect of the reform on the age-adjusted long-run income of type A students. Among type A students, the positive income premium associated with a STEM university education, equal to 28 percent before 1961, decreased by 12.6 percentage points from 1965 to 1968. The estimated decline is robust to controlling for the positive selection of type A students out of STEM majors (by restricting the sample to low-achieving students). The model—in combination with the previous estimates— suggests that crowding of university resources could explain 19 percent of the income decline of type A STEM students, while higher class heterogeneity could explain another 17.5 percent. In the absence of other unaccounted effects, the remaining share can be attributed to lower skill prices.

This paper is related to three strands of the literature. First, a few papers study how education policies might have general equilibrium effects on skill prices (Lee, 2005; Lee and Wolpin, 2006; Abbott et al., 2013). Heckman, Lochner and Taber (1998*a*; 1998*b*) build a life-cycle model, in which educational expansion affects skill prices through changes in the relative supply of different types of human capital. In a different context, Duflo (2004) shows how a large school-construction program in Indonesia decreased the wages of older and untreated individuals by increasing the supply of educated workers in the economy. This paper adds to these findings, highlighting how educational expansion can also decrease the returns to education through congestion of university resources and higher class heterogeneity.

Second, a few papers explore how cohort size affects academic outcomes (Stapleton and Young, 1988; Bound and Turner, 2007). Bound and Turner (2007) show that larger cohorts within US states lead to overcrowding of public postsecondary institutions' resources and, therefore, have lower graduation rates. This paper relates to the literature on cohort size by estimating how educational expansion congests university resources. However, by combining a shock to the number of university students with preexisting differences of school inputs across university courses within a major, the analysis in this paper does not rely primarily on cross-cohort variation. This paper identifies the effect of crowding based solely on within-major, within-cohort, between-course variation.

Third, a large literature examines how the composition of a class affects students' outcomes (Figlio and Page, 2002; Hoxby and Weingarth, 2006; Lavy, Paserman and Schlosser, 2012; Anelli and Peri, 2013). Most of these papers exploit natural variations in class composition across the units of observation (usually classes, schools, or neighborhoods) or time, while a few rely on policy changes (Hoxby and Weingarth, 2006; Cooley, 2010) or randomized control trials (Duflo, Dupas and Kremer, 2011). The literature on class composition focuses primarily on precollegiate education, in part because self-selection into different universities or majors makes identification more problematic. This paper, however, is able to estimate the effect of class heterogeneity at the university by exploiting within-major, within-cohort, between-course variation in student preparedness.[4]

The rest of the paper is organized as follows. Section 2 outlines the policy change. Section 3 describes the data. Section 4 presents a model of educational expansion. Section 5 shows how type A students modified their education choices after the reform. Section 6 shows evidence that the educational expansion lowered human capital in STEM majors. Section 7 presents results on long-run income. Section 8 concludes.

[4] The literature on peer effects at the university is mainly based on random housing assignments. For this reason, it identifies the effect of roommates—and not necessarily classmates—on achievement. De Giorgi, Pellizzari and Woolston (2012), however, estimate the effects of class composition at the university in a context with random assignment of students across sections within a course. The identification strategy in this paper is different because it relies on a variation in the student precollegiate preparedness across courses within a major.

2 Institutional Details

2.1 The Italian High School System

Italian high schools offer different diplomas. General education schools (*licei*; type A schools) focus on either the humanities (*licei classici*) or the sciences (*licei scientifici*) and traditionally prepare students for the university. Their curricula range from philosophy, Latin, and Ancient Greek to mathematics and physics (Appendix Table B1). In addition to type A schools, there are technical high schools, which train professionals for specific economic sectors.[5] Technical schools include industrial schools (*istituti industriali*; type B), which prepare students for jobs such as chemists and surveyors, and commercial schools (*istituti commerciali*; type C), which prepare students for jobs in accounting and the service sector. The technical curricula focus on applied disciplines and have a narrower scope than academic-track programs.

At age 14, students choose a high school. They can self-select in different tracks, because admission into public high schools does not depend on past performance and is typically granted to all applicants. On the one hand, type A schools provide a better preparation for most university majors but are characterized by a heavy workload and are academically more challenging. On the other hand, technical schools grant access to well-paid professions that do not require a university degree, but they do so at the expenses of a more general education. As a result, family background is a strong predictor of high school choice. For instance, data from the Bank of Italy's Survey of Household Income and Wealth (SHIW) show that 74.9 percent of technical graduates have a father with 8 or fewer years of completed education, compared with only 53 percent of academic-track students (Appendix Table C1). Similarly, 80.1 percent of technical graduates have a mother with 8 or fewer years of completed education, compared with 66 percent of academic-track students.

2.2 Expanding Access to University STEM Programs

After the end of World War II, enrollment in technical education rose dramatically. In 1950, type B students constituted only 1.5 percent of the high school–age population (14 to 18 years old), while type A students made up 4.1 percent (Appendix Figure A1). In the following 10 years, enrollment in type B schools grew at a much faster rate. By 1960, type B students made up 4.7 percent and type A 5.7 percent of the high school–age population. Despite these changes in the number and composition of high school graduates, access to the university

[5] Technical schools are different from vocational and trade schools, which last only 3 years (instead of 5) and have a narrower scope.

was still regulated by laws enacted during the Fascist regime. Type A graduates could enroll in any university major, while type B could enroll only in business economics and statistics.[6] These restrictions made the Italian university accessible to only a small share of high school graduates. In 1960, 18.6 percent of the high school–age population was still enrolled in high school. However, only 4.1 percent of the university-age population was enrolled in university studies.

In July 1960, a shift of the coalition government towards center-left parties put university reform on the political agenda. In July 1961, law 685 allowed type B students to enroll in university STEM programs for the first time. Specifically, the majors that became accessible to type B students were engineering, mathematics, physics, chemistry, biology, geology, natural science, and agricultural science. The reform applied retroactively: all students with a type B diploma, even if received before 1961, were allowed to enroll in STEM majors. Until 1964, the number of type B students was capped and type B applicants were selected with an admission test. As mandated by the original law, the enrollment caps were eliminated in 1965.

During the first phase, the number of STEM freshmen rose by 35.3 percent from 12,222 students in 1960 to 16,643 in 1964 (Figure 1, Panel A). In terms of share of 19-year-olds, STEM freshmen increased from 1.5 percent in 1960 to 2.4 percent in 1964 (Appendix Figure A2). During the second phase, freshmen increased by an additional 132.1 percent to 38,627 students (4.6 percent of the 19-year-old population) in 1968. Data that categorize freshmen by high school diploma confirm that the increase in STEM freshmen was driven by the entry of type B students. Type B STEM freshmen rose from 2,713 in 1963 to 17,310 in 1968 with an overall 6.4-fold increase. This led to a major compositional change in the student population. In 1964, there were 0.28 type B freshmen for every type A freshman in STEM fields (Figure 1, Panel B). By 1967, type B freshmen had become more numerous than type A. The policy was also associated with an increase in the outflow of STEM students. The number of STEM graduates increased from 5,362 in 1960 to 10,196 in 1968, a 90.2 percent increase.

The reform left many majors accessible only to type A students: law, medicine, the humanities, the social sciences (with the exception of business economics, accessible to type B students before 1961), and architecture. In this period, the number of freshmen in majors with restricted access increased by only 24 percent, from 20,382 students in 1960 to 25,280 in 1964. By 1968, it increased by 64.3 percent to 41,533 students.

[6] The major in business economics (*economia e commercio)* focused primarily on accounting and was preparatory for becoming an accountant or tax preparer. All technical students had access to this major, even though only type C graduates had received a precollegiate preparation in accounting.

2.3 Effects on High School Choice

The reform increased the value of a type B diploma and gave students an alternative path to pursue a STEM degree. The policy, therefore, might have affected how students sorted into type A and B schools. In particular, some students who would have enrolled in a type A school without the policy might have decided to enroll in a type B school. Data on high school enrollment, however, do not confirm this hypothesis (Appendix Figure A1). After 1964, enrollment in type B schools slowed down, while enrollment in type A schools started increasing at a faster pace.

In addition, the SHIW data can be used to test whether paternal characteristics (education and occupation) of students who enrolled in type A and B schools changed after the policy implementation (Appendix Table C2). With high school lasting 5 years and transfers across school types not admissible, the students in the 1966 cohort were the first who knew about the policy at the time they enrolled in high school. Technical students (the SHIW groups graduates of type B, type C, and other technical schools in one category) who graduated after 1965 were 6.3 percentage points less likely to have a father with a high school diploma, relative to earlier cohorts. The effect is significant at the 10 percent level (p-value 0.095) but is not matched by an offsetting increase in the same probability for type A students, suggesting that all students in later cohorts had more educated fathers (rather than students with more educated fathers switching to type B schools). Furthermore, the average paternal occupation did not change after 1965 for both type A and type B students. Overall, looking at changes in parental characteristics of enrolled students, there is no evidence of a significant enrollment shift from type A to type B schools.

It might surprise some readers that after 1961 enrollment in type B schools, relative to type A, did not increase. The two schools, however, differed in many aspects that were not affected by the reform. First, type A schools provided a solid preparation for the university, while type B schools did not teach important disciplines like math and physics. Second, type A schools were still granting access to a wider array of majors. For a high-achieving student, especially one who was not sure about what major to pursue at the university, a type A school was clearly the best choice even after 1961. Low-achieving low-income students were less likely to attend a type A school to begin with. On the other hand, low-achieving high-income students were probably attending type A schools primarily for nonacademic reasons (higher social prestige of a type A diploma) and were not inclined to switch to type B schools, due to parental pressure.

3 Data

To analyze the effects of the policy, I collected the school transcripts of 27,236 students who completed high school between 1958 and 1968 in Milan, Italy. Traditionally an industrial powerhouse, Milan is also a major financial and commercial center. Disposable income per capita is the highest in Italy at €25,866, 49.2 percent higher than the national average of €17,336 (Unioncamere, 2013). Similarly, 55 percent of its population has at least a high school diploma, compared with 41 percent for Italy (2011 Census).

3.1 High School Registries

I collected and digitized official registries containing the grades of all students who completed high school between 1958 and 1968 in Milan. The final sample includes 27,236 students from 17 high schools (7 type A, 6 type B, and 4 type C) out of 19 public schools that were operating throughout this time period (Appendix Figure B1). Of type A students, 66.8 percent were male, compared with 98.6 percent of type B students (Table 1). On average, type B students were one year older than type A.

At the end of their fifth year, students take a national examination (*maturità*) in order to graduate. The registries report the outcome of the exit exam (a numerical score from 0 to 10 with 6 as passing grade) for each discipline in the curriculum. On average, type A students graduated with a 6.48 GPA and type B with a 6.36 GPA. The registries identify home-schooled students (7.2 percent of type A and 8.4 percent of type B) and repeaters (9.7 percent of type A and 8.2 percent of type B). The high school score standardized by year and school and the binary indicators for home-schooled students and repeaters are used as measures of precollegiate ability.

In each high school, cohorts were divided randomly into classes of 20–30 students at the beginning of freshman year. Usually, the initial assignment remained unchanged in the following years, except for students who moved to new classes after failing a grade. For each student, her other classmates' average score is used as a measure of precollegiate peer quality.

3.2 University Transcripts

For the same sample of students who completed high school in Milan between 1958 and 1968, I collected and digitized full transcripts from the two public universities of Milan—*Università*

Statale (State University) and *Politecnico* (Polytechnic)—and from the private *Università Cattolica* (Catholic University).[7]

The transcripts are an incredibly rich source of information. For each course attended, they contain the course title, the exam date, and the grade received. Grades (from 0 to 30 cum laude with 18 as passing grade) are not curved: in this time period, most courses had oral tests and students were graded sequentially. The transcripts include the final mark (*voto di laurea*), a function of the GPA and a final thesis, that is salient to potential employers during a job search. The transcripts also contain the start and end dates of each student's university career, together with a description of the final outcome (graduation, dropout, or transfer). Of type A students, 86.7 percent enrolled in university and 63.7 percent graduated (Table 1). In comparison, only 40.8 percent of type B students enrolled and 16.1 percent graduated.

The fact that the transcripts are only available from the universities located in Milan could raise some concerns about the representativity of the sample. If, for example, a large share of high school students chose to attend university in another city or country, limiting the data collection to local universities would return a selected group of students. High school graduates in Milan, however, did not travel out of the area to receive a university education. In 1967, for instance, 93.5 percent of university freshmen who had attended high school in Milan enrolled in a local university (ISTAT).

4 A Model of Educational Expansion

This section proposes a model in which educational expansion can lower learning and returns through three channels. First, higher enrollment can congest university resources and lead to a decrease in the quality of education. Second, the admission of less-well-prepared students might increase the degree of class heterogeneity, which in some cases can generate negative peer effects and make teaching more difficult. Both effects weaken human-capital accumulation and lower returns. Third, higher enrollment leads to a higher supply of STEM human capital, which in turn decreases the price of STEM skills.

In the model, students choose between a university major or work (HS). The majors are divided into three groups: STEM ($STEM$), majors not accessible to type B students after 1961 (R for restricted majors), and majors accessible to type B students before and after

[7] The sample does not contain transcripts for Università Bocconi, a private university located in Milan. This does not affect the analysis for two reasons. First, Bocconi specializes in business and economics and admission into these majors was not restricted before 1961. Second, Bocconi was the only highly selective university in the Italian system, due to restricted admission and high tuition fees.

1961 (NR for nonrestricted majors). Aggregate production in the economy is determined by a CES production function that uses the four types of human capital:

$$Y = (S_{HS}H_{HS}^{\rho} + S_{STEM}H_{STEM}^{\rho} + S_R H_R^{\rho} + S_{NR}H_{NR}^{\rho})^{\frac{1}{\rho}}, \tag{1}$$

where H_k is the aggregate supply of human capital with education k, S_k are share parameters, $\rho = \frac{\phi-1}{\phi}$, and ϕ is the elasticity of substitution between the four types of human capital. Assuming perfect competition in the labor market, the skill price of human capital $k = \{HS, STEM, R, NR\}$ is

$$w_k = Y^{1-\rho} S_k H_k^{\rho-1}. \tag{2}$$

In turn, the wage of individual i with education k is the product of skill prices, which are the same for all workers with the same education, and human capital, which varies across individuals ($W_i^k = w_k \cdot h_i^k$). Individual human capital is a function of the knowledge acquired in each university course:

$$h_i^k = \sum_{c \in N_k} \mu_c \cdot k_{ic}^k, \tag{3}$$

where μ_c are weights and N_k is the set of courses in major k. Knowledge acquired in course c is a function of individual and course characteristics:

$$k_{ic}^k = \gamma_0^k + \gamma_1^k X_i + \gamma_2^k C_{ic} + \gamma_3^k Q_c + \gamma_4^k \mathrm{CH}_{ic} + u_{ic}^k, \tag{4}$$

where X_i are individual characteristics, C_c are course characteristics, Q_c is quality of education in course c, and CH_{ic} measures class heterogeneity. The utility of choosing major k is the sum of net nonmonetary preferences and the log of monetary returns:

$$u_i^k = \alpha^k X_i + \log(W_i^k) + \varepsilon_i^k, \tag{5}$$

where ε_i^k is a random idiosyncratic shock.

In this setting, the entry of type B students has three main effects. First, educational expansion can congest university resources. Knowledge acquired in course c is a function of the quality of education, which depends positively on the amount of public resources assigned to the course (r_c) and negatively on the number of enrolled students (E_c). If resources do not vary with enrollment,[8] a marginal increase in the enrollment of type B students affects the quality of education according to

[8] This case fits the Italian scenario, where tertiary education was heavily subsidized by lump-sum transfers and tuition fees were low. For example, tuition fees were equal to 20 percent of total revenues at *Università*

$$\frac{\mathrm{d}Q_c}{\mathrm{d}E_{B,STEM}} = \frac{\partial Q_c}{\partial E_{STEM}} \cdot (1 + \frac{\partial E_{A,STEM}}{\partial E_{B,STEM}}). \tag{6}$$

As total enrollment increases, resources are shared among a larger amount of students, access to university inputs becomes more crowded, and quality of education decreases ($\frac{\partial Q_c}{\partial E_{STEM}}$). The sign of equation (6), however, hinges on how total enrollment changes. Type A students, in fact, might decide to move out of STEM fields towards majors with restricted access after 1961, where quality of education is unaffected. The overall effect is negative if more than one type A student leaves STEM for each incoming type B student ($\frac{\partial E_{A,STEM}}{\partial E_{B,STEM}} > -1$).

Second, educational expansion can modify the composition of skills in a classroom by admitting less-well-prepared students. In the resulting classroom, teaching might be more challenging and negative peer effects more likely (Lazear, 2001). For student i in course c, class heterogeneity is a function of the number of students of the same type ($E_{i,STEM}$)—in this case, with the same high school diploma—and the number of students of a different type ($E_{-i,STEM}$). A marginal increase in type B enrollment modifies class heterogeneity according to

$$\frac{\mathrm{dCH}_{ic}}{\mathrm{d}E_{B,STEM}} = \frac{\partial CH_{i,c}}{\partial E_{B,STEM}} + \frac{\partial CH_{i,c}}{\partial E_{A,STEM}} \cdot \frac{\partial E_{A,STEM}}{\partial E_{B,STEM}}. \tag{7}$$

For type A students in STEM majors, class heterogeneity increases after the reform: type B students enter and make classes more diverse ($\frac{\partial CH_{i,c}}{\partial E_{B,STEM}}$), while some type A students who would have chosen STEM might decide to enroll elsewhere to avoid the negative consequences of educational expansion ($\frac{\partial CH_{i,c}}{\partial E_{A,STEM}} \cdot \frac{\partial E_{A,STEM}}{\partial E_{B,STEM}}$).

Third, an increase in STEM enrollment drives up the aggregate supply of STEM skills, which can decrease prices of STEM skills. The change in prices of STEM skills following a marginal increase in type B enrollment is

$$\frac{\mathrm{d}log(w_{STEM})}{\mathrm{d}E_{B,STEM}} = -\phi^{-1} \cdot \frac{1}{H_{STEM}} \cdot (\frac{\partial H_{STEM}}{\partial E_{B,STEM}} + \frac{\partial H_{STEM}}{\partial E_{A,STEM}} \cdot \frac{\partial E_{A,STEM}}{\partial E_{B,STEM}}) \tag{8}$$

with aggregate production Y fixed. As seen above, the sign depends on two offsetting changes. On the one hand, more type B students enroll in STEM fields and drive up the aggregate supply of STEM skills ($\frac{\partial H_{STEM}}{\partial E_{B,STEM}}$). On the other hand, some type A students switch to other fields and decrease the supply of STEM human capital ($\frac{\partial H_{STEM}}{\partial E_{A,STEM}} \cdot \frac{\partial E_{A,STEM}}{\partial E_{B,STEM}}$).

Statale and 11 percent at *Politecnico*. This assumption can be relaxed to allow for cases in which university resources respond to enrollment, although not elastically.

5 Major Choice of Type A Students

This section shows how the major choice of type A students, who were not directly targeted by the reform, changed after 1961. Assuming that the shocks ε_{it}^k in equation (5) follow a type I extreme value distribution[9] and that type A students choose only between STEM and restricted majors, a marginal increase in type B enrollment changes the probability of type A students choosing STEM according to

$$\frac{\partial P_{iA}^{STEM}}{\partial E_{B,STEM}} = P_{iA}^{STEM}(1 - P_{iA}^{STEM})(ME_i^{STEM} - ME_i^R), \tag{9}$$

where ME_i^k is the marginal change in the returns to major k due to educational expansion and P_{iA}^k is the ex-ante probability of type A choosing major k. According to equation (9), if returns decrease more in STEM than in restricted majors, some type A students who would have enrolled in STEM without the policy choose a restricted major. In addition, the change in probability is larger among the students who are ex-ante uncertain between STEM and restricted majors. Both predictions are confirmed by the data.

In Milan, in fact, the share of type A students enrolling in STEM and restricted majors followed a diverging trend after the reform (Figure 2, Panel A). In 1958, 39.6 percent of type A enrolled in STEM, while 37.7 percent enrolled in restricted majors. The two shares stayed constant until 1961. After 1961, however, the share of type A students enrolling in STEM started decreasing: enrollment in STEM decreased to 36.5 percent in 1962 and to 33.4 percent in 1965. In the same period, the share of type A students choosing a restricted major increased to 44.9 percent in 1962 and 53.5 percent in 1965.

A multinomial logit model can be estimated to control for individual characteristics:

$$\log\left(\frac{Pr(\text{major}_{it} = k)}{Pr(\text{no university})}\right) = \alpha_k + \sum_t \beta_{kt} Y_t + \gamma_k X_{it}, \tag{10}$$

where the choice is between STEM, restricted, and nonrestricted majors, and no university is the baseline. Y_t is a full set of cohort fixed effects. X_{it} includes gender, high school fixed effects, the high school exit score, the mean score of high school classmates, a dummy for home-schooled students, and a dummy for students who did not repeat a grade in high school.

[9] Therefore, the choice probabilities take the form of a multinomial logit (ML) model (Train, 2009). If instead the shocks follow a generalized extreme value distribution, the resulting nested logit (NL) could have a nest with the three schooling options and another nest with the work option. In this case, the ML is just an NL with an additional parameter constraint (the dissimilarity parameter of the schooling nest, λ_S, equal to 1). A likelihood-ratio test fails to reject the ML ($\lambda_S = 0.925$, p-value 0.573).

The cohort effects follow the same diverging trends shown by the raw probabilities (Figure 2, Panel B). Cohort effects are not statistically significant until 1961 and do not show any preexisting trend. Type A students who completed high school in 1963, however, were 3.7 percentage points less likely to enroll in STEM and 8.8 percentage points more likely to enroll in a restricted major, compared with the 1958 cohort. The cohort effects keep diverging until 1966 and then reach a plateau. Type A students who completed high school in 1968 were 9.6 percentage points less likely to choose STEM and 17.8 percentage points more likely to enroll in restricted majors, relative to the 1958 cohort.

To estimate unbiased cohort effects in equation (10), adjacent cohorts may differ only in their exposure to the reform. Section 2.3 already ruled out the hypothesis that the reform changed the choice of high school. Additional tests (Appendix D) suggest that the results are not driven by changes in the characteristics of type A students (for example, the increasing number of female students enrolled in university studies) or by exogenous changes in the returns to different majors (such as unrelated decreases in the returns to STEM majors).

The coefficients estimated from equation (10) using pre-1961 data can be used to predict the major choice of students who completed high school after 1961. The resulting predicted and actual major choices of type A students can be compared to identify the students who were more likely to abandon STEM majors (Appendix Table D2). First, the graduates from type A humanities schools (*licei classici*) were more likely to move to restricted majors after 1961, compared with the graduates from type A scientific schools (*licei scientifici*): the predicted decrease in STEM enrollment was equal to 10.2 percentage points among humanities students and only to 3.1 percentage points among scientific students. This finding holds at any level of precollegiate ability and suggests that educational expansion affected disproportionately the students with stronger preferences for STEM disciplines. Second, the type A students who scored in the top quartile of their high school class (high achieving) were more likely to move out of STEM majors after 1961, compared with the students who scored in the bottom quartile (low achieving): the predicted decrease in STEM enrollment was 11.2 percentage points among high-achieving students and only 4.1 percentage points among low-achieving students. This finding suggests that the high-achieving students had the skills to succeed in different fields and that they potentially suffered the most from the deterioration of the signal attached to STEM degrees.

Estimating equation (10) with the restricted majors divided into different subcategories make it possible to investigate where type A students moved after 1961 (Appendix Table D3). Humanities students moved in similar proportions towards medicine and humanities majors, while scientific students moved exclusively to medicine. In medicine, enrollment grew monotonically with precollegiate ability: among humanities students, for example, enrollment in medicine increased by 11.7 among high-achieving students and by only 6.1 percentage points among low-achieving students. In the humanities majors, instead, enrollment increased more among low-achieving students (+ 6.8 percentage points), compared with high-achieving students (+5.4 percentage points).

6 Human Capital in STEM Majors

This section presents evidence suggesting that human-capital accumulation in STEM majors decreased as a result of fewer resources per student and higher class heterogeneity.

6.1 Congestion of University Resources

If university resources do not fully adjust to enrollment changes, educational expansion congests access to university inputs, lowers resources per student, and may decrease the quality of education. An enrollment increase, for example, may lower the amount of time that a teacher can dedicate to each student, increasing the probability that some students do not receive needed help with coursework.

In the Italian context, access to teaching assistants was very important for students. While professors had mainly a lecturing role, teaching assistants had more responsibilities. In addition to holding office hours and teaching sections, assistants helped with oral examinations, delivered the lectures when the professor was unavailable, supervised undergraduate theses (mandatory to graduate), and carried out important administrative tasks (Marbach, Rizzi and Salvemini, 1969). In summary, teaching assistants were the best chance for students to receive assistance with coursework.

In spite of the large enrollment increase following the 1961 reform, the number of teaching fellows (professors and assistants) did not increase accordingly. Italian universities, in fact, were funded through government grants, which were not affected by the educational expansion: more students did not bring more resources. As a result, the entry of type B students congested university resources in STEM majors. In 1961, the average student–faculty ratio in first-year compulsory courses in Milan was equal to 19.6 in STEM and 18.8 in restricted majors (Appendix Figure E2, Panel A). In 1968, however, the average student–faculty ratio was equal to 43.9 in STEM and to 21.6 in restricted fields. In the same period, the number of teaching fellows per first-year course slightly decreased from 5.3 in 1961 to 4.1 in 1968 (Appendix Figure E2, Panel B).

This paper takes advantage of the fact that the educational expansion congested university resources differentially between courses within a major. In Italian universities, in fact, two otherwise similar compulsory courses (within the same major, within the same field of study, within the same curriculum year, and with similar enrollment) could have been assigned a different amount of assistants. This variation depended primarily on the tenure status of the professor teaching each course (Clark, 1977). Assistants, in fact, were a coveted resource, and tenured professors had more power to hire them.[10] Data from universities in Milan confirm that a compulsory STEM course taught by a tenured professor had on average 3.1 additional teaching fellows, compared with other compulsory STEM courses (Appendix Table E1, Column 1). Other course variables have limited explanatory power: for example, increasing enrollment by 10 units correlates to only 0.29 additional teaching fellows. Therefore, in spite of the same enrollment increase after 1961, the courses with fewer assistants experienced larger increases in student–faculty ratios (more crowding and larger quality decrease), relative to the courses in the same major with more assistants.

The empirical analysis assumes that quality of education in course c is a function of the student–faculty ratio, $Q_c = f\left(\frac{E_c}{fac_c}\right)$, where $f' < 0$, $f'' \leq 0$, E_c is the number of students enrolled in c, and fac_c is the number of teaching fellows (professors and teaching assistants).[11] There are, in fact, indications that student–faculty ratios were correlated with student achievement. Before 1961, for example, grades in STEM courses with a student–faculty ratio below the 25th percentile were 6.4 percent higher than grades in courses with a student–faculty ratio above the 75th percentile (Appendix Table E2).

The baseline specification estimates the within-major change in achievement after 1961 across similar courses with different levels of congestion:

$$g_{cmt} = \alpha + \beta \frac{E_{cm}^{pre}}{fac_{cm}^{pre}} + \sum_t \gamma_t A_t + \sum_t \delta_t \left(\frac{E_{cm}^{pre}}{fac_{cm}^{pre}} \times A_t \right) + \zeta Z_{cmt} + \psi_m + u_{cmt}, \qquad (11)$$

where g_{cmt} is the standardized grade received in the compulsory course c, which belongs to institute m, in academic year t.[12] $\frac{E_{cm}^{pre}}{fac_{cm}^{pre}}$ is the preexisting student–faculty ratio in course c, measured as the average between 1958 and 1964. A_t are academic-year fixed effects: either a full set from 1958 to 1968, or just two binary variables (Post 61_t for years between 1961 and 1964 and Post 65_t for years between 1965 and 1968). Z_{cmt} are student and course

[10] First, since they sat on the hiring committees, they were more likely to have their requests approved. Second, tenured professors were permanently assigned to the same course and, therefore, had a much larger pool of former students to hire as assistants.

[11] If crowding happened on other dimensions that determine the quality of education, relying only on the student–faculty ratio might under-measure the effect of educational expansion on academic outcomes.

[12] The sample is restricted to compulsory courses to avoid self-selection into electives, which could have changed after 1961. The subscript c identifies different courses—for example, Math I and Math II—and not different sections of the same university course. An institute is a bureaucratic entity with a director, an administrative staff, and a dedicated budget that groups courses within a major in the same field of study. In equation (11), I omitted the individual subscript i for ease of notation.

characteristics. The student variables are gender, a quadratic polynomial of age in year t, high school fixed effects, measures of precollegiate ability, and major and university fixed effects. Course variables are the tenure and gender of the professor, and an indicator variable that identifies the institute directors. The regression also includes institute fixed effects (ψ_m). The key parameter δ_t measures the differential change in grades after 1961 across courses with different amounts of resources and, therefore, with different levels of congestion.

The results show that grades decreased after 1961 in courses with a higher student–faculty ratio, relative to other courses in the same major (Table 2, Columns 1 and 2). A marginal increase in the student–faculty ratio decreased grades by 0.004σ between 1961 and 1964 and by 0.003σ between 1965 and 1968. Since the average student–faculty ratio increased by 24.3 between 1961 and 1968, the average effect is much larger. In the average course, grades decreased by 0.090σ from 1961 to 1964 and by 0.082σ from 1965 to 1968. Yearly estimates indicate that, in the average course, grades decreased by 0.113σ in 1961, by 0.137σ in 1962, and by 0.050σ in 1963 (Appendix Figure E3). After 1963, estimates increased in magnitude. In 1968, grades decreased by 0.130σ in the average STEM course.

The standardized course grade as a proxy for human capital could raise some concerns, even in the Italian setting where exams were not graded on a curve (exams were mostly oral and students were graded sequentially). After 1961, for example, professors could have started grading more leniently in order to avoid failing a large share of the enrolled students.[13] If the change in grading was the same across all courses within a major, the difference-in-differences setup of equation (11) would cancel this effect. If, instead, lenient grading after 1961 was more pronounced in courses that experienced more congestion, the previous results should be interpreted as upper bounds of the negative effect of crowding on human capital.

These results are robust to a variety of robustness checks. Including student fixed effects, grades decreased by 0.133σ from 1961 to 1964 and by 0.156σ from 1965 to 1968 in the average STEM course (Table 2, Columns 3 and 4). Replacing institute fixed effects in equation (11) with course fixed effects, grades decreased by 0.083σ from 1961 to 1964 and by 0.073σ from 1965 to 1968 (Table 2, Columns 5 and 6). In both cases, the estimates are statistically significant and very close to the baseline.

As explained above, the main driver of the differences in the number of assistants was the tenure status of the professor in charge of the course. If tenured professors were better prepared to deal with higher enrollment, the estimates of δ_t would confound two effects: the increase of student–faculty ratios and the unpreparedness of junior professors to handle bigger classes. Estimate equation (11) with the inclusion of professor fixed effects addresses

[13] The opposite scenario—grading more harshly—was less likely, because it would have just induced the failed students to take the exam again (often only few weeks after the first attempt).

this concern. Since tenure was linked to a specific course, a professor could be tenured in one course and untenured in a second course during the same academic year. Then, professor fixed effects control for any unobservable that is constant across all courses taught by the same faculty member. Including professor fixed effects, grades decreased by 0.089σ from 1961 to 1964 and by 0.107σ from 1965 to 1968 in the average STEM course (Table 2, Columns 7 and 8).

In addition, the effect is not driven by the academic outcomes of type B students after 1961. Restricting the sample to type A students only, the estimates do not change significantly. Grades decreased by 0.082σ from 1961 to 1964 and by 0.063σ from 1965 to 1968 (Appendix Table E3). Further restricting the sample to type A students from scientific schools (who were less likely to select out of STEM majors after 1961) leads to similar conclusions. Grades of scientific type A students decreased by 0.075σ from 1961 to 1964 and by 0.045σ from 1965 to 1968 in the average STEM course. Similarly, limiting the sample to the students with a precollegiate GPA in the bottom quartile of their high school class yields negative, although less precise coefficients. Including institute-specific linear pre-trends, grades decreased by 0.11σ from 1961 to 1964 and by 0.10σ from 1965 to 1968. Clustering the standard errors at the institute level decreases the precision of the estimated decrease from 1965 to 1968.

In the previous regressions, the student–faculty ratios are computed using the number of enrolled students who attended high school in Milan, because data on total enrollment by course and academic year (including students who attended high school in other cities) are not available. To address this measurement error, enrollment in each course can be adjusted using information about the coverage of the dataset in different university majors.[14] The resulting augmented student–faculty ratio followed the same increasing trend after 1961, but the average increase was equal to 84.3 from 71.5 in 1961 to 155.8 in 1968 (Appendix Figure E2, Panel D). Estimating equation (11) with the augmented student–faculty ratio, grades decreased by 0.085σ from 1961 to 1964 and by 0.072σ from 1965 to 1968 in the average STEM course (Appendix Table E3).

In addition to using data from STEM majors, equation (11) can be used to analyze the indirect effects of the reforms in restricted and nonrestricted majors. The average student–faculty ratio increased only by 2.8 in restricted majors and decreased by 61 in nonrestricted majors (partially due to type B students choosing STEM majors after 1961). The estimates suggest that the reform did not affect student outcomes in restricted majors,

[14]For example, on average, the number of engineering freshmen who completed high school in Milan made up 25.5 percent of the total. Enrollment in each engineering course can be divided by 0.255 to estimate total enrollment.

while it increased grades in nonrestricted majors by 0.135σ as a result of more resources per student (Appendix Table E3).

6.2 Class Heterogeneity and High School Curricula

Relative to the pre-reform years when all students in STEM majors had a type A diploma, educational expansion increased significantly the heterogeneity of STEM classes. At the time of university enrollment, in fact, type A and B students differed greatly in their scientific knowledge (Appendix Table B1). On the one hand, type A students had studied formal, physical, and life sciences (for example, math, physics, chemistry, and biology) during high school. On the other hand, type B students had studied only applied sciences such as mechanics, basic engineering, and topography.

The empirical analysis exploits the fact that the composition of enrolled students changed in all STEM majors after 1961, but the negative effects on learning were larger in courses that were not included in the high school curriculum of type B students. Since type B students were less well prepared in these areas,[15] their entry was more likely to disrupt teaching practices and student-to-student interactions. Engineering students, for example, had to take "Technical Drawing" and "Mathematics I" in their freshman year. Type B students were trained in technical drawing, but not prepared for university-level math. Unlike technical drawing, in fact, math was not part of the curriculum in type B schools. To isolate the effect of increased class heterogeneity, then, this paper measures how the grades of type A students changed after 1961 in STEM courses where type B students were not prepared (like "Mathematics I"), relative to other courses in the same major (like "Technical Drawing"):

$$g_{cmt} = \alpha + \beta \mathit{Not\ in\ B\ cv}_{cm} + \sum_t \gamma_t A_t + \sum_t \delta_t \left(\mathit{Not\ in\ B\ cv}_{cm} \times A_t\right) + \zeta Z_{cmt} + \psi_m + u_{cmt}, \quad (12)$$

where g_{cmt} is the standardized grade received in the compulsory course c of institute m during academic year t and $\mathit{Not\ in\ B\ cv}_{cm}$ is equal to 1 if course c was not included in the high school curriculum of type B students. As described in the previous section, Z_{cmt} are student and course characteristics, and ψ_m are institute fixed effects. Before 1961, the average student–faculty ratio was not statistically different between STEM courses included and not included in the type B curriculum (Appendix Table F3). This finding is important,

[15] Once type B students were admitted into STEM majors, their grades in the STEM courses not included in their precollegiate curriculum were 0.243σ lower, compared with other STEM courses (Appendix Table F2). In these same courses, grades of type B students were 0.279σ lower than grades of type A students.

because it suggests that equation (12) is not exploiting the same source of variation used to estimate the effect of decreased quality of education.[16]

Negative and statistically significant estimates of δ_t indicate that the grades of type A students decreased after 1961 in courses not included in the type B curriculum, relative to other courses in the same majors. In courses not in the precollegiate curriculum of type B students, grades of type A students decreased by 0.100σ between 1961 and 1964 and by 0.115σ between 1965 and 1968, relative to other compulsory courses (Table 3, Column 1). Yearly estimates show that the effect of class heterogeneity becomes more negative over time (Appendix Figure F1). Grades decreased by 0.056σ in 1961, by 0.077σ in 1963, and by 0.021σ in 1965, even though the last estimate is not statistically significant (p-value 0.533). After 1965, the effect of higher class heterogeneity started increasing in magnitude: grades of type A students decreased by 0.062σ in 1966, by 0.097σ in 1967, and by 0.170σ in 1968.

The inclusion of institute fixed effects leads to slightly smaller estimates: grades decreased by 0.067σ between 1961 and 1964 and by 0.086σ between 1965 and 1968 (Table 3, Column 2). Similarly, including student fixed effects, grades decreased by 0.104σ from 1961 to 1964 and by 0.106σ from 1965 to 1968 (Table 3, Column 3). Replacing institute fixed effects with course fixed effects, estimates do not change significantly. In courses not in the precollegiate curriculum of type B students, grades of type A students decreased by 0.072σ from 1961 to 1964 and by 0.090σ from 1965 to 1968 (Table 3, Column 4). The inclusion of professor fixed effects, however, leads to slightly smaller estimates: grades of type A students decreased by 0.050σ from 1961 to 1964 and by 0.062σ from 1965 to 1968 (Table 3, Column 5).

Limiting the sample to type A students from scientific schools (who were less likely to shift away from STEM majors after 1961), results do not change. In courses not included in the type B curriculum, grades of scientific type A students decreased by 0.073σ between 1961 and 1964 and by 0.076σ between 1965 and 1968 (Appendix Table F4). Restricting the sample to type A students with a high school GPA in the bottom quartile of their class reduces the precision of the estimates. In courses not in the precollegiate curriculum of type B students, grades of low-achieving type A students decreased by 0.029σ (p-value 0.53) between 1961 and 1964 and by 0.084σ (p-value 0.11) between 1965 and 1968. Including institute-specific linear pre-trends, grades of type A students decreased by 0.118σ (p-value 0.12) from 1961 to 1964 and by 0.138σ from 1965 to 1968. Clustering the standard errors at the institute level decreases the precision of the estimated decrease from 1961 to 1964.

[16]The opposite is also true. The share of courses in the high school curriculum of type B students is not statistically different between courses with a high and low preexisting student–faculty ratio (Appendix Table E2).

7 Long-run Income

This section describes the available income measure and studies how the educational expansion affected the long-run income of students with a STEM education.

7.1 Income Tax Returns in 2005

The income from personal income tax returns in 2005 is the sum of all individual earnings that are taxable under the Italian personal income tax after allowed deductions. It includes labor earnings for the employees, profits for the self-employed, pensions, rents, and interests. The main excluded categories are dividends and capital gains, both taxed separately. Because the release of the 2005 income tax data was an extraordinary event, similar data from other years are not available.[17]

Using name and birthdate, I uniquely match 83 percent of the high school graduates to income earners in year 2005, when the estimated survival rate for these cohorts was equal to 91 percent.[18] The difference between the matching rate and the estimated survival rate indicates that some individuals who were alive in 2005 did not file a tax return either because they had a disposable income below the taxable threshold (€7,500) or because they were living and working abroad. Matched individuals, however, are more likely to be men and have higher high school grades (Appendix Table B2).[19] These findings suggest that the attrition in matching students to income earners might be primarily driven by individuals (women and low-achieving students) with income below the minimum threshold, and not by high-skilled expatriates. Importantly, selective attrition based on gender and school achievements does not vary across cohorts (Appendix Table B3).

In 2005, 95 percent of the students in the dataset were between 56 and 67 years old. Therefore, the income is observed at—or just after—the peak of the earning curve, when the returns to education are realized in full. This income measure is also less likely to be affected by temporary shocks and allows me to evaluate the long-run effect of the educational expansion. However, having only one cross section limits the analysis in two dimensions. First, the effect of the policy on income dynamics cannot be examined. Second, in a single cross section, any comparison between students from different cohorts combines age and

[17] In March 2008, the Italian Treasury published the 2005 income tax data (with identifiers like name, birthdate, and city of residence) on its website with the intention to fight tax evasion. The data files were removed within 24 hours due to strong opposition from the public. If downloaded in March 2008, the data can now be used for research purposes.

[18] The estimated survival rate can be found at http://www.mortality.org.

[19] The gender difference is statistically significant, but small (81.8 percent for women and 83.3 for men), because Italian couples file separately and married women are required to use their maiden name.

cohort effects. That is, after controlling for observable characteristics, the incomes of two otherwise similar students in different cohorts could differ both because they completed high school in different years (cohort effects) and because their ages were different in 2005 (age effects).

To measure the effect of the policy, then, the age effects need to be eliminated. To deal with this problem, this paper uses repeated cross sections of the SHIW data to estimate age effects as a function of observable characteristics, allowing for cohort and year effects. Of course, owing to the fact that cohort plus age equals year, fully free year effects cannot be estimated. This paper therefore assumes that year effects are smooth. This assumption is reasonable because the period under consideration does not contain sharp year events (like a war). The results, however, are robust to different specifications of the age, cohort, and year effects. The resulting out-of-sample estimates of age effects serve two purposes. First, they can be used to predict income at age 65 (the age of retirement for men) for all the in-sample observations (Appendix G). Then, throughout the analysis, this paper uses this age-adjusted income to measure the effects of the policy. Second, the out-of-sample estimates reveal the importance of age effects among Italian individuals aged 56 to 67. If the age effects are trivial in the SHIW dataset, they should also be trivial in the cross section based on income tax returns.

In fact, age effects for older Italian workers are unimportant, both because their preretirement earnings are hardly rising with age and because their postretirement earnings tend to be a strict percentage of their pay in their last several years before retirement. In this case, the rigidity of the Italian pay and pension systems (Holden and Wulfsberg, 2008) is useful. Indeed, the age effects are so trivial that using age-adjusted income leads to results that are similar to the results based on raw income (Appendix Table G4).

7.2 Income of Type B Students

The reform was successful in expanding access to the university among type B students who would not have enrolled otherwise. The evidence, however, suggests that type B students did not earn higher income in the long run, relative to earlier cohorts who were denied access to STEM majors.

The baseline specification compares outcomes of adjacent cohorts of type B students:

$$\text{outcome}_{it} = F(\alpha + \sum_t \beta_t Y_t + \gamma X_{it}), \tag{13}$$

where outcome_{it} is either university enrollment, university graduation, or log income in 2005 of student i in cohort t. $F()$ is the logistic function when the dependent variable is binary and a linear function otherwise. Y_t is a full set of cohort fixed effects. X_{it} includes the usual student controls.

University enrollment rates of type B students increased by 9 percentage points from 1961 to 1964 and by 22.5 percentage points from 1965 to 1968, relative to the baseline cohorts (Figure 3, Panel A). The reform, then, did not merely shift type B students across majors (from nonrestricted to STEM), but induced more students to enroll in the university. Higher university enrollment then translated into more completed education. University graduation rates of type B students increased by 5.9 percentage points from 1961 to 1964 and by 15 percentage points from 1965 to 1968 (Figure 3, Panel B). Compared with an average graduation rate of 8.2 percent before 1961, these estimates imply 72 percent and 183 percent increases, respectively. Long-run income, instead, followed a different path. Type B students who completed high school from 1961 to 1964 earned 9.5 percent more in 2005, relative to earlier cohorts (Figure 3, Panel C). However, the following cohorts, for whom enrollment and graduation rates increased the most, did not earn higher income, relative to the baseline.

These findings are robust to a variety of difference-in-differences specifications, which do not rely only on cross-cohort variation. For example, it is possible to measure how outcomes of type B students who scored in the top quartile of their high school class (and, therefore, should have benefited more from the opportunity to enroll in STEM majors) changed across cohorts, relative to type B students who scored in the bottom quartile.[20] In the pre-reform cohorts, high-achieving type B students were 15.1 percent more likely to enroll in the university and 9.3 percent more likely to receive a university degree, and they earned 33.8 percent more in 2005. The gap in university enrollment increased by 7.1 percentage points from 1961 to 1964 and by 6 percentage points from 1965 to 1968 (Appendix Table H2). Similarly, the gap in university graduation increased by 7.9 percentage points from 1961 to 1964 and by 9.7 percentage points from 1965 to 1968. The income gap, however, did not change after 1961. This is an important finding because it does not rely on the strong assumption that adjacent cohorts differ only in their exposure to the reform (it compares changes in education and income across time and within cohorts), but confirms that the policy did not increase the incomes of type B students.

Alternatively, the following equation measures more directly the income change among type B students who enrolled in university studies after 1961:

[20]There are additional ways to identify more and less treated individuals within a cohort: for example, (1) type B and type A students, or (2) type B and type C students (graduates from commercial schools). In all these cases, the findings suggest that, after 1961, type B students acquired more education, relative to the comparison group. The long-run income of type B students, however, did not increase differentially (Appendix H).

$$\log(\text{income}_{it}) = \beta_0 + \beta_1 \text{degree}_{it} + \beta_2 X_{it} + \eta_{it}, \tag{14}$$

where degree_{it} is 1 for university graduates and X_{it} is the usual set of individual characteristics.

The OLS estimator of β_1 is equal to 0.337 and statistically significant (Appendix Table H3): in the case of 5-year degrees, one year of university studies is associated with an 8 percent higher income. When Post 61_t (1 for the 1961–1964 cohorts) and Post 65_t (1 for the 1965–1968 cohorts) serve as instruments for university graduation (degree_{it}), the IV estimator of β_1 in equation (14) measures the effect of the policy on the incomes of type B students who received a university degree after 1961. The IV estimator is lower than the OLS and not statistically significant (-0.225, p-value 0.343). Although the IV estimator has a large confidence interval, these results can rule out returns to a university education for type B students above 5.4 percent per academic year.[21] Moreover, estimating equation (14) on different quartiles of the ability distribution does not modify these findings (Appendix Table H3). The OLS estimator is negative and significant for the bottom quartile and, for these students, the results rule out positive returns to a STEM education after 1961. For the remaining quartiles, the IV estimators are not statistically different from zero, but are less precisely estimated.

In addition, there is no evidence that the policy modified other moments of the income distribution. For instance, the income distribution of the 1958 cohort, the first in the data, can be compared with the income distribution of the 1968 cohorts, for whom the rate of university graduation increased the most (Figure 3, Panel D). A Kolmogorov–Smirnov test fails to reject the hypothesis that the two distributions are statistically equivalent at the 5 percent level (p-value 0.064).

Stigma or the timing of graduation could explain these results, even in the absence of any negative consequence of educational expansion. To start, stigma or discrimination towards a technical diploma could explain the lack of returns to a university education for type B students. Indeed, a type A diploma is associated with 36.1 percent higher income, compared with a type B diploma (p-value < 0.001). However, the type of high school loses its predictive power after controlling for university graduation (coefficient 0.028, p-value 0.457). In addition, the younger cohorts might have entered the labor force around the 1973 oil crisis. The literature that examines the costs of graduating in a recession, however, found that the negative effects fade out a few years after graduation and do not apply to graduates

[21] These upper-bound estimates are much lower than what found, for example, by Maurin and McNally (2008). They find that, in France, one year of university studies increased earnings by 14 percent among cohorts born between 1947 and 1950.

from high-return majors like STEM (Oreopoulos, von Wachter and Heisz, 2012; Altonji, Kahn and Speer, 2014). Therefore, it is unlikely that graduating in a recession could have generated negative income effects large enough to wipe out returns to a STEM education 30 years after graduation. Overall, there is no evidence supporting these hypotheses.

7.3 Income of Type A Students

The baseline specification examines how the long-run income of STEM students with a type A diploma changed after 1961, relative to other type A students:

$$log(\text{income}_{ft}) = \alpha + \sum_f \beta_f \text{C}_f + \sum_t \gamma_t Y_t + \sum_t \delta_t [\text{STEM}_f \times Y_t] + \zeta X_{ft} + u_{ft}, \tag{15}$$

where the unit of observation is a student (i, omitted) in a field of study (f) and in an high school cohort (t). The dependent variable income_{ft} is the taxable personal income in 2005, Y_t is a set of fixed effects for high school graduation year, C_f are major fixed effects, STEM_f is equal to 1 if the student enrolled in a STEM major, and X_{ft} is the usual set of student characteristics. A negative estimate of δ_t would suggest that the income of STEM students who completed high school in year t decreased, relative to type A students in the same cohort and with a different education.

Before 1961, type A students who enrolled in a STEM major earned 28 percent more than other type A students (Table 4, column 1). The income gap between STEM and other fields remained unchanged from 1961 to 1964, but decreased by 12.6 percentage points from 1965 to 1968. Estimates for single cohorts indicate that the STEM premium decreased by 28.9 percentage points in 1966, by 27.3 percentage points in 1967, and by 33.9 percentage points in 1968 (Appendix Figure H2). Estimating equation (15) on the sample of type A students who received a university degree leads to the same findings (Table 4, column 2). The STEM premium, equal to 17.4 percent before 1961, decreased by 12.9 percentage points between 1965 and 1968.

In the previous results, the estimates of δ_t might confound the income effect of the educational expansion (lower STEM human capital and skill prices) with the variation resulting from a compositional change of type A students in STEM majors. To control for changing selection, the sample can be restricted to type A students with a high school GPA in the bottom quartile of their class, because these students were less likely to move out of STEM majors after 1961 (section 6.1). Before 1961, low-achieving type A students who enrolled in a STEM major earned 24 percent more, relative to other low-achieving type A students (Table 4, column 3). The STEM premium, however, decreased by 21.8 percentage points from 1965 to 1968. Similarly, the STEM premium among low-achieving university graduates, which was equal to 35.1 percent before 1961, decreased by 34.4 percentage points from 1965 to 1968 (Table 4, column 4). These results indicate that not accounting for selection underestimates the effect of the educational expansion on the returns to STEM majors and suggest that the reform might have increased the skill mismatch among type A students who were induced to enroll in restricted majors.

Within low-achieving type A students, graduates from humanities schools can be compared with graduates from scientific schools. On the one hand, low-achieving scientific students had a high probability of enrolling in STEM (42.3 percent) but did not switch to restricted majors after 1961 (Appendix Table D2). On the other hand, most low-achieving humanities students enrolled in restricted majors before 1961 (62.2 percent) and, therefore, were not directly affected by the entry of type B students in STEM majors. Before 1961, low-achieving scientific students earned 30.3 percent more than low-achieving humanities students (Table 4, column 5). The scientific premium did not change until 1964, but decreased by 26.7 percentage points from 1965 to 1968. Similar findings hold if the sample is restricted to students with a university degree (Table 4, column 6).

To estimate the change in income among low-achieving scientific students who enrolled in STEM majors, graduation from a scientific high school can be used to instrument STEM enrollment in equation (15). The STEM premium, equal to 40.6 percent before 1961, decreased by 67.5 percentage points from 1965 to 1968 (Table 4, column 7). Restricting the sample to university graduates, the STEM premium decreased by 53.1 percentage points from 1961 to 1964 and by 55.3 percentage points from 1965 to 1968 (Table 4, column 8), relative to a baseline of 57.3 percent.

The proposed model of educational expansion—combined with the previous estimates—can relate lower human capital in STEM majors to income losses. Income of STEM students responds to the entry of type B graduates in STEM majors according to

$$\frac{\partial \log(W_i^k)}{\partial E_B^k} = \frac{\partial log(w^k)}{\partial E_B^k} + \frac{\partial log(h_i^k)}{\partial E_B^k}, \tag{16}$$

where h_i^k, the human capital of student i, is the sum of skills (measured by the grade received in each university course) acquired in major k. The estimated effects of congestion of university resources (section 6.1) and higher class heterogeneity (section 6.2) are used to compute the grade change generated by educational expansion per student and course (leave-one-out estimator, Abadie, Chingos and West, 2014). For each student, then, the grade changes in each university course are summed to compute the total variation in human capital.

The congestion of university resources in STEM majors decreased income by 2.4 percentage points. Similarly, higher class heterogeneity decreased income by 2.2 percentage points. Since the income of type A students with a STEM education declined by 12.6 percentage points from 1965 to 1968, lower human capital accounts for 36.5 percent of the total income loss. Among low-achieving type A students, instead, lower human capital accounts for 33 percent of the total income decrease. In the absence of other effects, lower skill prices (due to a higher supply of STEM-educated workers) would account for the remaining share.

8 Conclusions

This paper studied a 1961 Italian reform that increased university enrollment in STEM majors by 216 percent in only 8 years. Using data from high school registries, university transcripts, and long-run income for 27,236 Italian students who completed high school in Milan between 1958 and 1968, the paper found that educational expansion lowered learning in STEM majors through congestion of university resources and higher class heterogeneity. There is evidence that these effects may have induced some of the best students who would have otherwise enrolled in STEM majors to choose other programs. Finally, the students' income information suggests that the educational expansion lowered the returns to a STEM university education in the long run.

Although the consequences of educational expansion were long-lasting for the students who enrolled in university studies immediately after the policy implementation, the negative effects may have dwindled for individuals in the following cohorts. The government, for example, could have increased the resources of public universities so that there was less crowding. However, some of the adverse effects of creating such an abrupt expansion in access might have lingered. For example, hiring new faculty would not have necessarily restored the pre-policy level of education quality because the new professors would have been trained in the "expanded" university system and, therefore, would have had lower human capital.[22] In addition, if the entry of type B students in STEM fields caused the (aptitude or other) signal provided by STEM degrees to deteriorate, especially talented type A students would still have deserted STEM fields and gone into restricted fields (like medicine) even if the expansion had been gradual.

The findings in this paper have broader implications for investments in human capital. Public intervention is potentially needed to overcome three market failures that may

[22] A basic cost–benefit analysis indicates that hiring new faculty in 1961 to prevent any congestion would have been beneficial (appendix I). This strategy, however, would have required a large stock of unemployed professors in Italy, because hiring foreign faculty was not a viable option.

lead to suboptimal human-capital investment: (i) liquidity constraints that prevent people from investing in their own education, (ii) the spillovers associated with certain types of education (for example, inventiveness for university-level STEM education), and (iii) information failures that prevent people from understanding their likely returns to human-capital investment. The remedies for these failures are programs like (i) means-tested financial aid, (ii) scholarships for people who might generate positive externalities, and (iii) information campaigns. Economics, however, does not suggest that the solution to these human-capital investment failures is greatly expanded education provision by state-controlled universities that attempt to force students into certain fields selected by the government. Indeed, "in-kind" provision of university education by state schools can have distortionary, even perverse, effects on human-capital accumulation relative to the same resources being directed toward student-specific financial aid.[23] Peltzman (1973) and others (Long, 2004; Cellini, 2009; Cohodes and Goodman, 2014) writing about this disconnect between what economic logic suggests and what states often do generally focus on the US case, but in fact the issue is probably minimized in America where state universities compete with a robust private sector (Aghion et al., 2010). In most countries, state universities lack competition so that any distortion they introduce via in-kind provision is unlikely to be offset by private universities. In short, countries that currently aspire to increase university-level human capital may be reminded by Italy's example that dramatic, field-specific expansions of access are not what most economic models would indicate.

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[23] If private universities offer higher-quality education and students are willing to forego quality in exchange for cheaper (but lower-quality) education, in-kind subsidies to state universities might lead to an overall decrease in quality-adjusted human capital.

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TABLES AND FIGURES

Table 1: Summary Statistics

	Type A	Type B	Type C
Male	0.668	0.986	0.568
Birth year	1944	1943	1943
High school			
HS exit score (6-10)	6.48	6.36	6.38
Home schooled	0.072	0.084	0.169
Non-repeater	0.903	0.918	0.918
College			
Enrolled	0.867	0.408	0.402
Enrolled—STEM major	0.359	0.204	0.002
Enrolled—restricted major	0.465	0.045	0.037
Enrolled—nonrestricted major	0.043	0.159	0.363
University degree	0.637	0.161	0.105
University degree—STEM major	0.269	0.101	0.001
University degree—restricted major	0.353	0.029	0.016
University degree—nonrestricted major	0.015	0.031	0.088
Grades (18–31)—STEM major	23.93	24.56	21.79
Grades (18–31)—restricted majors	25.64	24.82	25.12
Grades (18–31)—nonrestricted majors	22.80	23.19	22.86
Income in 2005 (€)			
Income	58,657	47,628	41,892
Adjusted Income	65,749	53,812	48,095
Log Income	10.55	10.48	10.04

Notes: Summary statistics of students that completed high school in Milan, Italy; 1958–1968. The sample is composed of 11,433 type A, 8,813 type B, and 6,690 type C. The number of course–student combinations from university transcripts is 144,572 for type A, 15,493 for type B, and 12,456 for type C students. STEM majors are engineering, physics, mathematics, biology, geology, natural science, chemistry, and agricultural science. The restricted majors are medicine, the humanities, political science, law, and architecture. Nonrestricted majors are business, economics, and statistics. Income is winsorized at the 2nd and 98th percentiles. *Adjusted income* is taxable income in 2005 adjusted for age effects. Details on this procedure can be found in appendix G.
Sources: High school archives, university transcripts, and income tax returns in 2005.

Table 2: Quality of Education, Effect on Standardized Grades

	Baseline		Student FEs		Course FEs		Professor FEs	
	Coeff.	Average Effect	Coeff.	Average Effect	Coeff.	Average Effect	Coeff.	Average Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\frac{E_{cm}^{pre}}{fac_{cm}^{pre}}$ x $Post61_t$	-0.004***	-0.090	-0.005***	-0.133	-0.003***	-0.083	-0.004***	-0.089
	(0.001)	(0.024)	(0.001)	(0.015)	(0.001)	(0.024)	(0.001)	(0.025)
$\frac{E_{cm}^{pre}}{fac_{cm}^{pre}}$x $Post65_t$	-0.003***	-0.082	-0.006***	-0.156	-0.003***	-0.073	-0.004***	-0.107
	(0.001)	(0.024)	(0.001)	(0.018)	(0.001)	(0.024)	(0.001)	(0.029)
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Course controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institute FEs	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Observations	62,417	62,417	62,418	62,418	62,417	62,417	62,417	62,417

Notes: Effect on STEM grades of a decrease in the quality of education. The "Average Effect" is the effect of an average increase in the student–faculty ratio (+24.3). The coefficients are computed from $g_{cmt} = \alpha + \beta \frac{E_{cm}^{pre}}{fac_{cm}^{pre}} + \sum_t \gamma_t A_t + \delta_1 \left(\frac{E_{cm}^{pre}}{fac_{cm}^{pre}} \times \text{Post } 61_t \right) + \delta_2 \left(\frac{E_{cm}^{pre}}{fac_{cm}^{pre}} \times \text{Post } 65_t \right) + \zeta Z_{cmt} + \psi_m + u_{cmt}$, using data from STEM compulsory courses. g_{cmt} are standardized grades, and $\frac{E_{cm}^{pre}}{fac_{cm}^{pre}}$ is the preexisting student–faculty ratio (average over 1958–64). A_t is a set of academic-year fixed effects. Post 61 is equal to 1 for the years between 1961 and 1964, while Post 65 is 1 for the years between 1965 and 1968. Z_{cmt} is a set of student and course characteristics. The student variables include gender, high school fixed effects, a quadratic polynomial of age, high school exit score, the average score of high school classmates, a dummy for home-schooled students, a dummy for students that did not repeat a grade in high school, and major and university fixed effects. The course characteristics are the tenure and gender of the professor, a binary variable that identifies professors that are institute directors, and institute fixed effects. An institute is a group of homogeneous courses within a major. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: University transcripts of students who completed high school in Milan, Italy; 1958–1968.

Table 3: Class Heterogeneity, Effect on Standardized Grades of Type A Students

	Baseline		Student FEs	Course FEs	Professor FEs
	(1)	(2)	(3)	(4)	(5)
Not in B cv_c x $Post61_t$	-0.100***	-0.067***	-0.104***	-0.072***	-0.050*
	(0.025)	(0.026)	(0.024)	(0.026)	(0.029)
Not in B cv_c x $Post65_t$	-0.115***	-0.086***	-0.106***	-0.090***	-0.062*
	(0.025)	(0.026)	(0.025)	(0.027)	(0.033)
Student controls	Yes	Yes	Yes	Yes	Yes
Course controls	Yes	Yes	Yes	Yes	Yes
Academic year FEs	Yes	Yes	Yes	Yes	Yes
Institute FEs	No	Yes	Yes	No	Yes
Observations	53,651	53,651	53,651	53,651	53,651

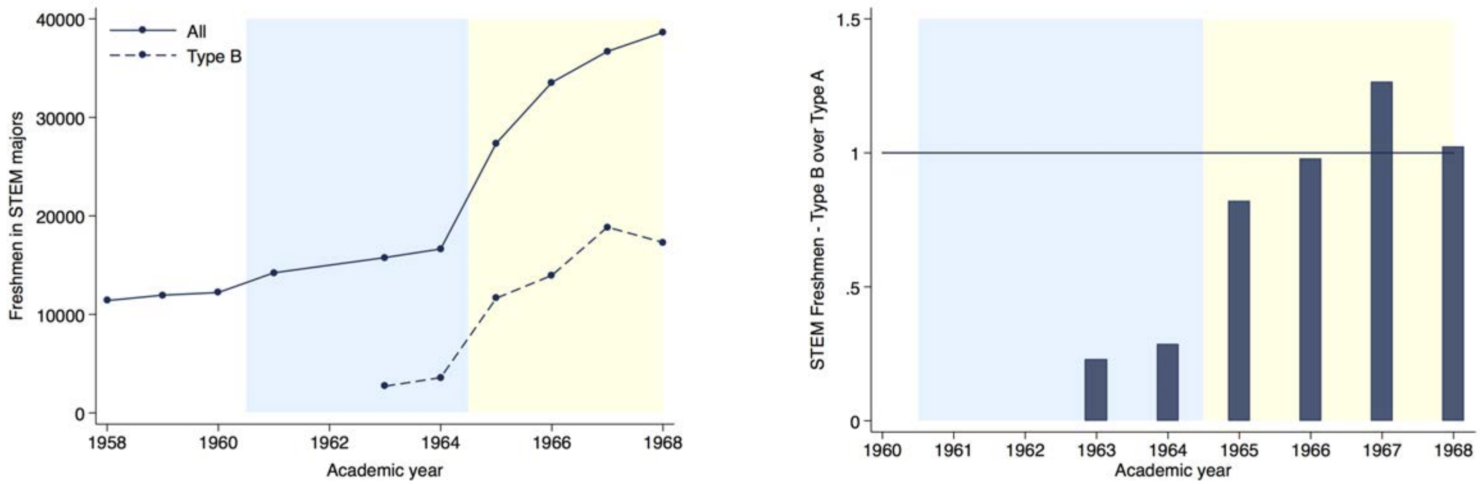
Notes: Effect of an increase of class heterogeneity on STEM grades. The coefficients are computed from $g_{ct} = \alpha + \beta Not\ in\ B\ cv_c + \sum_t \gamma_t A_t + \delta_1 (Not\ in\ B\ cv_c \times \text{Post } 61_t) + \delta_2 (Not\ in\ B\ cv_c \times \text{Post } 65_t) + \zeta Z_{cmt} + \psi_m + u_{cmt}$, using data from type A students enrolled in STEM compulsory courses. g_{cmt} are standardized grades and *Not in B* cv_c is 1 if course c was not included in the curricula of type B schools. A_t is a set of academic-year fixed effects. Post 61_t is equal to 1 for the years between 1961 and 1964, while Post 65_t is 1 for the years between 1965 and 1968. Z_{cmt} is a set of student and course characteristics. The student variables include gender, high school fixed effects, a quadratic polynomial of age, high school exit score, the average score of high school classmates, a dummy for home-schooled students, a dummy for students that did not repeat a grade in high school, and major and university fixed effects. The course characteristics are the tenure and gender of the professor, a binary variable that identifies professors that are institute directors, and institute fixed effects. An institute is a group of homogeneous courses within a major. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: University transcripts of students who completed high school in Milan, Italy; 1958–1968.

Table 4: Income of Type A Students

	All Type A		Low-Achieving Type A					
							IV	
	All	Grads	All	Grads	All	Grads	All	Grads
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
STEM x Post 61	-0.006	0.017	0.021	-0.196			-0.437	-0.758*
	(0.076)	(0.081)	(0.147)	(0.155)			(0.544)	(0.439)
STEM x Post 65	-0.135*	-0.138*	-0.246*	-0.421***			-1.125**	-0.806**
	(0.070)	(0.075)	(0.140)	(0.141)			(0.497)	(0.400)
Scientific x Post 61					-0.123	-0.289*		
					(0.145)	(0.164)		
Scientific x Post 65					-0.311**	-0.293**		
					(0.135)	(0.146)		
STEM/Scientific premium (logs)	0.247	0.160	0.215	0.301	0.265	0.359	0.341	0.453
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HS fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HS graduation year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Major FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,495	6,398	3,072	1,731	3,072	1,731	3,072	1,731

Notes: The coefficients are estimated from $log(\text{income}_{ct}) = \alpha + \sum_c \beta_c \text{C}_c + \sum_t \gamma_t Y_t + \delta_1[\text{STEM}_c \times \text{Post } 61_t] + \delta_2[\text{STEM}_c \times \text{Post } 65_t] + \zeta X_{ct} + u_{ct}$. Y_t is a set of fixed effects for high school graduation year, and C_c are major fixed effects (STEM, restricted major, nonrestricted major, and no university as omitted category). STEM_c is equal to 1 if a student enrolled in STEM. Post 61_t is 1 for the cohorts who graduated between 1961 and 1964, and Post 65_t is 1 for the cohorts who graduated between 1965 and 1968. X_i is a set of student characteristics. Columns (1) and (2) use the whole sample of type A students. Columns (3) to (8) use only type A students with a high school GPA in the bottom quartile of their class. Columns (5) and (6) compare low-achieving graduates from the scientific schools with low-achieving graduates from the humanistic schools. Columns (7) and (8) show IV estimates: the instruments for STEM_c is Scientific_c, which is 1 for the low-achieving graduates from scientific schools. "Grads" restricts the sample to university graduates. The STEM/Scientific premium is the income premium in 2005 (in logs) associated with a STEM degree/scientific diploma for different subgroups of students in pre-1961 cohorts. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: University transcripts and income in 2005 of type A students who completed high school in Milan, Italy; 1958–1968.

Figure 1: Freshmen in STEM Majors



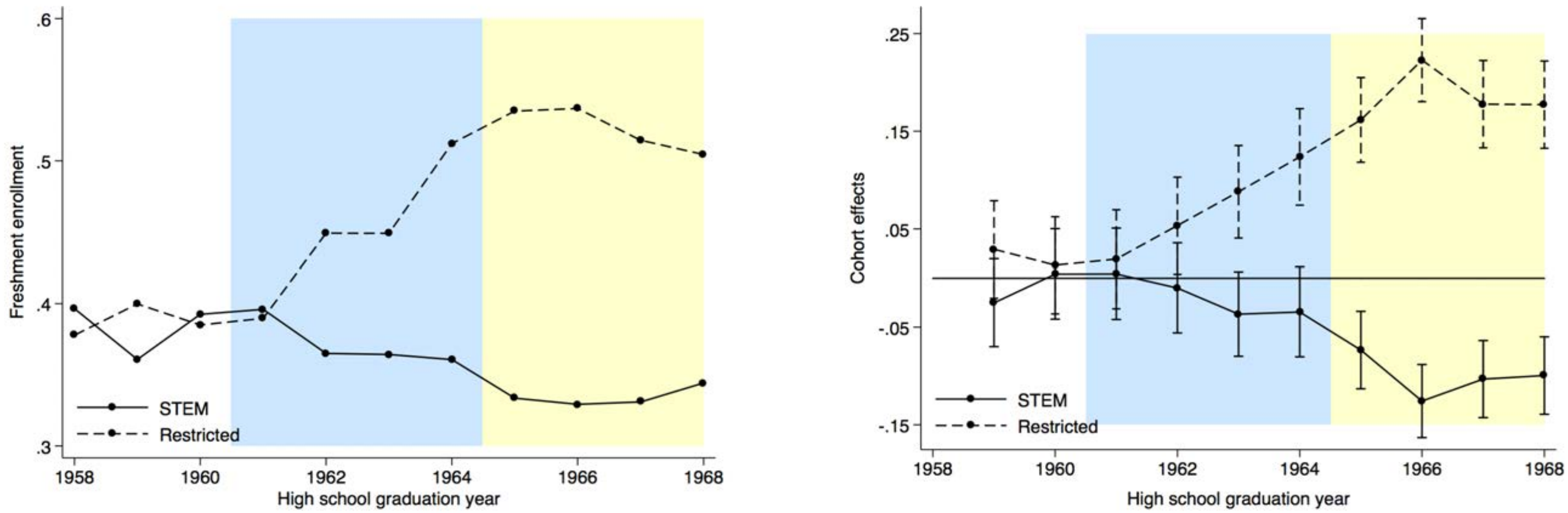
A. Number of Freshmen in STEM Majors

B. Ratio of Type B to Type A

Notes: Panel A: "All" counts the total number of students enrolled in the freshman year of a STEM major for each academic year; "Type B" is the number of students with a type B diploma enrolled in the freshman year of a STEM major. STEM majors are engineering, mathematics, physics, chemistry, biology, geology, natural science, and agricultural sciences. Observations in 1961 and 1962 are missing. The blue shaded area denotes the first phase of the reform: between 1961 and 1964, enrollment of type B students was capped. The yellow shaded area denotes the second phase of the reform: in 1965, the cap to type B enrollment in STEM fields was lifted.

Sources: Annuario Statistico dell'Istruzione Italiana, 1958–1968, Istituto Nazionale di Statistica.

Figure 2: Type A, Enrollment in STEM and Restricted Majors

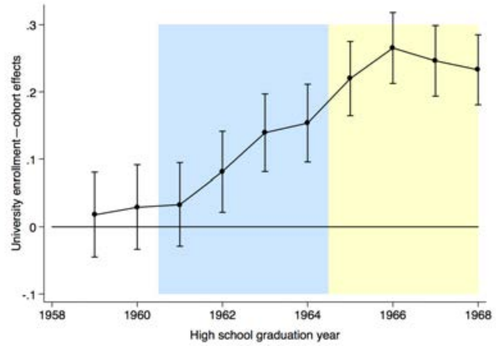


A. Raw Shares

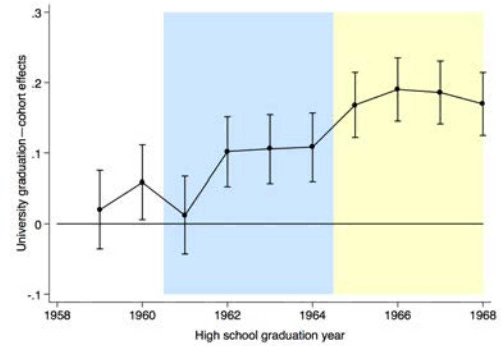
B. Marginal Cohort Effects

Notes: Panel A shows the raw shares of type A students enrolling in STEM and restricted majors by year of high school graduation. Panel B shows marginal cohort effects from the multinomial logit $ln(\frac{Pr(\text{major}_{it}=k)}{Pr(\text{no university})}) = \alpha_k + \beta_k X_{it} + \sum_t \gamma_{kt} \cdot Y_t$, where k is either a STEM, restricted, or nonrestricted major, or no university (baseline). Y_t is a set of year of high school graduation fixed effects with 1958 as omitted category. X_{it} includes gender, the high school exit score, the average exit score of the high school classmates, high school fixed effects, a dummy for home-schooled students, and a dummy for non-repeaters. The bars represent 95 percent confidence intervals. STEM majors are engineering, physics, mathematics, biology, geology, natural science, chemistry, and agricultural science. The restricted majors are medicine, the humanities, political science, law, and architecture. The blue shaded area denotes the first phase of the reform: between 1961 and 1964, enrollment of type B students in STEM majors was capped. The yellow shaded area denotes the second phase of the reform: after 1965, the enrollment cap was lifted. Sources: School data of type A students who completed high school in Milan, Italy; 1958–1968.

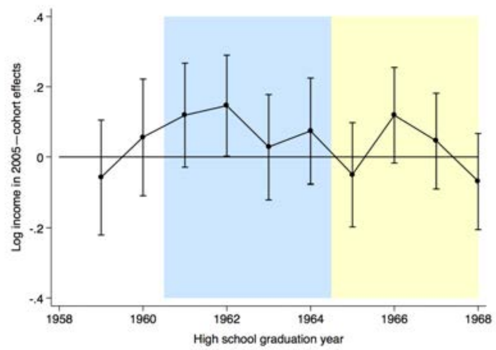
Figure 3: Cohort Effects, Education and Income of Type B Students



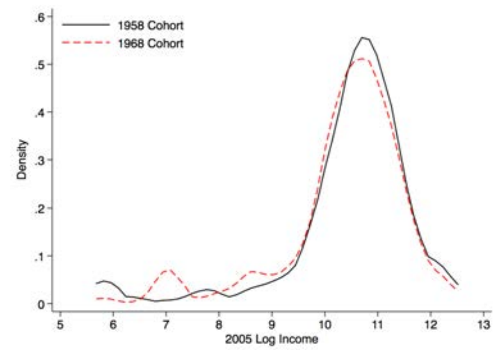
A. University Enrollment



B. University Graduation



C. Income in 2005



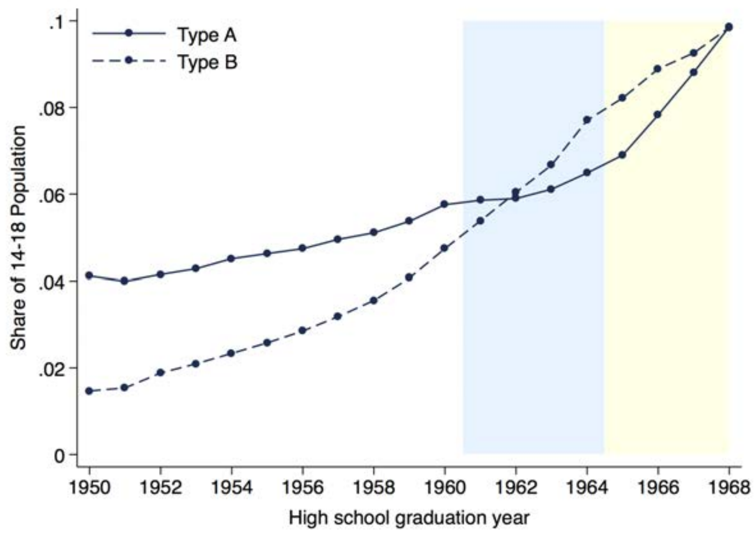
D. Income Distribution

Notes: The marginal effects (Panels A–C) are computed from $\text{outcome}_{it} = F(\alpha + \sum_t \beta_t Y_t + \gamma X_{it})$, where outcome_{it} is either a dummy for university enrollment, a dummy for university graduation, or log income. The function F is logit for university enrollment and graduation and linear for log income. Y_t is a set of year of high school graduation fixed effects with 1958 as omitted category. X_{it} is a set of student characteristics that include gender, the high school exit score, the average exit score of the high school classmates, a dummy for home-schooled students, a dummy for students that did not repeat a grade in high school, and high school fixed effects. The blue shaded area denotes the first phase of the reform: between 1961 and 1964, enrollment of type B students in STEM majors was capped. The yellow shaded area denotes the second phase of the reform: after 1965, the enrollment cap was lifted. The bars represent 95 percent confidence intervals. Panel D shows the income distribution for the 1958 and 1968 cohorts of type B students.
Sources: School data of type B students who completed high school in Milan, Italy; 1958–1968.

Online Appendix - Not For Publication

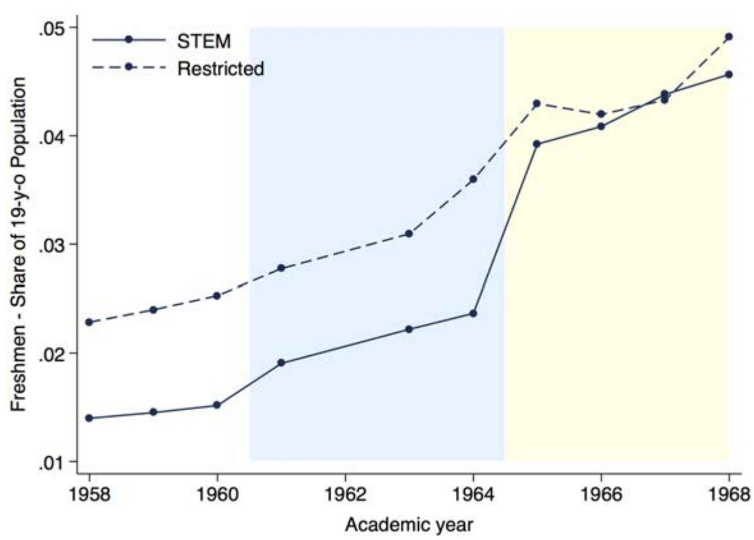
A Additional Figures on the Italian Education System

Figure A1: Type A and Type B Students



Notes: Type A and type B students as a share of 14–18-year-olds.
Sources: Annuario Statistico dell'Istruzione Italiana, 1958–1968, Istituto Nazionale di Statistica.

Figure A2: Freshmen in STEM and Restricted Majors



Notes: Freshmen enrolled in STEM and restricted majors as a share of 19-year-olds.
Sources: Annuario Statistico dell'Istruzione Italiana, 1958–1968, Istituto Nazionale di Statistica.

B Data Collection

The data collection targeted the population of high school students who graduated from a public high school in the city of Milan between 1958 and 1968. The whole process constituted of three main phases.

Between September 2012 and January 2013, I contacted all 19 public high schools in Milan that were granting either a type A (*licei classici e scientifici*), type B (*istituti tecnici industriali*), or type C (*istituti tecnici commerciali*) diploma between 1958 and 1968. 18 schools approved my request to make copies of the student registries (Appendix Figure B2, Panel A), but in one case the archive did not contain the registries for the period under consideration. In some isolated instances, the registries of single school years could not be located in the archives of participating schools. For these reasons, the data cover 74 percent of the high school population in Milan.

Between January 2013 and April 2013, I copied university transcripts from the archives of the three local universities (Appendix Figure B2, Panel B). Two of these universities are public, *Università Statale di Milano* and *Politecnico di Milano*, while the third is private, *Università Cattolica del Sacro Cuore*. The two public universities offered non-overlapping sets of majors: *Politecnico (*Polytechnic*)* focused on engineering and architecture, while *Università Statale* (State University) offered all other majors with the exception of business and economics. *Università Cattolica del Sacro Cuore* (Catholic University of the Sacred Hearth) focused on the humanities majors and the social sciences. The fourth university in Milan, Università Bocconi, was not included in the data collection. Differently from the other Italian universities, Bocconi was charging high tuition fees and admission was highly selective. In addition, Bocconi offered exclusively business and economics majors, which were accessible to type B and type C graduates even before the 1961 reform of university admissions.

Photographic copies of the data were digitized during the months between January 2013 and December 2013 with the help of freelancers hired on a popular online marketplace. The fact that significant portions of the data were hand-written made necessary to hire Italian-speaking typists in order to minimize mistakes in the data entry. The high school registries were transcribed directly into excel spreadsheets. The same procedure, however, was not an option for university transcripts, due to their complex structure and high number of variables. For this reason, I provided each contractor with a data-entry software that I specifically designed to visually reproduce the fields of university transcripts. In addition, I pre-loaded drop-down lists for many string variables, such as course titles. This software sped up the digitization process, lowered the incidence of mistakes, and made data-checking easier.

The resulting dataset of high school graduates was matched with a complete list of personal income tax returns in 2005. Income observations in Italy are extremely rare. The complete list of income tax returns that I used in this paper was published online by the Italian Treasury on March 5, 2008. The goal was to fight tax evasion, allowing every citizen to check the income reported by acquaintances, coworkers, and neighbors. The Italian public strongly opposed this way of disseminating income observations and the data files remained available online for less than 24 hours. The academics that downloaded the data on March 5, 2008 can now use the income observations for research purposes. The data are organized in separate text files for each Italian city. Each file contains the complete list of local income earners with full name, birthdate, the total taxable income after deductions, the due income tax, a coarse indication of the main source of income, and the city of residence in 2005.

Acknowledgments

Many people helped before and during the data collection in Milan. I thank Giovanni Peri for sharing with me his contacts at the Province of Milan and his experience acquired in a similar data collection. I thank Prof. Bruna Sinnone and Prof. Sandra Favi at the Province of Milan. I thank the principals and the staff of the participating schools for their help in the collection of the high school data. For their help in the collection of the university transcripts, I thank Prof. Mauro Santomauro and the staff of the Archivio Storico at Politecnico di Milano; Prof. Daniele Checchi, Emanuela Dellavalle, Idilio Baitieri and the staff of Segreteria Studenti at Università Statale di Milano; Aldo Piacentini, Maurizio Zambon, Claudio Maderna and the staff of Segreteria Studenti at Università Cattolica del Sacro Cuore di Milano. Prof. Aldo Carera shared with me his data on the assignment of faculty to university courses at Università Cattolica.

Table B1: High school curricula

	Type A (1)	Type B (2)	Type C (3)
Humanities	Italian, philosophy, history, Latin, Ancient Greek, art history	Italian	Italian
Sciences	Mathematics, physics, chemistry, geography, biology	Chemistry	Financial mathematics, geography
Applied disciplines (type B: not exhaustive)	Technical drawing	Technical drawing, topography, land appraisal, mechanics, optics, electro-chemistry, thermal eng., aeronautical eng., electrical eng., nuclear eng., training in workshops and labs	Accounting
Law and economics	No	Law (basics)	Law, economics
Foreign languages	Yes (1)	No	Yes (2)
Non-academic	P.E.	P.E.	P.E.

Notes: Type A schools are university-prep high schools, which focus on either the humanities or science. Type B and C are technical high schools, which train professionals for specific economic sectors: type B are industrial schools, which prepare students for jobs in the industry and construction, and type C are commercial schools, which prepare students for jobs in the service sector.

Table B2: Test of Means, Students With and Without an Income Observation

	Matched	Not Matched	Difference
	(1)	(2)	(3)
Male	0.749	0.730	0.019***
Age in 2005	61.663	62.474	-0.811***
Type A	0.421	0.415	0.006
Type B	0.327	0.304	0.023***
Type C	0.252	0.281	-0.029***
HS exit score	6.420	6.384	0.036***
Observations	22,579	4,657	

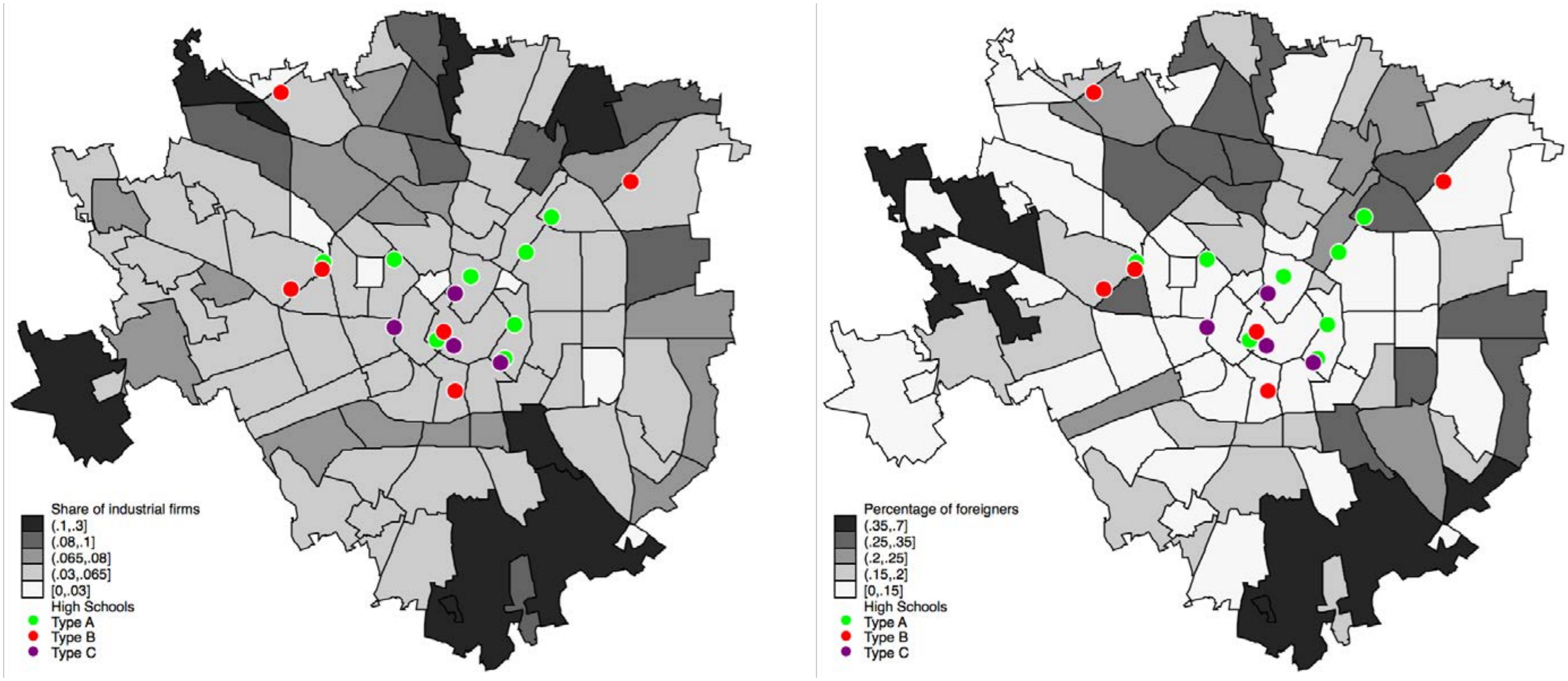
Notes: *Matched* are the 22,579 students that are matched with an income earner from the complete list of personal income tax returns in 2005. Similarly, *Not Matched* are the remaining 4,657 students that did not have a correspondence in the list of income tax returns. *** p<0.01, ** p<0.05, * p<0.1.

Table B3: OLS, Test for Selective Attrition in Matching Students to Income Earners

	Matched	Matched
	(1)	(2)
F-test on student controls	19.43	14.80
	(<0.001)	(<0.001)
F-test on cohort FEs	5.87	0.074
	(<0.001)	(0.689)
F-test on high school FEs	2.38	2.32
	(0.001)	(0.002)
F-test on cohort FEs x Male	Not included	0.91
		(0.527)
F-test on cohort FEs x HS score	Not included	0.16
		(0.999)
Observations	27,206	27,206

Notes: Each column shows the F-statistics and the corresponding p-values (in parenthesis) from separate OLS regressions. The dependent variable is 1 if a student was matched with an income earner in 2005. *** p<0.01, ** p<0.05, * p<0.1.

Figure B1: Location of High Schools in Milan



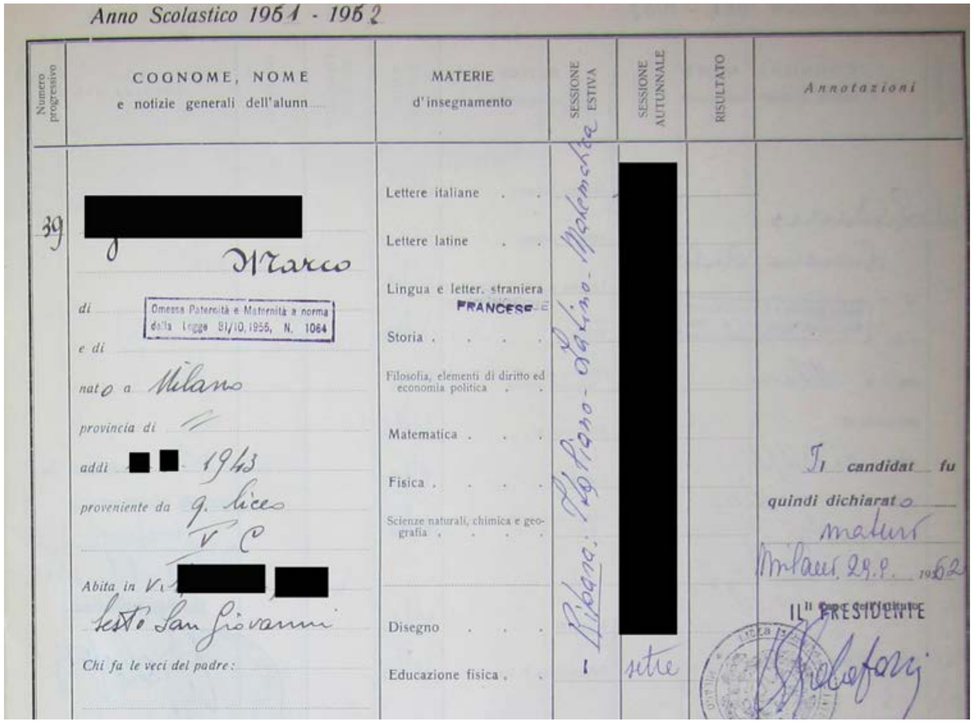
A. Share of Industrial Firms (2011)

B. Share of Foreigners (2011)

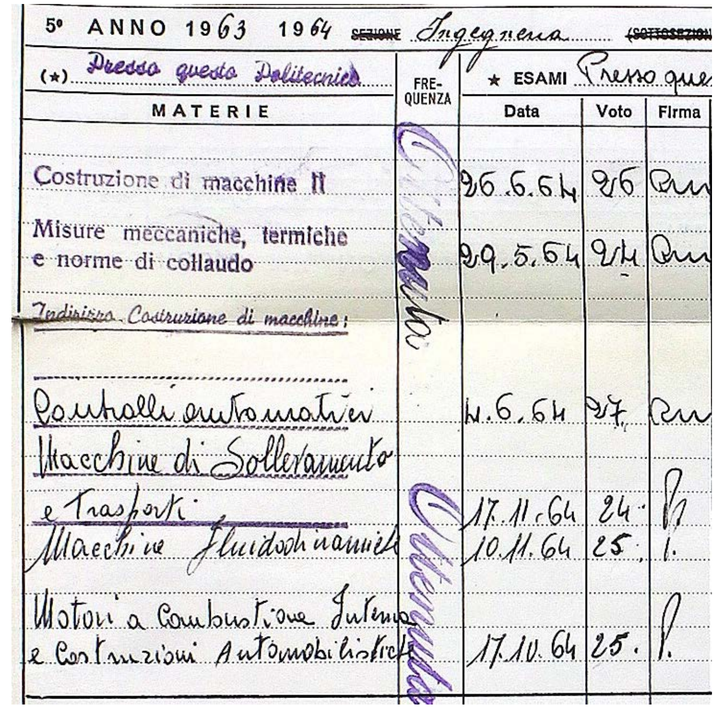
Notes: Map of Milan, Italy. The boundaries identify the 88 neighborhoods (*Nuclei di Identità Locale*). The share of industrial firms is the percentage of industrial firms out of all economic activities in the neighborhood (Panel A). The percentage of foreigners is the share of non-nationals over total population living in each neighborhood in year 2011 (Panel B). The colored dots report the location of the public high schools of type A, B, and C that were active throughout the years between 1958 and 1968.

Sources: Data available at http://dati.comune.milano.it/dato/item/61 and http://dati.comune.milano.it/dato/item/201-201-imprese-numero-di-unita-locali-per-settore-di-attivita-e-quartiere-2010.html.

Figure B2: Examples of High School and University Data



A. High School Data



B. University Transcripts

Notes: Panel A shows the information available for one high school students. I blacked out several parts to guarantee anonymity. Panel B shows an excerpt of a university transcript. Specifically, it shows courses attended during the fifth year of engineering with exam dates and outcomes for one university student.

C Parental Characteristics of High School Graduates

The administrative data collected in school and university archives do not contain any information about the family background. To run some additional tests on the changing characteristics of high school graduates, I use out-of-sample observations from the Bank of Italy's Survey of Household Income and Wealth (SHIW). I restrict the sample to the four more recent waves of the SHIW (2006, 2008, 2010, 2012), because previous waves contain only information about the highest level of completed education and the high school diploma of university graduates cannot not be observed.

Table C1: Parents' Characteristics and High School Choice

	Father			Mother		
	Type A (1)	Technical (2)	Difference (3)	Type A (4)	Technical (5)	Difference (6)
Education						
No education	0.057	0.075	-0.018	0.126	0.094	0.032
Low education	0.530	0.749	-0.219***	0.660	0.801	-0.141***
High education	0.413	0.176	0.237***	0.246	0.072	0.174***
Occupation						
Low income	0.275	0.408	-0.133***	0.823	0.856	-0.033
Middle income	0.438	0.461	-0.023	0.148	0.139	0.009
High income	0.287	0.131	0.156***	0.029	0.007	0.022*
Sector						
Public servant	0.277	0.232	0.045	0.483	0.132	0.351***

Notes: Data from 1,802 individuals born between 1931 and 1950 and with at least a high school diploma; 710 individuals have a type A diploma, while 1,092 have a technical diploma. The SHIW does not distinguish between types of technical diplomas. Respondents were asked the education, employment status, and sector of activity of their parents at their current age (or earlier, if deceased or retired at that age). Low education: a primary school or lower secondary school certificate. High education: high school diploma or higher. Low income: production workers and not employed. Middle income: clerical workers, teachers, self-employed. High income: managers, professionals, and entrepreneurs. *** p<0.01, ** p<0.05, * p<0.1.
Sources: Bank of Italy's SHIW; 2006, 2008, 2010, 2012 waves.

Table C2: Change of Father's Characteristics

	Technical students			Type A		
	Pre 1965 (1)	Post 1965 (2)	Post-Pre (3)	Pre 1965 (4)	Post 1965 (5)	Post-Pre (6)
Education						
No education	0.075	0.074	-0.001	0.067	0.029	-0.038
Low education	0.727	0.791	0.064	0.507	0.592	0.085
High education	0.198	0.135	-0.063*	0.425	0.379	-0.046
Occupation						
Low income	0.411	0.403	-0.008	0.301	0.209	-0.092
Middle income	0.448	0.485	0.037	0.414	0.499	0.085
High income	0.141	0.112	-0.029	0.285	0.292	0.007
Sector						
Public servant	0.242	0.216	-0.026	0.270	0.296	0.026

Notes: See table C1, *** p<0.01, ** p<0.05, * p<0.1.
Sources: Bank of Italy's SHIW; 2006, 2008, 2010, 2012 waves.

Table C3: Change of Mother's Characteristics

	Technical students			Type A		
	Pre 1965 (1)	Post 1965 (2)	Post-Pre (3)	Pre 1965 (4)	Post 1965 (5)	Post-Pre (6)
Education						
No education	0.130	0.119	-0.011	0.105	0.065	-0.040
Low education	0.773	0.856	0.083**	0.658	0.663	0.005
High education	0.097	0.026	-0.071***	0.237	0.272	0.035
Occupation						
Low income	0.860	0.841	-0.019	0.810	0.857	0.047
Middle income	0.132	0.153	0.021	0.153	0.135	-0.018
High income	0.008	0.006	-0.002	0.037	0.008	-0.029
Sector						
Public servant	0.145	0.114	-0.031	0.478	0.501	0.023

Notes: See table C1, *** p<0.01, ** p<0.05, * p<0.1.
Sources: Bank of Italy's SHIW; 2006, 2008, 2010, 2012 waves.

D Additional Results on Major Choice of Type A Students

The identifying assumption to estimate cohort effects in equation (10) is that adjacent cohorts differ only in relation to their exposure to the reform. The remaining threats to identification can be grouped in two classes: changes in the characteristics of type A students and exogenous changes in the returns to different majors.

Initially, I estimate the multinomial logit model in equation (10) dropping controls for precollegiate ability. The cohort effects do not change (Appendix Figure D1, Panel A). In 1968, type A students were 10 percent less likely to enroll in STEM and 18 percent more likely to enroll in restricted majors, relative to the 1958 cohort.

The increasing education of female students over this time period could explain the shift towards restricted majors, which include fields with high female participation like the humanities. To test this hypothesis, I estimate the model in (10) using data on male students only. The main findings hold (Table D1 and Appendix Figure D1, Panel B). Male type A students who completed high school from 1965 to 1968 were 10.7 percentage points more likely to enroll in STEM and 16.6 percentage points less likely to choose a restricted major, relative to the 1958 cohort.

In a separate test, I estimate the model in (10) using only the pre-1961 cohorts. I then use the estimated coefficients to predict the major choice of type A students who completed high school after 1961. If changes in students' characteristics do not drive the diverging trends in major choice, predicted and actual shares should follow different paths. The predicted share of type A students enrolling in STEM majors follows a slightly increasing path after 1961 (Appendix Figure D2, Panel A), while the predicted share in restricted majors is stable (Appendix Figure D2, Panel B).

The second group of robustness checks tests for the role of concurrent and exogenous changes in the returns to higher education.[24] To address this concern, I estimate a conditional multinomial logit model in which I control simultaneously for students' characteristics and returns to different university majors. As a proxy for returns to education, I use the sectoral value added per full-time equivalent worker in the industry, finance, and service sectors (Baffigi, 2011).[25] Controlling for contemporaneous changes in the economy does not affect the path of the marginal cohort effects (Appendix Figure D1, Panel C). In 1968, type A

[24]The economic downturn that affected Italy during the 70's could have affected the industry sector more than services and government, therefore inducing more students to abandon STEM majors.

[25]I use the SHIW dataset to show that different majors lead to occupations in different sectors: STEM to industry, restricted majors to services and government, non-restricted majors to banking and finance, and a high school diploma to retail. I, then, assign to each major the corresponding sectoral value added.

students were 16.7 percentage points less likely to enroll in STEM and 26.5 percentage points more likely to enroll in restricted majors, relative to the 1958 cohort.

Lastly, I estimate the model in (10) using more disaggregated choices to show that enrollment in fairly different STEM (restricted) majors follow the same decreasing (increasing) trend after 1961 (Appendix Figure D1, Panel D).[26] In 1968, type A students were 2.1 percentage points less likely to enroll in engineering and 5.1 percentage points in physics, relative to the 1958 cohort. At the same time, they were 6.3 percentage points more likely to enroll in the humanities and 13.6 percentage points in medicine.

[26]I divide majors with restricted access in 4 groups: (1) medicine - medicine, pharmacy, veterinary, (2) humanities - Italian, history, philosophy, foreign languages, (3) law and political science, (4) architecture. Similarly, I divide STEM in: (1) physics, (2) mathematics, (3) sciences - geology, biology, natural science, and chemistry, (4) engineering.

Table D1: Multinomial Logit, Probability of Type A Students Enrolling in STEM and Restricted Majors

	STEM Majors				Restricted Majors			
	All Type A		Males		All Type A		Males	
	Coeff.	Marginal Effects	Coeff.	Marginal Effects	Coeff.	Marginal Effects	Coeff.	Marginal Effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 61	0.294***	-0.009	0.259***	-0.002	0.449***	0.059***	0.373***	0.043***
	(0.079)	(0.013)	(0.096)	(0.016)	(0.078)	(0.014)	(0.099)	(0.016)
Post 65	0.183**	-0.085***	-0.141	-0.107***	0.803***	0.172***	0.548***	0.166***
	(0.080)	(0.013)	(0.095)	(0.015)	(0.078)	(0.013)	(0.096)	(0.015)
Male	0.429***	0.079***			0.012	-0.087***		
	(0.069)	(0.011)			(0.064)	(0.011)		
HS exit score	0.339***	0.070***	0.336***	0.089***	0.073**	-0.031***	0.011	-0.042***
	(0.035)	(0.005)	(0.043)	(0.006)	(0.035)	(0.005)	(0.045)	(0.006)
HS class score	-0.023	-0.004	-0.082	-0.020	-0.008	0.002	0.012	0.018
	(0.119)	(0.019)	(0.141)	(0.023)	(0.115)	(0.020)	(0.142)	(0.023)
Home schooled	-0.582***	-0.110***	-0.779***	-0.150***	-0.163	0.048**	-0.275**	0.052**
	(0.133)	(0.023)	(0.154)	(0.028)	(0.116)	(0.022)	(0.137)	(0.025)
Non-repeater	0.691***	0.125***	0.843***	0.180***	0.193**	-0.064***	0.145	-0.100***
	(0.104)	(0.019)	(0.121)	(0.022)	(0.093)	(0.018)	(0.110)	(0.020)
Mean, 1958–60	0.383	0.383	0.436	0.436	0.387	0.387	0.340	0.340
HS fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,425	11,425	7,632	7,632	11,425	11,425	7,632	7,632

Notes: Coefficients and marginal effects are estimated from a multinomial logit model where the choice is either a STEM, restricted, or nonrestricted major, or no university (baseline). STEM majors are engineering, physics, mathematics, biology, geology, natural science, chemistry, and agricultural science. The restricted majors are medicine, the humanities, political science, law, and architecture. Post 61_t is equal to 1 for the cohorts that completed high school between 1961 and 1964, while Post 65_t is 1 for the cohort that graduated starting in 1965. The omitted category is represented by the cohorts that graduated between 1958 and 1960. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: School data of type A students who completed high school in Milan, Italy; 1958–1968.

Table D2: Type A, Decrease in STEM Enrollment after 1961

	Humanities high schools			Scientific high schools		
	Actual Shares	Predicted Shares	Difference	Actual Shares	Predicted Shares	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
All type A students	0.192	0.294	-0.102***	0.501	0.532	-0.031***
HS exit score						
Quartile 1 (1-25)	0.149	0.218	-0.069***	0.425	0.439	-0.014**
Quartile 2 (26-50)	0.169	0.256	-0.087***	0.470	0.492	-0.022***
Quartile 3 (51-75)	0.196	0.302	-0.106***	0.518	0.554	-0.036***
Quartile 4 (76-100)	0.272	0.435	-0.163***	0.631	0.691	-0.060***

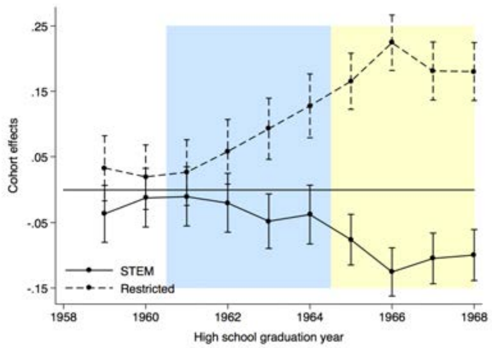
Notes: Columns (1) and (4) show the actual share of type A from the humanistic and scientific high schools choosing STEM fields after 1961. Columns (2) and (5) show the predicted share of type A students from humanistic and scientific schools that would choose a STEM major after 1961, using the coefficients estimated from equation (10) with data of the pre-1961 cohorts. *** p<0.01, ** p<0.05, * p<0.1.
Sources: School data of type A students who completed high school in Milan, Italy; 1958–1968.

Table D3: Type A, Enrollment Shares in Restricted Majors After 1961

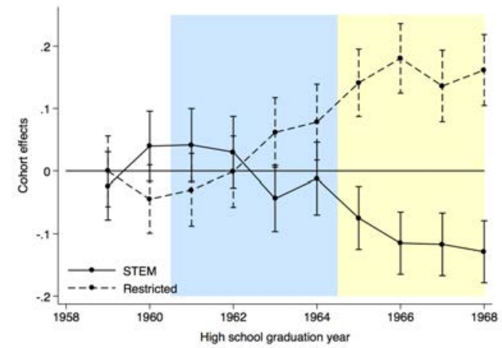
	Humanities high schools			Scientific high schools		
	Actual Shares	Predicted Shares	Difference	Actual Shares	Predicted Shares	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
			Medicine			
HS score - Q1	0.138	0.077	0.061***	0.158	0.069	0.089***
HS score - Q2	0.139	0.055	0.084***	0.156	0.049	0.107***
HS score - Q3	0.141	0.034	0.107***	0.153	0.036	0.117***
HS score - Q4	0.131	0.014	0.117***	0.135	0.012	0.123***
			Humanities			
HS score - Q1	0.300	0.232	0.068***	0.010	0.007	0.003**
HS score - Q2	0.316	0.252	0.064***	0.011	0.010	0.001
HS score - Q3	0.350	0.290	0.060***	0.011	0.015	-0.004**
HS score - Q4	0.372	0.318	0.054***	0.011	0.017	-0.006***

Notes: See table D2. Two groups of restricted majors (Architecture, Law and PoliSci) are not reported. *** p<0.01, ** p<0.05, * p<0.1.

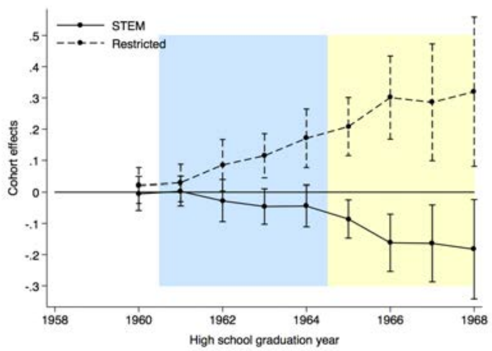
Figure D1: Multinomial Logit, Major Choice of Type A Students



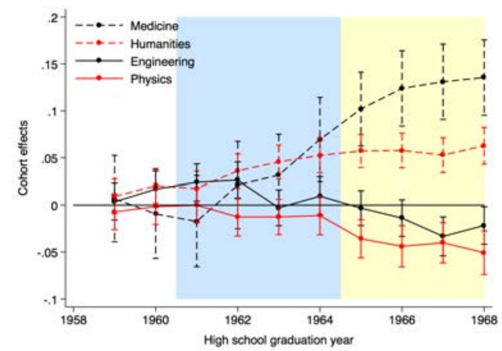
A. No precollegiate ability



B. Males



C. Conditional Multinomial Logit

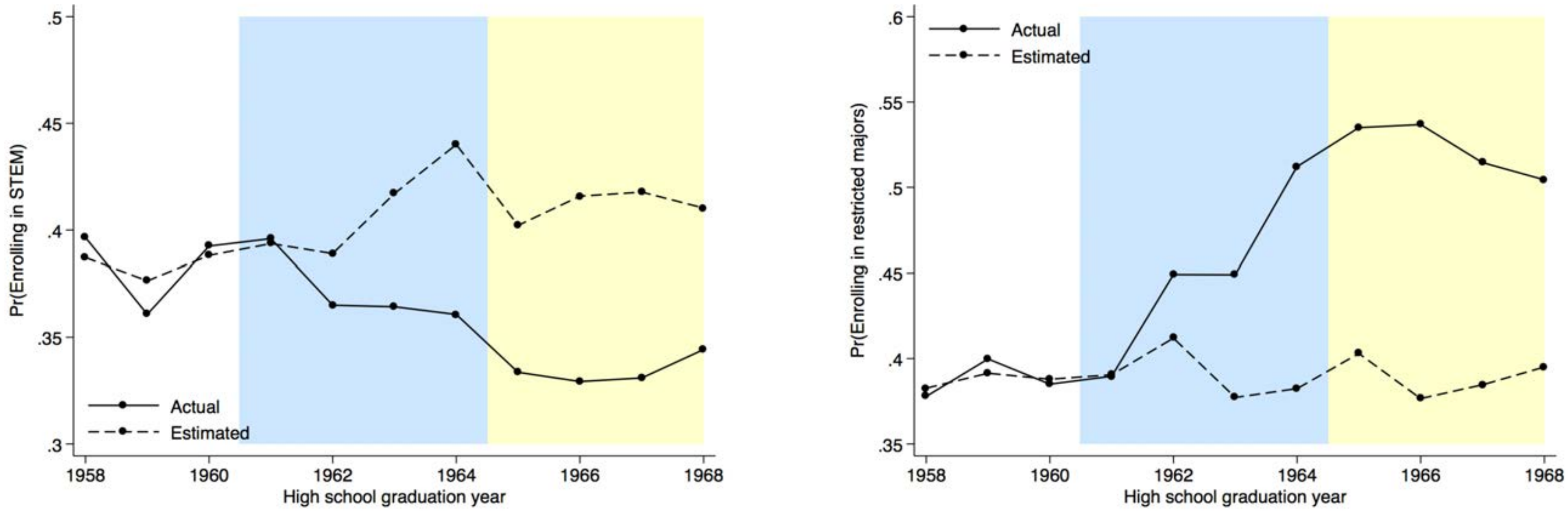


D. Disaggregated Major Choice

Notes: Robustness checks of the marginal cohort effects on the major choice of type A students from equation (10). The bars represent 95 percent confidence intervals. Panel A shows marginal cohort effects without controlling for measures of precollegiate ability. Panel B includes only males. Panel C shows marginal cohort effects from a conditional multinomial logit model with controls for concurrent returns to different majors. Panel D estimates a multinomial logit model with a more disaggregated major choice.

Sources: School data of type A students who completed high school in Milan, Italy; 1958–1968.

Figure D2: Predicted and Actual Probability of Type A Enrollment



A. STEM Majors

B. Restricted Majors

Notes: Actual and predicted probabilities of type A students enrolling in STEM and restricted majors. The predicted probabilities are constructed estimating the equation (10), using only the cohorts that completed high school until 1961.
Sources: School data of type A students who completed high school in Milan, Italy; 1958–1968.

E Additional Results on Congestion of University Resources

In section 6.1, I assumed that the quality of education in each university course is a function of the student–faculty ratio ($Q_c = f\left(\frac{E_c}{fac_c}\right)$). The function f is decreasing ($f^{'} < 0$) and either linear and concave ($f^{''} \leq 0$). This parametrization is consistent with contexts in which congestion costs increase as quality decreases. It is, however, not consistent with the existence of a steep quality decrease at low levels of the student–faculty ratio. The empirical relationship between the quality of education (average grade) and the student–faculty ratio in STEM compulsory courses is fairly linear and decreasing (Figure E1). Importantly, it does not show evidence of a steep quality decrease at low levels of the student–faculty ratio.

The entry of type B students and a fixed number of professors and assistants imply that the quality of education decreases more in courses with fewer teaching fellows and, therefore, a higher preexisting student–faculty ratio. For example, the function f is consistent with a scenario in which students compete to access fixed school inputs and courses with low preexisting student–faculty ratio have unused capacity. Let's consider two courses in the same major: both have 20 students enrolled, but course I has 1 teaching fellow (1 professor) and course II has 2 teaching fellows (1 professor and 1 assistant). Professors have two weekly tasks: (1) preparing and giving lectures, and (2) holding office hours. Professors have a fixed amount of time every week (20 units): lectures take 10 units of time and the remaining units are allocated to office hours. Assistants have 20 units of time for office hours, in case the professor cannot accommodate all students. Finally, all students want to attend office hours and each student takes up one unit of time. In this scenario, 10 students could not attend office hours in course I and 0 in course II. After an enrollment increase of 20 units, 30 students could not attend office hours in course I and 10 in course II. In course II, there was unused capacity which allowed to accommodate more students after the enrollment increase. As a result, quality of education decreased more in course I.

Table E1: STEM, Determinants of the Number of Teaching Fellows

	Compulsory Courses		All Courses	
	(1)	(2)	(3)	(4)
Tenured professor	3.096***	2.646***	3.814***	3.902***
	(0.533)	(0.599)	(0.476)	(0.506)
Institute director	2.518***	1.405	2.172***	0.899
	(0.611)	(1.324)	(0.422)	(0.736)
Female professor	-0.528*	0.468	-0.571**	0.114
	(0.309)	(1.124)	(0.243)	(0.943)
Compulsory course			-0.009	0.326
			(0.334)	(0.466)
Number of students	0.029***	0.025***	0.030***	0.024***
	(0.006)	(0.006)	(0.005)	(0.005)
Lagged average grade	0.019	0.296**	0.018	0.150*
	(0.163)	(0.115)	(0.101)	(0.081)
Mean, 1958–68	4.38	4.38	3.39	3.39
Major-university FEs	Yes	Yes	Yes	Yes
Academic year FEs	Yes	Yes	Yes	Yes
Professor FE	No	Yes	No	Yes
Observations	1,388	1,388	2,440	2,440

Notes: The dependent variable is the number of teaching fellows assigned to each STEM course. Standard errors clustered by course in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: Annals of Università Statale di Milano, Politecnico di Milano, and Università Cattolica del Sacro Cuore. University transcripts of students that completed high school in Milan, Italy; 1958–1968.

Table E2: STEM, Courses with Low and High Student–Faculty Ratio

	Bottom Quartile	Top Quartile	Difference
	(1)	(2)	(3)
Tenured professor	0.691	0.214	0.477***
Institute director	0.709	0.233	0.476***
Female professor	0.073	0.252	-0.179***
Grades (18-31)	24.382	22.924	1.458***
Not in B cv	0.653	0.713	-0.060

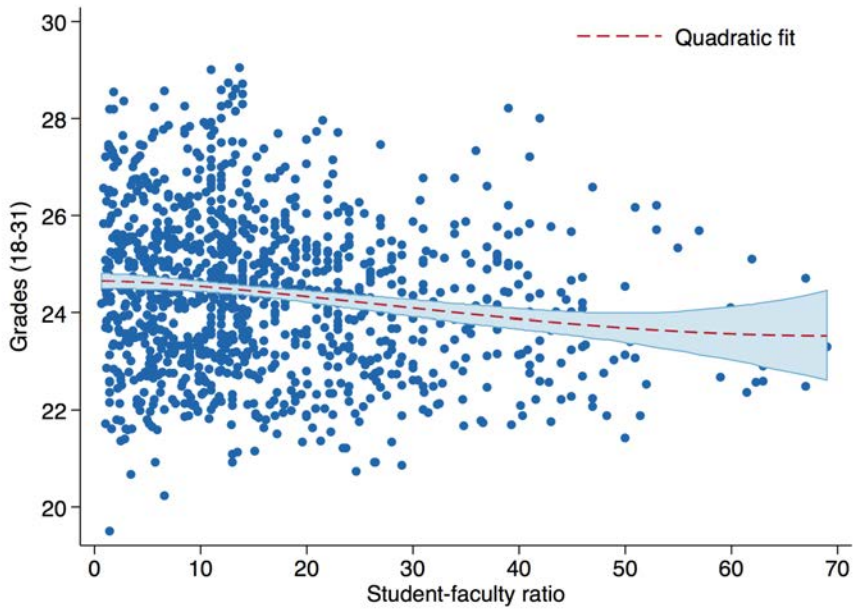
Notes: Only compulsory courses. Below (Above) Median are courses with pre-1964 student-faculty ratio below (above) median. “Grades” are non-standardized grades. “Tenured”, “Institute director”, “Female professor” are characteristics of the professor assigned to the course. “Not in B cv” is a binary variable for courses not included in the curricula of type B high schools. Means computed for academic years 1958–1960. *** p<0.01, ** p<0.05, * p<0.1.
Sources: Annals of Università Statale di Milano, Politecnico di Milano, and Università Cattolica del Sacro Cuore. University transcripts of students that completed high school in Milan, Italy; 1958–1968.

Table E3: Quality of Education, Additional Results and Robustness Checks

	Academic Years 1961-1964		Academic Years 1965-1968		
	Coeff.	Average Effect	Coeff.	Average Effect	Obs.
	(1)	(2)	(3)	(4)	(5)
STEM Majors					
Only type A students	-0.003***	-0.082	-0.003***	-0.063	52,815
	(0.001)	(0.024)	(0.001)	(0.024)	
Scientific type A students	-0.003***	-0.075	-0.002	-0.045	38,644
	(0.001)	(0.029)	(0.001)	(0.029)	
Low-achieving students	-0.002	-0.038	-0.003*	-0.084	13,041
	(0.002)	(0.049)	(0.002)	(0.048)	
No precollegiate ability	-0.004***	-0.101	-0.004***	-0.093	62,418
	(0.001)	(0.026)	(0.001)	(0.025)	
Institute-specific linear pre-1961 trends	-0.005***	-0.110	-0.004***	-0.100	62,417
	(0.001)	(0.029)	(0.001)	(0.029)	
S.e. clustered at the institute level	-0.004**	-0.090	-0.003	-0.082	62,417
	(0.001)	(0.035)	(0.003)	(0.068)	
Augmented student–faculty ratio	-0.001***	-0.085	-0.001***	-0.072	62,417
	(0.000)	(0.023)	(0.000)	(0.022)	
Restricted Majors	-0.001	-0.001	-0.003***	-0.008	39,357
	(0.001)	(0.002)	(0.001)	(0.002)	
Nonrestricted Majors	-0.001	0.069	-0.002**	0.135	12,888
	(0.001)	(0.055)	(0.001)	(0.055)	

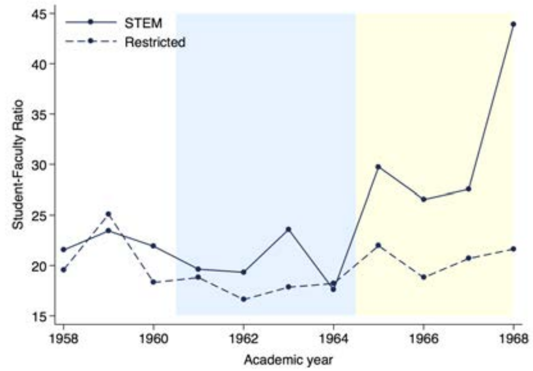
Notes: Each row shows coefficients and average effects estimated from a different regression. The specification is $g_{cmt} = \alpha + \beta \frac{E_{cm}^{pre}}{fac_{cm}^{pre}} + \sum_t \gamma_t A_t + \delta_1 \left(\frac{E_{cm}^{pre}}{fac_{cm}^{pre}} \times \text{Post } 61_t \right) + \delta_2 \left(\frac{E_{cm}^{pre}}{fac_{cm}^{pre}} \times \text{Post } 61_t \right) + \zeta Z_{cmt} + \psi_m + u_{cmt}$. g_{cmt} are standardized grades and $\frac{E_{cm}^{pre}}{fac_{cm}^{pre}}$ is the preexisting student–faculty ratio (average over 1958-64). Column (1) shows the estimator of δ_1, while column (3) shows the estimator of δ_2. "Average Effect" is the effect of an average change in the student-faculty ratio between 1961 and 1968: +24.3 in STEM (+84.3 in case of the augmented student-faculty ratio), +2.8 in restricted majors, and -61 in nonrestricted majors. "Low-achieving students" identifies the students who scored in the bottom quarter of their class in the high school exit exam. "Scientific type A students" graduated from type A schools, whose curriculum focus on the sciences. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: University transcripts of students who completed high school in Milan, Italy; 1958–1968.

Figure E1: Empirical Relationship Between The Quality of Education and The Student–Faculty Ratio

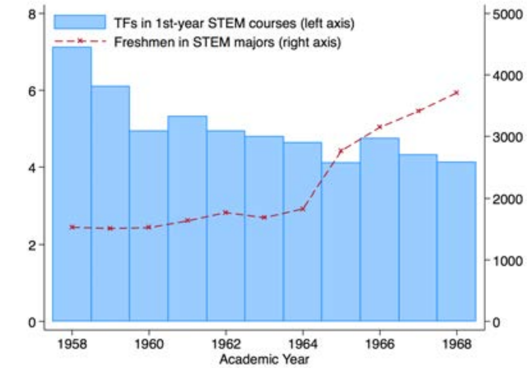


Notes: Each point is a course-academic year combination. Sample includes all compulsory courses in STEM majors with more than 10 students enrolled.
Sources: Annals of Università Statale di Milano, Politecnico di Milano, and Università Cattolica del Sacro Cuore; 1958-1968. University transcripts of students that completed high school in Milan, Italy; 1958–1968.

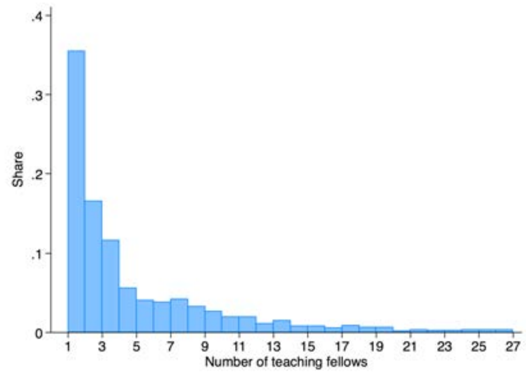
Figure E2: Trends of Students–Faculty Ratios



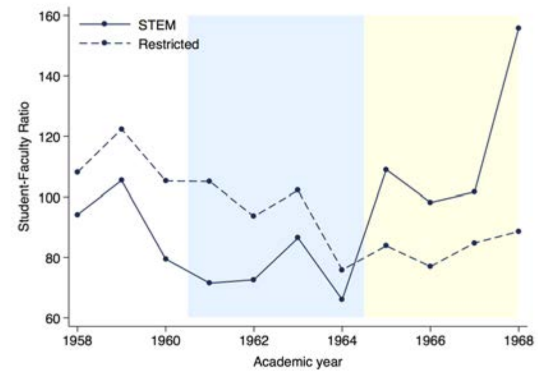
A. Mean Student–Faculty Ratio of First-Year Courses



B. Students Enrolled and Number of Teaching Fellows



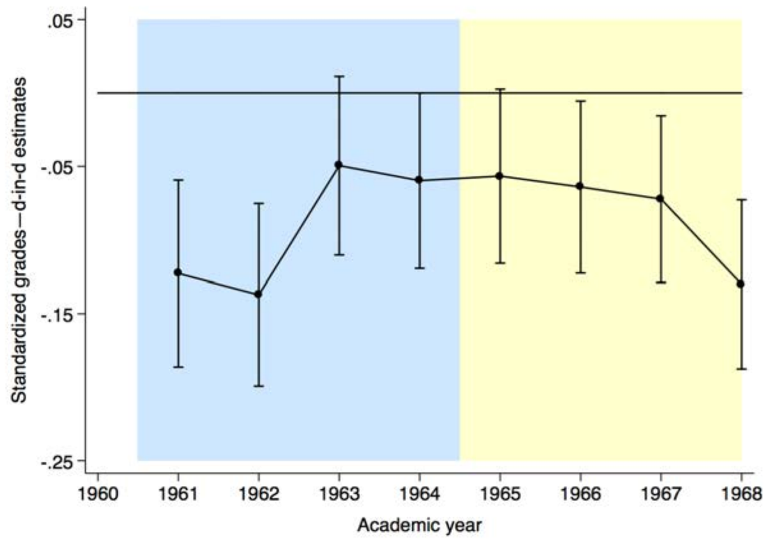
C. Distribution of Teaching Fellows in Compulsory STEM Courses



D. Mean Augmented Student–Faculty Ratio

Notes: The student–faculty ratio of course c in academic year t is the number of enrolled students who completed high school in Milan between 1958 and 1968 over the number of teaching fellows. The teaching fellows is the sum of the professor and the teaching assistants assigned to each course.
Sources: Annals of Università Statale di Milano, Politecnico di Milano, and Università Cattolica del Sacro Cuore; 1958–1968. University transcripts of students who completed high school in Milan, Italy; 1958–1968.

Figure E3: Quality of Education, Average Effect on Grades in STEM Majors



Notes: Effect on grades in STEM compulsory courses of an average decrease in the quality of education (+24.3 student–faculty ratio). The coefficients are computed from $g_{cmt} = \alpha + \beta \frac{E^{pre}_{cm}}{fac^{pre}_{cm}} + \sum_t \gamma_t A_t + \delta_t \left(\frac{E^{pre}_{cm}}{fac^{pre}_{cm}} \times A_t \right) + \zeta Z_{cmt} + \psi_m + u_{cmt}$, using data from STEM compulsory courses. g_{cmt} are standardized grades, and $\frac{E^{pre}_{cm}}{fac^{pre}_{cm}}$ is the preexisting student–faculty ratio (average over 1958–64). A_t is a set of academic-year fixed effects. Z_{cmt} is a set of student and course characteristics. The student variables include gender, high school fixed effects, a quadratic polynomial of age, high school exit score, the average score of high school classmates, a dummy for home-schooled students, a dummy for students that did not repeat a grade in high school, and major and university fixed effects. The course characteristics are the tenure and gender of the professor, a binary variable that identifies professors that are institute directors, and institute fixed effects. An institute is a group of homogeneous courses within a major. The blue shaded area denotes the first phase of the reform: between 1961 and 1964, enrollment of type B students in STEM majors was capped. The yellow shaded area denotes the second phase of the reform: after 1965, the enrollment cap was lifted. The bars represent 95 percent confidence intervals. The omitted academic years are 1958–1960.
Sources: University transcripts of students who completed high school in Milan, Italy; 1958–1968.

F Additional Results on Class Heterogeneity

To create the variable *Not in B* cv_{cm}, I use the institute of affiliation to determine whether a course was taught in type B schools. Unlike course titles, in fact, institutes have unambiguous denominations from which it is easy to infer the field of study. As an example, consider the engineering course "Analytical Mechanics" that studies a branch of mathematical physics and belongs to the institute of mathematical sciences. Based on its title, "Analytical Mechanics" could be misinterpreted as a course in applied mechanics, an area of expertise of type B students. Using the institute of affiliation, however, I correctly categorize this course as not being included in the precollegiate curriculum of type B students.

Table F1: List of Institutes in Type B Cv

Fields	Institutes in type B Cv	Institutes not in type B Cv
Agricultural science	"Chimica agraria", "Chimica organica", "Economia e politica agraria", "Idraulica agraria", "Meccanica agraria"	"Agronomia", "Anatomia e fisiologia degli animali domestici", "Coltivazioni arboree", "Entomologia agraria", "Fisiologia della nutrizione animale", "Industrie agrarie", "Ispezione degli alimenti di origine animale", "Istologia ed embriologia", "Microbiologia agraria", "Morfologia e fisiologia vegetale", "Patologia vegetale", "Scienze botaniche", "Scienze fisiche", "Scienze matematiche", "Tecnologie alimentari", "Zootecnica generale"
Engineering	"Chimica", "Chimica fisica, elettrochimica e metallurgia", "Chimica industriale", "Disegno generale", "Edilizia", "Elettrotecnica ed elettronica", "Elettrotecnica industriale", "Geodesia, topografia, e fotogrammetria", "Idraulica", "Ingegneria aerospaziale", "Ingegneria nucleare", "Macchine", "Meccanica", "Scienza e tecnica delle costruzioni", "Vie e trasporti"	"Costruzioni di ponti", "Fisica", "Fisica tecnica", "Matematica"
Sciences	"Chimica fisica", "Chimica generale ed inorganica", "Chimica industriale", "Chimica organica", "Topografia e cartografia"	"Fisiologia generale", "Geologia", "Igiene", "Istologia ed embriologia", "Mineralogia, petrografia e geochimica", "Paleontologia", "Pedagogia", "Scienze botaniche", "Scienze fisiche", "Scienze matematiche", "Zoologia"

Table F2: Grades of Type B students in STEM Courses

	(1)	(2)	(3)	(4)	(5)	Obs.
Type B only						
Not in B cv_c	-0.243***	-0.256***	-0.212***	-0.202***	-0.210***	9,857
	(0.020)	(0.022)	(0.024)	(0.024)	(0.022)	
Type B vs Type A						
Not in B cv_c x $TypeB_i$	-0.279***	-0.294***	-0.293***	-0.277***	-0.279***	63,522
	(0.022)	(0.022)	(0.022)	(0.021)	(0.021)	
Student controls	No	Yes	Yes	Yes	Yes	
Course controls	No	No	Yes	Yes	Yes	
Academic year FEs	No	No	No	Yes	Yes	
Student FEs	No	No	No	No	Yes	

Notes: Each cell shows the coefficient from a separate regression. Coefficient measure the average grades of type B students in university courses that were not included in their high school cv, compared with other courses. The first row shows coefficients from $g_{ict} = \alpha + \beta \textit{Not in B } cv_c + \zeta Z_{ict} + \psi_t + u_{ict}$, using data on type B only. g_{ict} are standardized scores in STEM compulsory exams. *Not in B* cv_c is 1 if a course is included in the high school cv of type B. Z_{ict} is a set of student and course characteristics and ψ_t are academic year fixed effects. The second row shows coefficients from $g_{ict} = \alpha + \beta \textit{Not in B } cv_c + \gamma \text{Type B}_i + \delta \textit{Not in B } cv_c \times \text{Type B}_i + \zeta Z_{ict} + \psi_t + u_{ict}$, using data on type A and B. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: University transcripts of students who completed high school in Milan, Italy; 1958–1968.

Table F3: STEM, Courses in the High School Cv of Type B Students

	In type B cv	Not in type B cv	Difference
	(1)	(2)	(3)
Tenured professor	0.261	0.433	-0.172**
Institute director	0.500	0.356	0.144
Female professor	0.152	0.212	-0.060
Grades (18-31)	22.864	23.444	-0.580***
Student-faculty ratio	18.469	17.906	0.563

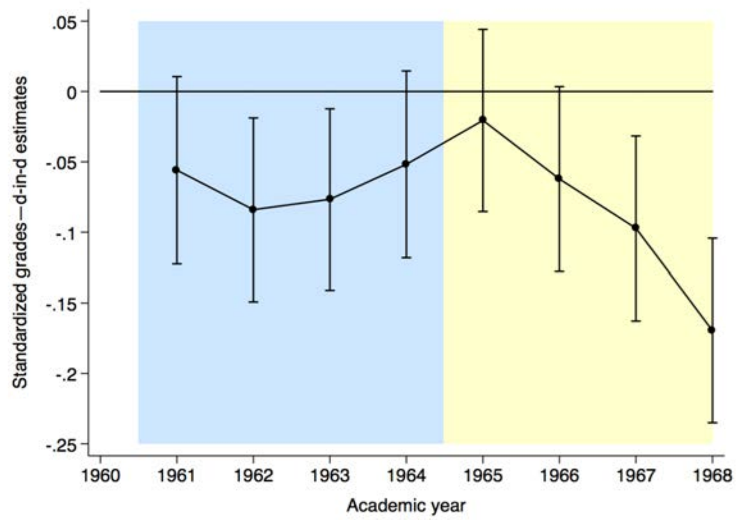
Notes: Only compulsory courses. "Not in type B cv" are courses not included in the curricula of type B high schools. "Grades" are non-standardized grades. "Tenured", "Institute director", "Female professor" are characteristics of the professor assigned to the course. Means computed for academic years 1958-1960. *** p<0.01, ** p<0.05, * p<0.1.
Sources: Annals of Università Statale di Milano, Politecnico di Milano, and Università Cattolica del Sacro Cuore. University transcripts of students who completed high school in Milan, Italy; 1958–1968.

Table F4: Class Heterogeneity, Additional Results and Robustness Checks

	Academic Years 1961–1964	Academic Years 1965–1968	Obs.
	(1)	(2)	(3)
Scientific type A students	-0.073**	-0.076**	39,154
	(0.030)	(0.030)	
Low-achieving students	-0.029	-0.084	11,848
	(0.053)	(0.053)	
Institute-specific linear pre-1961 trends	-0.118	-0.138*	53,651
	(0.076)	(0.076)	
S.e. clustered at the institute level	-0.067	-0.086*	53,651
	(0.043)	(0.044)	
No pre-collegiate ability	-0.073***	-0.088***	53,651
	(0.027)	(0.028)	

Notes: Each row shows coefficients estimated from a different regression. The coefficients are computed from $g_{ct} = \alpha + \beta Not\ in\ B\ cv_c + \sum_t \gamma_t A_t + \delta_1 \left(Not\ in\ B\ cv_c \times \text{Post } 61_t\right) + \delta_2 \left(Not\ in\ B\ cv_c \times \text{Post } 65_t\right) + \zeta Z_{cmt} + \psi_m + u_{cmt}$, using data from type A students who enrolled in STEM compulsory courses. g_{cmt} are standardized grades and *Not in B* cv_c is 1 if course c was not included in the curricula of type B schools.. Column (1) shows the estimator of δ_1, while column (2) shows the estimator of δ_2. "Scientific type A students" graduated from type A schools that focuses on the sciences. "Low-achieving students" identifies the students who scored in the bottom quarter of their class in the high school exit exam. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: University transcripts of students who completed high school in Milan, Italy; 1958–1968.

Figure F1: Class Heterogeneity, Effect on Grades of Type A in STEM Majors



Notes: Effect of an increase in the degree of class heterogeneity on STEM grades. The coefficients are computed from $g_{ct} = \alpha + \beta Not\ in\ B\ cv_c + \sum_t \gamma_t A_t + \sum_t \delta_t\,(Not\ in\ B\ cv_c \times A_t) + \zeta Z_{cmt} + \psi_m + u_{cmt}$, using data from type A students enrolled in STEM compulsory courses. g_{cmt} are standardized grades and $Not\ in\ B\ cv_c$ is 1 if course c was not included in the curricula of type B schools. A_t is a set of academic-year fixed effects. Z_{cmt} is a set of student and course characteristics. The student variables include gender, high school fixed effects, a quadratic polynomial of age, high school exit score, the average score of high school classmates, a dummy for home-schooled students, a dummy for students that did not repeat a grade in high school, and major and university fixed effects. The course characteristics are the tenure and gender of the professor, a binary variable that identifies professors that are institute directors, and institute fixed effects. The blue shaded area denotes the first phase of the reform: between 1961 and 1964, enrollment of type B students in STEM majors was capped. The yellow shaded area denotes the second phase of the reform: after 1965, the enrollment cap was lifted. The bars represent 95 percent confidence intervals. The omitted academic years are 1958–1960.
Sources: University transcripts of students who completed high school in Milan, Italy; 1958–1968.

G Income Adjustment

The problem of estimating age-cohort effects in a cross-section

After controlling for observable characteristics, the difference between the average income of type B students who completed high school in year b, before the policy implementation, and b', after the policy implementation, is the function of two elements: $E(y_b - y_{b'} \mid X) = f(A\, , \, B)$, where A are age effects, and B are cohort effects.

Since most of the students in the dataset were between 55 and 67 years old in 2005, there are two main age effects (A) at play. Older cohorts were more likely to be retired. In Italy, pensions were computed as a fraction of the last 10 wages. For this reason, pension earners received a lower income, compared with similar individuals that are still in the labor force. In addition, income of younger cohorts could still be on an increasing trajectory.

The cohort effects (B), instead, measure the income change due to the fact that one group of students completed high school in year b (before the policy), while the other group graduated in year b' (after the policy).

Figure G1 (Panel A) visualizes this problem. In one cross section, the difference between two observations at different ages can be caused by age effects only, cohort effects only, or a combination of the two. In this paper, I am interested in isolating cohort effects. To do so, I need to predict the income that individuals from different cohorts would earn at the same age (65 years old, the retirement age for men). As Figure G1 (Panel A) suggests, cohort and age effects cannot be disentangle in a single cross section. To circumvent this problem, I use out-of-sample observations from repeated cross-sections to estimate the age effects. However, moving from one cross-section to repeated cross-sections is not a solution in itself. In fact, this procedure adds a dimension ("period" or "survey year" effects) that is a linear combination of age and cohort (cohort = survey year - age): this means that it is impossible to observe individuals from the same cohorts at different ages in the same survey year. As a consequence, period-cohort-age effects cannot be estimated simultaneously. Figure G1 (Panel B) represents this problem graphically. In this case, repeated cross sections and assumptions on the structure of period-cohort-age effects are necessary.

Repeated cross-sections: description of the sample

I pool 11 waves of the Bank of Italy's Survey of Household Income and Wealth (SHIW) collected between 1991 and 2012. This representative dataset of the Italian population contains information on 245,184 individuals. I keep household heads and their spouses/partners (91,700 observations deleted), individuals born between 1930 and 1955 years old (-72,871 obs), with at least a high school diploma (-57,265 obs), and with positive income (-2,879 obs). This procedure leaves 20,469 observations. Table G1 shows the characteristics of the cohorts born between 1930 and 1950 in the survey years 2004 and 2006, compared with the characteristics of the high school graduates from Milan.[27] The SHIW sample has a higher share of females. This is due to the fact that the SHIW sample contains graduates from all high schools, including the female-oriented education schools, while the sample of high school graduates from Milan is focused on high schools that were either equally split between men and women (type A and C schools) or men-only schools (type B). On average, individuals from the SHIW sample are less likely to have a university degree and earned lower incomes. These differences could be due to the fact that Milan has higher returns to university education, compared with the rest of Italy. Unfortunately, the sample cannot be restricted to individuals living in the north of Italy (the geographical aggregation included in the dataset that is closer to Milan), because of small sample size. The average income of university graduates is not statistically different across the two dataset.

Procedure and assumptions to disentangle age and cohort effects

As stated previously, the age effects are mainly two. Older cohorts had a higher probability of being pension earners. The income of younger cohorts, instead, could still be increasing with age. I find that the first effect is larger than the second.

In the empirical analysis, I estimate these two parts separately, because the SHIW contains detailed information about pensions. The procedure uses several assumptions: (1) period effects can be captured by macroeconomic indicators (the unemployment rate observed in the survey year), (2) age effects are constant across cohorts, (3) age effects have a specific functional form. In the next section, I will show how results would change with different assumptions or just using non-adjusted income.

First, I estimate (1) the probability of being a pension earner, (2) the ratio of pensions to total income, and (3) the replacement rate (the ratio of pensions to last wage) as a function of age, gender, completed education, unemployment rate (u_t), and birth year fixed effects (B_b):

[27] This subsample is close (both in characteristics and in time) to the group of income earners in 2005 that completed high school in Milan between 1958 and 1968. To improve precision, however, the estimating sample is bigger, including also individuals born between 1951 and 1955 and more survey years. The characteristics of the full sample are reported in the last column of Table G1.

$$p_{abt} = F(\text{age}_{abt}\,,\ \text{age}^2_{abt}\,,\ \text{age}^3_{abt}\,,\ \text{male}_{abt}\,,\ \text{college}_{abt}\,,\ \text{age}_{abt}{\cdot}\text{male}_{abt}\,,\ \text{age}_{abt}{\cdot}\text{college}_{abt}\,,\ u_t\,,\ B_b), \tag{G1}$$

where F is the logit function for the probability of being retired, and linear for the remaining two dependent variables. I set the probability of retirement equal to 1 above age 70. As expected, the estimated probability of retirement decreases among younger cohorts (Appendix Figure G2). Male individuals with a university degree that completed high school in 1958 have a 90.3 percent probability of being retired, while similar individuals that completed high school in 1968 have a 31.5 percent probability. Conditional on education, women have a higher probability of being retired. In the Italian system, in fact, women could retire at 55 years old, five years before men. Conditional on gender, high school graduates have a higher probability of being retired, relative to university graduates. A higher investment in education induces individuals to stay longer in the labor market to recoup the initial investment in human capital. In addition, it can also increase productivity later in life.

Second, I estimate how income increases with age in the years before retirement. On the subsample of individuals who are not retired and below 65 years old, I estimate the following income equation

$$y_{abt} = \alpha + \beta \text{age}_{abt} + \sum_b \gamma_b B_b + \delta u_t + \epsilon_{abt} \tag{G2}$$

separately for men (1) with and (2) without a university degree, and women (3) with and (4) without a university degree. B_b are cohort fixed effects, while u_t is the unemployment rate in each survey year. The estimated $\hat{\beta}$ varies with gender and completed education. One additional year increases income by €786 for male university graduates and by €524 for female university graduates. The coefficients are smaller for high school graduates: one additional year increases income of male high school graduates by (€202) and of female high school graduates by (€91).

Using these estimates, I adjust the taxable income in 2005 according to the following formula:

$$\tilde{y}_b = \hat{\pi}_b \cdot \left[\frac{\hat{\%}_b \cdot y_b}{\hat{\text{replace}}_b} + (1 - \hat{\%}_b) \cdot y_b \right] + (1 - \hat{\pi}_b) \cdot \left[y_b + \hat{\beta} \text{age}_b \right], \tag{G3}$$

where $\hat{\pi}_b$ is the probability of being retired, $\hat{\%}_b$ is the ratio of pensions to total income, and $\hat{\text{replace}}_b$ is the replacement rate.

To test the validity of these estimates, I split randomly the SHIW dataset into two groups: a portion of the observations (75 percent) is employed to estimate the age effects, while the remaining part is used to validate the results. The predicted and actual means are close. For example, the average predicted probability of retirement is 33.7 percent, compared with an actual 34.5 percent of individuals in the sample being retired. The difference is significant at the 10 percent level, but is small. The average incomes predicted by equation (G2) are above the actual means, but the difference is not significant for men with and without a university degree. For women with a university degree, however, the actual income is on average €3,095 (14 percent) lower than predicted.

I winsorize the adjusted income in 2005 at the 2nd and 98th percentiles to limit the influence of outliers on the analysis. Figure G3 compares the average adjusted and unadjusted income by cohorts, both winsorized. The plot shows that the adjustment increased income for both older and younger cohorts, but the first effect prevailed. The results in the paper can be replicated qualitatively using unadjusted income.

Sensitivity analysis

In this section, I show how different assumptions on the structure of period-cohort-age effects would change the results. For comparison, I use the IV estimator of β_1 in equation (14), which measures the returns to a STEM education for type B students that enrolled after 1961 (Table G4). For each set of assumptions, I also plot the average adjusted income (Figure G4). This exercise shows that the different assumptions lead to results that are qualitatively similar.

A. Period effects do not exist

I estimate equations (G1) and (G2) excluding the unemployment rate. The results are virtually unchanged. The average adjusted income follows the same path and the adjustment is larger for older cohorts. The IV estimator of β_1 is very close to the baseline. It is negative (-0.258) but not statistically significant.

B. Normalization of period effects

Deaton and Paxson (1994) suggested a normalization for the period effects in which period dummies sum up to 1 and are orthogonal to a time trend. In practice, the normalization implies that any growth in income is attributed to age and cohort effects. I estimate equations (G1) and (G2) replacing the unemployment rate with these set of modified survey year dummies. The average adjusted income follows the same path. The IV estimator of β_1 is positive (0.079), but not statistically significant. The point estimate lies in the 95 percent confidence interval of the baseline estimate.

C. Quadratic age effects on income

In this case, I estimate equation (G2) including both a linear and quadratic age variable. All the other assumptions are unchanged, which means that I assume that period effects are captured by the unemployment rate and age effects are constant across cohorts. The quadratic age component in equation (G2) lowers significantly the income increase in the years that lead to retirement, compared with the baseline. This implies that the adjustment for pension earners, in this case, is predominant. Age-adjusted income of younger cohorts is very close to the non-adjusted incomes, while there is a large gap among older cohorts. The IV estimator of β_1 is smaller, compared with the baseline. The point estimate is negative (-1.940) and statistically significant.

D. Age effects change with cohorts

I estimate equations (G1) and (G2) for separate cohorts grouped in 5-year bins. This procedure allows age effects to differ across (groups of) cohorts. All the remaining assumptions are unchanged. In this case, income is almost stable in the years that lead to retirement. The IV estimator of β_1 is negative (-0.886) and statistically significant.

Table G1: Summary Statistics of the SHIW Sample

	SHIW 1930-1950	HS Graduates from Milan	Difference	SHIW 1930-1955
	(1)	(2)	(3)	(4)
Birth year	1943.38	1943.20	0.18	1946.10
Males	0.64	0.75	-0.11***	0.61
University graduates	0.25	0.35	-0.10***	0.24
University graduates—males	0.24	0.33	-0.09***	0.24
Income	29,508	35,348	-5,840***	28,824
Income—males	34,703	39,730	-5,027***	34,372
Income—university graduates	42,038	43,403	-1,365	41,584
Income—male university graduates	53,162	49,838	3,326	52,416
Income—HS graduates	25,334	30,650	-5,316***	24,770
Income—male HS graduates	29,017	34,304	-5,827***	28,823

Notes: The table shows the characteristics of the cohorts born between 1930 and 1950 in the survey years 2004 and 2006. They represent a subsample of a larger estimating sample; details on the sample construction can be found in appendix G. Column (2) shows the characteristics of the dataset of students that completed high school in Milan between 1958 and 1968. The last column shows the summary statistics for the whole estimating sample from the SHIW; this includes individuals with a high school diploma born between 1930 and 1955. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sources: Bank of Italy's SHIW; 2004–2006 waves.

Table G2: Marginal Age Effect on Income

	University Degree	High School Diploma
Males	786.30***	202.34***
	(191.77)	(58.11)
Females	524.43***	91.25**
	(92.69)	(39.78)

Notes: The table shows the $\hat{\beta}$ from the equation (G2), separately estimated on men (1) with and (2) without a university degree, and women (3) with and (4) without a university degree. *** p<0.01, ** p<0.05, * p<0.1.
Sources: Bank of Italy's SHIW; 1991-2012 waves.

Table G3: Income Adjustment, Actual and Predicted Age Effects

	Actual (1)	Predicted (2)	Difference (1) - (2)
Retired workers			
Probability of being retired ($\hat{\pi}_b$)	0.345	0.337	0.008*
Ratio of pensions to income ($\hat{\%}_b$)	0.742	0.723	0.019***
Replacement rate ($\widehat{\text{replace}}_b$)	0.752	0.751	0.001
Income of employed workers			
Male with university degree	43,892	46,078	-2,186
Male with HS diploma	28,694	28,885	-191
Female with university degree	21,556	24,651	-3,095***
Female with HS diploma	17,114	17,948	-834***

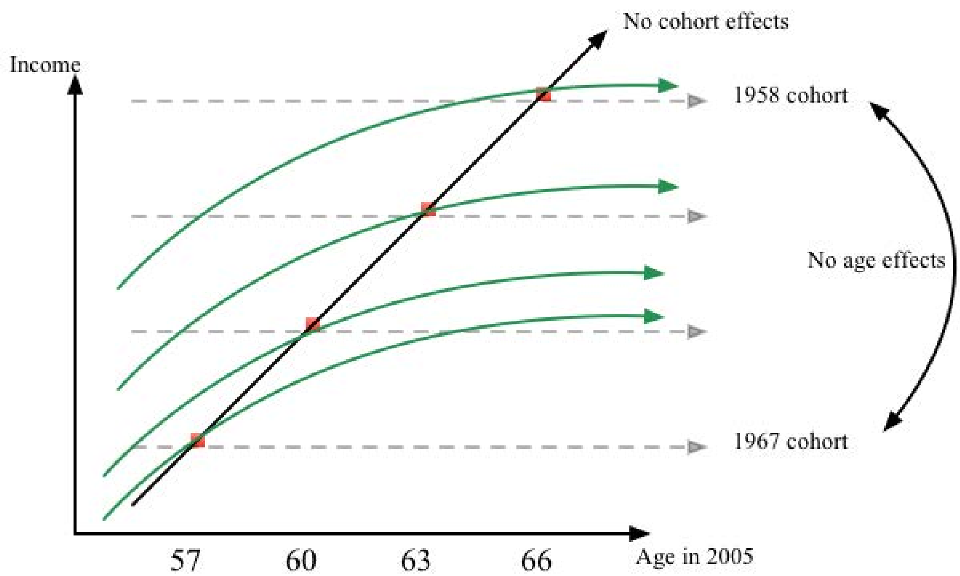
Notes: For a random subsample (25 percent) of the SHIW dataset, the table compares the actual means to those predicted with the coefficients estimated using the remaining 75 percent of the sample. The probability of being retired, the ratio of pensions to total income, and the replacement rate are estimated using equation (G1). Income is estimated using equation (G2). *** p<0.01, ** p<0.05, * p<0.1.
Sources: Bank of Italy's SHIW; 1991-2012 waves.

Table G4: Sensitivity Analysis of Assumptions on Age-Cohort-Period Effects

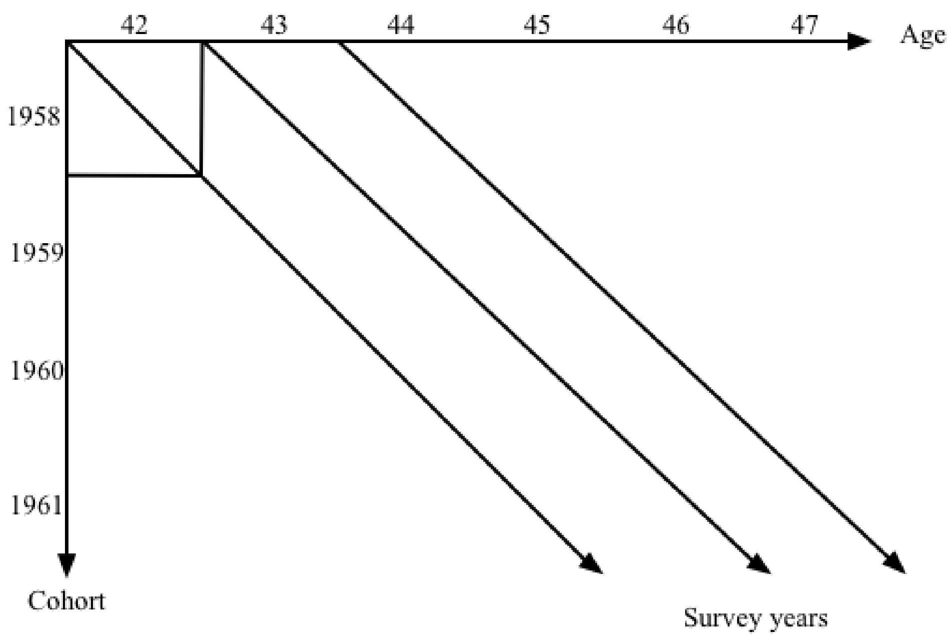
Baseline	Not Adjusted
-0.238	-1.208***
(0.238)	(0.457)
A. No Period Effects	B. Normalization
-0.258	0.079
(0.237)	(0.230)
C. Quadratic Age Effects	D. Changing Age Effects
-1.940***	-0.886***
(0.472)	(0.198)

Notes: The table shows the IV estimator of β_1 in equation (14), which measures the returns to a STEM education for type B students that enrolled after 1961. Different estimators use different assumptions about the structure of age - cohort - period effects, as described in section G. *** p<0.01, ** p<0.05, * p<0.1.
Sources: School data of type B students that completed high school in Milan, Italy; 1958-1968.

Figure G1: Identification of Age and Cohort Effects



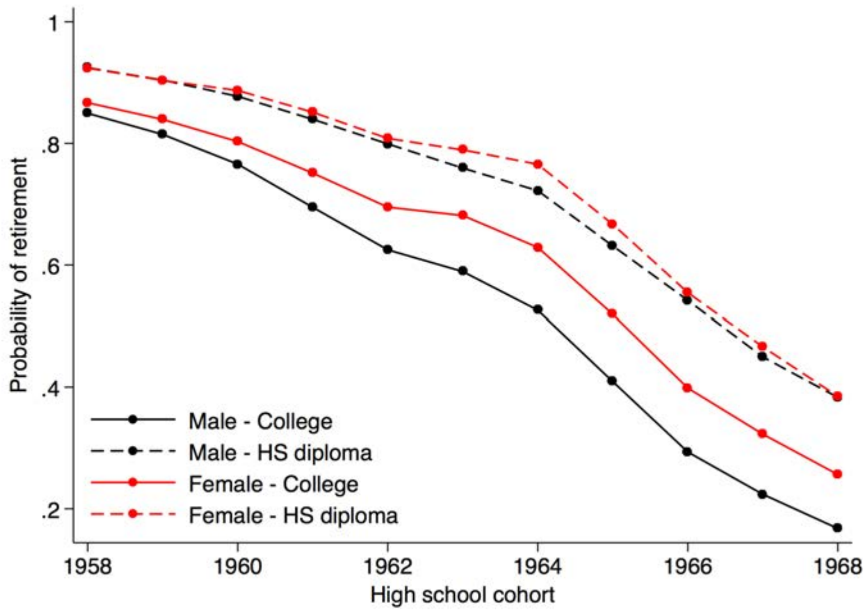
A. Age and Cohort Effects in One Cross Section



B. Age, Cohort and Year Effects in Repeated Cross Sections

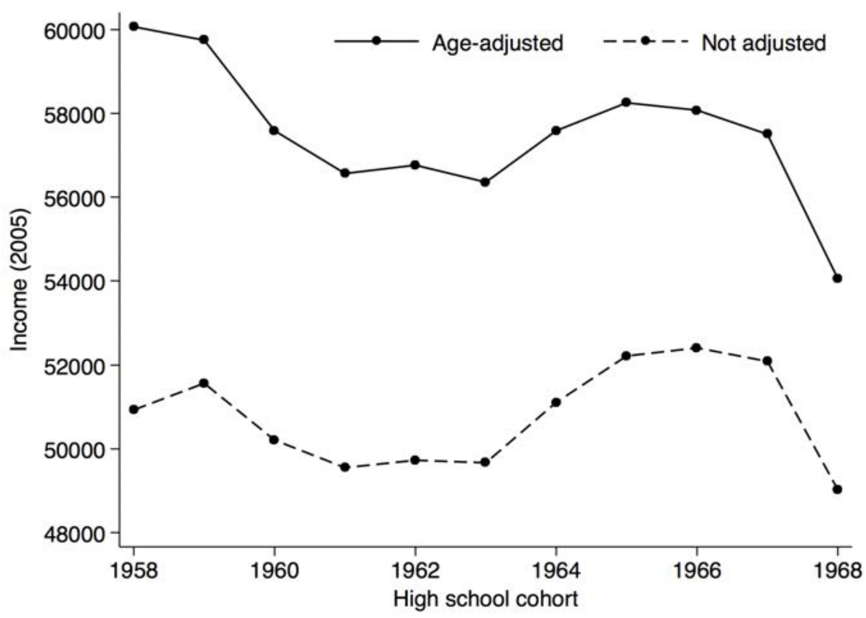
Notes: Panel A shows how age and cohort effects are confounded in a single cross-section. Panel B shows that in repeated cross-sections there is an additional dimension (period or survey year), which is a linear combination of cohort and age. Therefore, the simultaneous estimation of all three is not possible with additional assumptions.

Figure G2: Estimated Probability of Retirement



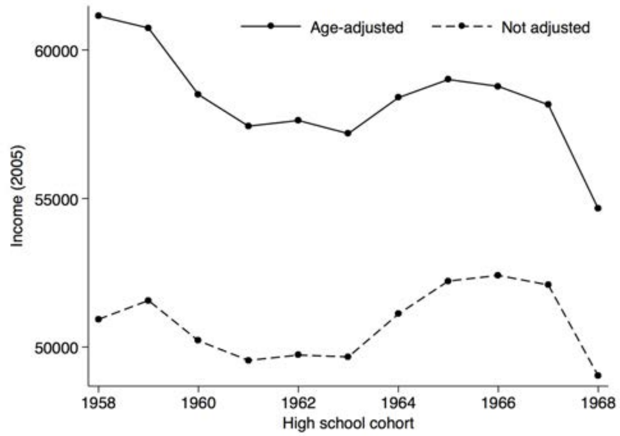
Notes: Estimated probability of retirement by completed education and gender. Probability of retirement is estimated by equation (G1).
Sources: Bank of Italy's SHIW; 1991-2012 waves.

Figure G3: Age-adjusted vs. Unadjusted Income

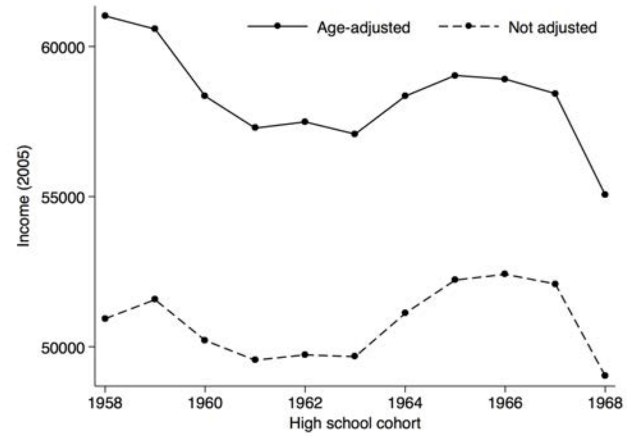


Notes: The figure shows means by high school cohorts. Income is adjusted to account for age effects using equation (G3).
Sources: Bank of Italy's SHIW; 1991-2012 waves.

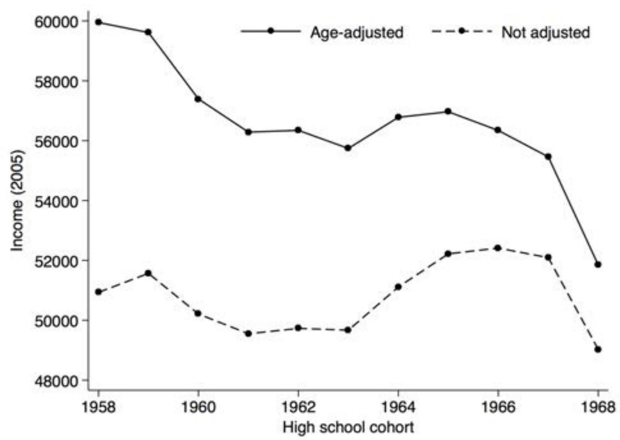
Figure G4: Age-adjustment: Different Assumptions on Age-Cohort-Period Effects



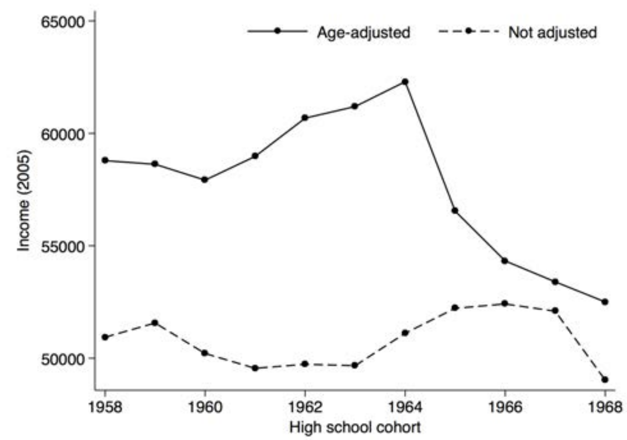
A. No Period Effects



B. Normalized Period Effects



C. Quadratic Age Effects



D. Varying Age Effects

Notes: The figure shows average adjusted and unadjusted income by cohorts, using different assumptions on the structure of age-cohort-period effects. The different assumptions are described in section G.

H Additional Results on Income

H.1 Type B Students

Instead of relying exclusively on inter-cohort comparisons, I can adopt a difference-in-differences approach:

$$\text{outcome}_{it} = \alpha + \beta \mathrm{B}_i + \sum_t \gamma_t Y_t + \sum_t \delta_t [\mathrm{B}_i \times Y_t] + \zeta X_{it} + \eta_{it} \tag{H1}$$

where B_i is a binary variable that identifies type B students who are substantially more "treated" within each cohort. There are multiple ways to identify more and less treated individuals within a cohort.

In the text, I compared type B students who scored in the top quartile of their high school class with type B students who scored in the bottom quartile. The results suggest that within each post-reform cohort the type B students with higher aptitude completed more education relative to the type B students with lower aptitude, but did not earn more in the long run. Alternatively, I use cohorts who completed high school after 1961 to predict the propensity to enroll in STEM majors as a function of observable characteristics. I adopt a leave-one-out estimator in order to avoid overfitting bias (Abadie, Chingos and West, 2014). Then, I compare the type B students who have a high propensity to enroll in STEM (top third) with the type B students who have a low propensity (bottom third). "High-propensity" type B students completed more education after 1961, but did not earn more (Appendix Table H2).

In addition, I compare type B with type A students. In the pre-reform cohorts, type A students were 56.2 percent more likely to enroll in university, 49.8 percent more likely to receive a university degree, and earned 39 percent more in 2005 relative to type B students. The enrollment gap fell by 15.9 percentage points from 1965 to 1968. Consequently, the graduation gap fell by 3.2 percentage points from 1965 to 1968. The preexisting income gap, however, remained unchanged. After the policy implementation, then, type B students invested more in education relative to type A students, but did not earn higher incomes in the long-run. This finding is another piece of evidence, which does not simply rely on inter-cohort comparisons, suggesting that the policy did not increase the earnings of type B students.

Furthermore, I can compare type B and C students, the graduates from commercial schools. These two groups of students had access to the same university programs up until the policy implementation (mainly business economics and statistics). Then, the reform allowed type B students to enroll in university STEM majors, but did not have any direct effect on type C students. This comparison leads to similar conclusions. In the pre-reform cohorts, type C students were 27.2 percent more likely to enroll in university, 4.8 percent more likely to graduate, and earned 22.5 percent more in 2005. The enrollment gap fell by 22 percentage points from 1965 to 1968. The graduation gap decreased by 12.5 percentage points from 1965 to 1968. Nevertheless, the income gap remained unchanged. Although surprising, these results are consistent with the previous finding of no income effect on type B students. After the policy implementation, type B students completed much more education (relative to type C students) to the extent that the pre-reform gap in university enrollment had reversed by 1968. However, they did not earn higher incomes in the long-run.

If I restrict the sample to male students, the main findings hold. In only one case (male type B vs male type C students), I find that the income gap fell (in favor of type B students), but this result is driven exclusively by a marked income decrease among type C students.

Table H1: Cohort Effects, Education and Income of Type B Students

	University Enrollment		University Graduation		Log Income
	Coeff.	Marginal Effects	Coeff.	Marginal Effects	Coeff.
	(1)	(2)	(3)	(4)	(5)
Post 61	0.386***	0.090***	0.499***	0.059***	0.091**
	(0.070)	(0.016)	(0.111)	(0.013)	(0.042)
Post 65	0.965***	0.225***	1.271***	0.150***	0.015
	(0.065)	(0.014)	(0.101)	(0.012)	(0.040)
Male	0.444**	0.103**	0.317	0.037	0.952***
	(0.190)	(0.044)	(0.227)	(0.027)	(0.166)
HS exit score	0.324***	0.075***	0.438***	0.052***	0.112***
	(0.024)	(0.006)	(0.028)	(0.003)	(0.013)
HS class score	0.196**	0.046**	0.040	0.005	0.030
	(0.079)	(0.018)	(0.106)	(0.012)	(0.046)
Home schooled	-0.269**	-0.063**	-0.662***	-0.078***	-0.131*
	(0.105)	(0.025)	(0.171)	(0.020)	(0.068)
Non-repeater	0.499***	0.116***	1.004***	0.118***	0.192***
	(0.092)	(0.021)	(0.172)	(0.020)	(0.052)
Mean, 1958-60	0.292	0.292	0.082	0.082	10.447
HS fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	8,791	8,791	8,791	8,791	7,381

Notes: Coefficients and marginal effects are estimated from outcome$_{it} = F(\alpha + \beta_1\text{Post } 61_t + \beta_2\text{Post } 65_t + \gamma X_{it})$, where outcome$_{it}$ is a dummy for university enrollment in columns (1) and (2), a dummy for university graduation in columns (3) and (4), and log income in column (5). The function F is logit for university enrollment and graduation and linear for log income. Post 61_t is equal to 1 for the cohorts that completed high school between 1961 and 1964, while Post 65_t is 1 for the cohort that graduated between 1965 and 1968. The omitted category is represented by the cohorts that graduated between 1958 and 1960. X_{it} is a set of student characteristics that include gender, the high school exit score, the average exit score of the high school classmates, a dummy for home-schooled students, a dummy for students that did not repeat a grade in high school, and high school fixed effects. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: School data of type B students who completed high school in Milan, Italy; 1958–1968. Income tax returns in 2005.

Table H2: Difference-in-Differences, Education and Income of Type B

	University Enrollment		University Graduation		Log Income	
	1961-64	1965-68	1961-64	1965-68	1961-64	1965-68
	(1)	(2)	(3)	(4)	(5)	(6)
Type B						
High- vs Low-achieving	0.071*	0.060	0.079***	0.097***	-0.000	0.079
	(0.040)	(0.038)	(0.029)	(0.028)	(0.108)	(0.103)
High- vs Low-propensity	0.001	0.121***	0.031	0.136***	0.054	0.160
	(0.037)	(0.035)	(0.024)	(0.024)	(0.102)	(0.098)
Type B vs Type A						
All	0.036**	0.159***	-0.000	0.032**	0.102*	-0.074
	(0.017)	(0.016)	(0.015)	(0.015)	(0.057)	(0.054)
Males	0.049***	0.199***	0.009	0.061***	0.150**	-0.055
	(0.018)	(0.017)	(0.017)	(0.017)	(0.061)	(0.058)
Type B vs Type C						
All	0.062***	0.220***	0.046***	0.125***	0.136**	0.059
	(0.020)	(0.019)	(0.013)	(0.013)	(0.066)	(0.066)
Males	0.086***	0.260***	0.053***	0.131***	0.147**	0.284***
	(0.024)	(0.024)	(0.016)	(0.016)	(0.067)	(0.070)
Mean, 1958-60	0.292	0.292	0.082	0.082	10.447	10.447
HS fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

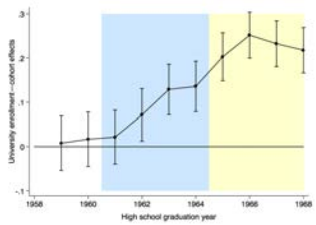
Notes: Each row contains difference-in-differences estimators from three regressions, using different subsamples of students. Coefficients are estimated from outcome$_{it} = \alpha + \sum_t \beta_t Y_t + \gamma \mathrm{B}_i + \delta_1[\mathrm{B}_i \times \text{Post } 61_t] + \delta_2[\mathrm{B}_i \times \text{Post } 65_t] + \zeta X_{it} + u_{it}$. The dependent variable is university enrollment in columns (1) and (2), university graduation in columns (3) and (4), and log income in columns (5) and (6). Odd columns show estimates of δ_1, while even columns of δ_2. Y_t is a set of cohort effects. Post 61 is equal to 1 for the cohorts who completed high school between 1961 and 1964, while Post 65 is 1 for the cohort who graduated between 1965 and 1968. The omitted category is represented by the cohorts who graduated between 1958 and 1960. X_{it} is a set of student characteristics that include gender, the high school exit score, the average exit score of the high school classmates, a dummy for home-schooled students, a dummy for students that did not repeat a grade in high school, and high school fixed effects. "High- vs Low-achieving" compares type B in the top quartile and bottom quartile of the ability distribution. "High- vs Low-propensity" compares type B with highest (top third) and lowest propensity to enroll in STEM majors. The predicted propensity is computed from observable characteristics and the enrollment choices of post-1961 cohorts. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Sources: School data of type B students who completed high school in Milan, Italy; 1958–1968. Income tax returns in 2005.

Table H3: IV, Returns to University Education for Type B Students

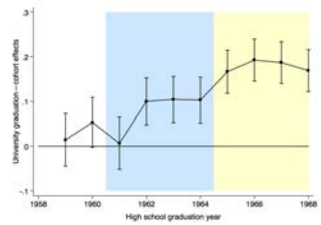
	OLS	IV	Confidence Interval (IV)	F-Stat	Obs.
	(1)	(2)	(3)	(4)	(5)
IV: Post 61_t, Post 65_t					
All Type B	0.337***	-0.225	[-0.691, 0.240]	101.96	7,381
	(0.032)	(0.237)			
Controls for region of residence in 2005	0.330***	-0.262	[-0.724, 0.201]	101.93	7,381
	(0.032)	(0.236)			
1^{st} Quartile of Ability	0.305***	-1.349**	[-2.582, -0.116]	23.39	2,561
	(0.070)	(0.629)			
2^{nd} Quartile of Ability	0.221***	0.189	[-0.714, 1.092]	26.65	1,807
	(0.068)	(0.461)			
3^{rd} Quartile of Ability	0.473***	-0.046	[-0.876, 0.784]	28.54	1,512
	(0.065)	(0.424)			
4^{th} Quartile of Ability	0.347***	0.079	[-0.636, 0.795]	26.11	1,501
	(0.058)	(0.365)			
IV: Post 65_t					
All Type B	0.282***	-0.458*	[-0.872, 0.089]	177.85	7,381
	(0.034)	(0.247)			
IV: Separate cohort dummies (1961-1968)					
All Type B	0.337***	-0.109	[-0.532, 0.314]	30.35	7,381
	(0.032)	(0.216)			
IV: Post $61_t \times 4^{th}$Q, Post $65_t \times 4^{th}$Q					
High- vs Low-achieving	0.356***	0.726	[-1.306, 2.758]	5.05	4,062
	(0.046)	(1.037)			
STEM degrees; IV: Post 61_t, Post 65_t					
All Type B	0.337***	-0.231	[-0.666, 0.205]	191.80	7,381
	(0.037)	(0.222)			

Notes: The table shows estimations of equation (14). Column (1) shows the OLS estimator of β_1. Column (2) shows the IV estimator of β_1 and column (3) the corresponding 95 percent confidence interval. University education is instrumented by two dummy variables: Post 61_t is 1 for the cohorts who completed high school between 1961 and 1965, while Post 65_t is 1 for the cohorts who completed high school between 1965 and 1968. The quartiles of ability are computed from the distribution of high school scores. The next-to-last section restricts the sample to type B in the top and bottom quartile of the ability distribution. The instrumental variable is the interaction of Post 61_t and Post 65_t with a dummy that is 1 for students in the top quartile of the ability distribution. The last section replaces degree$_{it}$ with STEM degree$_{it}$, which is 1 for students that received a STEM university degree. The F-Statistic tests for the joint significance of the instruments. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sources: School data of type B students that completed high school in Milan, Italy; 1958-1968. Income tax returns in 2005.

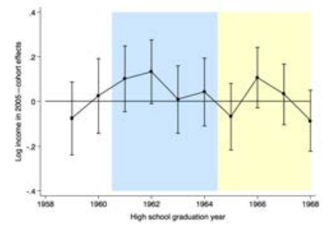
Figure H1: Cohort Effects, Education and Income of Type B Students



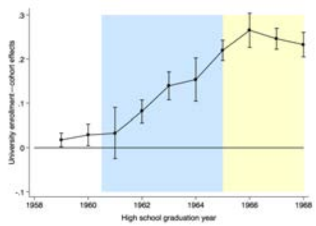
A. Enrollment—No precollegiate ability



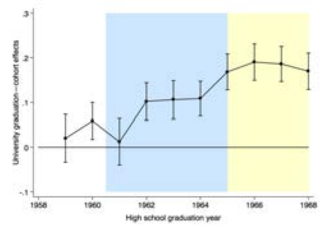
B. Graduation—No precollegiate ability



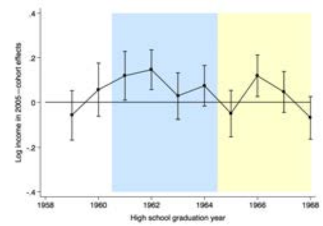
C. Log Income—No precollegiate ability



D. Enrollment—Clustered SEs



E. Graduation—Clustered SEs

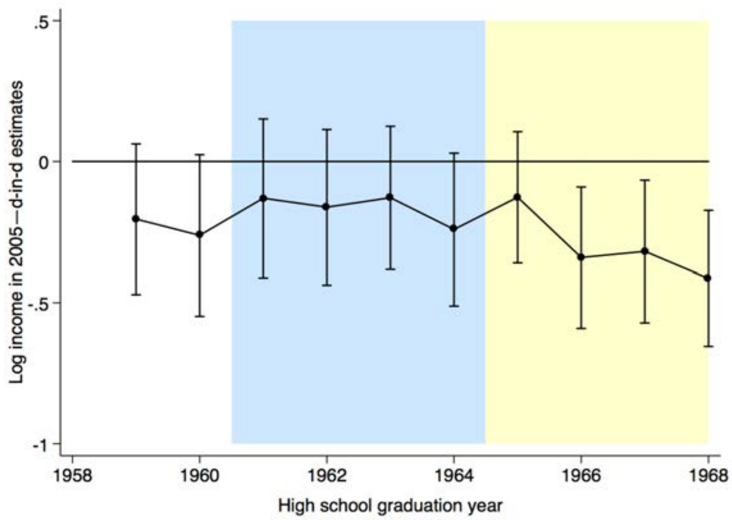


F. Log Income—Clustered SEs

Notes: Robustness checks of the marginal cohort effects on education choices and income of type B students from equation (13). The bars represent 95 percent confidence intervals. The first row shows marginal cohort effects computed without including measures of precollegiate ability on university enrollment (Panel A), on university graduation (Panel B), and on log income in 2005 (Panel C). The second row shows marginal cohort effects with standard errors clustered by high school and graduation year on university enrollment (Panel D), on university graduation (Panel E), and on log income in 2005 (Panel F).
Sources: School data of type B students who completed high school in Milan, Italy; 1958–1968.

H.2 Type A Students

Figure H2: Type A, Income Premium of STEM Education



Notes: The marginal cohort effects are estimated from $log(\text{income}_{ct}) = \alpha + \sum_c \beta_c \mathrm{C}_c + \sum_t \gamma_t Y_t + \sum_t \delta_t[\mathrm{STEM}_c \times Y_t] + \zeta X_{ct} + u_{ct}$ on all type A students. Y_t is a set of fixed effects for high school graduation year, and C_c are major fixed effects (STEM, restricted major, and nonrestricted major, and no university as omitted category). STEM_c is equal to 1 if a student enrolled in STEM. X_i is a set of student characteristics that includes gender, high score exit score, the average score of high school classmates, dummy for home schooled, and dummy for non-repeaters. STEM majors are engineering, physics, mathematics, biology, geology, natural science, chemistry, and agricultural science. The blue shaded area denotes the first phase of the reform: between 1961 and 1964, enrollment of type B students in STEM majors was capped. The yellow shaded area denotes the second phase of the reform: after 1965, the enrollment cap was lifted. The bars represent 95 percent confidence intervals. The omitted academic years are 1958–1960.
Sources: School data and income in year 2005 of type A students who completed high school in Milan, Italy; 1958–1968.

I Cost-Benefit Analysis

In this section, I examine whether the costs of keeping quality of education and class heterogeneity at their pre-reform level would have surpassed the benefits for STEM students. Initially, I compute the discounted present value at age 25 (age of university graduation) of the income losses caused by lower human capital. The average long-run income of STEM students in pre-reform cohorts was equal to €57,632. I assume that the effects of lower quality of education (-2.4 percentage points) and higher class heterogeneity (-2.2 percentage points) do not change with age, that income increases by 1.8 percent every year, and that the discounting rate is equal to 10 percent.[28] Based on these assumptions, the yearly income loss at age 25 was €678 due to lower quality of education and €621 due to higher class heterogeneity. Their discounted present values over the lifetime (in this case, until retirement at age 65) were €7,891 and €7,233 respectively.

At this point, I estimate the costs of hypothetical actions that the Italian government could have taken to prevent lower human capital. I only consider the costs of hiring more teaching fellows (professors and assistants). Although this procedure involves less arbitrary assumptions, it could underestimate the total costs. In addition, I will use the empirical model described in section 4 to base my analysis. This will lead to some simplifications. For example, the model suggests that the only way to keep class heterogeneity fixed is to establish a strict tracking system in which type A and B students do not interact. This, however, ignores any potential benefit of more diverse classes. Moreover, I will assume that quality of education depends linearly on the student-faculty ratio.

First, I consider a scenario in which the government intends to keep class heterogeneity unchanged but allows the quality of education to vary. For each STEM course, the government creates a separate section for type B students and assigns only one professor to it. In doing so, the government hires the minimum amount of faculty to keep type A and B students in separate classes. For a degree with 25 university courses, this plan costs €690,875 or €5,074 per student.[29] In this case, the benefits of keeping class heterogeneity fixed (€7,233) are greater than the costs (€5,074) for the government.

Second, I consider an alternative scenario in which the government wants to keep the quality of education constant but allows class heterogeneity to increase. Specifically, type B students join the lectures attended by type A, but the government hires new teaching assistants to keep the student-faculty ratio fixed. In Milan, the average STEM major had 4.81 teaching fellows per course, 71.91 students per year before 1965, and 64.25 new type B students per year after 1965. To keep the student–faculty ratio constant, the government needs to hire 4.3 assistants per course. In a degree with 25 courses, this plan costs €1,666,406 or €12,239 per student.[30] In this case, the benefits of keeping quality of education fixed (€7,891) are lower than the costs (€12,239) for the government.

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[28] The yearly increase of income is estimated using SHIW data on males with a university degree (appendix G). The discounting rate is computed as the average discount rate in Italy between 1958 and 2003 (available from the Bank of Italy at this link).

[29] All the figures are in 2005 €. The average annual salary of a professor in 1965 was equal to €27,635 (Supplemento alla Gazzetta Ufficiale 108, 30/04/1965, Tabella C). In Milan, the average STEM major had 136.16 students enrolled in each academic year.

[30] The average annual salary of an assistant in 1965 was equal to €15,510 (Supplemento alla Gazzetta Ufficiale 108, 30/04/1965, Tabella C; Marbach, Rizzi and Salvemini 1969).

Third, I consider the case in which the government intends to keep both the quality of education and class heterogeneity fixed. The plan consists in creating two sections for each course, one for type A and one for type B students, in which the student–faculty ratios do not exceed the pre-reform level. The government still needs to hire 4.3 new teaching fellows per course: in this case, however, 1 professor responsible for the teaching and 3.3 new assistants per course. In a major with 25 courses, this plan costs €1,969,531 or €14,465 per student. In this last case, the benefits of keeping both the quality of education and class heterogeneity fixed (€15,124) are slightly higher than the costs (€14,465) for the government.

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