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Do Reforms Aimed at Reducing Time to Graduation Work? Evidence from the Italian Higher Education System

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Abstract

This paper examines the impact of a reform aimed at expediting graduation times in Italian universities by reducing the number of exams students must pass in order to graduate. Using eventstudy estimates that leverage the reform's staggered implementation, we find that this policy led to an increase in on-time graduation rates but also resulted in a decreased probability of employment one-year post-graduation. However, this negative effect reverses into a positive one in the medium run. We show that these patterns are explained by students using the time gained from earlier graduation to pursue additional educational qualifications.

JEL classification

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Keywords

higher education, policy evaluation, time to graduation, labor outcomes

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1 Introduction

Completing a college degree within the normal completion time is the exception rather than the rule. For instance, in 2016, only 44% of first-time U.S. bachelor's degree recipients completed their degree within four years (NCES, 2019; Bound et al., 2012).

This fact has been recognized by policymakers (e.g., U.S. Department of Education, 2016) but has received limited attention from economists. Related studies mostly focus on the determinants of increased time to graduation (e.g., Garibaldi et al., 2012) or completion rates (Bound et al., 2010; Denning et al., 2022) and highlight the costs associated with staying an extra year in college: more tuition to be paid and delayed entry in the labor market (Dynarski, 2016; Oreopoulos and Petronijevic, 2013; Bound et al., 2012). However, a longer time to graduate could also have positive effects on labor market outcomes, particularly if students use that extra time to increase their human capital while still enrolled in post-secondary education. It is therefore hard to know, ex-ante, whether policies aimed at reducing time to graduate—which are often debated by policymakers, see for instance Johnson (2011)—ultimately lead to improved labor market prospects.

This paper studies the consequences of an Italian reform signed into law in 2004 that attempted to reduce time to graduation by harmonizing the course offerings across all universities. Before this reform, all students had to obtain a predetermined number of credits to get a degree. This number of credits was common across all universities (120 credits) but each institution was free to decide the number of courses (and thus exams) students must pass to obtain the necessary credits. As a result, the number of exams required for the same degree could differ significantly between universities. For example, it was possible to observe one student having to pass 20 exams to obtain their degree whereas another student in a different university could obtain the same degree after passing only 12 exams.

The reform forced all universities to offer at most 12 courses to obtain the necessary 120 credits. This was achieved by consolidating the course offerings. If before the reform a student would have to pass two, somewhat similar, exams to obtain 10 credits, now after the reform the

student has to pass only one exam to obtain the 10 credits.[1] This could speed up graduation time because in Italy university students decide when to take an exam. The pre-reform student might therefore decide to take one exam in the fall session and the other exam in the following spring session thus potentially delaying their graduation date.

We study the effects of this reform using data for (close to) the universe of Italian university students. We have access to administrative records on the time to graduation, detailed field of study, grades, and other academic information of these students. We can also measure their labor market outcomes via follow-up surveys conducted 1, 3, and 5 years after graduation. The staggered implementation of the reform across universities and majors enables a policy evaluation of the reform via a difference-in-differences approach where changes in outcomes for cohorts of students affected by the reform are compared to changes in outcomes of cohorts of students still enrolled under the pre-reform regime.

We find that the reform successfully streamlined course structures and significantly increased on-time graduations, with immediate post-reform cohorts experiencing a 4.2 percentage point increase in on-time graduation rates (from a pre-reform rate of about 39%). This effect is even stronger in later cohorts, with increases up to 12.7 percentage points. However, the reform significantly reduced the likelihood of being employed one year after graduation and also had a negative, but noisier, effect on earnings. These negative effects fade in the following years, and five years after graduation, the earnings for post-reform cohorts are slightly higher (around 4%).

In terms of mechanisms, the reform did not impact the quantity, quality, and attrition rate of enrolled students, nor did it lead to a decline in the human capital accumulated by students. Post-reform cohorts have similar grades, propensity to study abroad, and completion times for their final thesis as pre-reform cohorts. In addition, the reform did not significantly impact the likelihood of students having job-related experiences, such as internships, while still in school. Finally, in majors where the reform had no impact on the probability of graduating on time, we

[1] This reform is therefore different from the one analyzed by Arteaga (2018). She studies a reform that reduces the required credits for an Economics degree at Universidad de Los Andes (Colombia) up to 20%. The reform studied here instead does not alter the number of credits—remaining fixed at 120 before and after the reform, with each credit in Italy typically corresponding to 8 hours of classroom instruction—so it did not entail any decrease in instructional time, but only a change in the number of exams a student must pass in order to obtain those 120 credits.

observe no change in post-graduation labor market outcomes. Therefore, the primary effect of the reform on short-term labor market outcomes seems to come from its enhancement of students' probability of graduating on time.

But how can a reform that caused an increase in on-time graduation—a positive signal used by employers to screen between job market candidates (Aina and Casalone, 2020)—lead to *worse* labor market outcomes in the year after graduation? We rule out general equilibrium explanations based on employers decreasing the weight assigned to on-time graduation as a signal to evaluate job market candidates. Instead, our findings suggest that the observed labor market effects are more likely due to changes in graduates' job search behavior.

Compliers of the reform—students who graduated on time under the new regime but would have been delayed in the pre-reform regime—appear to use the additional time gained from the earlier graduation to invest in obtaining additional educational qualifications (e.g., a professional master degree). We find no effect on substitution towards leisure, as the fraction of students who are not searching for a job one year after graduation and are not in education does not change after the implementation of the reform. Therefore, students in the post-reform cohorts are less likely to seek employment immediately after graduation because they choose to pursue further educational qualifications, such as one-year professional programs. This behavior helps account the lower employment rate found one year after graduation for post-reform cohorts but higher earnings observed five years after graduation, relative to pre-reform cohorts. The last finding suggests that the additional education obtained after completing the degree generates long-term financial benefits for post-reform cohorts.

2 Institutional Background

2.1 The Italian Higher Education System

Italy's higher education system follows a "3+2" model, implemented as part of the Bologna Process. A first-level degree (*laurea triennale*, henceforth LT) with a statutory duration of three years is followed by a second-level degree (*laurea specialistica/magistrale*, henceforth LM) with a

statutory duration of two years.[2] Most Italian students with an LT continue their studies and thus enroll in an LM. For instance, in 2014, about 77% of the students graduating from an LT declared that they wanted to continue their studies (Alma Laurea, 2014). This figure had been remarkably stable over time—among students obtaining an LT in 2007, it was equal to 78% (Alma Laurea, 2007).

2.2 The Reform

The Bologna Process established that students graduating from an LM must obtain 120 academic credits (180 for LT). However, it also gave significant autonomy to universities in designing their course offerings.[3] As a result, one university could require students to take 20 or more courses to obtain the necessary 120 credits, whereas another might only require 12 courses.

Many have observed (e.g., Stefani and Zara, 2009) that taking 20 courses instead of 12 often leads to delayed graduation. Unlike the U.S., in Italy, students are not required to take the exam right after a course ends and can decide when to take it. Moreover, they have the option to reject an unsatisfactory grade and retake that exam in the next available exam session.[4] This system, where passing at least one exam per course is necessary to earn credits, combined with the flexibility in exam scheduling and the option to retake exams, can significantly extend the time students take to graduate.

Concerned about the potential impact of this fragmented course offering on graduation times, the Italian government enacted Act 270/04 (henceforth "the reform").[5] This reform imposed a cap on the number of courses required for graduation without, however, changing the number of credits. The cap was fixed at 20 in an LT (for a total of 180 credits) and 12 in an LM (for a total of 120 credits).[6] Importantly, these new dispositions applied only to new enrollees. Students who

[2]For some degrees, most prominently in law and medicine, there is a single cycle with nominal length equal to five (law) or six (medicine) years, the "*lauree a ciclo unico*".

[3]When enrolling in either an LT or LM, students immediately declare their major (e.g., engineering) and field of study (e.g., mechanical engineering).

[4]There are typically three exam sessions, one in the winter, summer, and fall.

[5]The rules detailing the implementation of the reform were described out in DM 16/03/2007.

[6]The reform also facilitated the transition to a second-level degree by allowing students to enroll in an LM during the academic year and separating the accumulation of credits across LT and LM (see chapter 1.3.2 of Stefani and Zara, 2009).

enrolled before the enactment of new regime could finish their degree under pre-reform rules.[7] The law only required the universities to complete the reform process by the academic year 2011, resulting in the reform's staggered implementation across both universities and majors.

2.3 Implications of the Reform on Course Offerings, Graduation Time, and Labor Market Outcomes

Table 1 provides qualitative evidence on how course offerings were changed. The table shows the list of courses required to graduate with a degree in economic science from the University of Tor Vergata—a large public university in Rome—before and after the reform. Post-reform, the number of courses required to graduate dropped from 20 to 12, while the total number of credits remained the same.

The re-structuring induced by the reform appears to follow a "clustering rule": two previously separated courses (e.g., growth theory and development economics) were now merged into one (called "development"). As a result, a student under the new regime would only need to pass one qualifying exam instead of two to obtain the necessary credits. Therefore, while the post-reform student might just take this exam, say, in the fall exam session, the pre-reform student might instead decide to take one exam in the fall session and the other exam in the following spring session, thus delaying their graduation.

However, whether this reform was successful in reducing graduation time is ultimately an empirical question (e.g., the post-reform student can instead decide to take the now longer and potentially more complex exam on development in the spring as opposed to the fall). Even more uncertain is an *ex ante* evaluation of the reform's impact on labor market trajectories. The changes to the course structure induced by the reform—as summarized in Table 1—can directly impact students' human capital and thus labor market outcomes. Moreover, the reform's direct effect on graduation timing could significantly impact post-graduation labor market outcomes, with effects that are challenging to predict *ex ante*. While employers might view delayed graduation

[7] Although students can switch between regimes, this possibility may be complicated and costly (Stefani and Zara, 2009). Thus, the fraction of "switchers" is small, and we drop these few cases when building our final estimation sample.

as a negative signal when assessing a candidate's potential in the labor market, this delay could also indicate a student's greater investment in human capital.

Table 1: Changes in the Course Structure Induced by the Reform

University of Rome - Tor Vergata - LM Degree in Economic Science

Pre Reform		**Post Reform**	
Name of the Exam	No. of Credits	Name of the Exam	No. of Credits
Game Theory	5	Game Theory	5
Information Economics	5	Information Economics	5
Economic History	5	Economic History	5
Mathematical Methods for Economics	5	Mathematical Methods	10
Optimization	5	- Mathematics for Economics	
Public Finance	5	- Optimization	
Macroeconomics	5	Macroeconomics	5
International Economics	5	International Economics	5
Statistical Methods for Economics	5	Statistical Methods	10
Econometrics	5	- Statistics	
Growth Theory	5	- Econometrics	
Development Economics	5	Development	10
Monetary Economics	5	- Growth Theory	
International Monetary Economics	5	- Development Economics	
Competition Law	5	International Monetary Economics	5
Corporate Law	5	Law of Economics Activities	10
Administrative Law	5	- Competition Law	
International Accounting	5	- Corporate Law	
Finance and Governance	5	Accounting	10
Management of Service Firms	5	- International Accounting	
		- Finance and Governance	
		Management of Service Firms	5
		Extra Activities	11
Final Thesis	20	Final Thesis	24
Total No. of Credits	120	Total No. of Credits	120
Total No. of Exams	20	Total No. of Exams	12

Notes: This table lists the courses that a student enrolled in the field of study "Economic Science" in the University of Rome Tor Vergata has to perform if enrolled under the old and the new regime (introduced in the academic year 2009-2010). The plan of study for the student under the pre-reform assumes that the extra exam corresponds to economic history. The plan of study for the student under the new regime assumes that the student takes as optional exam Game theory. The source for this table is the student service of the University of Tor Vergata (Department of Economics and Finance), see https://economia.uniroma2.it/cdl/biennio/clese/piano-di-studi.

3 Data

Our data come from the Alma Laurea (AL) consortium. In 2014, 91% of the Italian college graduate population got their degree from a university that belongs to the AL consortium. The AL data combine three sources: (i) administrative data provided by the universities (e.g., duration of studies, reform status, field of study, university, and year of enrollment); (ii) additional student demographics (e.g., province of residency, parents' education); and (iii) follow-up surveys one, three, and five years after graduation on labor market outcomes.

3.1 Estimation Sample

Our core estimation sample includes 221,336 students who graduated with an LM between 2007 and 2014, had non-missing values for several key baseline characteristics (e.g., information on whether the student is studying under the new reform), and responded to all follow-up surveys on their labor market outcomes conducted one, three, and five years post-graduation.[8] Similarly to Meluzzi (2024), we focus on LM graduates because (i) most students completing an LT enroll in an LM, (ii) we have information on labor market outcomes post-graduation for a large set of individuals graduating from an LM (whereas for most LT graduates, this information is not readily available as the majority would still be in school) and (iii) we find that the reform had very little effect on the probability to enroll in a LM (Figure 5), suggesting that the selection of students that decide to continue their studies was not affected by this policy change.[9]

Because the AL samples students who eventually graduate, we do not observe students who enrolled but did not complete their degree, either because they are still enrolled in school or because they dropped out.[10] To assess whether the reform impacted attrition rates, we combine the AL data with administrative data from the Italian Ministry of University and Research (MUR), which provides counts of all students who enroll in a given year-major-university combination.

[8] Response rates were 88% (1-year), 78% (3-year), and 70% (5-year), with a 58.7% rate for all surveys combined. The reform did not affect response rates (Figure A.1). High response rates are likely encouraged by AL's role as a major job board where students can upload their CV for free to be screened by firms (Bagues and Labini, 2009).

[9] In a study conducted after ours, Aina et al. (2024) examine the impact of the reform on LT students enrolled in STEM fields at a single university in Northeast Italy, and find improved labor market outcomes. By contrast, we study LM students across all fields of study and almost the universe of Italian universities.

[10] This provides another reason for focusing on LM students as the attrition rates in LMs are approximately half of those observed for students enrolling in a first-level degree (see Torrini, 2013).

3.2 Summary Statistics

Table 2 provides summary statistics for our sample. Around 41% of students graduate under the new regime established by the reform. The table shows minimal differences between pre- and post-reform students in terms of pre-determined characteristics. For instance, age at enrollment and grades from the LT do not appear to change systematically between pre- and post-reform cohorts, suggesting that the reform did not systematically change the composition of students.

Table 2: Summary Statistics

	Regime Status		Graduation Time		
Variable	Pre-Reform	Post-Reform	On-time	1yr Late	2yrs Late or more
Share	0.59	0.41	0.52	0.34	0.14
Background Characteristics					
Age at Enrollment	24.13	24.13	24.15	23.85	24.72
	(4.63)	(4.46)	(5.02)	(3.72)	(4.51)
Female	0.58	0.60	0.60	0.57	0.56
Foreign	0.01	0.02	0.01	0.01	0.02
Scholarship	0.22	0.22	0.23	0.23	0.18
High School Grade	85.88	85.48	86.23	85.57	84.19
	(12.15)	(12.20)	(12.22)	(12.03)	(12.21)
Grade (LT)	101.55	101.07	101.99	100.87	100.19
	(9.01)	(9.30)	(9.22)	(8.80)	(9.39)
Scientific HS	0.43	0.43	0.44	0.43	0.40
Letters HS	0.16	0.15	0.15	0.16	0.15
STEM Field	0.32	0.30	0.29	0.34	0.34
Same Province	0.46	0.45	0.45	0.46	0.49
Parent's College	0.28	0.29	0.29	0.28	0.26
Total Number of Students	221,336				
Number of Fields x Universities	1,733				
Share of Never-takers	.068				
Share of Always-takers	.027				
Share of Compliers	.906				

Notes: This table provides summary statistics for the sample of students who graduated with a second-level degree "Laurea Magistrale" (LM) in the Alma Laurea data between the years 2007-2014 and who responded to all post-graduation surveys conducted by Alma Laurea 1, 3, 5 years after graduation. The first row reports the shares corresponding to the variable indicated in each column. For instance, 0.59 corresponds to the fraction of students in our sample who graduated under the pre-reform regime. "Grade (LT)" is the final grade obtained by the student when graduating from the first level degree, denoted as LT (*Laurea Triennale*). "Same province" is an indicator of whether the student's resident address is in the same province as the university that they attend. Standard deviations are in parenthesis.

Nearly 58% of the pre-reform students are women, entering at an average age of 24. About 1.3% are international students, 43% attended a scientific high school, 16% attended a humanities high school, and 28% have parents with a college degree. These figures are very similar to those for the post-reform sample. The table also shows the averages of key covariates according to when a student graduates. Around half of the observed students graduate on time, and they tend to have higher prior grades, come from a more favorable socio-economic background (as measured by parents' education), and are less likely to have graduated from a STEM field.

Table 3: Summary Statistics - Labor Market Outcomes

	Years After Graduation		
Variable	1yr	3yrs	5yrs
Labor Market Condition			
Employed	0.72	0.84	0.86
Additional Information			
Permanent	0.25	0.46	0.62
Temporary	0.47	0.38	0.30
No Contract	0.06	0.03	0.02
Full Time	0.79	0.82	0.84
Monthly Wage	1051.79	1216.65	1383.61
	(504.28)	(517.66)	(556.85)
Total Number of Students	221,336		
Number of Fields x Universities	1,733		

Notes: This table provides summary statistics on the labor market outcomes of students who graduated with a second-level degree "Laurea Magistrale" (LM) in the Alma Laurea data between the years 2007-2014 and who responded to all post-graduation surveys conducted by Alma Laurea 1, 3, 5 years after graduation. Standard deviations are in parenthesis. All statistics regarding type of contract, wage, full time, and months to find job are reported only for students who are employed.

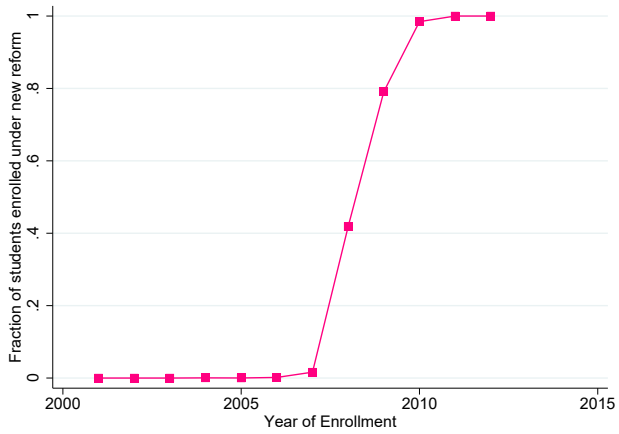
Table 3 shows descriptive patterns of the labor market outcomes one, three, and five years after graduation for the sample. Immediately after graduation, 72% of graduates are employed: 25% have permanent positions, 47% work with a temporary employment contract, and 79% are employed full time. Moreover, the average monthly wage (conditional on working) for graduates

is slightly above 1,000 euros, consistent with recent figures on job market prospects for younger and more educated generations in Italy (Rosolia and Torrini, 2007). Wages, employment shares, and the percentage of students with a permanent employment contract tend to increase as the years pass following graduation.

4 Research Design

Whether a student is covered by the reform depends on their year of enrollment, major, and university. Figure 1 illustrates the fraction of students registered under the new reform for a given year of enrollment in an LM. Most universities adopted the new reform during the 2008–2009 biennium. In line with central government requirements, by 2011, nearly all students in the AL database were studying under the new regime.

Figure 1: Roll-out of the Reform



Notes: This graph shows, for a given year of enrollment in a LM degree, the fraction of students who are registered under the new reform.

The reform's staggered implementation can be leveraged in an event-study framework as follows:

$$y_i = \alpha_{f(i),u(i)} + \lambda_{u(i),c(i)} + \sum_{j=a}^{b} \beta_j R^j_{f(i),u(i),c(i)} + X_i^\top \gamma + r_i, \tag{1}$$

where y_i represents an outcome of interest of student i, $c(i)$ denotes of their year of enrollment,

$u(i)$ represents their university, and $f(i)$ denotes their field of study. Therefore, $\lambda_{u(i),c(i)}$ controls for year of enrollment $\times$ university fixed effects, while $\alpha_{f(i),u(i)}$ controls for field of study $\times$ university fixed effects.[11] X_i includes a set of student demographics used to increase our estimates' precision.[12] Standard errors are clustered at the university level (there are 64 universities in our estimation sample).

The term $R^j_{f,u,c}$ is an event-study indicator, i.e., $R^j_{f,u,c} = E_{f,u}\mathbb{1}\{c = c^*_{f,u} + j\}$, where $E_{f,u}$ is a dummy for whether the field of study f in university u implemented the reform, and $c^*_{f,u}$ represents the year in which the reform was implemented in a particular field by university u.[13] The coefficients of interest, β_j, for $j \geq 0$, capture the effects of the reform under the assumption that differences in outcomes between cohorts that enrolled before or after the implementation would have remained constant in the absence of the policy. One can gauge the plausibility of this parallel trends assumption by comparing the relative changes in outcomes of the cohorts of students who enrolled in different years in the pre-reform regime, i.e., by evaluating the pre-trends coefficients β_j for $j < 0$ in Equation (1).

To deal with potential violations of the parallel trends assumption, we use the estimates of β_j for $j < 0$ to construct a linear time trend based on data from pre-reform cohorts only. We then report deviations of β_j from this linear time trend, extrapolated to post-reform years, to assess any breaks in outcomes following the reform's implementation (for a similar approach, see Dustmann et al., 2022). To assess the robustness of the results to alternative assumptions about how outcomes would have evolved for pre- and post-reform cohorts without the reform, we report the bounds from the "honest approach" to parallel trends outlined by Rambachan and Roth (2023).[14]

[11] The staggered adoption across both universities and majors/fields of studies within the same university allows us to flexibly control for university-by-cohort of entry fixed effects. Regressing the reform enactment year for field of study f in university u on university fixed effects yields an R^2 of about 0.5, suggesting considerable within-university variation in timing across fields within the same university.

[12] These controls include gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high school, and an indicator for whether the province of residence is the same as the province of study.

[13] About 7% of the field by universities in our data are only observed in the pre-reform regime. Around 2% are only observed in the post-reform regime, and the remaining 91% switched at some point from the pre-reform to the post-reform regime.

[14] In particular, we report results after allowing the slope of the pre-trend to change by an amount M across consecutive periods.

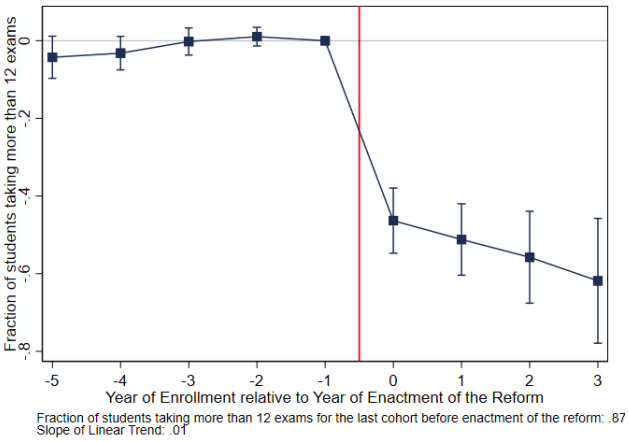
5 The Impact of the Reform

5.1 Number of Exams Taken and On-Time Graduations

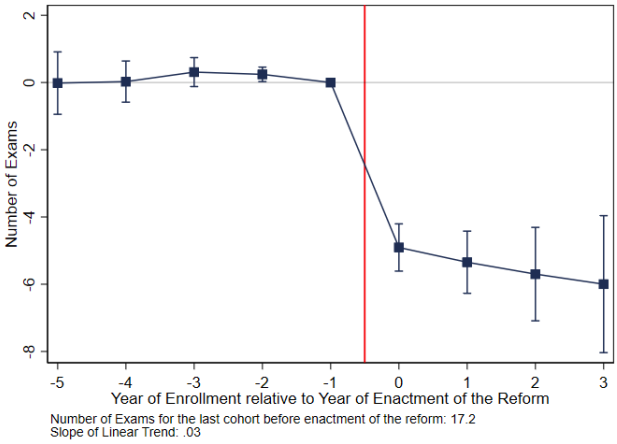
We begin by examining the impact of the reform on the number of exams taken by pre-reform and post-reform cohorts. Figure 2 displays estimates from Equation (1) for two different outcomes: an indicator of whether students took more than 12 exams (panel (a)), and the total number of exams taken by students (panel (b)).[15]

Figure 2: The Impact of the Reform on the Number of Exams

(a) Taking more than 12 Exams



(b) Number of Exams



Notes: This figure reports estimates from Equation (1). In panel (a), the outcome is an indicator for whether the student took more than 12 exams. In panel (b), the outcome is the total number of exams taken. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year prior to the enactment of the new regime. The vertical line indicates the first entry cohort that studies under the new reform. Below each graph, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Two key observations stand out. First, pre-reform cohorts show a stable trend in the number of exams taken, indicating that the reform's introduction was largely unanticipated by students and institutions alike. Second, the reform is linked to significant changes in course structure, as evidenced by the substantial reduction in the number of exams. Specifically, for the first cohort under the new regime, there is a notable decrease of approximately 55 percentage points in the

[15] Appendix Table A.1 reports event estimates at $j = 0$ and $j = 3$ from Equation (1) on the probability of taking more than 12 exams and on the number of exams. Appendix Figure A.2 shows the sensitivity analysis based on Rambachan and Roth (2023).

proportion of students taking more than 12 exams—a 63% reduction from the 87% share of students taking more than 12 exams observed in the last pre-reform cohort. Additionally, there is an average drop of roughly 5 exams from a baseline average of 17.2 exams. This suggests that the reform successfully streamlined the course structure.

Figure 3 shows the reform's impact on graduation rates. Panel (a) provides estimates on the proportion of students graduating on time, defined as those completing their studies within two years of initial enrollment. Panel (b) reports the results on the proportion graduating within three years, while panel (c) provides estimates on graduation rates (i.e., number of students graduating divided by number of students enrolling).[16] Differences in graduation rates seem to trend positively in the years leading up to the implementation of the reform. To account for these trends, a linear time trend estimated from the pre-reform data and extrapolated into subsequent years is depicted as a dashed line to the left of each panel. The deviations between actual coefficient estimates and this linear trend are shown in the graphs to the right of each panel.

We find that the reform sharply increased the share of on-time graduates. For the first cohort of students enrolling in the new regime, the share of on-time graduates increases by 4.2 percentage points. For the last observed cohort of post-reform students, the increase is higher, around 12.7 percentage points (the fraction of on-time graduates in the last pre-reform cohort is 39%). When we consider students graduating within three years from initial enrollment, the graduation rate increases from 2.8 to 6.3 percentage points, depending on the cohort analyzed.[17] Moreover, the reform had no significant effect on attrition rates, as shown in Figure 3, panel (c). This finding simplifies the interpretation of the results on labor market outcomes (presented in the next section) since the AL collects data on labor market outcomes conditional on graduation.

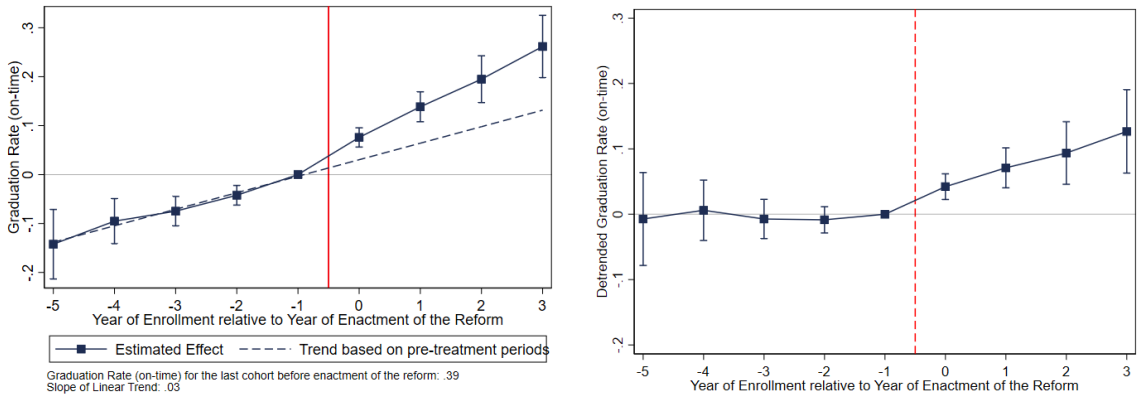
To summarize, the empirical evidence suggests that the reform led to a large restructuring and harmonization of Italian universities' course offerings. While this change did not impact *whether* students graduated, it did impact *when* they graduated. The rate of on-time graduation increased significantly after the reform, consistent with its initial intent (Stefani and Zara, 2009).

[16] Appendix Table A.2 reports estimates at $j = 0$ and $j = 3$ from the event studies shown in Figure 3.

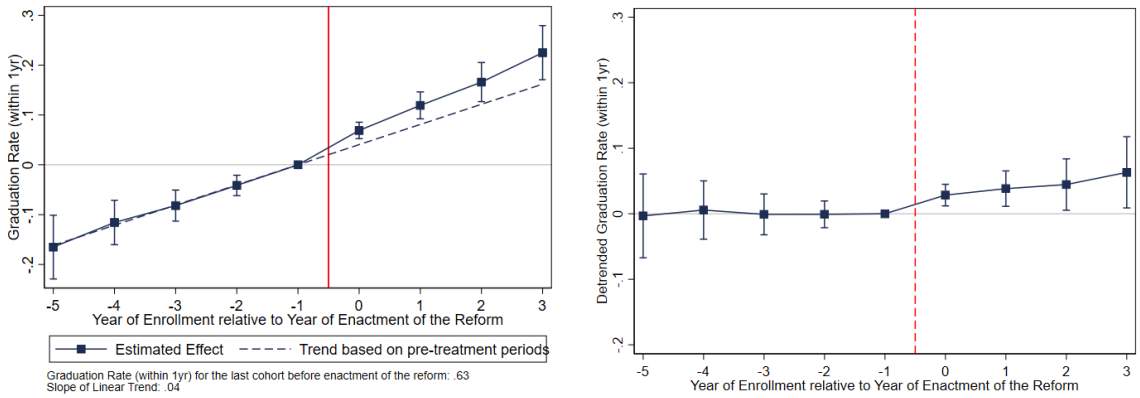
[17] Figure A.3 reports estimates from the honest approach to parallel trends, proposed by Rambachan and Roth (2023). The average impact of the reform on-time graduation rate becomes insignificant for a value of $M = 0.01$, which corresponds to a $\pm 33\%$ deviation per cohort from the linear trend found in the pre-reform era.

Figure 3: The Impact of the Reform on Time to Graduation and Graduation Rates

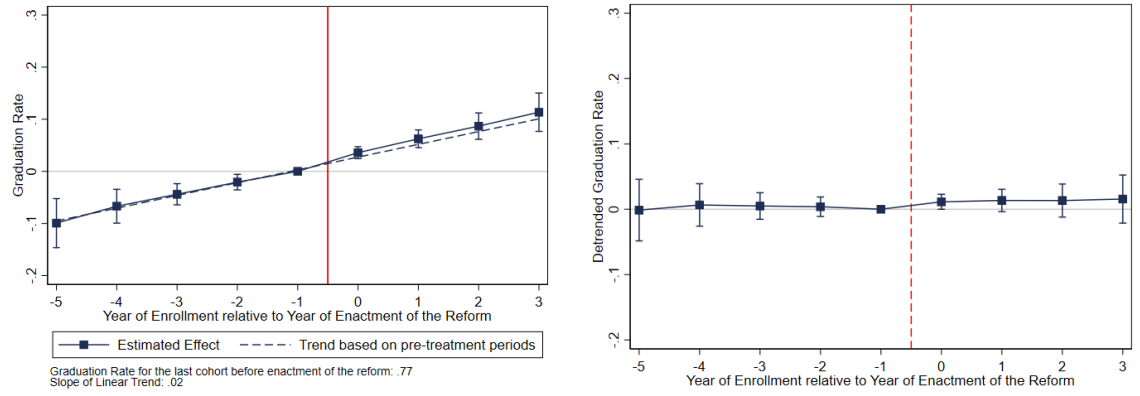
(a) Fraction of Students Graduating in Two Years or Less (On-Time Graduation)



(b) Fraction of Students Graduating in Three Years or Less



(c) Graduation Rate



Notes: This figure reports estimates from a version of Equation (1) collapsed at the university × major × year of enrollment that controls for university by major and university by year of enrollment fixed effects. Majors are groups of fields of study defined by Italian National Statistic Institute (ISTAT) and include: Scientific, Chemistry-Pharmaceutical, Geo-Biological, Medical, Engineering, Architecture, Agricultural-Veterinary, Economics-Statistics, Political-Social, Law, Literary, Education, Psychological, and Physical Education. In panel (a), the outcome is the fraction of students graduating on-time, i.e. the fraction of students that obtained their degree within the nominal duration of an LM degree (which is two years). In panel (b), the outcome is the fraction of students that graduated in three years. In panel (c), the outcome is the fraction of students graduating. For all these outcomes, the denominator is represented by the number of students *enrolling* in a given year × major × university. Estimates are weighted by the number of enrolled students. On the left, the dashed lines represent the linear trend estimated using pre-reform event-study coefficients only which is then extrapolated to post-reform cohorts. On the right, we report the deviations of the event-study coefficients from this linear time trend. Figure A.3 reports estimates from the "honest approach" to parallel trends proposed by Rambachan and Roth (2023) to probe the robustness of our finding to alternative assumptions about different trends between pre-reform and post-reform cohorts that would have emerged in the absence of the reform. Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph on the left, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

5.2 Labor Market Outcomes

Figure 4 reports the results on labor market outcomes. All event study coefficients reported in this figure are computed net of any linear trend that is found among the pre-event cohorts— Figure A.4 reports the unrotated event-study coefficients along with the estimated linear trend. Panel (a) shows that exposure to the reform substantially decreased the probability of employment one year after graduation, with an impact ranging from 1.7 percentage points one year post-reform to almost 10 percentage points three years later, compared to a baseline employment probability of 72%. A similar pattern is also found when evaluating the earnings of students one year after graduation, albeit the estimates are somewhat noisier.[18] However, we do not find these negative effects five years post-graduation.[19] If anything, the post-reform cohorts seem to have slightly better labor market outcomes (both in terms of employment and earnings). Figure A.5 displays the sensibility of our estimates significant to alternative assumptions on the evolution of outcomes between treated and control cohorts in the absence of the reform. The qualitative conclusion is that the reform negatively impacted the employment of post-reform cohorts 1 year after graduation but raises their earnings 5 years after graduation is fairly robust to significant deviations from the corresponding linear trends.

Our dataset measures labor market outcomes only after graduation has occurred. To estimate the overall impact of the reform on earnings—without conditioning on graduation—we calculate earnings after nominal graduation (for a student enrolled in year t, the nominal graduation would occur in year $t+2$) assuming that students earn zero euros while in school. The results are displayed in the last two panels of Figure 4. The reform appears to have raised earnings, especially in the long-run. The overall present discounted value (PDV) of the reform is around 118 euros, about 3% of the PDV cumulative average earnings observed in the 5 years following the nominal graduation.[20]

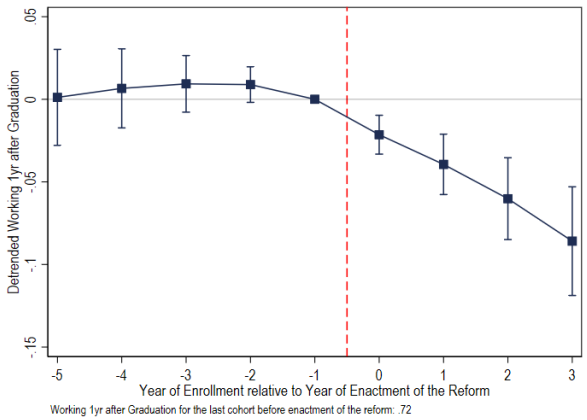
[18] Figure A.10 shows that these results are robust to including various proxies for unemployment rates in the analysis to capture differences in labor demand across cohorts—see also Figure A.11 and A.12.

[19] The results on labor market outcomes at three years post-graduation show marginal increases compared to one year after graduation but are generally not statistically significant. For simplicity, these findings are not included in the paper but are available upon request. Appendix Table A.3 reports event estimates at $j = 0$ and $j = 3$ from Equation (1) on the probability of working and earnings.

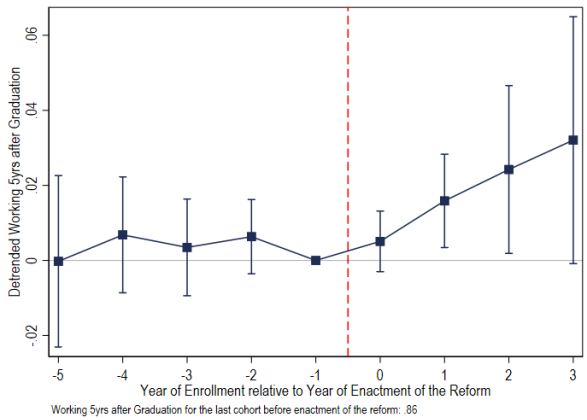
[20] The PDV effect of the reform is calculated as $\sum_{\tau=1}^{5} \delta^{\tau} \bar{\beta}_{\tau}$, where τ measures years since nominal graduation and $\bar{\beta}_{\tau}$ is the average of the event-study coefficients for post-reform cohorts in Equation (1), i.e. $\bar{\beta}_{\tau} = \frac{1}{4} \sum_{j=0}^{4} \beta_j$ when using earnings τ years from nominal graduation as the outcome. The calculations assume a 1% discount rate, in line with the average interest rate observed in Europe during this period.

Figure 4: The Impact of the Reform on Post-Graduation Labor Market Outcomes

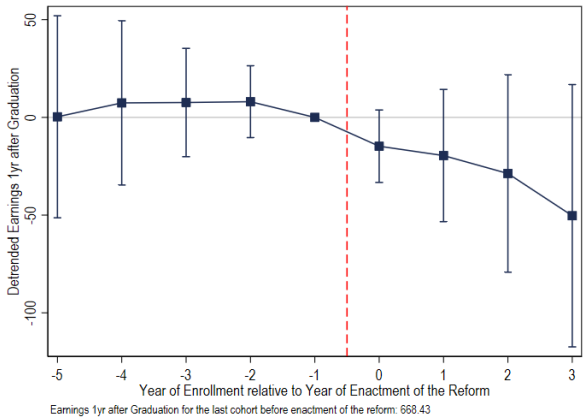
(a) Working–1yr after graduation



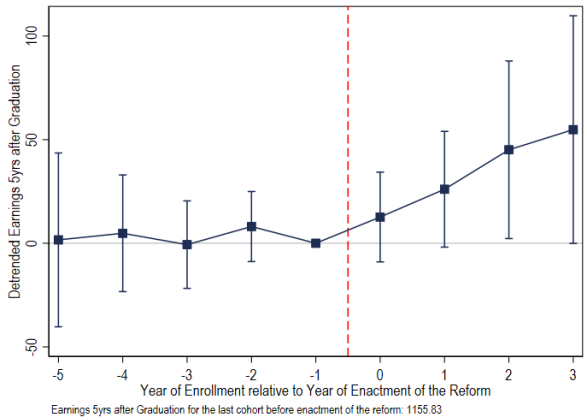
(b) Working–5yrs after graduation



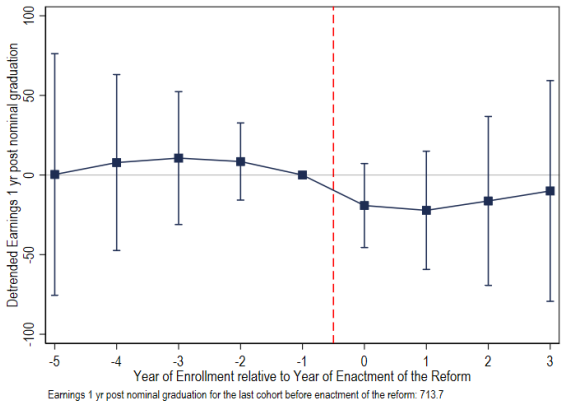
(c) Earnings–1yr after graduation



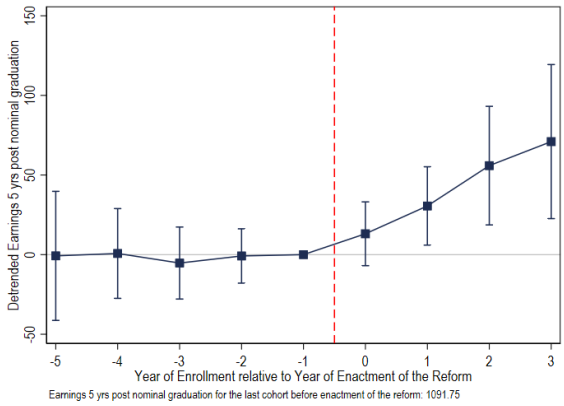
(d) Earnings–5yrs after graduation



(e) Earnings–1yr after *nominal* graduation



(f) Earnings–5yrs after *nominal* graduation



Notes: This figure reports estimates from Equation (1). In panels (a) and (b), the outcome is an indicator equal to 1 if the worker has a job one year after graduation (panel a) or 5 years after graduation (panel (b)). This indicator is equal to zero if the student remains in school (e.g., the student is enrolled in a professional or master's program or a PhD). Panel (c) and (d) are similar but look at earnings (with earnings set equal to zero for students not working or missing if the student decides to not disclose their earnings to Alma Laurea if employed) 1 and 5 years after graduation. Panel (e) and (f) looks at earnings measured after the nominal graduation. That is, for a student enrolled in year t the nominal graduation corresponds to year $t+2$. We assign earnings equal to zero if the student is still enrolled in university. Since our measure of earnings is observed 1, 3, 5 years after graduation, we linearly interpolate earnings to derive earnings at 2 and 4 years when necessary. Each graph reports the deviations from the event-study coefficients to the linear time trend estimated from the pre-reform cohorts. Figure A.4 reports event-study coefficients with the linear time trend (dashed line) estimated from the pre-reform cohorts. Figure A.5 reports estimates from the "honest approach" to parallel trends proposed by Rambachan and Roth (2023) to probe the robustness of our finding to alternative assumptions about different trends between pre-reform and post-reform cohorts that would have emerged in the absence of the reform. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph, we report the the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

In sum, the reform negatively affected labor market outcomes one year after graduation. However, this negative shock faded out over time, turning into a small but positive effect on both employment and earnings when measuring these outcomes five years after graduation.

6 Mechanisms

Section 6.1 shows that on-time graduation is the primary channel through which the reform impacts post-graduation labor market outcomes. Section 6.2 argues that the negative short-term effects on employment are driven by the reform's compliers—students who graduate on time due to the reform but would have otherwise graduated late—reducing their job search efforts immediately after graduation, relative to the counterfactual where they graduate later.

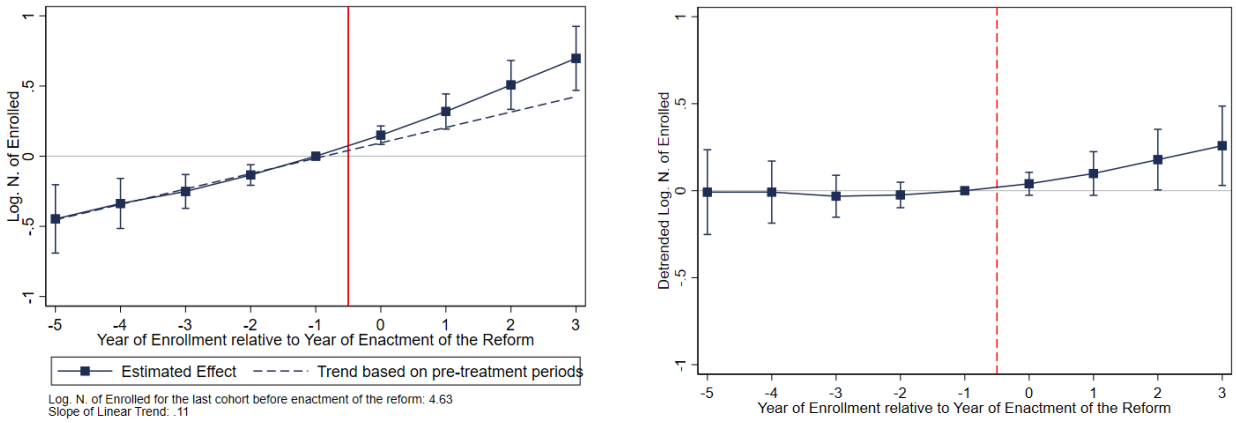
6.1 Channels Unrelated to On-Time Graduation

Selection. The reform aimed to facilitate student enrollment in an LM (Stefani and Zara, 2009). As a result, it might have shifted the composition of students, potentially impacting the labor market trajectories of post-reform cohorts. However, Table 2 shows minimal differences between pre- and post-reform students across various pre-determined characteristics that are good predictors of post-graduation labor market outcomes, such as age at enrollment or grades in high school and during the LT. Moreover, Figure 5 shows the reform's impact on LM enrollment numbers. The event-study estimates show no significant changes for all but the final post-reform cohort, but only marginally. The magnitude of the average effect is economically small and not statistically different from zero when allowing for minimal alterations to the linear trend extrapolation (see the bounds based on the approach of Rambachan and Roth (2023) displayed in Appendix Figure A.6). This lack of significant change in the student composition aligns with the reform's null impact on attrition rates (see Figure 3, panel (c)).

Crowding-out effects. As discussed in Section 5.1, the reform did not increase graduation rates. This finding, combined with the aforementioned negligible impact on enrollment numbers, suggests that crowding-out effects—which could explain some of the negative short-term

outcomes—are unlikely. The reduced impact of the reform over the medium term further diminishes the likelihood of such effects, contrasting with the crowding-out effects observed in another major educational reform in Italy, as documented by Bianchi (2020).

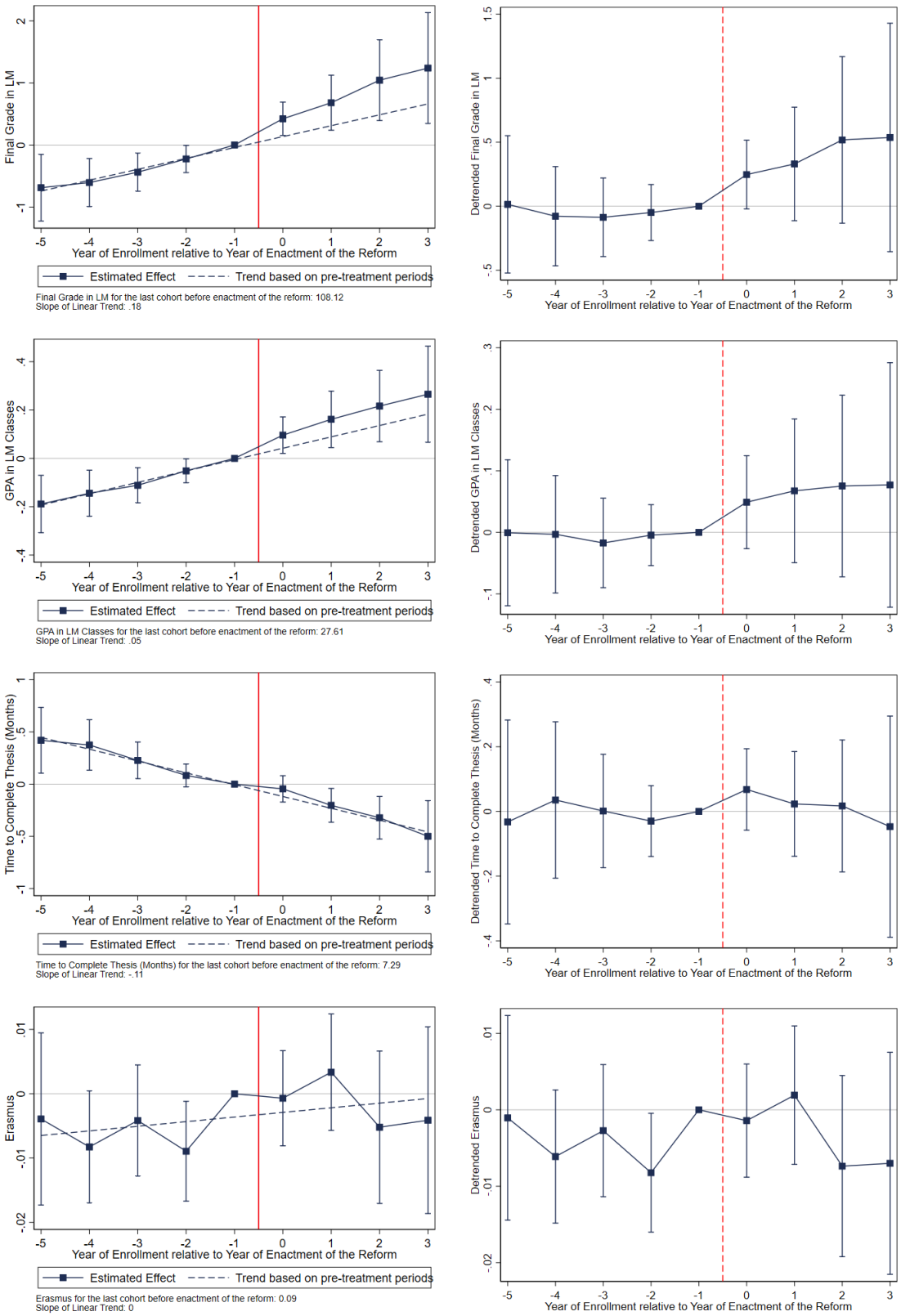
Figure 5: Event Study on Number of Enrolled



Notes: This figure reports estimates from a version of Equation (1) collapsed at the university × major × year of enrollment that controls for university by major and university by year of enrollment fixed effects. The outcome variable is the logarithm of the number of enrolled students. Majors are groups of fields of study defined by the Italian National Statistic Institute (ISTAT) and include: Scientific, Chemistry-Pharmaceutical, Geo-Biological, Medical, Engineering, Architecture, Agricultural-Veterinary, Economics-Statistics, Political-Social, Law, Literary, Education, Psychological, and Physical Education. In the left-graph, we also report the linear trend estimated using pre-reform event-study coefficients only extrapolated to post-reform cohorts (dashed line). In the right-graph, we report the deviations from the event-study coefficients to this linear time trend. Estimates are weighted by the number of enrolled students. We omit the coefficient for those students who have enrolled in the year prior to the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below the left-graph, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

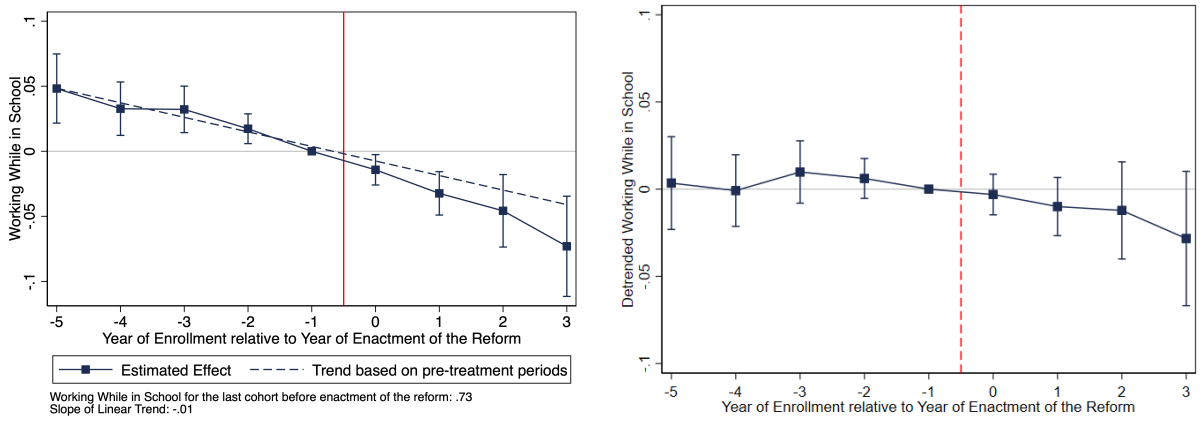
Human capital. While the reform did not change the number of credits necessary for a degree, its changes to course structure—as shown in Table 1—might decrease the human capital of post-reform students, similar to what happened in Arteaga (2018). However, our findings do not support this interpretation. First, in the longer run, our evidence points to positive effects on labor market outcomes. Moreover, Figure 6 indicates no significant effect of the reform on proxies that could measure the quality of human capital accumulated by students, such as final LM grades, LM exam GPAs, final thesis completion times, and the likelihood of studying abroad (*Erasmus*). Additionally, Figure 7 shows that the reform had no significant impact on the probability of working while in school (see also Appendix Figures A.7 and A.8). Taken together, this evidence contrasts with the idea that the reform reduced students' human capital.

Figure 6: Quality of Human Capital



Notes: This figure reports estimates from Equation (1). Outcomes measure the quality of human capital accumulated by students, such as the final LM grade that ranges from 66 to 110, the grade point average (GPA) obtained in the exams of the LM and ranging from 18 to 30, the time to complete the final thesis, and the likelihood of studying abroad (*Erasmus*). On the left, we report the linear trend estimated using pre-reform event-study coefficients only extrapolated to post-reform cohorts. On the right, we report the deviations from the event-study coefficients to this linear time trend. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year prior to the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph on the left, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Figure 7: Effect on the Probability to Work While Still Enrolled in School



Notes: This figure reports estimates from Equation (1). The outcome is an indicator of whether students worked while still enrolled in school (e.g., having an internship while still enrolled in the LM degree). In the left-graph, we also report the linear trend estimated using pre-reform event-study coefficients only extrapolated to post-reform cohorts (dashed line). In the right-graph, we report the deviations from the event-study coefficients to this linear time trend. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year prior to the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below the left-graph, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Heterogeneity across majors. Figure 8, panel (a) reports the post-reform coefficients on the probability of having a job one year after graduation (y-axis) against the reform's effect on the probability of graduating on time (x-axis) for a given major observed in our data. Panel (b) is similar but focuses on earnings. The post-reform, major-specific, coefficients depicted in each scatter plot are obtained by running an augmented version of Equation (1) that reads as follows:

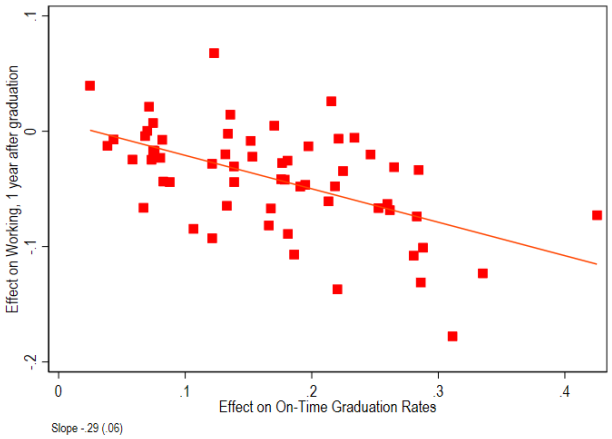
$$y_i = \alpha_{f(i),u(i)} + \lambda_{u(i),c(i)} + \sum_{j=a}^{b} \beta_j R^j_{f(i),u(i),c(i)} + \sum_{j=a}^{b} \theta_j \left(R^j_{f(i),u(i),c(i)} \times M_i \right) + X_i^\top \gamma + r_i \qquad (2)$$

where M_i is an indicator equal to 1 when considering a given major. Scatter plots display the linear combination ($\phi_j \equiv \beta_j + \theta_j$ for $j \geq 0$) of the coefficients from Equation (2). In majors where the reform had no impact on the probability of graduating on time, we observe no effects on labor market outcomes. Negative short-term effects are seen only in majors where the reform had a

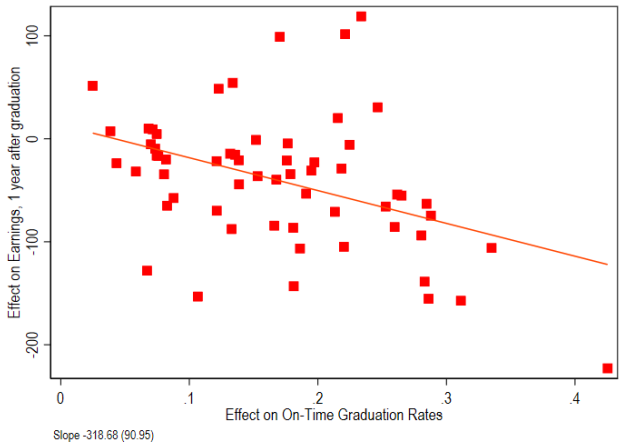
large impact on the probability of graduating on time, reinforcing the interpretation that other factors are not responsible.

Figure 8: Mechanisms - Heterogeneity across Majors

(a) Working and On-time Graduation across Majors



(b) Earnings and On-time Graduation across Majors



Notes: In panels (a) and (b), each figure reports the post-reform coefficients on the probability of having a job one year after graduation (y-axis) and the effect of the reform on the probability of graduating on time (x-axis) for a given major observed in our data. Panel (b) is similar but focuses on earnings. The post-reform, major-specific, coefficients depicted in each scatter plot are obtained by running Equation (2) for each of the following majors: Scientific, Chemistry-Pharmaceutical, Geo-Biological, Medical, Engineering, Architecture, Agricultural-Veterinary, Economics-Statistics, Political-Social, Law, Literary, Education, Psychological, and Physical Education. The scatter plots report the linear combination given by $\phi_j \equiv \beta_j + \theta_j$ for $j \geq 0$ for the aforementioned majors. We also report the associated linear fit (which includes a constant) and print below each graph the associated slope and robust standard error.

6.2 Channels Related to On-Time Graduation

On-time graduation is a positive signal used by employers when evaluating job market candidates.[21] However, despite increasing on-time graduation rates, the reform has a negative effect on employment probabilities one year after graduation. We consider two alternative theories—one based on labor demand, the other on labor supply—that could rationalize this finding.

[21] Cross-sectional regressions of earnings post-graduation on an indicator of on-time graduation rate show an economically and statistically strong coefficient, even when this regression controls flexibly for fields of studies × university fixed effects and other student characteristics such as high school grades, age upon initial enrollment, and gender (see also Aina and Casalone, 2020).

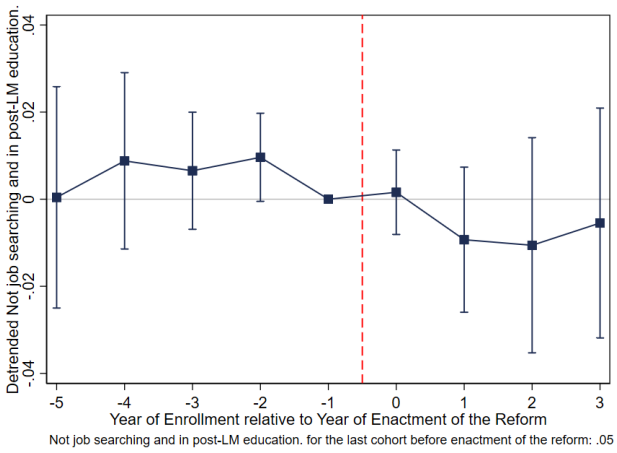
Employers. By increasing the number of students graduating on time, the reform has potentially decreased the value of on-time graduation as a signal for employers to assess student quality (Farber and Gibbons, 1996).[22] Therefore, when facing post-reform graduates, firms may be more likely to converge to a pooling type of equilibrium with lower offered wages relative to a counterfactual where they instead use on-time graduation as an effective signal to discriminate between new graduates (Spence, 1973). Yet, Appendix Figure A.9 provides evidence partly at odds with this interpretation. Conditional on having a job, post-reform cohorts do not seem to receive a systematically lower wage one-year after graduation.

Students. Post-reform cohorts differ from pre-reform cohorts on two key margins: (i) the share of on-time graduates and (ii) the probability of employment one year after graduation. Conditional on having a job, treated cohorts are not systematically different from control cohorts in terms of log wages, as shown in Figure A.9. Moreover, five years after graduation, post-reform cohorts tend to have better labor market outcomes in terms of both employment and earnings. A plausible explanation for this set of findings is that some of the reform's compliers might devote the extra time due to the earlier graduation date to either leisure or additional educational activities (e.g., getting a professional master degree). In a counterfactual where they were not exposed to the reform, these individuals would instead graduate late and as a result might decide to start searching for a job right after graduation to compensate for the delayed entry into the labor market. To test this idea—and whether in particular reform compliers devote their extra time to leisure vs. additional human capital—we analyze how the reform impacts the probability that a graduated student who does not have a job one year after graduation is *not* looking for a job one year after graduation and is not enrolled in any additional courses. Figure 9, panel (a) shows that this probability is not significantly different between pre and post-reform cohorts. Conversely, Figure 9, panel (b) shows a positive effect on the probability to observe post-reform students not looking for a job but who are enrolled in some form of education, one year after having obtained their LM degree.

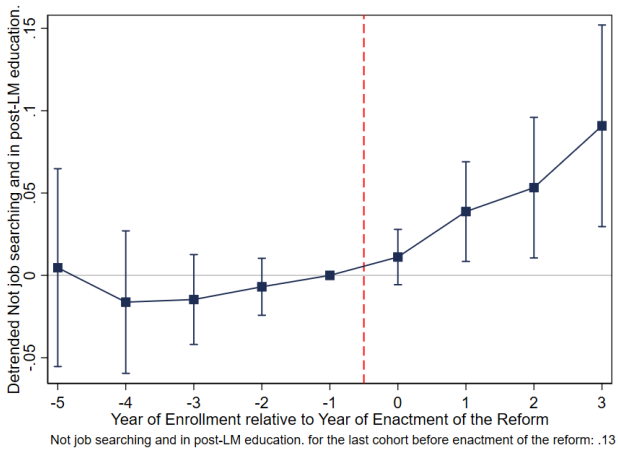
[22] On-time graduation is an easy-to-observe signal for employers and is directly available to those hiring via the AL job board.

Figure 9: Mechanisms - Searching for a Job and Education Status

(a) Not Searching for a Job and not in Education



(b) Not Searching for a Job and in Education



Notes: In panels (a) and (b), the figure reports estimates from Equation (1) using the sample of students without a job 1 year after graduation. In panel (a), the outcome is an indicator equal to 1 if the student is not looking for a job 1 year after graduation and the student is not pursuing any additional extra-educational activities (e.g. obtaining a professional master degree). In panel (b), the outcome is an indicator equal to 1 if the student is not looking for a job 1 year after graduation and the student is pursuing some additional extra-educational activities (e.g., obtaining a professional master degree). In both panel (a) and (b) we report the deviations of the event-study coefficients from linear time trend estimated from the pre-event cohorts. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph, we report the the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

The average effect for post-reform cohorts—after adjusting for linear pre-trends—is around 3 percentage points which is $\approx$ 24% of the baseline share of pre-reform cohorts who are not looking for a job one year after graduation and who are in some form of post-graduate education. But we should stress that this finding is somewhat sensitive to deviations from the linear trend extrapolation as shown by the honest analysis displayed in Figure A.13, panel (b). Overall, this evidence is suggestive that post-reform students are less likely to be job-seeking immediately after graduation as they are instead pursuing additional educational qualifications, such as professional degrees, which typically last around one year. This pattern helps explain why post-reform cohorts exhibit: (i) a lower probability of employment one year after graduation, (ii) no significant effect of the reform on their wages one year post-graduation, conditional on working, and (iii) higher earnings five years after graduation compared to pre-reform cohorts. The latter suggests that the

additional education undertaken after the LM degree yields positive returns in the long run.

7 Conclusion

Our evaluation of an Italian university reform aimed at consolidating course offerings to decrease time to graduation shows that the reform successfully streamlined course structures and increased the on-time graduation rate. Students graduating under this reform are less likely to have a job one year after graduation but have slightly higher earnings five years post graduation. These patterns are plausibly explained by reform compliers delaying their job search after graduation to pursue additional post-graduate educational qualifications, an investment that ultimately leads to higher earnings in the long run.

An important takeaway of our analysis is that policies that aim to shorten the time to graduation could in principle generate positive effects in earnings via two mutually exclusive channels. The first is mechanical: by graduating on time, students enter the labor market earlier, allowing them to start earning sooner than they would have if they had remained in school. From a government perspective, this would result in higher tax revenues and lower education expenditures, as students exit the education system earlier. Our findings provide limited evidence for this channel, as earnings do not increase much immediately following nominal graduation. The reason is related to the second channel that we document. In response to the reform, some students adjust their behavior and use the extra time gained from earlier graduation to pursue additional post-graduate educational qualifications, which ultimately lead to higher future earnings. Understanding the behavioral responses to different policies designed to accelerate graduation with alternative incentives—such as tuition increases for students exceeding the prescribed time to graduate, see Garibaldi et al., 2012—is an interesting avenue for future research.

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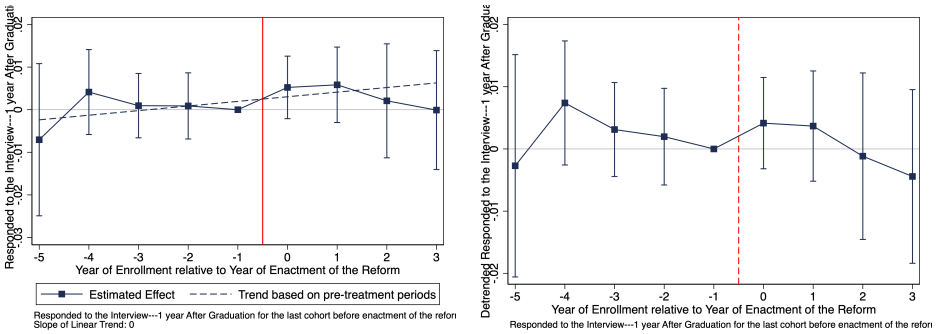
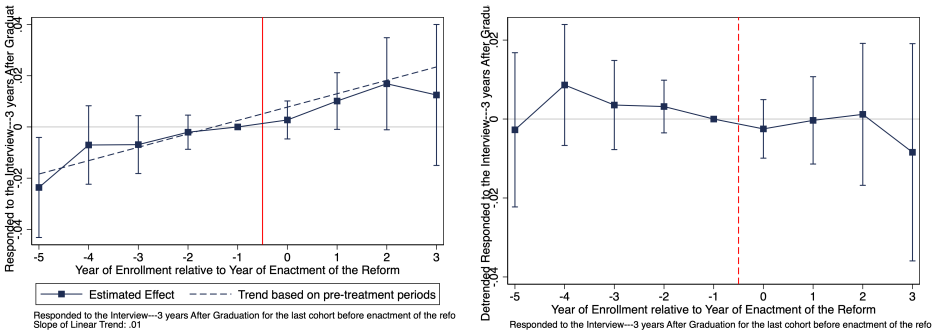
Appendix. Additional Figures and Tables

Figure A.1: Response Rates and The Reform

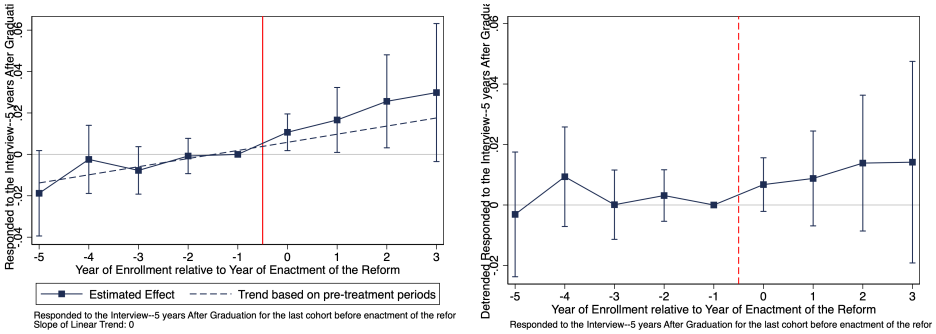
(a) Responding to the AL Survey—1 year after graduation



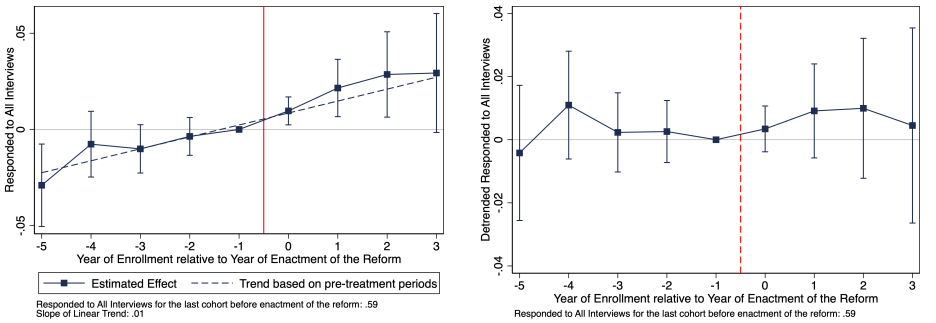
(b) Responding to the AL Survey—3 years after graduation



(c) Responding to the AL Survey—5 years after graduation



(d) Responding to all AL Surveys



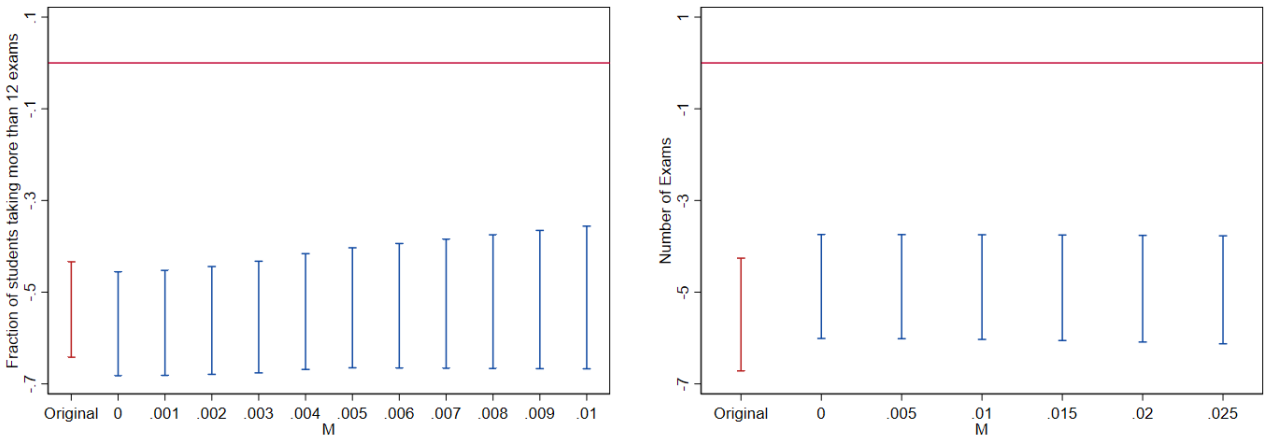
Notes: This figure reports estimates from Equation (1). In panel (a), the outcome is an indicator equal to 1 if student i responded to the AL survey after their graduation. Panel (b) and (c) measure the response rates 3, 5 years after graduation, respectively. Panel (d) shows the impact of the reform on the probability to respond to all AL surveys. On the left, we report the linear trend estimated using pre-reform event-study coefficients only which is then extrapolated to post-reform cohorts. On the right, we report the deviations from the event-study coefficients to this linear time trend. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph on the left, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Table A.1: Evidence on the Reform

	(1) Taking More than 12 Exams	(2) Number of Exams
First cohort enrolled in the reform ($j = 0$)	-0.463***	-4.907***
	(0.035)	(0.359)
Last cohort enrolled in the reform ($j = 3$)	-0.618***	-5.998***
	(0.070)	(1.039)
Observations	206,812	206,812
Pre-reform Mean	0.87	17.2
University by Fields of Study FEs	YES	YES
University by Cohorts of Entry FEs	YES	YES
Students' Characteristics	YES	YES

Notes: This tables reports events estimates at $j = 0$ and $j = 3$ from Equation (1). In column (1), the outcome is an indicator for whether the student took more than 12 exams. In column (2), the outcome is the total number of exams taken. The regression controls for university by fields of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year prior to the enactment of the new regime. Pre-reform mean reports the average of a given outcome for the last cohort who studied under the pre-reform regime. Standard errors are clustered at the university level. *** p<0.01, ** p<0.05, * p<0.1

Figure A.2: Sensitivity of the results on the number of exams according to the honest bounds of Rambachan and Roth (2023)



Notes: This figure reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the outcomes described in Figure 2 when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control are exactly linear.

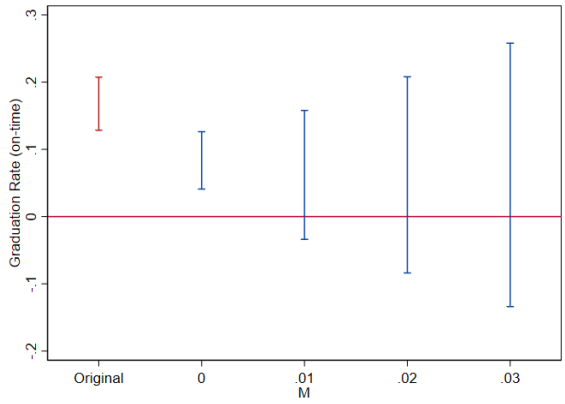
Table A.2: Reform Effect on Graduation Rate

	(1) Fraction of Students Graduating in Two Years or Less	(2) Fraction of Students Graduating in Three Years or Less	(3) Graduation Rate
First cohort enrolled in the reform ($j = 0$)	0.042***	0.028***	0.011*
	(0.010)	(0.008)	(0.006)
Last cohort enrolled in the reform ($j = 3$)	0.127***	0.063***	0.016
	(0.033)	(0.028)	(0.019)
Observations	3,044	3,080	3,084
Pre-reform Mean	0.39	0.63	0.77
University by Major FEs	YES	YES	YES
University by Cohorts of Entry FEs	YES	YES	YES

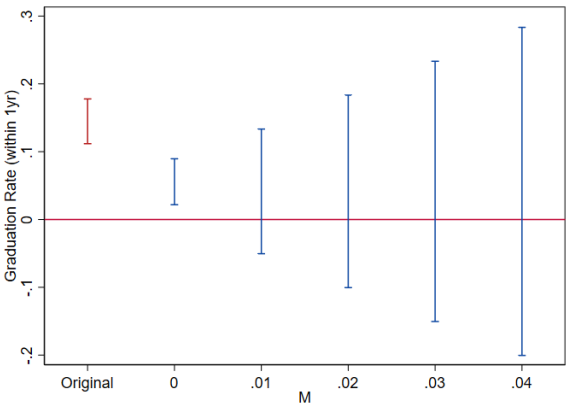
Notes: This table reports event estimates at $j = 0$ and $j = 3$ from the event study depicted in Figure 3. Majors are groups of fields of study defined by Italian National Statistic Institute (ISTAT) and include: Scientific, Chemistry-Pharmaceutical, Geo-Biological, Medical, Engineering, Architecture, Agricultural-Veterinary, Economics-Statistics, Political-Social, Law, Literary, Education, Psychological, and Physical Education. In column (1), the outcome is the fraction of students graduating on-time, i.e. the fraction of students that obtained their degree within the nominal duration of an LM degree (which is two years). In column (2), the outcome is the fraction of students that graduated in three years. In panel (3), the outcome is the fraction of students graduating. For all these outcomes, the denominator is represented by the number of students *enrolling* in a given year × major × university. Estimates are weighted by the number of enrolled students. Event-study estimates are normalized relative to the cohort of students that enrolled in the year prior to the enactment of the new regime. Pre-reform mean reports the average of a given outcome for the last cohort who studied under the pre-reform regime. Standard errors are clustered at the university level. *** p<0.01, ** p<0.05, * p<0.1

Figure A.3: Sensitivity of the results on the graduation rate according to the honest bounds of Rambachan and Roth (2023)

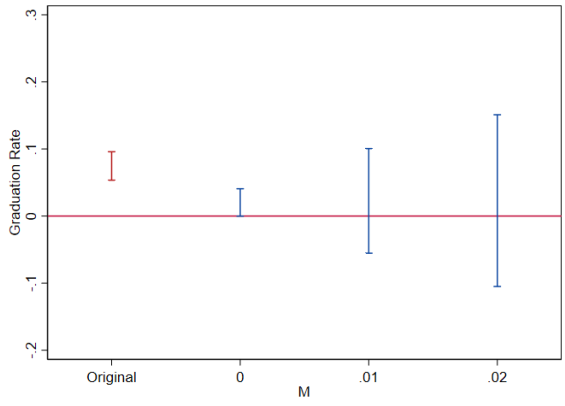
(a) Fraction of Students Graduating in Two Years or Less (On-Time Graduation)



(b) Fraction of Students Graduating in Three Years or Less



(c) Graduation Rate



Notes: This figure reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the outcomes described in Figure 3 when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control are exactly linear.

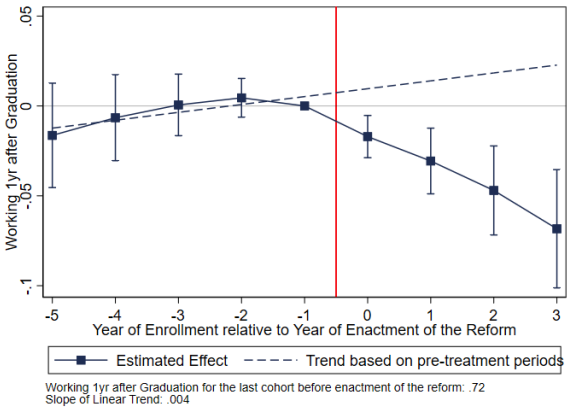
Table A.3: Labor Market Outcomes

	Working		Earnings from graduation		Earnings from *nominal* graduation	
	(1) 1yr after	(2) 5yrs after	(3) 1yr after	(4) 5yrs after	(5) 1yr after	(6) 5yrs after
First cohort enrolled in the reform ($j = 0$)	-0.017***	0.011**	-14.735	24.620**	-28.238**	23.085**
	(0.006)	(0.004)	(9.454)	(11.054)	(13.450)	(10.222)
Last cohort enrolled in the reform ($j = 3$)	-0.068***	0.055***	-50.265	102.720***	-46.242	110.967***
	(0.017)	(0.017)	(34.234)	(28.032)	(35.363)	(24.683)
Observations	221,336	221,336	182,410	204,026	92,989	205,697
Pre-reform Mean	0.72	0.86	668.4	1155.8	713.7	1091.75
University by Fields of Study FEs	YES	YES	YES	YES	YES	YES
University by Cohorts of Entry FEs	YES	YES	YES	YES	YES	YES
Students' Characteristics	YES	YES	YES	YES	YES	YES

Notes: This tables reports events estimates at $j = 0$ and $j = 3$ from Equation (1). In columns (1) and (2), the outcome is an indicator equal to 1 if the worker has a job one year after graduation (column 1) or 5 years after graduation. This indicator is equal to zero if the student remains in school (e.g., the student is enrolled in a master's program or a PhD). Columns (3) and (4) are similar but look at earnings (with earnings set equal to zero for students not working, and missing when a student with a job decides to not report their wage). Columns (5) and (6) look at earnings from *nominal* graduation. That is, for a student enrolled in year t the nominal graduation corresponds to year $t + 2$. We assign earnings equal to zero if the student is still enrolled in university. Since our measure of earnings is observed 1, 3, 5 years after graduation, we linearly interpolate earnings to derive earnings at 2 and 4 years when necessary. The regression controls for university by fields of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year prior to the enactment of the new regime. Standard errors are clustered at the university level. *** p<0.01, ** p<0.05, * p<0.1

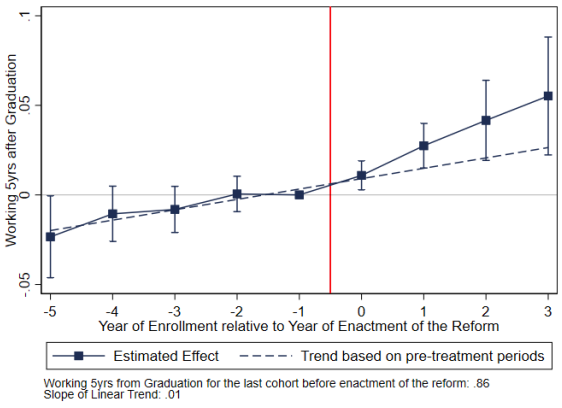
Figure A.4: Event Study on Labor Market Outcomes

(a) Working–1yr after graduation



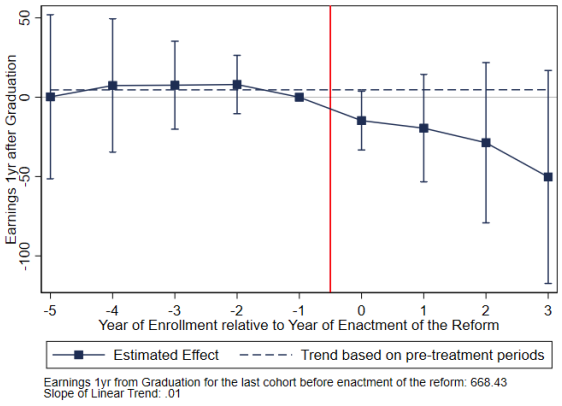
Working 1yr after Graduation for the last cohort before enactment of the reform: .72
Slope of Linear Trend: .004

(b) Working–5yrs after graduation



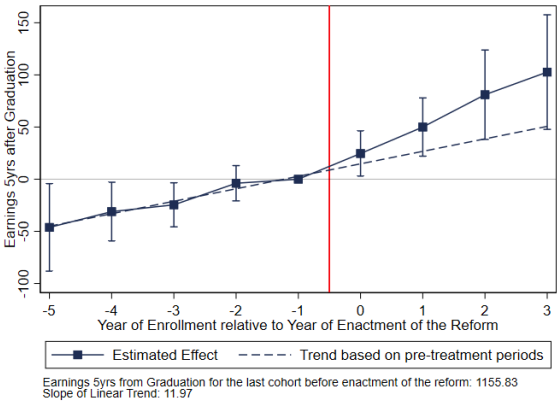
Working 5yrs from Graduation for the last cohort before enactment of the reform: .86
Slope of Linear Trend: .01

(c) Earnings–1yr after graduation



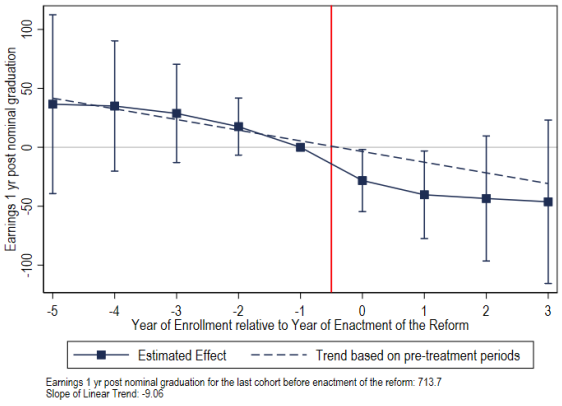
Earnings 1yr from Graduation for the last cohort before enactment of the reform: 668.43
Slope of Linear Trend: .01

(d) Earnings–5yrs after graduation



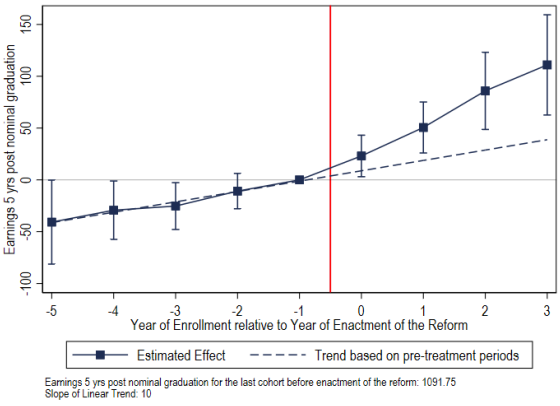
Earnings 5yrs from Graduation for the last cohort before enactment of the reform: 1155.83
Slope of Linear Trend: 11.97

(e) Earnings–1yr after *nominal* graduation



Earnings 1 yr post nominal graduation for the last cohort before enactment of the reform: 713.7
Slope of Linear Trend: -9.06

(f) Earnings–5yrs after *nominal* graduation

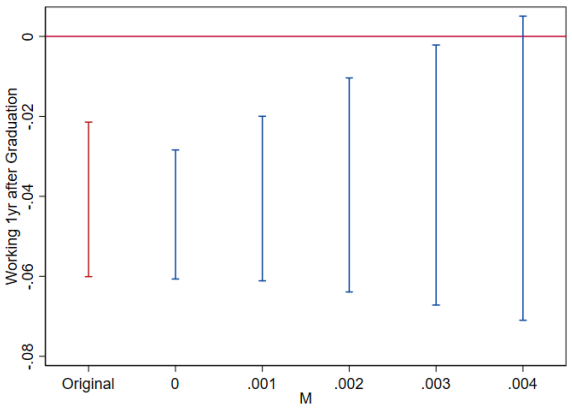


Earnings 5 yrs post nominal graduation for the last cohort before enactment of the reform: 1091.75
Slope of Linear Trend: 10

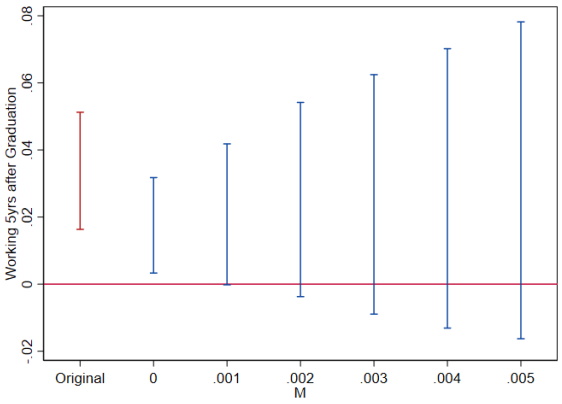
Notes: This figure reports estimates from Equation (1). In panels (a) and (b), the outcome is an indicator equal to 1 if the worker has a job one year after graduation (panel a) or 5 years after graduation (panel (b)). This indicator is equal to zero if the student remains in school (e.g., the student is enrolled in a professional or master's program or a PhD). Panel (c) and (d) are similar but look at earnings (with earnings set equal to zero for students not working or missing if the student decides to not disclose their earnings to Alma Laurea if employed) 1 and 5 years after graduation. Panel (e) and (f) looks at earnings measured after the nominal graduation. That is, for a student enrolled in year t the nominal graduation corresponds to year $t+2$. We assign earnings equal to zero if the student is still enrolled in university. Since our measure of earnings is observed 1, 3, 5 years after graduation, we linearly interpolate earnings to derive earnings at 2 and 4 years when necessary. Figure A.5 reports estimates from the "honest approach" to parallel trends proposed by Rambachan and Roth (2023) to probe the robustness of our finding to alternative assumptions about different trends between pre-reform and post-reform cohorts that would have emerged in the absence of the reform. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The dashed line reports the linear trend estimated using pre-reform event-study coefficients only which is then extrapolated to post-reform cohorts. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph, we report we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Figure A.5: Sensitivity of the results on the labor market outcomes according to the honest bounds of Rambachan and Roth (2023)

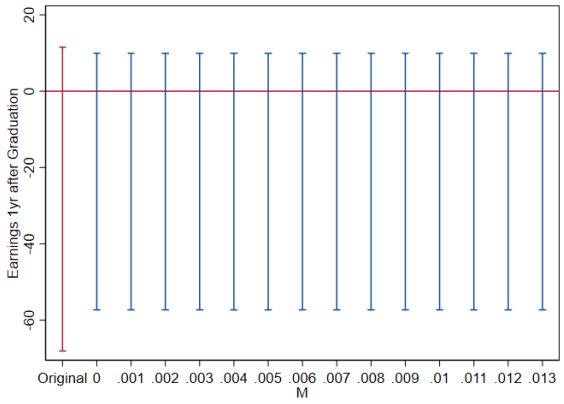
(a) Working–1yr after graduation



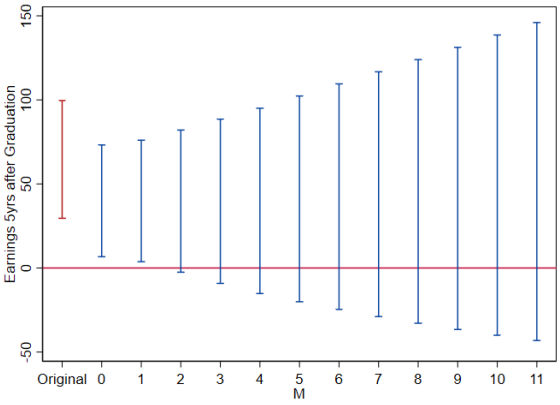
(b) Working–5yrs after graduation



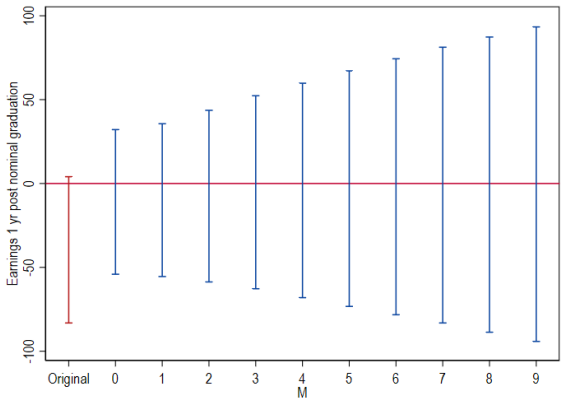
(c) Earnings–1yr after graduation



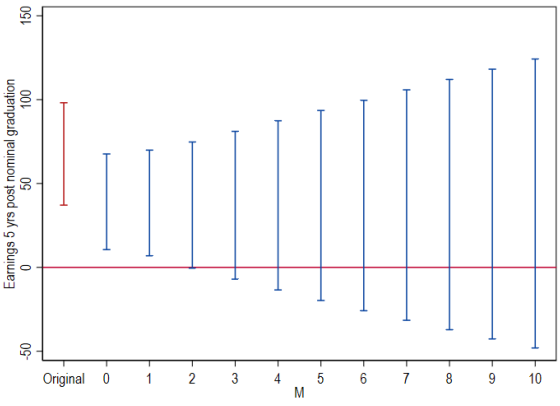
(d) Earnings–5yrs after graduation



(e) Earnings–1yr after *nominal* graduation



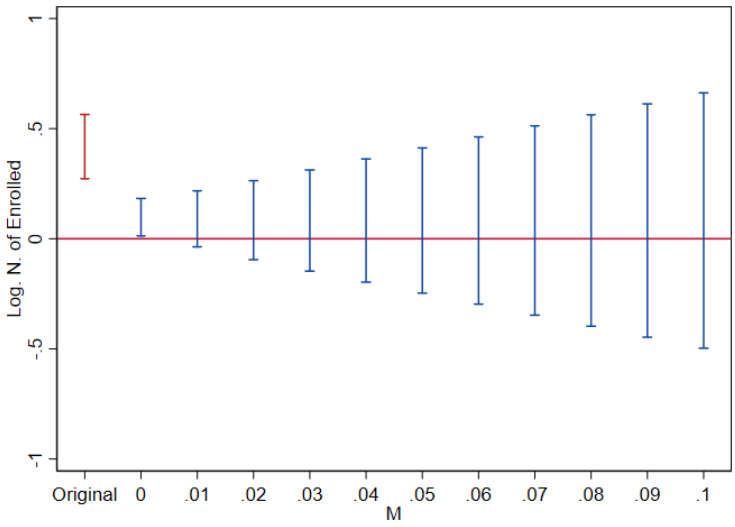
(f) Earnings–5yrs after *nominal* graduation



Notes: This figure reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the outcomes described in Figure 4 when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control are exactly linear. Labor market outcomes one year post-graduation are reported on the left-graphs, while those five years from graduation are reported on the right graphs.

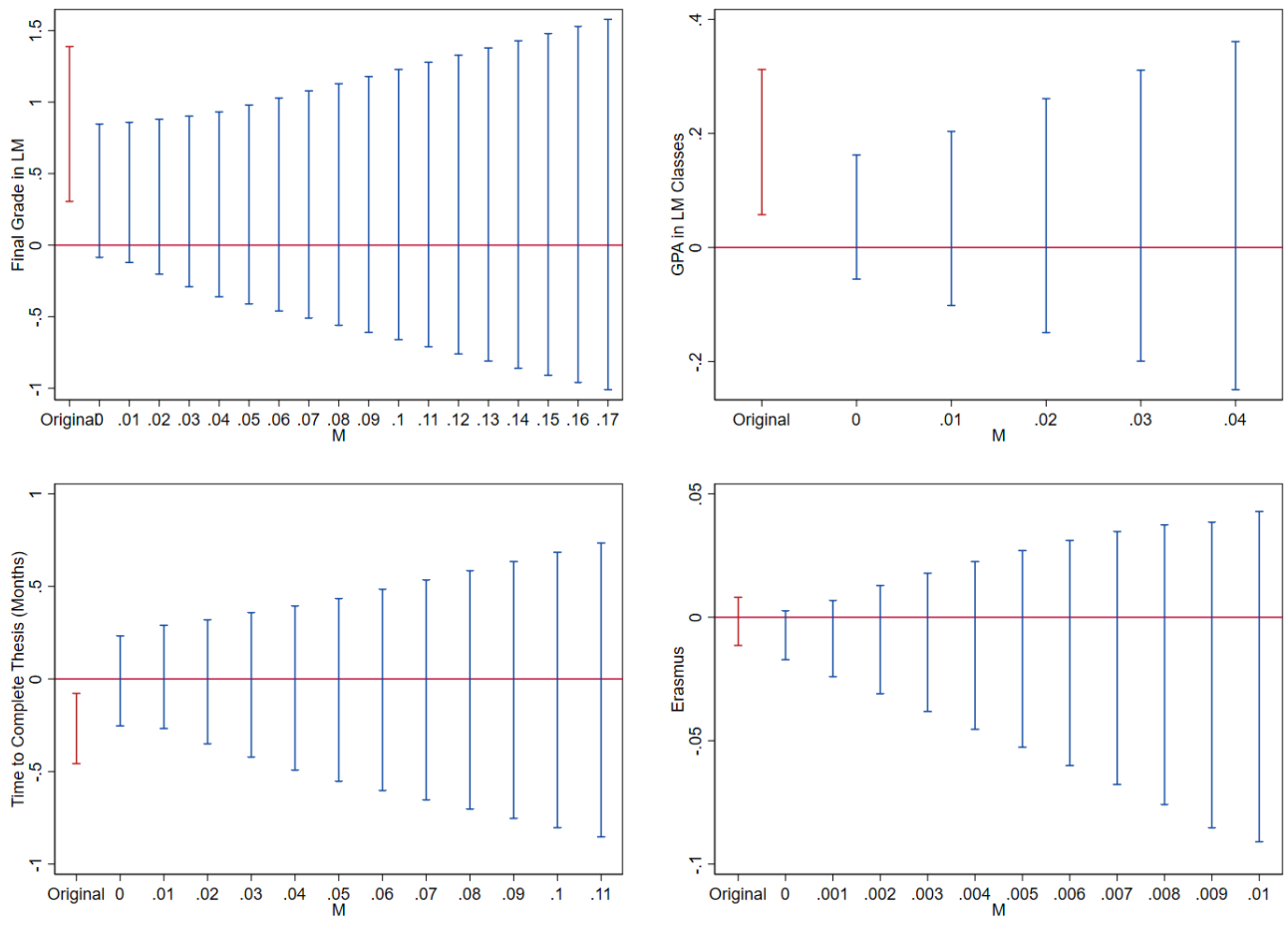
Figure A.6: Sensitivity of the results on the Number of Enrolled according to the honest bounds of Rambachan and Roth (2023)

(c) Honest Bound on the N. of Enrolled



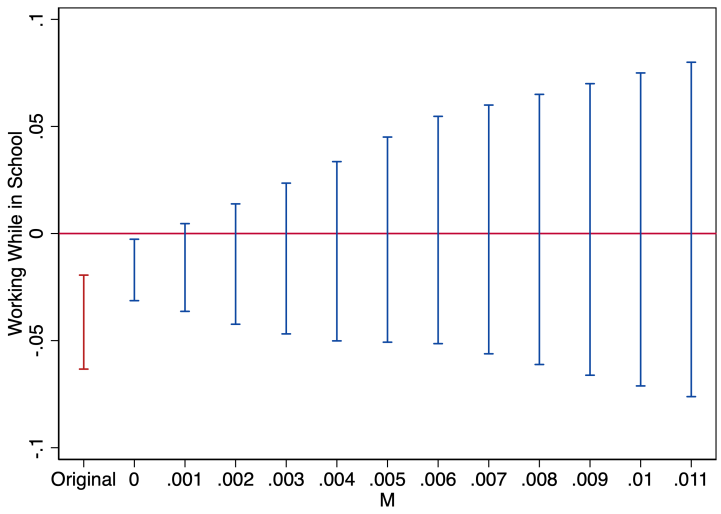
Notes: This figure reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the outcome described in Figure 5 when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control is exactly linear.

Figure A.7: Sensitivity of the results on the quality of human capital according to the honest bounds of Rambachan and Roth (2023)



Notes: This figure reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the outcomes described in Figure 6 when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control is exactly linear.

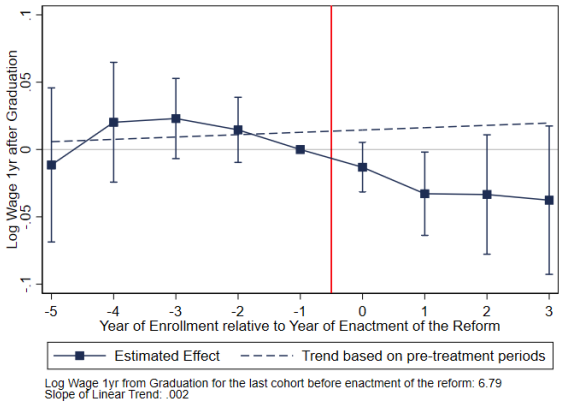
Figure A.8: Effect on the Probability to Work While Still Enrolled in School



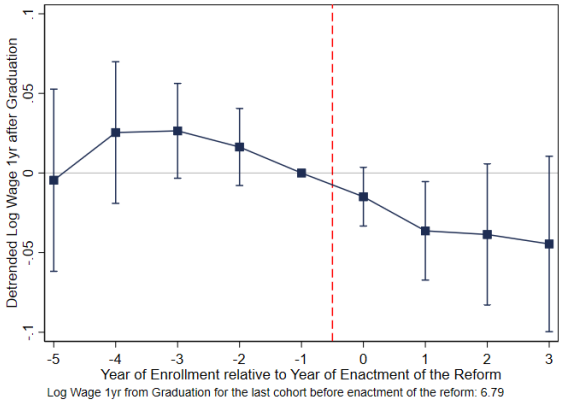
Notes: This figure reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the outcomes described in Figure 7 when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control is exactly linear.

Figure A.9: Effect on the Log. Wages One Year After Graduation

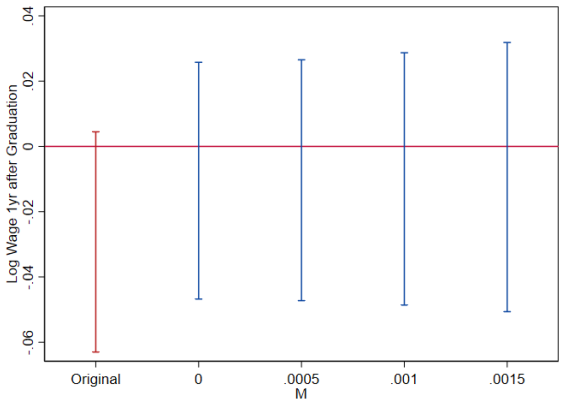
(a) Log. Wage



(b) Detrended Log. Wage



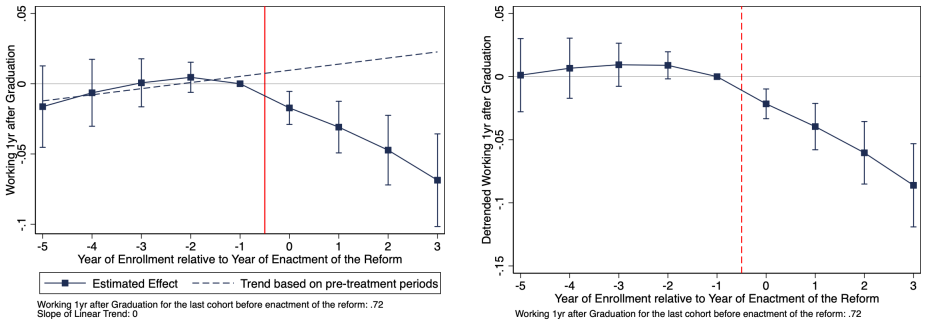
(c) Honest Bound on Log. Wage



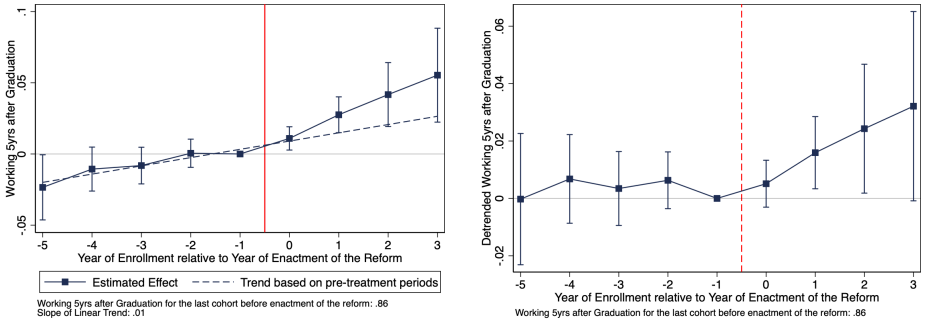
Notes: This figure reports estimates from Equation (1). The outcome is the student's log monthly wage one year after graduation. In panel (a), we also report the linear trend estimated using pre-reform event-study coefficients only extrapolated to post-reform cohorts (dashed line). In panel (b), we report the deviations from the event-study coefficients to this linear time trend. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year prior to the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below the graph of panel (a), we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level. Panel (c) reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the number of enrolled students when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control are exactly linear.

Figure A.10: The Impact of the Reform on Post-Graduation Labor Market Outcomes, Controlling for Unemployment Rate at the Time of Graduation

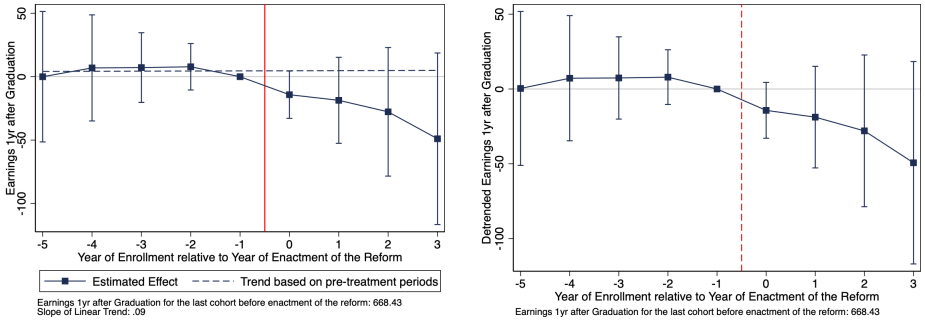
(a) Probability of Working - 1yr After Graduation



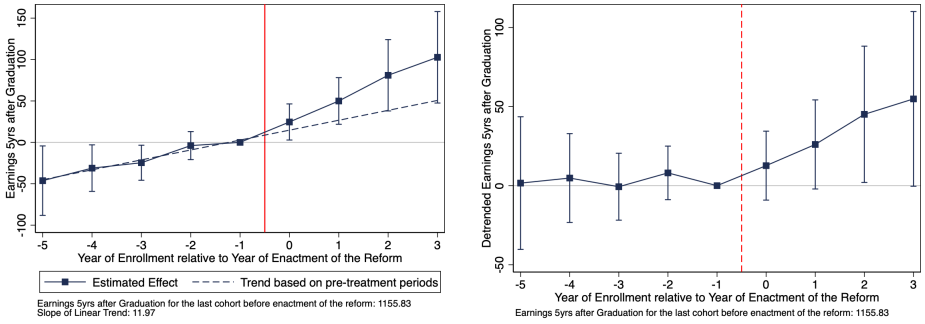
(b) Probability of Working - 5yrs After Graduation



(c) Earnings - 1yr After Graduation



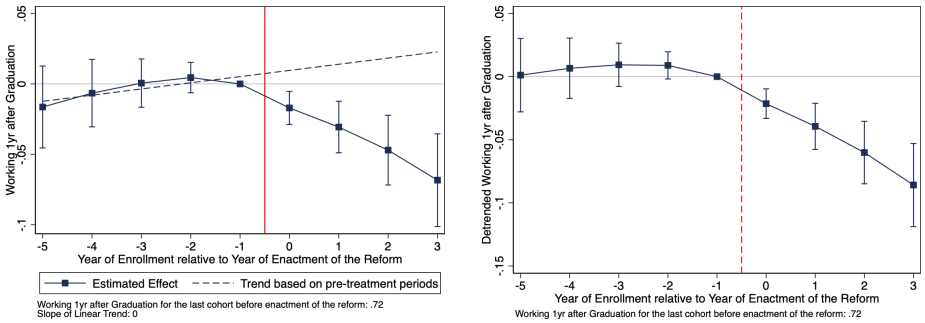
(d) Earnings - 5yrs After Graduation



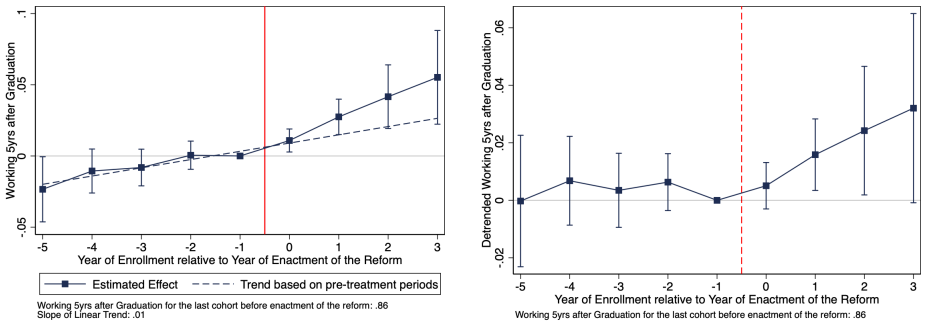
Notes: This figure reports estimates from Equation (1), after adding as an additional control the unemployment rate observed in the province of study and in the year of graduation of a given student. In panels (a) and (b), the outcome is an indicator equal to 1 if the worker has a job one year after graduation (panel a) or 5 years after graduation. This indicator is equal to zero if the student remains in school (e.g., the student is enrolled in a master's program or a PhD). Panel (c) and (d) are similar but look at earnings (with earnings set equal to zero for students not working or missing if the student decides to not disclose their earnings to Alma Laurea if employed). On the left, we report the linear trend estimated using pre-reform event-study coefficients only which is then extrapolated to post-reform cohorts. On the right, we report the deviations from the event-study coefficients to this linear time trend. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph on the left, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Figure A.11: The Impact of the Reform on Post-Graduation Labor Market Outcomes, Controlling for Unemployment Rate in the year before graduation

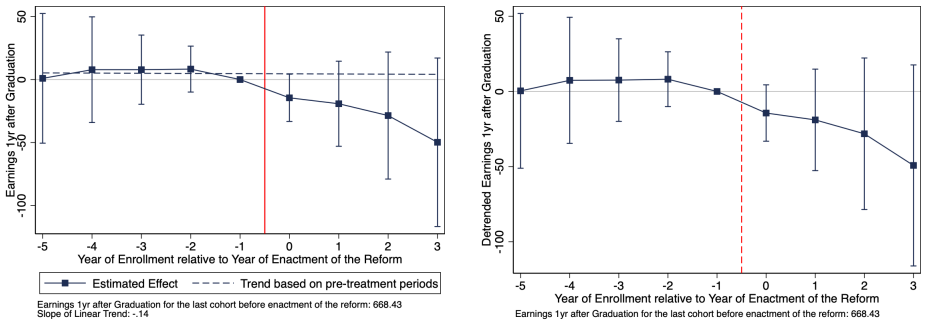
(a) Probability of Working - 1yr After Graduation



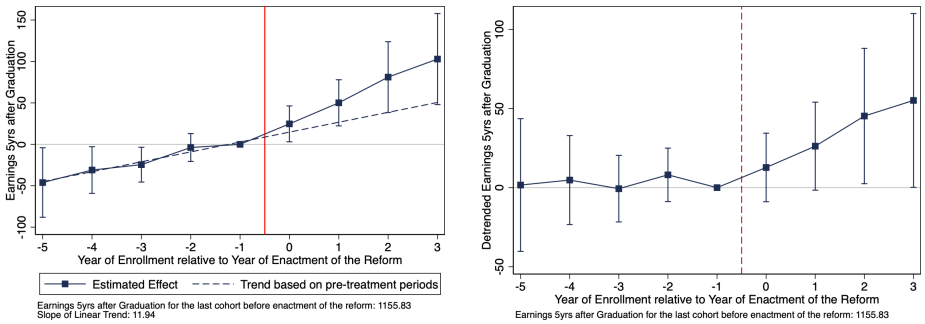
(b) Probability of Working - 5yrs After Graduation



(c) Earnings - 1yr After Graduation



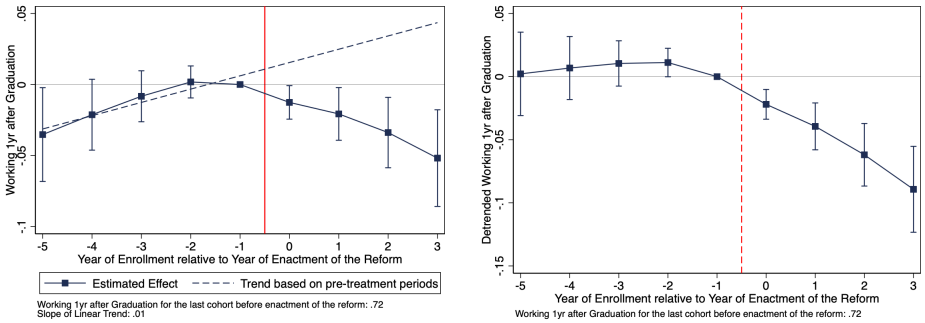
(d) Earnings - 5yrs After Graduation



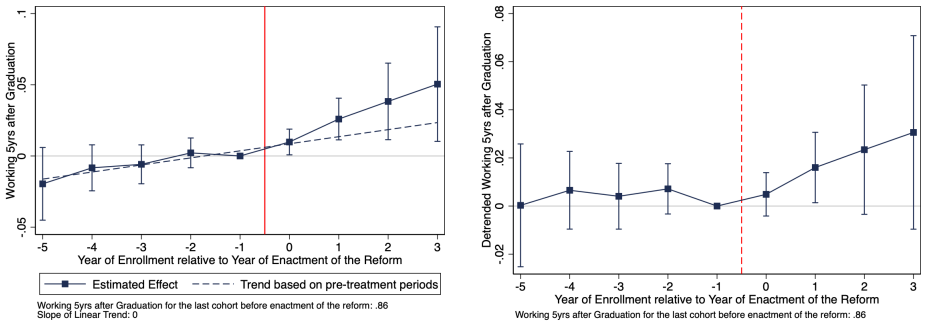
Notes: This figure reports estimates from Equation (1), after adding as an additional control the unemployment rate observed in the province of study and in the year before graduation of a given student. In panels (a) and (b), the outcome is an indicator equal to 1 if the worker has a job one year after graduation (panel a) or 5 years after graduation. This indicator is equal to zero if the student remains in school (e.g., the student is enrolled in a master's program or a PhD). Panel (c) and (d) are similar but look at earnings (with earnings set equal to zero for students not working or missing if the student decides to not disclose their earnings to Alma Laurea if employed). On the left, we report the linear trend estimated using pre-reform event-study coefficients only which is then extrapolated to post-reform cohorts. On the right, we report the deviations from the event-study coefficients to this linear time trend. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph on the left, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Figure A.12: The Impact of the Reform on Post-Graduation Labor Market Outcomes, Controlling for Unemployment Rate of Previous Cohorts

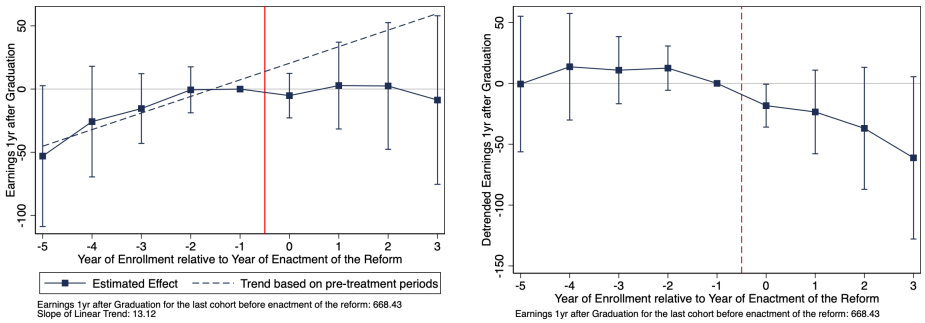
(a) Probability of Working - 1yr After Graduation



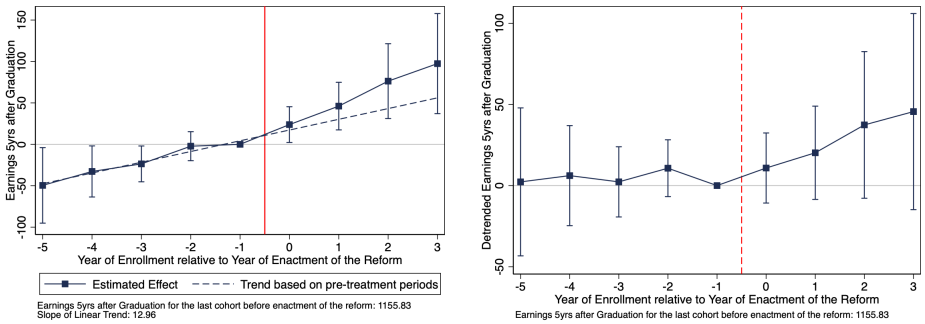
(b) Probability of Working - 5yrs After Graduation



(c) Earnings - 1yr After Graduation



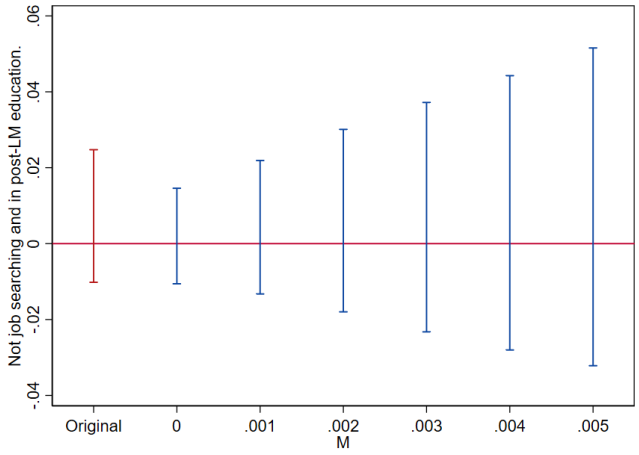
(d) Earnings - 5yrs After Graduation



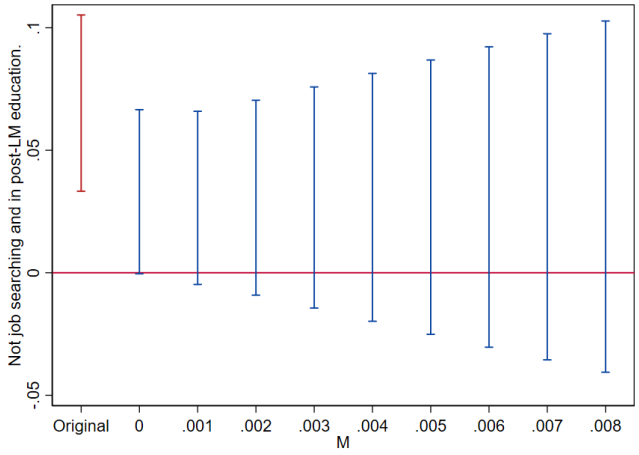
Notes: This figure reports estimates from Equation (1), after adding as an additional control the unemployment rate observed in the province of study and in the year before graduation of a given student. In panels (a) and (b), the outcome is an indicator equal to 1 if the worker has a job one year after graduation (panel a) or 5 years after graduation. This indicator is equal to zero if the student remains in school (e.g., the student is enrolled in a master's program or a PhD). Panel (c) and (d) are similar but look at earnings (with earnings set equal to zero for students not working or missing if the student decides to not disclose their earnings to Alma Laurea if employed). On the left, we report the linear trend estimated using pre-reform event-study coefficients only which is then extrapolated to post-reform cohorts. On the right, we report the deviations from the event-study coefficients to this linear time trend. The regression controls for university by field of study fixed effects and university by year of enrollment fixed effects as well as students' pre-determined characteristics (gender, age at entry, foreign, parent's college, a cubic in high school final grade, dummies for type of high-school, and an indicator for whether the province of residence is the same as the province of study). Event-study estimates are normalized relative to the cohort of students that enrolled in the year before the enactment of the new regime. The vertical lines indicate the first entry cohort that studies under the new reform. Below each graph on the left, we report the slope of the pre-event event-study coefficients as well as the average of the outcome variable for the last cohort of students who enrolled in the pre-reform regime. 95% confidence intervals are obtained after clustering the standard errors at the university level.

Figure A.13: Sensitivity of the results on the probability of not searching for a job according to the honest bounds of Rambachan and Roth (2023)

(a) Not Searching for Job and Not in Education



(b) Not Searching for Job and in Education



Notes: This figure reports the confidence sets described in Rambachan and Roth (2023) for the average of all post-reform coefficients on the outcomes described in Figure 9 (panels (a) and (b)) when we allow the slope of the pre-trend coefficients to change by no more than M—reported on the x-axis—across consecutive cohorts. Values of $M = 0$ correspond to cases where differences between treated and control is exactly linear. Labor market outcomes one year post graduation are reported on the left-graphs, while those at five years from graduation are reported on the right-graphs.