Students' Reactions to more Flexible University Systems: the Bologna Process in Italy

Emma Scandolo

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Abstract

This paper studies how students react when they are exposed to a more flexible university system, with the option of getting a less specialized university degree in shorter time. Using a Regression Discontinuity design that exploits the roll-out of the Bologna Reform in Italy, I compare education and labor market outcomes for cohorts of individuals who were deciding to go to university right before and right after the reform was implemented. Ex-ante, the impact of the reform on university attainment is ambiguous: schooling might increase because more people go to university, but it might also decrease if students who previously would have completed a longer degree now opt to finish studying after a shorter 3-year degree. I show that both mechanisms are at play but that females, on net, spend 0.15 more years in university. This increases female earnings by 1.5-2% while keeping their employment probability constant, implying a return to one year of university of at least 10%. The results are mostly driven by more women graduating from healthcare degrees - a 30% increase - which has important implications on their occupation. They are more likely to work in the healthcare sector, for which a short 3-year degree provides sufficient preparation, and less likely to work in administrative roles. These findings highlight the importance of tailoring educational reforms to the specific demands of different career paths.

1 Introduction

Up until the early 2000s, two disctinct higher education systems were coexisting in Europe. On the one hand, the Anglo-Saxon system was praised for offering a shorter first cycle of university degrees, therefore more accessible and better able to attract students to university. On the other hand, the Continental European system was praised because its longer first cycle university degrees provided more training for university students entering the labor market. These different features of the two systems have sparked a long-standing debate on which system is more effective and better prepares students for the labor market. This paper contributes to the debate by studying the Bologna Process — a reform intended to harmonize higher education systems across Europe converging towards the Anglo-Saxon model — as a natural experiment. Despite this reform involving a significant restructuring for more than 20 countries (OECD, 2011), there is surprisingly little empirical evidence on how it has influenced students' educational and career choices (Kroher et al., 2021). This paper addresses this gap.

I focus on two competing margins at play that are brought about by the reform. The first margin consists of those who, prior to the reform, may have refrained from attending university due to financial or time-related constraints. The introduction of shorter, 3-year bachelor's degrees effectively reduced the cost of obtaining a degree, encouraging more of these students to enroll, thereby increasing the average years of university in the population. The second margin comprises those who, under the old system, would have completed a 4- to 6-year degree but now choose to finish after just three years, reducing their years of university. Hence, whether the reform increases schooling, i.e. which of the two effects dominates, is an empirical question. It it also important to understand how the effects vary by field of study, as students who aim to work in professions where a 3-year degree suffices (e.g., healthcare) might benefit more from the reform compared to students aiming to work in professions where longer degrees are the norm. For this reason, I expect the reform's impact on employment and wages to vary by field of study.

This paper thus explores three key questions: (i) What are the effects of the increased flexibility of university on education choices, and average years of university in the population, on net?; (ii) What is the net effect on employment and wages?; and (iii) Do some occupations and related fields of study particularly benefit from the reform?

I focus on Italy as a case study but results can be informative for other countries that shifted towards the Anglo-Saxon system. Italy provides a compelling setting for two main reasons. First, gap years after high school are not common and hence most students decide whether or not to go to university at age 19. Second, the reform was implemented in a short time span, favoring the use of a Regression Discontinuity design using the year in which individuals turn 19 as a running variable. The idea is that individuals who were older than 19 by the time the reform was implemented were likely to have already decided whether or not to go to university and, at the time of this decision, the reform was not in place. On the other hand, individuals who were 19 or younger by the time of the reform implementation were more exposed to it when making their decision regarding university attendance. My findings suggest that the reform has been particularly effective

at increasing university graduation rates and average years spent in university for females, with this result being mostly driven by their higher participation in healthcare degrees, where the reform created a short 3-year path to access most healthcare related occupations. In turn, women shift from administrative roles to healthcare occupations while keeping constant their overall employment probability, and experiencing an increase in wages. The male sample, on the other side, experiences on mild effects on education and no effects on their employment probability and wages.

The results are particularly interesting because the reform has sparked a heated public debate in Italy. By 2017, almost 20 years after the reform, two of Italy's most prominent newspapers, La Repubblica[1] and Il Sole 24 Ore[2], still referred to the reform as a "flop", claiming that it did not increase enrollment nor employability of graduates and suggesting a reversal of the transition. However, the Italian National University Council responded to these articles emphasizing that empirical evidence so far does not support these claims[3]. In the case of Italy, an extensive causal (Bondonio and Berton, 2018; Di Pietro, 2012) and descriptive (Argentin and Triventi, 2011; Brunori et al., 2013; Cappellari and Lucifora, 2009; Di Pietro and Cutillo, 2008) literature documents positive effects of the reform on enrollment. Comparing cohorts of secondary school graduates before and after the reform, Cappellari and Lucifora (2009) estimate the increase to be around 15%. This is similar in magnitude to what is found by Bondonio and Berton (2018) when comparing first-year enrollment of students under the two systems, exploiting their coexistence due to the staggered implementation of the reform. Di Pietro (2012) compares university enrollment of secondary school leavers from disadvantaged backgrounds, and thus more likely to be affected by the reform, with those from more advantaged backgrounds, thus less affected, and estimate an increase of 7-8%. Other studies on social stratification, however, find an increase in social inequalities in terms of field chosen before and after the reform (Triventi et al., 2017). What remains unexplored, however, is the impact of the reform on graduation rates, and, more critically, how it affects the length of time students spend in education, particularly for those at the second margin discussed earlier. In the case of Italy, it is also still unknown whether the increase in demand for university education introduced by the reform is heterogeneous by field of study. In Portugal, Cardoso et al. (2008) exploit the coexistence of the two systems in the first years of the reform, and compare student's demand via their expressed preferences upon applying. They find that post-reform programs experience a 20% increase in demand compared to not yet reformed programs, and highlight heterogeneity by field, with the reform being particularly important for the education field. In Germany, however, findings suggest little effect of the reform on enrollment, likely driven by the fact that reformed programs in technical subjects such as engineering are significantly less popular when compared to pre-reform programs (Horstschräer and Sprietsma, 2015). An important take-away from studies in Portugal and Germany is the consideration that the effects of reform can significantly differ by field of study. Descriptive studies (Collins and Hewer, 2014; Davies, 2008) focused on specific occupations suggest, in fact, that the

[1] https://scriptsizeurl.com/4jfzv452
[2] https://scriptsizeurl.com/4ftd9txt
[3] https://scriptsizeurl.com/3eknaa2k

Bologna Process is particularly relevant for jobs like nursing and education, as it introduced specific courses tailored to these professions, shifting training for these careers from vocational to higher education. On labour market outcomes, several papers (Farčnik and Domadenik, 2012; Suleman and Figueiredo, 2020; Glauser et al., 2019) have provided a descriptive analysis of the reform, but only Bosio and Leonardi (2010) attempted to identify the causal link between the two. They find that in Italy the college wage premium has decreased after the reform, and that the relative employment probability of university graduates compared to high-school graduates increases but only for the male sample. Focusing only on these two groups of students, however, their findings do not address the selection introduced by the reform: the difference in characteristics between high-school and university graduates changes with the reform itself. Therefore, any observed change in the wage differential might encompass both the effect of selection and the causal impact of the reform on wages. Empirical estimates for the effect of the reform on student's employment and wages thus remains understudied, and this paper aims to fill this gap. I will do so also accounting for previous considerations on heterogeneities related to the field of study, as deciding what to study at university has been shown to be as important as deciding to study in the first place. The growing literature focused on accounting for heterogeneity by field of study when estimating returns to higher education (Kirkeboen et al., 2016; Hastings et al., 2013; Bleemer and Mehta, 2022), however, mostly identifies marginal students by exploiting field-specific admission cutoffs arising from high school GPA and university entrance exam scores or other measures of students' ability upon application. Instead of focusing on marginal students based on ability, I focus on marginal students based on the flexibility and accessibility of the university system. This is an important contribution, because, as Hastings et al. (2013) finds, socioeconomic characteristics can be important in determining returns to field of study and degree selectivity. For instance, students from disadvantaged backgrounds do not benefit from elite business degrees, when connections and socioeconomic status are important for gains in occupational status Zimmerman (2014). Instead, such students are more likely to benefit in less competitive and more safe health degrees. Lastly, this paper relates to the literature studying the effects of changes in length of schooling on earnings (Angrist and Krueger, 1991; Meghir and Palme, 1999; Acemoglu and Angrist, 2000; Oreopoulos, 2007; Pischke and Von Wachter, 2008), and the main contribution here lies in studying changes in minimum years required to complete a university degree rather than focusing on compulsory education changes.

I study this reform using Italian Labour Force Surveys and exploiting two levels of heterogeneity introduced by the reform. Firstly, I identify treated and control individuals using the year in which an individual turned 19 - the most common age for university enrollment in Italy - as a running variable. Secondly, I exploit the fact that the reform was implemented across two different academic years, and use data from the Ministry of Education (MIUR) to identify which regions were "early adopters" of the reform. This allows me to construct a new running variable that deals with any cohort-driven trends, as it depends both on cohorts and spatial implementation of the reform. The discontinuity that allocates people to treated or control groups is therefore based on whether an individual was 19 or younger by the time the reform was

introduced in their region of residence. Focusing on a sample of roughly 270.000 Italian citizens aged 30-36 at the time of interview, I firstly document the implementation of the reform by showing that my regression discontinuity setting is well able to capture the shift to the Anglo-Saxon system. Subsequently, I document that the reform has moderate effects on university attainment: the probability of graduating from university increases by 1.4 percentage points, from a baseline of 24%, while the probability of graduating from a degree that is 4 years or longer, i.e. a pre-reform degree or a post-reform master's degree, decreases by 0.7pp from a baseline of 18%, the net effect on average years spent in university is positive, with an increase of 0.1 from a baseline of 1.5 years. All these effects are stronger for the female sample, who experience a 0.15 year increase in their average years of university. In turn, while employment probabilities remain constant for both females and males, I document an increase in female log and average wages of roughly 1.5% to 2.5%, while no significant effect is detected for males. This implies, for the female sample, a 10% or higher return to one year of university. Lastly, turning to the field of study and occupational heterogeneity, I show that most of the increase in university graduation for the female sample is driven by healthcare degrees, and that this increase is matched with a very similar increase in the probability of working in a healthcare occupation. I present suggestive evidence that the increase in healthcare comes from a decrease in the probability of females working in administrative office roles. My paper is therefore the first to bridge together the three strains of literature that have so far, separately, analysed the Bologna Process, providing a complete picture of the causal impact of the reform on (i) education and field of study choices, (ii) employment and wages, and (iii) career path choices.

The rest of the paper is structured as follows: section 2 describes the reform and institutional background, section 3 the data and section 4 the empirical strategy and identification, section 5 describes the main findings and lastly section 6 concludes.

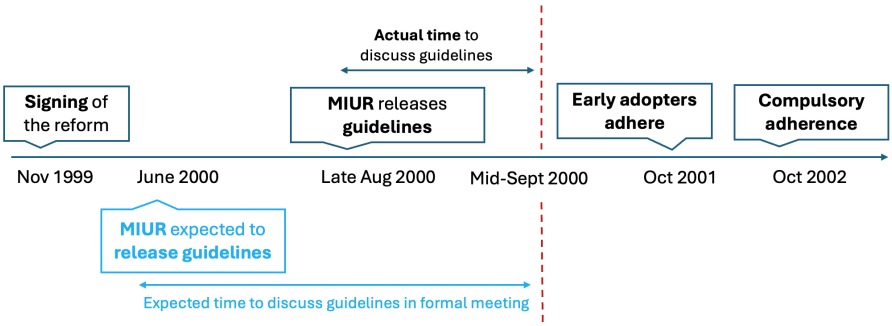
2 Reform background

Before delving into the institutional details of the reform adoption in Italy, it is important to understand why the Bologna Process chose to harmonize higher education with the Anglo-Saxon system rather than the Continental European system. This happened for three main reasons. Firstly, simple statistics indicated that the Anglo-Saxon system was more attractive for students. For example, in 2004, 39 percent of the US population aged 25–64 had attained tertiary education, compared to only 23 percent in Europe (Aghion, 2006). Secondly, the Continental European system was complex, with courses of varying lengths depending on the subject, and types of degrees varying by country. The Anglo-Saxon system is more transparent and comparable across countries, thereby reducing obstacles to workers' mobility. Lastly, the shorter bachelor's degrees offered by the Anglo-Saxon system allows students to complete their studies more quickly, reducing the costs of making a wrong choice when enrolling. This makes higher education more flexible and accessible (Jacobs and Van Der Ploeg, 2006).

2.1 Institutional Background in Italy

After the signing of the Bologna Process in 1999, Italy was the first among the signing countries to effectively introduce the reform. Compulsory adherence to the reform was set to 18 months from the signing of the reform, leaving universities to introduce the reform in the academic years 2000/1 and 2001/2. The timeline of the reform implementation is summarized in figure 1. To implement the reform, the new regulations introduced required department-level discussions, or faculty meetings, and approvals for each reformed curriculum degree. In Italy at the time, these meetings were usually pre-scheduled at the beginning of each term, meaning that the meetings for April to September were scheduled at the beginning of the summer term. To be able to implement the reform in the academic year 2000/01, these meetings had to take place at least two weeks in advance of the fall semester's beginning, typically starting in the first days of October 2000. This time was necessary to ensure the proper establishment of all the required administrative procedures, considering that the typical enrollment period spanned from mid-September to the end of October for most departments. As the Ministry of Education unexpectedly delayed the release of the final guidelines for the reform implementation to the beginning of August 2000 and as faculty meeting do not place in August, the only departments that were able to adopt the reform in the academic year 2000/1 were those whose faculty meeting happened to be scheduled in the first half of September 2000 (Bondonio and Berton, 2018). Because it is unlikely that there is a correlation between university quality and the probability that a faculty had pre-scheduled at a time that allowed for the transition in the academic year 2000/01, it is reasonable to assume the timing of the adoption of the reform is random. Throughout the paper, I will also show that faster adoption at the regional level is uncorrelated with better economic conditions of the region.

Figure 1: Timeline of the implementation of the reform in Italy



The new system implemented a two-cycle structure with undergraduate degrees of three years, followed by optional master's degrees of two years, and replaced the old single-cycle structure in which degree length varied from 4 to 6 years. Due to the provisions of the Italian constitution (as outlined in Article 34), which grant citizens the right to access any level of education, it was considered unconstitutional for the affected departments to reject enrollment applications from high school graduates. Consequently, these Italian departments were unable to impose enrollment caps or establish a selective admission process for high

school graduates. These unique features of the Italian university system not only facilitate the estimation of the reform's impacts without introducing attrition bias concerns but also ensure that the estimated effects are not restricted or exaggerated by institutional characteristics. Lastly, it is important to mention that not all fields of study adopted the reform: education, law, architecture, medicine and pharmacy paths kept the single-cycle structure even in the post-reform period.

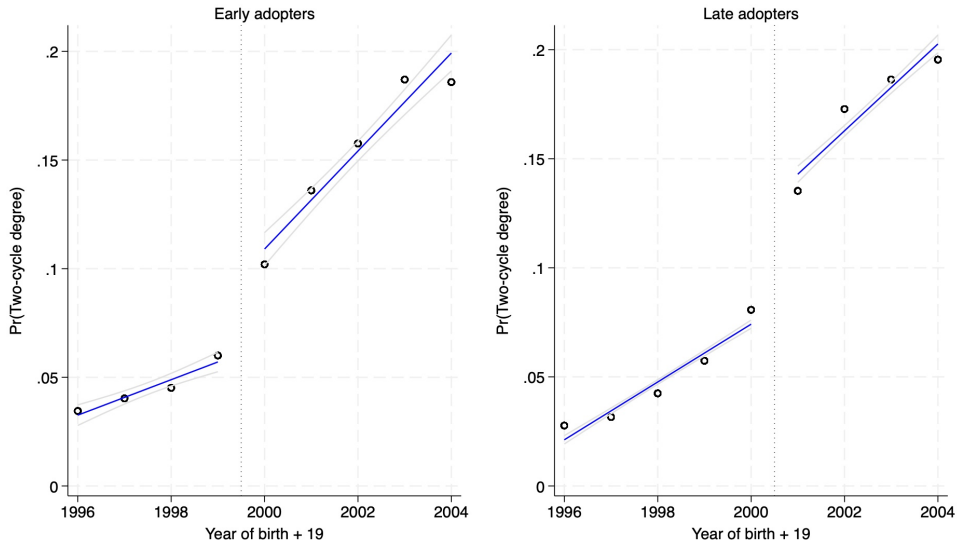
3 Data

I use individual-level data from the Labor Force Surveys (LFS), collected by the Italian National Institute of Statistics (ISTAT) every trimester, and restrict my analysis to the survey years 2009-2020, as the date of birth is not available for earlier surveys. The next step is to determine which individuals belong to the early and late adoption groups of the reform. To answer this question, I retrieve department-level data from the Ministry of Education (MIUR), which records the number of students enrolled in single-cycle or two-cycle degrees for each department of each university across academic years starting from 2000/2001.

Early and late adopters As my analysis does not only focus on individuals who attended university, but rather on the share of people attending university with respect to the whole sample of individuals I need to aggregate MIUR data at a higher level than just university-department, a level that is common for all individuals in the LFS data, regardless of their education level. I decide to aggregate MIUR data at the regional level, using university locations, to classify regions into early and late adopters of the reform, and use this information to allocate LFS individuals to one of the two groups based on their reported region of residence. The assumption underlying this is that current region of residence of individuals is a good proxy for the region in which they studied, and I note that this assumption is likely satisfied as mobility at the regional level in Italy is low: 84% of graduates reside in the region where they studied, 79% study in the region they were born, 78% have same birth and adulthood residence (Bondonio and Berton, 2018). The second decision is on how to classify regions as early or late adopters. Using MIUR data, I sum the number of students enrolled in two-cycle degrees and the total number of students enrolled in any university degree, and compute the ratio for each region. Figure 10 shows that, when rounding ratios to the nearest decimal, only four regions have a positive ratio in the academic year 2000/2001. Hence, individuals living in these four regions will form the "early" adopters of the reform.

I then proceed to check whether this staggered implementation matches the ISTAT data. To do so, I use a dummy that indicates whether an individual has completed a bachelor's or master's - and therefore a two-cycle degree - and plot this as an outcome variable against a running variable with birth cohorts. I would expect early adoption regions to experience a shift towards two-cycle degrees one cohort earlier than late adoption regions. This is indeed what we see in Figure 2. Cohorts who were 19 years old after 2000 in regions that adopted the reform in 2000/2001 are significantly more likely to have completed a two-cycle

Figure 2: Staggered implementation of the reform



degree compared to cohorts who were 19 years old right before 2000; for the late adopters the threshold is moved to cohorts who were 19 years old after 2001.

This distinction is needed to create a new running variable that indicates how far the cohort of birth of an individual is from the time in which the reform was adopted in that person's region of residence. In the next sections, I will refer to this running variable as Z_i, i.e. the distance, in years, between the year individual i was 19 and the year in which the reform was implemented in individual i's region:

$$Z_i = \begin{cases} c_i - 2000 \text{ if } i \in \text{Early Adopters} \\ c_i - 2001 \text{ if } i \in \text{Late Adopters} \end{cases}$$

where $c_i = \text{Year of birth} + 19$.

Sample definition and variables of interest Another important limitation of the ISTAT data is that, as I only use data from 2009 to 2020, for earlier cohorts I observe people that are on average older by the time of interview compared to later cohorts. Using the reconstructed running variable Z_i, Figure 11 in the Appendix shows the declining average age at interview on the unrestricted sample (blue line). I focus on individuals aged 30-36 by the time of interview in order to keep age constant at least 3 time periods before and after the reform, as illustrated by the red line.

To study the impact of the reform on the likelihood of completing a university degree, I create a dummy variable that takes the value of one if an individual's highest education achieved is any university-level degree. Additionally, I examine whether the reform has affected the proportion of people completing degrees that are 4 years or longer. Before the reform, this was the standard degree length, while post-reform, only those who complete a master's degree spend more than 3 years in university. If I observe an increase in

the probability of completing a university degree due to the reform, this latter variable will help determine whether the increase is primarily from more people obtaining 3-year degrees or a mix of both shorter and longer degrees. As previously mentioned, the impact on the average years of university is uncertain to determine ex-ante. I will study this using the difference between the age an individual reports leaving university and 19 if they went to university, assigning a value of 0 years of university to individuals who do not have a university degree. I split fields of study into 4 broad categories: (i) STEM, i.e. sciences, technology, engineering and mathematics, (ii) humanities, including languages and education; (iii) social sciences, including psychology, law and economics; and (iv) healthcare. Occupations related to those fields of study are: (i) specialists in science and informatics, engineers and architects, technical professions in STEM fields (ii) specialists in humanities, teachers and educators; (iii) specialists in social sciences and arts, lawyers and technical professions in admin, organizational and financial activities; and (iv) nurses, doctors, social carers and educators or teachers. To analyze the patterns, I create dummies taking value one if an individual has completed any university education in a specific field, and zero otherwise. Similarly, I create dummies taking value one if an individual works in a specific field and zero otherwise. I also look at effects on other occupations, and follow the CP2011 classification of occupations grouping occupations as: managers, professionals, technicians and associate professionals, office clerks, sales and service workers, craft workers, plant and machine operators, and elementary occupations. I refer to managers, professionals and technicians and associate professionals as high-skilled occupations, as those often require a university degree; and craft workers plant and machine operators, and elementary occupations as low-skilled occupations. Office clerks and sales and service workers, instead, are mid-skill occupations where some university graduates might end up working. Descriptives for these variables, for both the control and treatment groups are reported in table 3 in the Appendix.

4 Identification and empirical strategy

Using the data described in the previous section, I identify treated individuals, i.e. those more exposed to the reform, using the treatment dummy $D_i = \mathbf{1}[Z_i > 0]$. I also define TC_i to be a treatment indicator that takes value one if individual i has completed a two-cycle university degree. To the extent that the probability of completing a two-cycle degree changes discontinuously for individuals who were 19 or younger by the time the reform was implemented in their region (see figure 3), Z_i can be used as a running variable in a fuzzy RD setting where TC_i is the treatment variable, D_i is the treatment dummy and Y_i is the outcome. To estimate the impact of the reform, I estimate the first stage equation capturing the effect of the reform on the probability of completing a two-cycle university degree

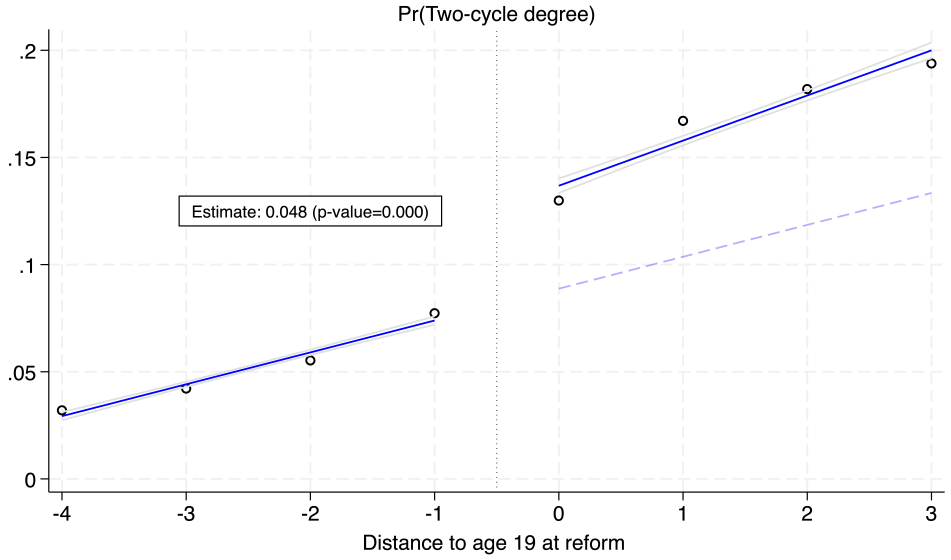
$$TC_i = \alpha_0 + \alpha_1 D_i + \alpha_2 Z_i + \alpha_3 Z_i \times D_i + \eta_i \tag{1}$$

and the second stage capturing the effect of the reform on education and labour market outcomes:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 Z_i + \beta_3 Z_i \times D_i + \epsilon_i \tag{2}$$

Throughout the paper, I will report estimates of the coefficients of interest, α_1 and β_1 for 3 and 4-cohort bandwidths, as age is roughly constant for such cohorts (see Figure 11). I also consider an additional specification in which I control for local unemployment rates, i.e. controlling for the unemployment rate in the region in which i lives, for the year in which i is interviewed. I estimate equations (1) and (2) on all individuals within the bandwidths, without restricting my sample to individuals with a certain education level. As individuals who pursue university education under the new system may differ from those who attended university under the previous system, the analysis should encompass all individuals randomized to the control group, without cutting the sample according to behavior that may have been affected by the random assignment (Duflo et al., 2007), i.e. going to university in the first place. This allows me to recover the impact of exposure to the reform on outcomes of interest.

Figure 3: First stage: transition to the two-cycle system



This identification strategy relies on three main assumptions. The first one is the first-stage assumption, stating that the treatment status TC_i should jump discontinuously at the threshold. Figure 3 plots the probability that an individual i has completed a two-cycle university degree (i.e. a bachelor's or master's) as a function of the running variable, $Pr(TC_i = 1|Z_i)$. We see that the reform increases the probability of completing a two-cycle university degree increases by 4.8 percentage points when considering a bandwidth of 4 cohorts above and below the cutoff. Table 4 in the Appendix reports an estimate for the first stage equation (1), with 3 and 4 cohort bandwidths, showing that the findings are robust to different specifications.

5 Results

I now turn to answering the core questions of this paper: (i) has the reform increased university graduation rates, has it decreased the share of students who graduate from degrees that are at least 4 years long, and what is the net effect on average years spent in university; (ii) on net, what is the effect on employment and earnings; and (iii) is there heterogeneity by field of study and occupation?

Effect of the reform on educational outcomes The first panel of Figure 4 illustrates that there is an increase in overall university attainment by 1.4pp at the threshold, while the second panel shows that the share of individuals who completed a university degree with a curriculum of 4 or more years, i.e. a pre-reform single-cycle degree or a post-reform master's degree, decreases by 0.7pp. Corresponding estimates

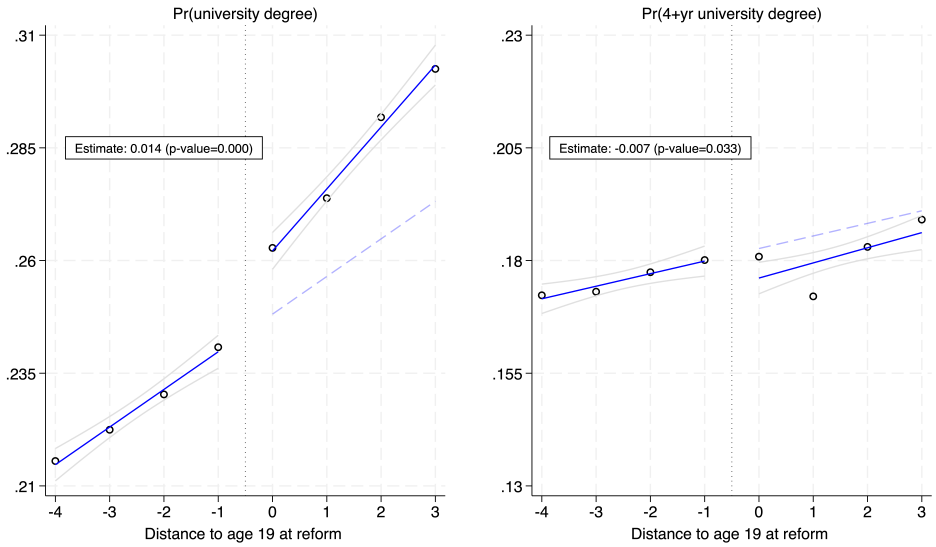


Figure 4: Effect of the reform on university completion and average age at end of studies

are reported in 1. The first two columns present the reform's impact on the probability of completing any university degree. With a 4-cohort bandwidth, the reform increases university graduation rates by 1.4pp for the overall sample, 1.8pp for females, and 0.8pp for males. These estimates correspond to the first group of marginal students affected by the reform. This group, previously deterred from enrolling in university due to the associated costs and time commitment, now takes advantage of the newly introduced shorter degree programs. The second pair of columns focuses on the second group of marginal students, showing the reform's effect on the probability of completing a university degree of 4 years or longer (i.e., single-cycle or master's degrees). For females, the reform reduces the likelihood of completing a longer degree by 1.2pp, while for males the effect is nearly zero. The last column shows the net effect on average years spent in university. Female spend statistically more time in university (0.147 years on average) after the reform, compared to males (0.041 years on average). Table 5 in the Appendix shows the same regressions when

Table 1: Effects on educational attainment

	Probability that highest education is:					
	Any university degree		4+ years university degree		Average years of university	
Bandwidth	3	4	3	4	3	4
	Whole sample					
RD Jump	0.012***	0.014***	-0.006*	-0.007**	0.070**	0.102***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.033)	(0.028)
Mean	0.241	0.241	0.180	0.180	1.667	1.667
BW obs	206,788	269,026	206,788	269,026	206,788	269,026
	Heterogeneity by gender					
	Female					
RD Jump	0.013**	0.018***	-0.013**	-0.012**	0.110**	0.147***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.047)	(0.040)
Mean	0.294	0.294	0.221	0.221	1.973	1.973
BW obs	105,429	137,193	105,429	137,193	105,429	137,193
	Male					
RD Jump	0.010*	0.008*	-0.000	-0.002	0.020	0.041
	(0.005)	(0.004)	(0.005)	(0.004)	(0.045)	(0.038)
Mean	0.186	0.186	0.138	0.138	1.352	1.352
BW obs	101359	131,833	101,359	131,833	101,359	131,833
p-value Female = Male	0.745	0.169	0.067	0.117	0.171	0.055

* p<0.10, ** p<0.05, *** p<0.01.

Notes: Standard errors are reported in parenthesis. RD Jump estimates reflect equation (2); for each dependent variable, 3 and 4 cohort bandwidths are reported. The first dependent variable is the probability of an individual going to university. The second one is the probability of going to university for 4 or more years, i.e. a single-cycle or a master degree. The third one is the average years spent in university. Mean is the mean of the outcome variable in the cohort right before the reform was implemented.

run on a subsample that excludes individuals studying subjects that were not affected by the reform, i.e. education, law, architecture, medicine and pharmacy, as those paths kept the single-cycle structure even in the post-reform period. Considering the 4-cohort bandwidth, we see that females are 2.7pp more likely to go to university, from a baseline of 24%, implying an increase of female university graduates of more than 10%. Lastly, Table 6 in the Appendix shows that estimates are robust to the inclusion of controls for local unemployment rates.

Effect of the reform on labor market outcomes We have established that, for females, the reform increased average years spent in university on net. Table 2 reports estimates for employment and wages, showing that females' average wages increase, yet this is not driven by an increase in employment but rather by an increase in wages conditional on employment. Males, on the other hand, do not experience any significant change in the labor market. Table 7 in the Appendix shows that the positive effects on female wages are robust to the inclusion of controls for the local unemployment rates. A graphical representation of

Table 2: Effect of the reform on employment and wages

	Employment Probability		Average Net Monthly Wage		Log Net Monthly Wage	
Bandwidths	3	4	3	4	3	4
			Whole sample			
RD Jump	0.001	-0.002	3.342	-0.635	0.006	0.008*
	(0.004)	(0.004)	(7.166)	(6.024)	(0.005)	(0.005)
Mean	0.687	0.687	769.629	769.629	7.038	7.038
BW obs	206,788	269,026	173,819	225,820	109,333	142,599
			Heterogeneity by gender			
			Female			
RD Jump	0.005	0.002	15.112*	9.576	0.024***	0.021***
	(0.007)	(0.005)	(9.000)	(7.562)	(0.008)	(0.007)
Mean	0.602	0.602	602.144	602.144	6.912	6.912
BW obs	105,429	137,193	93,117	121,007	51,177	66,675
			Male			
RD Jump	-0.001	-0.003	-6.979	-5.893	-0.007	0.001
	(0.006)	(0.005)	(10.705)	(9.006)	(0.006)	(0.005)
Mean	0.774	0.774	961.273	961.273	7.149	7.149
BW obs	101,359	131,833	80,702	104,813	58,156	75,924
p-value Female = Male	0.475	0.555	0.114	0.188	0.003	0.022

* p<0.10, ** p<0.05, *** p<0.01.

Notes: Standard errors are reported in parenthesis. RD Jump estimates reflect equation (2); for each dependent variable, 3 and 4 cohort bandwidths are reported. Employment Probability is a dummy taking value one if an individual is working. Average Net Monthly Wage is a variable taking the value of the individual's net monthly wage if working, and 0 otherwise, indicating average wage unconditional of employment. Log Net Monthly Wage is the logarithmic transformation of an individual's net monthly wage, conditional on working. Mean is the mean of the outcome variable in the cohort right before the reform was implemented.

the increase in wages and years of university for females, on net, is presented in Figure 5, where we see a jump in both outcomes. We see that a 0.15 year increase in average years of university in the female population corresponds to an increase in average wages of 9.6EUR per month, from a baseline of roughly 600EUR per

month, i.e. a 1.5% increase in wages unconditional on employment. This implies that the returns to one year of university education is 10% for the female sample.

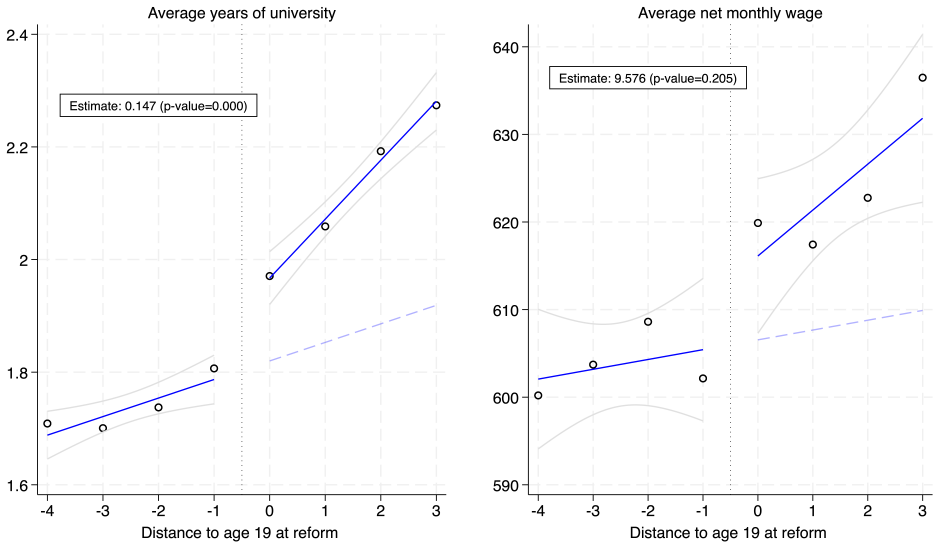


Figure 5: Effect of the reform on years of university and wages for females

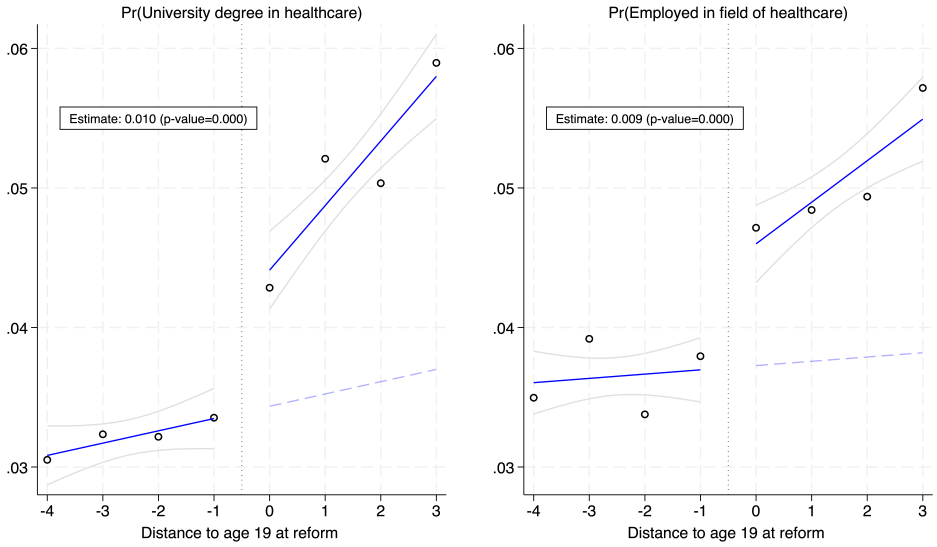


Figure 6: Effect of the reform on female studying and working in the field of healthcare

Heterogeneity by field of study and occupation As previously discussed, it is likely that the reform benefited some fields of study more than others, simply because some occupations might be well-suited to graduates with 3-year bachelor's degrees, while others may leave students underprepared. Figure 6 clearly

shows that the reform increases the probability of females studying and working in the healthcare sector. This is expected, as the reform introduced 3-year university degrees in nursing and other healthcare sectors that facilitated access to such occupations. Specifically, the reform increased the probability of a female graduating from a degree in healthcare by 1pp, from a baseline of roughly 3.3%. A very similar change is observed for the probability of working in the healthcare sector, which increased by 0.9pp from a baseline of 3.7%. This seems to show that the labor market has absorbed well the increase in supply of female graduates in the healthcare sector. Figures 7 and 8 explore other fields of study and their associated occupations. They plot the effect of the reform on dummies taking value 1 if an individual studied or works in a given field, and 0 otherwise. The estimated effects and confidence intervals relate to the specification with 4 cohort bandwidth. What stands out here is that, indeed, most of the effects of the reform are to be found in the healthcare sector. Lastly, I try to understand where the increase in healthcare workers is coming from.

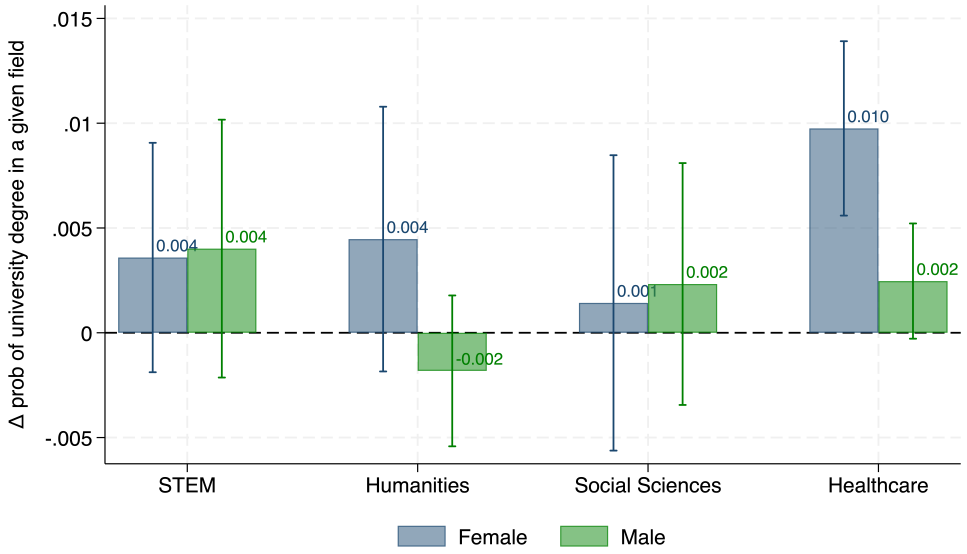


Figure 7: Effect of the reform on field of study

From Table 2, we know that the overall employment probabilities for females is unaffected by the reform, yet 1pp more of them work in the healthcare sector, implying that there must be a "missing mass" in some other sector. I explore this in Table 8 in the Appendix, where I estimate the effect of the reform on occupations. I find that, for females, the increase in probability of working in healthcare is matched with a decreased probability of working in administrative roles (office clerks) which is similar in size. This can be also seen in Figure 9, where both the jumps are similar in absolute value, but where it is also evident that the trends in the two sectors mirror each other for cohorts before and after the reform.

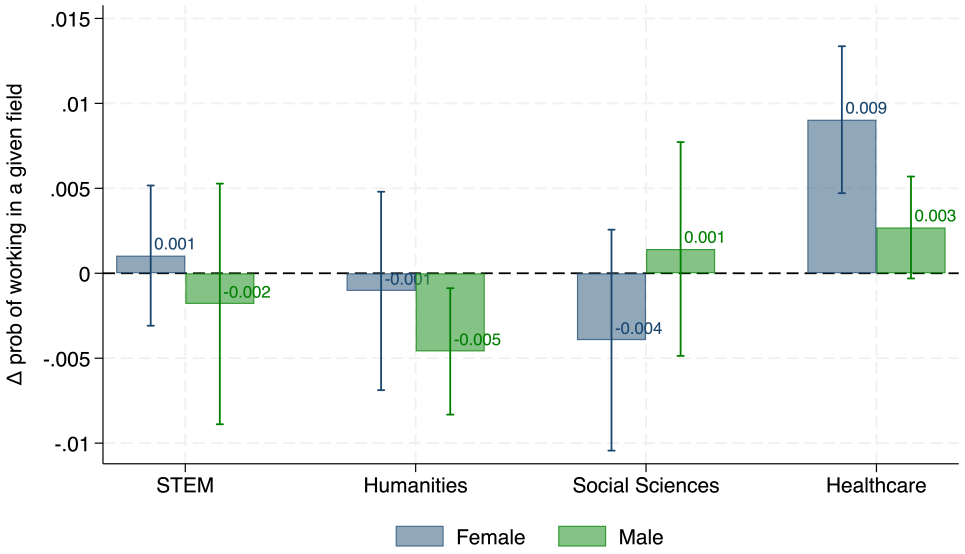


Figure 8: Effect of the reform on occupations

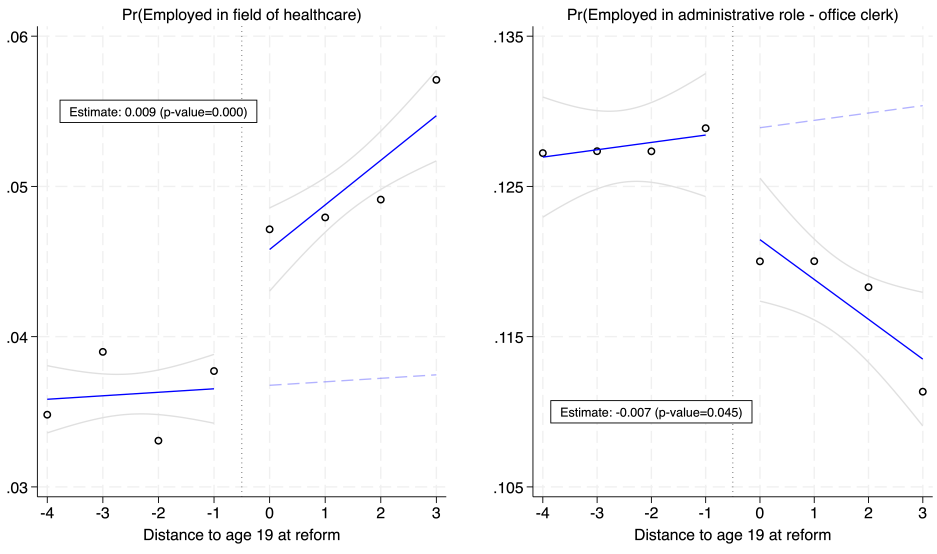


Figure 9: Effect of the reform on female occupation: the shift from administrative roles to healthcare

6 Conclusion

More than 20 countries in Europe and beyond have restructured their university system as part of the Bologna Process in the early 2000s. This implied transitioning from longer first cycle degrees of 4 to 6 years, to shorter first cycle degrees of 3 years. Ex-ante, the impact of the reform on university attainment is ambiguous: schooling might increase because more people go to university, but it might also decrease if students who previously would have completed a longer degree now opt to finish studying after a shorter 3-year degree. This paper provides the first robust and comprehensive overview of the effects of the Bologna Process, focusing on Italy as a case study. Exploiting the roll-out of the reform, I compare education and labor market outcomes for cohorts of individuals who were deciding to go to university right before and right after the reform was implemented. I document that, on net, the reform positively affects females, increasing the average years spent in university by 0.15 and, consequently, increasing their earnings by 1.5-2%, implying a 10% return to one year of university for this sample. No significant effects are found for the male sample. The findings are robust to different bandwidths considered, as well as to the inclusion of controls for local unemployment rates. Females are more likely to pursue university degrees in healthcare and, consequently, are more likely to be employed in healthcare occupations. I present suggestive evidence that the shift towards healthcare occupations comes from less women being employed in administrative office clerk roles. Overall, this emphasizes the importance of tailoring educational reforms to the specific demands of different career paths.

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Appendix

A Background of the reform

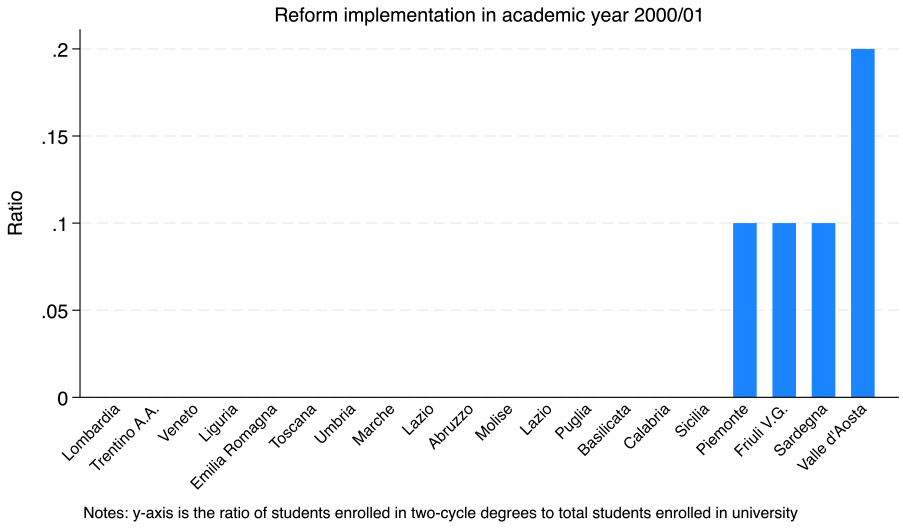

Figure 10: Early and late adopters by region

B Data and descriptive statistics

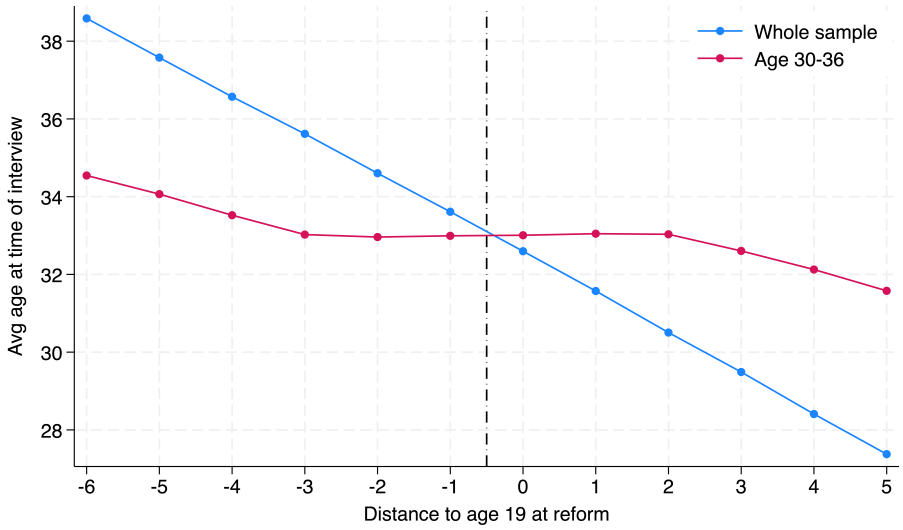

Figure 11: Explaining why focus on selected sample of individuals aged 30-36.

Table 3: Descriptives on selected sample

	Control			Treatment		
	N = 146,403			N = 122,623		
VARIABLE	Average	Min	Max	Average	Min	Max
Age	33.10	30	36	32.94	30	36
Year of birth	1979	1977	1981	1983	1981	1985
Female	0.51	0.00	1.00	0.51	0.00	1.00
University (two-cycle)	0.05	0.00	1.00	0.17	0.00	1.00
University	0.23	0.00	1.00	0.28	0.00	1.00
University (4+ years)	0.18	0.00	1.00	0.18	0.00	1.00
Years of university	1.63	0	17	1.91	0	17
Employment probability	0.69	0.00	1.00	0.68	0.00	1.00
Net monthly wage	773.88	0	3000	785.90	0	3000

C Results

Table 4: First stage estimates

	Probability that highest education is:	
	Two-cycle university degree	
Bandwidths	3	4
RD Jump	0.040***	0.048***
	(0.003)	(0.002)
Mean	0.077	0.077
BW Obs	206,788	269,026

* p<0.10, ** p<0.05, *** p<0.01.

Notes: Standard errors are reported in parenthesis. The two columns refer to the effect of the reform on the probability of having a two-cycle degree as highest education, with 3 and 4 year bandwidths above and below the threshold. As outlined in equation (1), estimates assume a linear trend above and below the threshold. Mean is the mean of the outcome variable in the cohort right before the reform was implemented.

Table 5: Effects on educational attainment excluding unaffected subjects

	Probability that highest education is:					
	Any university degree		4+ years university degree		Average years of university	
Bandwidth	3	4	3	4	3	4
	Whole sample					
RD Jump	0.015***	0.019***	-0.004	-0.002	0.095***	0.132***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.031)	(0.026)
Mean	0.201	0.201	0.137	0.137	1.368	1.368
BW Obs	196,953	256,089	196,953	256,089	196,953	256,089
	Heterogeneity by gender					
	Female					
RD Jump	0.020***	0.027***	-0.007	-0.003	0.162***	0.219***
	(0.006)	(0.005)	(0.005)	(0.004)	(0.045)	(0.038)
Mean	0.242	0.242	0.164	0.164	1.581	1.581
BW Obs	98,744	128,389	98,744	128,389	98,744	128,389
	Male					
RD Jump	0.007	0.009**	-0.003	-0.002	0.015	0.024
	(0.005)	(0.004)	(0.004)	(0.004)	(0.043)	(0.037)
Mean	0.161	0.161	0.111	0.111	1.159	1.159
BW Obs	98,209	127,700	98,209	127,700	98,209	127,700
p-value Female = Male	0.096	0.006	0.620	0.820	0.008	0.000

* p<0.10, ** p<0.05, *** p<0.01.

Notes: Standard errors are reported in parenthesis. RD Jump estimates reflect equation (2); for each dependent variable, 3 and 4 cohort bandwidths are reported. The first dependent variable is the probability of an individual going to university. The second one is the probability of going to university for 4 or more years, i.e. a single-cycle or a master degree. The third one is the average years spent in university. Mean is the mean of the outcome variable in the cohort right before the reform was implemented. Unaffected subjects include medicine, pharmacy, law, education and architecture.

Table 6: Effects on educational attainment controlling for local unemployment

	Probability that highest education is:					
	Any university degree		4+ years university degree		Average years of university	
Bandwidth	3	4	3	4	3	4
			Whole sample			
RD Jump	0.012***	0.015***	-0.006*	-0.006**	0.070**	0.106***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.033)	(0.028)
Mean	0.241	0.241	0.180	0.180	1.667	1.667
BW Obs	206,788	269,026	206,788	269,026	206,788	269,026
			Heterogeneity by gender			
			Female			
RD Jump	0.012*	0.018***	-0.014**	-0.012**	0.105**	0.148***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.047)	(0.040)
Mean	0.294	0.294	0.221	0.221	1.973	1.973
BW Obs	105,429	137,193	105,429	137,193	105,429	137,193
			Male			
RD Jump	0.010*	0.009**	0.000	-0.002	0.023	0.047
	(0.005)	(0.004)	(0.005)	(0.004)	(0.045)	(0.038)
Mean	0.186	0.186	0.138	0.138	1.352	1.352
BW Obs	101,359	131,833	101,359	131,833	101,359	131,833
p-value Female = Male	0.922	0.203	0.050	0.099	0.271	0.065

* p<0.10, ** p<0.05, *** p<0.01.

Notes: Standard errors are reported in parenthesis. RD Jump estimates reflect equation (2) with the addition of controls for local unemployment rates. For each dependent variable, 3 and 4 cohort bandwidths are reported. The first dependent variable is the probability of an individual going to university. The second one is the probability of going to university for 4 or more years, i.e. a single-cycle or a master degree. The third one is the average years spent in university. Mean is the mean of the outcome variable in the cohort right before the reform was implemented.

Table 7: Effect of the reform on employment and wages controlling for local unemployment

	Employment Probability		Average Net Monthly Wage		Log Net Monthly Wage	
Bandwidths	3	4	3	4	3	4
			Whole sample			
RD Jump	0.000	0.001	2.740	4.539	0.006	0.009**
	(0.004)	(0.004)	(6.613)	(5.561)	(0.005)	(0.004)
Mean	0.687	0.687	769.629	769.629	7.038	7.038
BW obs	206,788	269,026	173,819	225,820	109,333	142,599
			Heterogeneity by gender			
			Female			
RD Jump	0.001	0.002	9.252	11.152	0.019**	0.018***
	(0.006)	(0.005)	(8.250)	(6.925)	(0.008)	(0.007)
Mean	0.602	0.602	602.144	602.144	6.912	6.912
BW obs	105,429	137,193	93,117	121,007	51,177	66,675
			Male			
RD Jump	0.001	0.001	-1.211	3.523	-0.003	0.005
	(0.005)	(0.005)	(9.844)	(8.294)	(0.006)	(0.005)
Mean	0.774	0.774	961.273	961.273	7.149	7.149
BW obs	101,359	131,833	80,702	104,813	58,156	75,924
p-value Female = Male	0.973	0.849	0.415	0.480	0.028	0.128

* p<0.10, ** p<0.05, *** p<0.01.

Notes: Standard errors are reported in parenthesis. RD Jump estimates reflect equation (2) with the addition of controls for local unemployment rates. For each dependent variable, 3 and 4 cohort bandwidths are reported. Employment Probability is a dummy taking value one if an individual is working. Average Net Monthly Wage is a variable taking the value of the individual's net monthly wage if working, and 0 otherwise, indicating average wage unconditional of employment. Log Net Monthly Wage is the logarithmic transformation of an individual's net monthly wage, conditional on working. Mean is the mean of the outcome variable in the cohort right before the reform was implemented.

Table 8: Effects on all occupations

	Probability of working in:											
	Healthcare		**Other high-skill**		**Office Clerks**		**Sales and Services**		**Other**		**Any occupation**	
Bandwidths	3	4	3	4	3	4	3	4	3	4	3	4
	Whole sample											
RD Jump	0.007***	0.006***	-0.005	-0.005	-0.003	-0.004*	0.009***	0.012***	-0.008**	-0.010***	0.001	-0.002
	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
mean	0.028	0.028	0.231	0.231	0.094	0.094	0.128	0.128	0.207	0.207	0.687	0.687
BW Obs	206,788	269,026	206,788	269,026	206,788	269,026	206788	269,026	206,788	269,026	206,788	269,026
	Heterogeneity by gender											
	Female (F)											
RD Jump	0.012***	0.009***	-0.002	-0.004	-0.009**	-0.007**	0.004	0.003	-0.000	0.002	0.005	0.002
	(0.003)	(0.002)	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)	(0.007)	(0.005)
mean	0.038	0.038	0.215	0.215	0.129	0.129	0.150	0.150	0.070	0.070	0.602	0.602
BW Obs	105,429	137,193	105,429	137,193	105,429	137,193	105,429	137,193	105,429	137,193	105,429	137,193
	Male (M)											
RD Jump	0.001	0.003*	-0.008	-0.006	0.003	-0.002	0.015***	0.020***	-0.012*	-0.018***	-0.001	-0.003
	(0.002)	(0.002)	(0.006)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.005)	(0.006)	(0.005)
mean	0.017	0.017	0.246	0.246	0.058	0.058	0.106	0.106	0.347	0.347	0.774	0.774
BW Obs	101,359	131,833	101,359	131,833	101,359	131,833	101,359	131,833	101,359	131,833	101,359	131,833
p-value F = M	0.000	0.018	0.399	0.830	0.029	0.206	0.094	0.001	0.120	0.001	0.475	0.555

* p<0.10, ** p<0.05, *** p<0.01.

Notes: Standard errors are reported in parenthesis. RD Jump estimates reflect equation (2); for each dependent variable, 3 and 4 cohort bandwidths are reported. Healthcare refers to the probability of working in the healthcare sector. Other high-skill is the proability of working in a high-skill occupation that is not healthcare. Admin is the probability of working in the administrative sector. Sales is the probability of working in the sales and services sector. Other low-skill is the probability of working in low-skill occupations. Lastly, Any occupation is the overall probability of being employed. Mean is the mean of the outcome variable in the cohort right before the reform was implemented.