Balancing Study and Work: Heterogeneous Impact of the Bologna Reform on the Labor Market *

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Abstract

The Bologna reform, the largest European education reform, was implemented in Russia in 2011. The reform shortened the duration of some undergraduate programs by one year and compressed their curricula. Using a difference–in–differences design, I find that the reform had no short– or medium–term adverse effects on employment. Further, I find that null average effects on wages mask considerable heterogeneity. I find that female students with high relative returns studied more intensively, optimally invested in their human capital, and secured stable wages. In contrast, male students with low relative returns underinvested in human capital and experienced a decline in wages.

Keywords: Bologna reform; Higher education; Human capital; Russia

JEL Codes: I23, I26, I28, J24, J31

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1 Introduction

The Bologna reform, the most significant educational reform of the 21st century in Europe, aimed to create a European Higher Education Area by implementing a system of easily readable and comparable degrees and a credit transfer system to enhance student mobility. The reform resulted in a reduction of both the length of university programs and the content of curricula throughout Europe (Schomburg, 2011).[1] This study investigates the influence of the reform on graduates' labor market outcomes. The findings of this study are crucial for policymakers and university management to understand the benefits of the reform, as there has been ongoing debate about its effectiveness for graduates (Teichler, 2011). Kroher et al. (2021) review prior work on the impact of the Bologna reform, concluding that studies in the literature "often do not allow for causal conclusions and only provide a fragmented picture, which makes evidence–based adjustments in reform implementation difficult".

This paper investigates the impact of the Bologna reform on employment and wages of university graduates in Russia. The case of Russia is particularly suitable for identifying causal effects of the reform due to three key factors: the unexpected timing of the reform, the absence of a gap year culture, and high compliance with the nominal study period. The reform was announced in 2003, but its full implementation did not occur until 2011, due to the complexities of executing such an extensive reform across the nation. Furthermore, Russian students rarely take gap years, leaving little room for adjusting application strategies. This unanticipated timing, combined with the lack of a gap year culture, enables the elimination of anticipation effects among high school graduates. Additionally, in contrast to other countries implementing the Bologna reform, Russian university students typically graduate within the nominal study period, seldom exceeding their official study duration due to administrative barriers and financial costs. This factor mitigates concerns about students endogenously adjusting their graduation time, which could otherwise bias results. The paper presents evidence supporting these identifying assumptions based on administrative data.

In a nutshell, the Bologna reform transformed university degrees from five–

[1] The Bologna reform's impact is comparable to changes observed in higher education systems in Colombia (Arteaga, 2018) and the US (Denning et al., 2022).

year to four–year programs and changed the content of curricula, though some programs were not affected by the reform. The reform's unanticipated timing, the lack of a gap year culture, and high compliance with the nominal study duration create a unique opportunity to utilize programs that underwent changes as a treatment group and those that remained unchanged as a control group. These features provide a credible identification strategy to estimate the impact of the reform using a difference–in–differences design. The key identifying assumption is that, in the absence of the reform, the quantity and quality of university applicants and graduates would have evolved similarly in programs that experienced a policy change relative to programs that did not experience such a change.

This study employs data from two sources to investigate the impact of the Bologna reform on employment and wages of university graduates in Russia. The first dataset, the National Survey of Graduate Employment, contains wage and employment information for 11,768 individuals who graduated between 2010 and 2015. The second dataset, drawn from the Russian Labor Force Survey, includes employment data for 300,580 individuals who graduated between 2010 and 2020. Both datasets offer comprehensive information on demographics, educational background, and labor market outcomes.

The results indicate that the Bologna reform did not adversely affect the employment of graduates in the short– or medium–term. However, the reform's impact on wages displayed notable differences based on sex. Specifically, female graduates' wages were not negatively affected, whereas male graduates experienced a significant decline in wages. There were no heterogeneous effects on employment by sex.

This study explores a novel mechanism explaining heterogeneous impact of the Bologna reform on the wages of males and females. This phenomenon can be explained by considering the trade–off students make between investing in education or in working. I propose a model of students' decision–making where females have greater *relative* returns to education compared to returns to work experience than males do. If relative returns are high, it is optimal to study more and work less during the study program. When the reform forced students to have fewer years of education, female students, with higher relative returns, chose to study more intensively, at the expense of working while studying, in order to invest optimally in their human capital and secure stable wages after

graduation. On the other hand, males, with lower relative returns, prioritized working while studying, resulting in underinvestment in their human capital, which led to a decline in wages and the need for additional on–the–job training after graduation.

This paper makes two important contributions to the literature. First, it provides one of the first causal evidences of the impact of the Bologna reform on the labor market. Several papers have studied the effects of the reform on employment and wages of graduates, including Bosio and Leonardi (2011) in Italy, Farčnik and Domadenik (2012) in Slovenia, Garra (2013) in Portugal, Neugebauera and Weiss (2018) in Germany, and Domínguez and Gutiérrez (2022) in Spain. However, only one study provides credible causal evidence of the effect of the reform (Bosio and Leonardi, 2011). The authors use the variation in the introduction of the reform by universities as an instrument to identify changes in the supply of college graduates in 1998–2007 in Italy. They estimate the effects of the reform on the probability of employment and wage premium of college graduates relative to non–graduates and find that the reform increases the relative employment and decreases the wage premium of graduates. In contrast to Bosio and Leonardi (2011), the present study employs a more up–to–date dataset covering the years 2010–2020 and finds precisely estimated null effects of the reform on employment. By incorporating more recent data, this research contributes new causal evidence to the existing literature, elucidating the Bologna reform's implications within the context of the post–financial crisis period. The discovery of null effects on employment challenges the widely held, yet misguided, belief among policymakers and university management that the Bologna reform negatively influenced graduates' employability.

Second, this paper offers a novel mechanism to explain how students deal with the negative consequences of education reforms. To the best of my knowledge, this is the first Bologna reform study that explains why labor outcomes have been affected for some students while remaining unchanged for others. The findings indicate that students with high relative returns study more and work less, optimally investing in their human capital and securing stable wages. In contrast, students with low relative returns experience underinvestment in human capital and a decline in wages. These insights suggest that excessive working while studying can adversely impact academic performance and future labor market success

(Stinebrickner and Stinebrickner, 2003; Häkkinen, 2006; Avdic and Gartell, 2015).

The findings of this study bear significant implications for policymakers and university management contemplating similar education reforms. Understanding the ways in which students respond to the consequences of education reforms can help policymakers design better reforms (Hendren and Sprung-Keyser, 2020; Finkelstein and Hendren, 2020). One approach to mitigating the negative consequences of reforms on students who work excessively while studying is to offer more support for balancing academic and work responsibilities. This may include providing additional financial aid, which would enable students to focus on their performance and reduce the hours they work while studying (Bettinger et al., 2019; Anderson et al., 2020; Rothstein and Rouse, 2021). Another way to bolster student performance is to offer flexible class schedules or access to tutoring and other academic support services, allowing students to optimally allocate their time during their studies (Fryer, 2014; Xu and Ran, 2021; Canaan et al., 2022).

The paper is structured as follows. Section 2 reviews the institutional background of the Russian higher education system and the Bologna reform. Section 3 describes the data and the identification strategy. Section 4 presents the results. Section 5 concludes.

2 Institutional background and the reform

The Russian higher education system

In Russia, there are approximately 700 public and private universities. Private universities constitute a small proportion of the sector, with enrollment comprising less than 6% of total students. Apart from differences in funding, private universities are subject to the same regulations imposed by the Ministry of Science and Higher Education on public universities.[2]

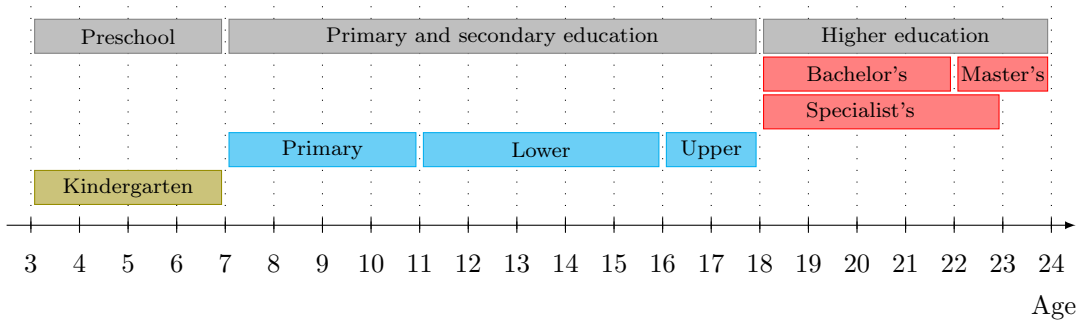
Traditionally, the majority of university applicants are high school graduates applying in the same year of graduation. In 2010 and 2011, 80.1% and 79.7% of admitted students respectively, were recent high school graduates. In Russia, the

[2]In this paper, I describe the system of higher education that is relevant for full–time students in public and private universities, who are the subject of the study. Hereinafter, by "universities" I mean all types of higher education institutions, including universities, institutes, and academies.

concept of taking a gap year does not prevail. This trend is particularly prevalent among male students, as there is a mandatory conscription system in place for Russian male citizens and avoiding conscription is considered a criminal offense.

Admission to universities in Russia is based on scores obtained on the Unified State Exam (similar to the SAT), taken during the final year of high school. The average age of applicants is 18 years old (see Figure 1). The Ministry of Science and Higher Education determines the number of state–funded places and the curricula at universities. State–funded places are financed by the state budget and allow students to attend university tuition–free (hereinafter, I refer to these places as "tuition–free places"). Figure A1 illustrates the inflow of full–time students admitted to tuition–free places from 2005–2020. The proportion of tuition–free places remains stable over time, while the total number of admitted students has decreased due to demographic changes.

Figure 1: The Russian education system



Notes: Age indicates student's age based on the nominal study duration.

In Russia, students can apply for either tuition–free places (more competitive) or fee–based places (less competitive). The application process allows students to submit a list of up to five universities, with a maximum of three programs at each university. The competition for tuition–free places is fierce, with admission determined by merit, as measured by scores on the admission exam. Typically, students with the highest exam scores enroll in public universities for tuition–free places, while those with lower scores attend private universities for fee–based places.

Approximately 60% of students enroll in tuition–free places at public universities. Some universities, under the supervision of various sectoral ministries such as the Ministry of Agriculture, Ministry of Culture and Ministry of Health, have

autonomy to set their own regulations. However, they have limited ability to influence the curricula and program quality. The Ministry of Science and Higher Education oversees all universities, including those under sectoral ministry supervision. This is due to the centralized nature of the higher education sector, as the Ministry holds the power to grant licenses, accredit universities, assign admission quotas, and controls a significant portion of the budget allocated to the higher education sector. For further information on the Russian higher education system, see Platonova and Semyonov (2018), Froumin and Leshukov (2019), and Kuzminov and Yudkevich (2022).

The Bologna reform in Russia

Historically, the Russian higher education landscape was represented by a unitary system. Under this system, students would typically enroll in a comprehensive five–year program leading directly to a specialist's degree. This system was straightforward but limiting in terms of academic transition and mobility.

In 2003, Russia committed to the Bologna Declaration, aiming to harmonize its higher education system with those of other European countries. However, the full realization of this commitment was not achieved until 2011, hindered by administrative complications associated with introducing such a big reform throughout the nation. When the Bologna reform was eventually implemented in 2011, it marked a significant transformation in the Russian higher education system. The country transitioned from its traditional unitary system, which was characterized by five–year specialist's degrees, to a two–cycle system comprising four–year bachelor's and two–year master's programs.

The universities that were under the supervision of the Ministry of Science and Higher Education, among other institutions, adapted by converting some of their specialist's programs into bachelor's degrees. However, universities overseen by sectoral ministries were not uniformly obligated to adhere to these changes.[3] The way universities adapted to the Bologna reform varied. Some institutions streamlined their course structures and curricula, often reducing optional components. In contrast, others simply tried to transfer the courses from the old specialist's

[3]For reference, the Decree of the Government of the Russian Federation N1136 specifies programs that retained their specialist's format.

curriculum to the new, condensed bachelor's format without significant changes.

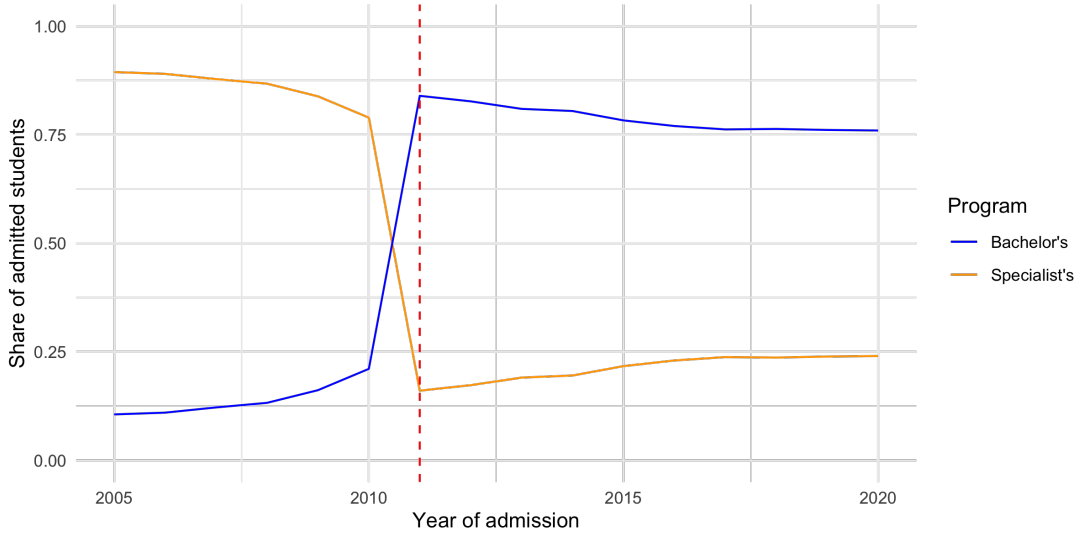
Central to the Bologna reform was the alignment with the European Qualifications Framework, which set out specific learning outcomes for each degree level. The goal was to achieve a more streamlined, efficient approach to higher education, reducing redundancies and focusing on core competencies. As a consequence, certain study programs underwent modifications to fit the new degree structures (such as services, education, social sciences, etc.). These changes often meant shortening programs that previously exceeded the standardized length. For example, only 27% of arts and humanities programs were bachelor's programs in 2010, while this share grew to more than 85% in 2011 (see Table A1). On the other hand, programs that already aligned with the new structure, or those rooted in academic cultures with traditionally longer duration like engineering and medicine, remained unchanged.

The Bologna reform, initiated in 2011, primarily targeted new enrollees, meaning those who started their studies post–implementation would experience the new structure. However, students who had enrolled prior to 2011 would have continued with their studies under the previous system. As a result, from 2011 to 2015, many universities managed two parallel systems. The older system for students who had started their education before the reform, ensuring they completed their studies as initially planned. At the same time, the new system was in place for the incoming students. This dual system ensured a smooth transition period, minimizing disruptions and ensuring that all students, regardless of when they enrolled, received the education they had anticipated.

The Bologna reform is one of the most significant institutional reforms of the Russian higher education system in the past decade and is a widely debated topic. The mass introduction of two–cycle higher education degrees occurred in the academic year 2010–2011 (see Figure 2). This creates a natural experiment for the 2011 intake, as there was a significant shift in the proportion of students admitted to specialist's programs (79%) versus bachelor's programs (21%) in 2010, to the opposite pattern in 2011 (16% specialist's students and 84% bachelor's students).

Numerous studies have investigated the returns to education in Russia (Lukiyanova, 2010; Roshchin and Rudakov, 2015; Kyui, 2016; Belskaya et al., 2020; Melianova et al., 2020; Rozhkova et al., 2021, 2023). However, these studies primarily offer correlational estimates, leaving a gap in the literature concerning

Figure 2: The share of bachelor's (4 year) and specialist's (5 year) full–time students admitted in 2005–2020



Notes: The red dashed line shows the period of the implementation of the reform.
Source: The Ministry of Science and Higher Education.

causal estimates of the Bologna reform's impact on labor market outcomes for graduates, particularly in relation to employment and wages.

3 Research design

3.1 Data

This study utilizes data from two sources. The first dataset is the National Survey of Graduate Employment conducted by the Russian Federal State Statistics Service. The sample was selected based on primary information from the census data and includes a repeated cross–sectional sample of students who graduated between 2010 and 2015. The sample is representative of the national level and provides detailed information on demographics (sex, age, region, type of settlement, and citizenship status), educational background (type of degree, year of graduation, type of university, funding, and combining study and work), and labor market outcomes (wages, number of working hours and days, employment, full–time job, permanent contract, and on–the–job training) in the years following

graduation. The survey includes weights, which were calculated by comparing the stratified sample by sex, age group, and type of settlement with the general population. In total, there are 11,768 observations.

The second dataset is obtained from the Russian Labor Force Survey conducted by the Russian Federal State Statistics Service. This dataset provides information on demographics (sex, age, region, type of settlement, and citizenship status), educational background (type of degree and year of graduation), and the labor market outcome (employment) in the years following graduation. This dataset has drawbacks as it does not provide information on whether a student graduated from a full–time or part–time program, and there is no information on wages. Although this may introduce bias, I expect the bias to be consistent for the treatment and control groups.[4] In total, there are 300,580 observations.

The study focuses on graduates with bachelor's and specialist's degrees, and only includes graduates from full–time programs. The education variable refers to the highest level of education attained.[5] I use both net monthly and hourly wages, calculated from the number of working days and hours per day.[6] The unemployed category includes both registered unemployed and economically inactive individuals.

The descriptive statistics in Table A2 show that about 40% of graduates are males, most of them (75%) live in urban areas, and nearly all have Russian citizenship. The majority (95%) of graduates studied at public universities, and 59% had tuition–free places. This is consistent with the administrative statistics by the Ministry of Science and Higher Education discussed in Section 2. In the year of the survey, 86% of graduates were employed, with a mean monthly wage of approximately 24,000 RUB (370 USD, with a 1 USD = 65 RUB exchange rate).

[4]In the main dataset, the percentage of full–time students among bachelor's students is 87.4% and among specialist's students is 88.4%. To check whether there could be possible biases due to the inclusion of part–time students, I compare the means of two groups. p–value of the t–test is 0.24. In Section 4.4, I use the main sample and include both full– and part–time students to estimate the short–term effect of the reform on wages and employment. The results remain unchanged.

[5]Note that although the survey only allows respondents to choose between "bachelor's" and "specialist's/master's" degrees, the share of master's graduates in the 2010–2015 cohort is negligible (between 4–8%).

[6]It should be noted that some employed graduates did not report their wages, but there are no statistically significant differences in observable characteristics between those who did and did not report their wages (see Table A3).

3.2 Identification strategy

This paper estimates the causal effect of the Bologna reform on employment and wages of university graduates in Russia by using the reform as a source of exogenous variation in the length of higher education and the content of the curricula. I compare two cohorts of students who graduated in the same year to estimate the differences in employment probabilities and wages for bachelor's and specialist's degree holders under similar labor market conditions. However, these simple estimates are confounded by other changes that may have affected students enrolled before and after the reform. To correct for this, I use a control group of graduates from programs that did not change due to the reform. The changes in the differences between the treated and control groups, under certain assumptions, can then be attributed to the effect of the Bologna reform. This DiD design allows me to estimate the average treatment effect on the treated.[7]

In summary, the announcement of the implementation of the Bologna reform occurred in 2003 but the actual reform took place in 2011. Due to these delays, high school graduates and their families were unable to predict when the reform would be implemented. Besides, students almost never take a gap year so there is no room to adjust their application strategies. This means that applicants could not affect their decision to participate in the reform or not by adjusting the year of application. Such unexpected timing, coupled with the absence of a gap year culture, allow to rule out the anticipation effect concerns. Additionally, the duration of programs is strictly limited to the nominal study period. This means that students must graduate within the specified time limit due to administrative barriers and financial costs. Therefore, any changes in the differences between the treated and control groups can be attributed to the effect of the Bologna reform.

These institutional features make the case of Russia particularly suitable for studying the effects of the Bologna reform on the labor market. The reform provides a source of exogenous variation in the length of the university study period

[7]Although a DiD design is used to estimate the causal effect of a policy intervention when some individuals are subject to treatment and others not, and outcomes are measured in each group before and after the policy reform, individuals in the treated and control groups should not be necessarily the same individuals (Bertrand et al., 2004; Athey and Imbens, 2006; Abadie et al., 2023).

and the content of the curricula. This means that the variation in the length of higher education and the content of studies is assumed to be uncorrelated with other factors that might affect labor market outcomes, such as ability or self–selection.

To obtain naïve OLS estimates of returns to education, I use the following model:

$$Y_{iprt} = \beta Edu_i + X_i'\gamma + \alpha_p + \eta_r + \lambda_t + \varepsilon_{iprt} \tag{1}$$

where Y_{iprt} is the labor market outcome (either log monthly wages or an indicator equal to 1 if the graduate is employed) of graduate i from program p in region r in year t, Edu_i is the imputed number of years of education of graduate i, X_i' represents individual characteristics of graduate i (sex, type of settlement, citizenship status, type of university, and funding), α_p are program fixed effects, η_r are region fixed effects, and λ_t are year of graduation fixed effects. The inclusion of the type of university and funding in the model allows to control for student's prior ability, as students with higher admission scores typically enroll in public universities for tuition–free places, while students with lower scores enroll in private universities for fee–based places. To estimate the probability of being employed, I use a linear probability model.

I estimate the short–term effect of the Bologna reform on labor market outcomes using the following difference–in-differences model:

$$Y_{iprt} = \delta D_{pt} + X_i'\gamma + \alpha_p + \eta_r + \lambda_t + \varepsilon_{iprt} \tag{2}$$

where D_{pt} is an indicator equal to 1 if a student graduated from a bachelor's program p after 2015. The parameter of interest δ gives the estimate of the reduced–form effect of the Bologna reform. Only δ is reported in the tables, unless stated otherwise.

I estimate the medium–term effect the Bologna reform using the following event study model:

$$Y_{iprt} = \sum_{\tau \in [-4,5], \tau \neq -1} \delta_\tau D_{p\tau} + X_i'\gamma + \alpha_p + \eta_r + \lambda_t + \varepsilon_{iprt} \tag{3}$$

where $D_{p\tau}$ is an indicator equal to 1 if a student i graduated from a bachelor's

program p in year τ relative to 4 years after the reform. All results reported in this paper are obtained using sample weights. Standard errors are clustered at the program and graduation year level to account for the treatment being assigned at this level (Abadie et al., 2023).

3.3 Evidence on the identifying assumptions

Table 1 illustrates that the majority of high school students apply to university in their graduation year, with approximately 80% of high school graduates being university applicants in the same year. These shares also indicate that there are no anticipation effects due to the reform, as the shares of high school students applying in their graduation year remain stable throughout the analyzed period.

Table 1: The share of high school students who applied to university in their graduation year in 2006–2011

	2006	2007	2008	2009	2010	2011
Bachelor's programs	0.79	0.79	0.78	0.79	0.79	0.79
Specialist's programs	0.80	0.80	0.79	0.79	0.80	0.80

Source: The Ministry of Science and Higher Education.

Panel A of Table 2 highlights that a high percentage of bachelor's (99–100%) and specialist's (95–97%) graduates complete their studies within the nominal study period, which is crucial for the identification strategy. Furthermore, Panel B shows that the dropout rate is low, at less than 11% for bachelor's graduates and less than 8% for specialist's graduates. This suggests that the estimates of the effect of the Bologna reform on the analyzed sample of graduates are not biased.

By making the programs shorter, the Bologna reform might have made it more attractive to enroll in university. Consequently, some might have opted for fields with a shorter study duration. Moreover, the intensified curricula might have presented challenges for students to graduate. These factors are crucial to consider in my identification strategy, given that the sample consists of university graduates. Since the primary data set only includes graduates, I use additional data from the Russian Longitudinal Monitoring Survey. This survey provides

Table 2: The share of students who did not graduate within the nominal study period and the drop out rate in 2010–2015

	2010	2011	2012	2013	2014	2015
Panel A: Students who did not graduate within the nominal study period						
Bachelor's programs	0.00	0.00	0.00	0.00	0.00	0.01
Specialist's programs	0.03	0.03	0.03	0.03	0.04	0.05
Panel B: Students who dropped out						
Bachelor's programs	0.09	0.09	0.09	0.11	0.10	0.09
Specialist's programs	0.07	0.07	0.07	0.08	0.07	0.07

Source: The Ministry of Science and Higher Education.

information on university enrollment, type of degree, field of study, and graduation. I focus on four cohorts born between 1991 and 1994, with those born in 1993 being the first cohort exposed to the reform (further details can be found in Section 4.4). As illustrated in Table 3, the reform did not impact the likelihood of university enrollment, chosen field of study, or graduation rates. These findings indicate that the reform had no impact on educational outcomes, suggesting that potential biases from only analyzing graduates or from sorting of students into different study fields are likely negligible.

The key identifying assumption of this study is that, in the absence of the Bologna reform, the quantity and quality of university applicants and graduates would have evolved similarly in programs that experienced a change in the length of education and curricula relative to programs that did not experience such changes. This assumption is based on the idea that graduates with a particular set of individual characteristics who received the treatment would have, on average, experienced the same changes in their outcomes had they not received the treatment as individuals with the same characteristics who did not receive the treatment.

I provide a test of the parallel pre–trends in Figures 3, 4, and A2-A11. These figures provide the estimates of the pre–treatment leads with 95% confidence intervals, i.e. the estimates of the interaction of the treatment indicator and pre–reform period dummies. The test of parallel pre–trends supports the parallel trend assumption, suggesting that before 2015, labor and education outcomes of the treated and control groups followed similar patterns.

Table 3: The effect of the Bologna reform on educational outcomes

Panel A: Extensive margin	*Enrolled in university*	*Enrolled in a bachelor's program*	*Graduated from university*
	(1)	(2)	(3)
Bologna reform	−0.019	0.241***	−0.001
	(0.012)	(0.024)	(0.017)
Pre–reform mean	0.95	0.25	0.89
Observations	2,408	2,253	2,253
Panel B: Intensive margin	*Enrolled in STEM*	*Enrolled in Medicine*	*Enrolled in Social sciences*
	(1)	(2)	(3)
Bologna reform	0.006	0.003	−0.003
	(0.004)	(0.002)	(0.002)
Pre–reform mean	0.22	0.02	0.31
Observations	2,253	2,253	2,253

Notes: Robust standard errors in parentheses. Bologna reform indicates whether a person is born in 1993 or 1994. *p<0.1; **p<0.05; ***p<0.01

4 Results

4.1 Main results

In this section, I discuss the naïve OLS estimates of returns to higher education and present the results of the DiD estimates of the effect of the Bologna reform on employment and wages.

OLS estimates of returns to higher education

Table 4 presents the estimates of the returns to one year of higher education in Russia. Panel A depicts the association between one year of higher education and log monthly wages for all graduates, as well as separately for males and females. Panel B presents the results of the linear probability model for employment. The estimated associations can be interpreted in a similar manner to returns to one year of higher education.

The results for log monthly wages reveal that returns to one year of higher education are 5.1% (s.e. = 0.010). Notably, returns are higher for females than

Table 4: OLS estimates of the association between years of higher education with wages and employment

Panel A: Wages	*All*	*Male*	*Female*
	(1)	(2)	(3)
Years of education	0.051***	0.031*	0.060***
	(0.010)	(0.014)	(0.016)
Observations	6,448	2,869	3,579
Panel B: Employment	*All*	*Male*	*Female*
	(1)	(2)	(3)
Years of education	0.021**	0.014	0.024*
	(0.007)	(0.009)	(0.010)
Observations	11,768	4,958	6,810

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. The dependent variable is imputed years of education. All columns include program, region, and year fixed effects. Column (1) includes all individual controls. Columns (2) and (3) include the same individual controls as Column (1), excluding sex. *p<0.1; **p<0.05; ***p<0.01

for males, which is consistent with previous literature (Montenegro and Patrinos, 2014). The overall results are largely driven by returns to higher education for females, with returns to one year of education estimated at 6.0% (s.e. = 0.016) for females and 3.1% (s.e. = 0.014) for males. All estimates are statistically significant.

The results for employment indicate that one year of education is associated with a 2.1 percentage point increase (s.e. = 0.007) in the probability of being employed. These results are primarily driven by female graduates, with one year of education associated with a 2.4 percentage point increase (s.e. = 0.010) in the employment probability for females, while the coefficient for males is not statistically significant.

Given substantial returns to education in terms of employment and wages, the Bologna reform potentially can have negative impact on the labor market due to the reduction of the length of study programs. This is a view shared by policymakers and university management, who blame the reform for causing negative labor market outcomes for bachelor's graduates (Teichler, 2011). However, it is important to keep in mind that the results from studies using OLS may be

biased upwards due to endogeneity bias.

First–stage estimates of the effect of the Bologna reform

To assess the impact of the Bologna reform on the average length of higher education, I examine the average age of graduates in the sample. Panel A of Table 5 shows that the average age of graduates after the reform decreased by 0.21 year, which is consistent with the expectation that the reform led to shortened programs.

Table 5: DiD estimates of the effect of the Bologna reform on graduation age, employment, and wages

Panel A: Graduation age	*All*		*Male*	*Female*
	(1)	(2)	(3)	(4)
Bologna reform	−0.214***	−0.205***	−0.219*	−0.225***
	(0.046)	(0.042)	(0.097)	(0.019)
Ind. controls		✓	✓	✓
Pre–reform mean	23.26	23.26	23.30	23.23
Observations	11,768	11,768	4,958	6,810
Panel B: Employment	*Employed in 2015*		*Employed in 2015–2020*	
	(1)	(2)	(3)	(4)
Bologna reform	−0.001	−0.004	−0.001	−0.001
	(0.005)	(0.006)	(0.005)	(0.005)
Ind. controls		✓		✓
Pre–reform mean	0.88	0.88	0.88	0.88
Observations	11,768	11,768	300,580	300,580
Panel C: Wages	*Log monthly wages*		*Log hourly wages*	
	(1)	(2)	(3)	(4)
Bologna reform	−0.003	0.000	−0.014	−0.007
	(0.013)	(0.012)	(0.017)	(0.022)
Ind. controls		✓		✓
Pre–reform mean	10.02	10.02	4.91	4.91
Observations	6,448	6,448	6,448	6,448

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. Bologna reform indicates whether a student graduated from a bachelor's program after 2015. All columns include program, region, and year fixed effects. Columns (2) and (4) include individual controls. *p<0.1; **p<0.05; ***p<0.01

The effect of the Bologna reform on employment

I estimate the effects of the Bologna reform on employment in two time periods: the short–term and the medium–term. The main variable of interest is *Bologna reform*, which captures the reduced–form effect of the reform. I start with a parsimonious specification in Column (1) of Panel B that includes only program, region, and year fixed effects. In Column (2), I add all individual controls. The results show that the Bologna reform does not affect the employment of university graduates in the short–term period.

To estimate the medium–term effect of the Bologna reform, I use different data from the same source for students who graduated in 2010–2020. In total, there are six cohorts of students who graduated after the reform. The estimates of the medium–term effect of the reform on employment are presented in Columns (3) and (4) of Panel B of Table 5, and the event study estimates are presented in Figure 3. Ultimately, there is no evidence of the medium–term effect of the reform on employment. The null effects are precisely estimated. These findings are surprising given the significant attention and concern surrounding the employment prospects of bachelor's degree holders (Teichler, 2011).

The effect of the Bologna reform on wages

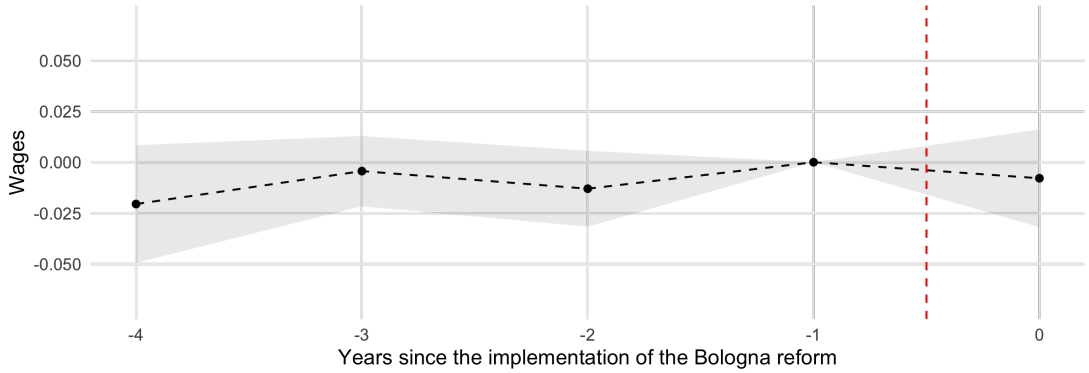
Panel C of Table 5 presents the effects of the Bologna reform on log monthly and hourly wages. Columns (1) and (2) yield the results for log monthly wages. Column (1) of Panel C of Table 5 includes only program, region, and year fixed effects, and I add all individual controls in Column (2). The event study estimates are presented in Figure 4. The results show that the Bologna reform has no adverse effects on log monthly wages.

One might expect that individuals with more years of education receive higher monthly wages because they work more hours, or because their productivity increases so their hourly wages increase. Therefore, I estimate Model 2, using log hourly wages as the dependent variable. Columns (3) and (4) of Panel C of Table 5 yield identical results, suggesting that the Bologna reform does not cause changes in the working hours.

Figure 3: Event study estimates of the effect of the Bologna reform on employment

Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure 4: Event study estimates of the effect of the Bologna reform on wages



Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

4.2 Heterogeneity results

To examine whether the effects of the Bologna reform on employment and wages are heterogeneous across different groups, I estimate Model 2 separately by sex and ability.

By sex

Panel A of Table A4 presents the results separately by sex. Columns (1) and (2) show the impact of the reform on employment for male and female graduates. Consistent with the results for the pooled sample, the reform does not reduce the probability of employment for male and female graduates. This is an important finding, as it contradicts the widely held belief that the Bologna reform hindered employment opportunities for bachelor's graduates.

Columns (3) and (4) show the effects of the Bologna reform on wages for male and female graduates, respectively. The results show that there is a negative, statistically significant effect of the reform on male graduates' wages, while the effect is not statistically significant for female graduates.

By ability

As a proxy for ability, I use an indicator for whether a graduate studied at a tuition–free or fee–based place, as applicants with higher scores on the standardized admission exam are more likely to study at a tuition–free place.

Panel B of Table A4 presents the results of the analysis by ability. Columns (1) and (2) show the effects of the Bologna reform on employment for graduates who studied at tuition–free and fee–based places, respectively. Columns (3) and (4) show the effects of the reform on wages for graduates from the same two groups. The results indicate that the effect of the reform is not statistically significant from zero for employment and wages for both groups of graduates who studied at tuition–free or fee–based places. This suggests that people with different ability levels do not respond differently to the reform.

4.3 Mechanism

The Bologna reform led to shortened degrees, which resulted in a notable disparity in wages between female and male graduates. Specifically, while wages for female graduates have remained relatively stable, wages for male graduates have fallen. To understand this phenomenon, it is crucial to consider the trade–off that students make between investing in education or in work experience.

As a response to the reform, universities compressed traditional five–year curricula into four–year programs and made certain courses optional. This increased the intensity of studies, forcing students to allocate more time to studying and less time to working. Given that both education and work experience increase human capital and wages, students are faced with the trade–off between the two activities.

Assume that the level of human capital, which is an important determinant for wages as highlighted in the seminal work by Becker (1962), is a concave function of invested time in education and in working:

$$H = f(E, W)$$

where E represents time investment in education and W represents time investment in working while studying. Assume that studying increases human capital more compared to working while studying, i.e.

$$H_E > H_W$$

where

$$H_E = \frac{\partial f(E, W)}{\partial E}$$
$$H_W = \frac{\partial f(E, W)}{\partial W}$$

Note, that this relation is *unobserved* to students as they do not know whether education or working while studying is more valuable for the accumulated human capital.

Assume that the wages are a concave function of invested human capital:

$$L = g(H)$$

where L is the labor market outcome, for example, wages. From this wage function, it is possible to derive returns to education and to work experience:

$$L_E = \frac{dg(H)}{dE} = \frac{\partial g(H)}{\partial H}\frac{\partial H}{\partial E}$$
$$L_W = \frac{dg(H)}{dW} = \frac{\partial g(H)}{\partial H}\frac{\partial H}{\partial W}$$

When students maximize their wages, they base their decision on what they observe. It is reasonable to assume that returns to education and to work experience are *observed* (Jensen, 2010). Hence, students can compare returns to education and returns to working to invest optimally in their human capital. I refer to this as *relative* returns, i.e. the ratio of returns to education over returns to work experience. Importantly, relative returns differ between female and male students. Returns to education tend to be higher for females, while returns to work experience are similar for females and males:

$$L_E^f > L_W^f \implies \frac{L_E^f}{L_W^f} > 1$$
$$L_E^m < L_W^m \implies \frac{L_E^m}{L_W^m} < 1$$

where superscripts denote females and males. This implies that female students may be more willing to invest in education at the expense of work as their relative returns are higher, while male students may be more inclined to prioritize work over education as their relative returns are lower.

Both male and female students maximize their wages given the time constraint:

$$\max_{(E,W)} g(H)$$
$$\text{s.t. } E + W \leq T$$

where T denotes the total time budget, which is the length of the study program Hence, students must choose optimal allocation of their time between studying and working.

The reform exacerbated this trade–off for students. Given that the curricula has been compressed and the length of study programs T decreased, students must allocate more time to studying in order to acquire the same amount of human capital as before since $H_E > H_W$. As a result, students must make a choice between studying more intensely or invest time in working while studying based on this relation. However, this relation is unobserved to students, and they base their decision on observed L_E and L_W which are different for males and females.

Given relative returns, female and male students choose to invest differently in their human capital. Notice that for females investment in education yields higher unobserved returns on human capital and higher observed returns on wages compared to investment in working, i.e their observed and unobserved returns are *aligned*:

$$H_E^f > H_W^f$$
$$L_E^f > L_W^f$$

However, for males observed and unobserved returns are *not aligned*. Investment in education yields higher unobserved returns on human capital but lower observed returns on wages compared to investment in working, i.e.

$$H_E^m > H_W^m$$
$$L_E^m < L_W^m$$

In response to the reform, female students choose to study more intensely, as their returns to education are higher than returns to working. Conversely, male students choose to work more while studying, as their returns to education are lower than returns to working. Due to aligned returns, females invest optimally in their human capital, while the accumulated human capital of male students is

lower than their optimal level of human capital, i.e.

$$H_f^{'} \approx H_f^*$$
$$H_m^{'} < H_m^*$$

where $H^{'}$ is the accumulated level of human capital and H^* is the optimal level of human capital.

These differential responses lead to a divergence in wages between female and male graduates, with female graduates maintaining relatively stable wages and male graduates experiencing a decline, i.e.

$$L_f^{'} \approx L_f^*$$
$$L_m^{'} < L_m^*$$

Empirical evidence supports this argument. Columns (1) and (2) of Table A5 show the associations between years of education with wages, and Columns (3) and (4) show the associations between combining study and work with wages. In terms of the model, these correspond to L_E for returns to education and L_W for returns to working. As argued, for females, returns to education are higher than returns to working ($L_E^f = 0.060$, s.e. $= 0.016$ and $L_W^f = 0.039$, s.e. $= 0.010$). For males, returns to education are lower than returns to working ($L_E^m = 0.031$, s.e. $= 0.014$ and $L_W^m = 0.036$, s.e. $= 0.012$).

As a result of different relative returns among females and males, the Bologna reform had significant influence on the rates of female and male students combining study and work (see Columns (1) and (2) of Panel A of Table A6). Before the reform, 21% of female and 29% of male students worked while studying. The reform decreased the share of females combining study and work (-0.054, s.e. $= 0.012$), while it increased the share of males doing so (0.093, s.e. $= 0.012$). This indicates that to compensate for human capital losses female students chose to work less during their shortened university program (a 26% decrease compared to the pre–reform mean), while male students decided to work more (a 32% increase).

To determine if the reform resulted in disparities in the accumulated human capital among females $H_f^{'}$ and males $H_m^{'}$, I examine whether it had a differen-

tial impact on the likelihood of female and male graduates receiving on–the–job training. If a graduate lacks the necessary accumulated human capital, on–the–job training may be required to make up for this deficiency. Prior to the reform, approximately 14% of both female and male graduates required on–the–job training. Columns (3) and (4) of Panel A of Table A6 indicate that the reform led to a decrease in the probability of on–the–job training for female graduates (-0.049, s.e. $= 0.015$) and an increase for male graduates (0.073, s.e. $= 0.026$). These coefficients, while substantial in their own right, are particularly noteworthy when compared to the pre–reform means. Specifically, the probability of receiving on–the–job training decreased by 37% for females and increased by 51% for males following the reform.

Optimal investing in human capital can help improve the job quality of graduates. Since female students optimally invest in their human capital compared to males, they can get a higher quality job after graduation. To test whether female and male graduates have different job quality, I examine two characteristics: whether a graduate has a full–time job and a permanent contract. These characteristics are considered desirable by many employees and are used as indicators of job quality. The results, presented in Panel B of Table A6, show that the Bologna reform has a positive effect on the probability of female graduates getting a full–time job (0.012, s.e. $= 0.003$) and a negative effect on the probability of male graduates getting a full–time job (-0.011, s.e. $= 0.001$). Additionally, I find suggestive evidence that females are more likely to get a permanent contract, while males are less likely, however, the estimates are not precise enough to draw conclusions.

In conclusion, the Bologna reform led to differing wage outcomes for female and male graduates as a result of differing relative returns to education and to working. The choices made by females and males in response to the reform resulted in divergent outcomes in terms of human capital investment and post–graduation wages.[8] I find that male graduates required a substantial increase in

[8]Another interesting question is whether the reform had a differential impact on enrollment in master's programs for males and females, which could explain the observed heterogeneity. The survey includes a question inquiring if respondents wish to pursue further education. Although this does not directly help studying the effect of the reform on master's program enrollment by sex, it offers insights into any differing educational aspirations between males and females for postgraduate studies. Prior to the reform, the aspiration for further educa-

on–the–job training, which is indicative of a significant decline in their level of human capital. This decline is attributed to an inefficient allocation of time during their studies.[9] This highlights the importance of effective time management for students in order to improve their human capital and enhance their chances of success in the job market (Fryer, 2014; Xu and Ran, 2021; Canaan et al., 2022).

4.4 Robustness checks

Inclusion of part–time students

In the main analysis, I use data on graduates from full–time programs to estimate the short–term effects of the Bologna reform on employment and wages. However, to estimate the medium–term effects, I use a different data source that does not allow to distinguish between full–time and part–time students. To ensure that the medium–term effects are not biased by the inclusion of part–time students, I compare the short–term effects of the reform on full–time students and then add in part–time students. The results, presented in Table A7, show that the effects of the reform on employment and wages are similar for both full–time and full–time together with part–time students. These findings suggest that the medium–term effects of the Bologna reform are not biased by the inclusion of part–time students.

tion was balanced between males and females: 6.3% of males and 6.5% of females expressed a desire to continue. Post–reform data does not show a significant shift, with an increase of 0.015 (s.e. = 0.009) for males and 0.028 (s.e. = 0.021) for females. Hence, the reform did not significantly alter the educational strategies of either males or females in relation to pursuing master's programs (at least, for the analyzed cohorts).

[9]The objective of the Bologna reform was to harmonize degree systems across European countries. One might thus wonder if the introduction of this reform catalyzed the migration of students from Russia to other European countries. Specifically, could there be a disproportionate migration of high–ability men that could have potentially influenced the observed negative effects on wages? Administrative statistics by the Ministry of Science and Higher Education show that in 2014 only 0.21% of students studied abroad for at least one term, with a slight increase to 0.22% in 2015. Such figures suggest a limited propensity among students to pursue education in other countries. I also analyze the migration patterns of the general population. In 2014, about 0.73% of men aged 20–29 migrated, with a modest increase to 0.78% in 2015. The corresponding figures for women stand at 0.26% and 0.29%. Although men demonstrate a marginally higher probability to migrate, it is unlikely to explain the heterogeneous effects.

Industry and occupation choices

To investigate whether the choice of occupation and industry could be contributing to wage disparities between male and female graduates, I examine the effect of the Bologna reform on graduates controlling for occupation and industry fixed effects. I include a set of occupation and industry fixed effects and the results are presented in Table A8. I show the baseline results from the main analysis in Columns (1) and (2). In Columns (3) and (4), I include occupation fixed effects. In Columns (5) and (6), I include industry fixed effects. The estimates remain unchanged across these different sets of fixed effects, indicating that the effects of the Bologna reform do not vary across different occupations and industries.

Different model specifications

To ensure that the main results of the study are robust, I examine the effect of including different sets of fixed effects. I first present the baseline results in Column (1) of Panel A of Table A9 for employment and Panel B for wages. In Column (2), I add the interaction between program and year fixed effects. In Column (3), I add the interaction between region and year fixed effects. In Column (4), I add the interaction between program and region fixed effects. Overall, the estimates do not change when different sets of fixed effects are included, indicating that the results are robust to these changes.

Compositional changes

One potential concern with the DiD analysis is that the distribution of characteristics among the cohorts may have changed over time, which could lead to bias in the estimation of the average treatment effect on the treated (Sant'Anna and Xu, 2022). To address this concern, I examine whether the distribution of covariates remains constant over time. Tables A10 and A11 show the descriptive statistics for students by the year of their graduation, and the results indicate that the shares of males, people who live in urban areas, have citizenship status, graduate from public universities, or have tuition–free places are similar throughout all years.

To formally assess whether there exists a compositional change in covariates due to the reform, I run a covariate regression using Model 2 with observable characteristics such as sex, the type of settlement, citizenship status, the type of university, and funding as the dependent variable. None of the coefficients are statistically significant (see Tables A10 and A11). Taken together, these results provide evidence that the observed characteristics of the cohorts are stable over time and not affected by the reform. This suggests that there is no bias in the estimation of the average treatment effect on the treated and supports the validity of the DiD analysis.

Regression discontinuity design

To complement the DiD estimates, I estimate the impact of the Bologna reform using the RDD analysis. The first reform–exposed cohort comprises those born in 1993. Thus, the treated group consists of the 1993–1994 cohorts, while the 1991–1992 cohorts serve as the control. I estimate the impact of the reform separately by sex using the following local linear regression equation:

$$Y_{ic} = \delta D_c + X_i'\gamma + \varepsilon_{ic} \tag{4}$$

where D_c indicates whether a person born in the cohort affected by the reform (born in 1993 or 1994) and X_i' includes controls from Model 2 and the running variable. To ensure similarity, only two cohorts to the left and right of 1993 are compared. The results are in Table A12. The first–stage estimates reveal people from the 1993–1994 cohort are more likely to graduate with a bachelor's degree than people from the 1991–1992 cohort: 17.1 percentage points (s.e. = 0.041) for males and 20.1 percentage points (s.e. = 0.030) for females. I find that employment remained unaffected: effects being −0.003 (s.e. = 0.005) for males and −0.009 (s.e. = 0.007) for females. The reform reduced male graduates' wages by 6.8% (s.e. = 0.006) while leaving female wages unaffected (0.8%, s.e. = 0.008). These results support the previous findings that the Bologna reform did not affect the employment of university graduates and had a heterogeneous impact on wages by sex.

5 Concluding remarks

This paper presents one of the first empirical investigations of the causal effects of the Bologna reform, a major higher education reform implemented in Europe, on employment and wages of university graduates. The reform aimed to create a more unified higher education system by reducing the length of university programs and changing the content of curricula. As such, it has been the subject of much debate among policymakers and university management.

This study finds that the reform did not negatively affect the employment of graduates in the short– or medium–term, nor did it have a detrimental effect on the wages of students who invested more time into their studies. These results suggest that students were able to compensate for the reduction in human capital by working less and studying more, thus mitigating any potential negative effects of the reform.

In addition, this study also highlights the potential benefits of the Bologna reform for graduates. The reduction in the university study period resulted in decreased forgone earnings and tuition fees, allowing graduates to gain an additional year of labor market experience compared to those who completed longer programs. Previous research has shown that additional work experience can have a positive impact on lifetime earnings, making the Bologna reform potentially beneficial for the long–term wealth of graduates (Topel, 1991; Dustmann and Meghir, 2005).

Further research could use the reform as a natural experiment to study the long–term effects of higher education on labor market outcomes, as well as other outcomes such as lifetime wealth, health, and fertility.

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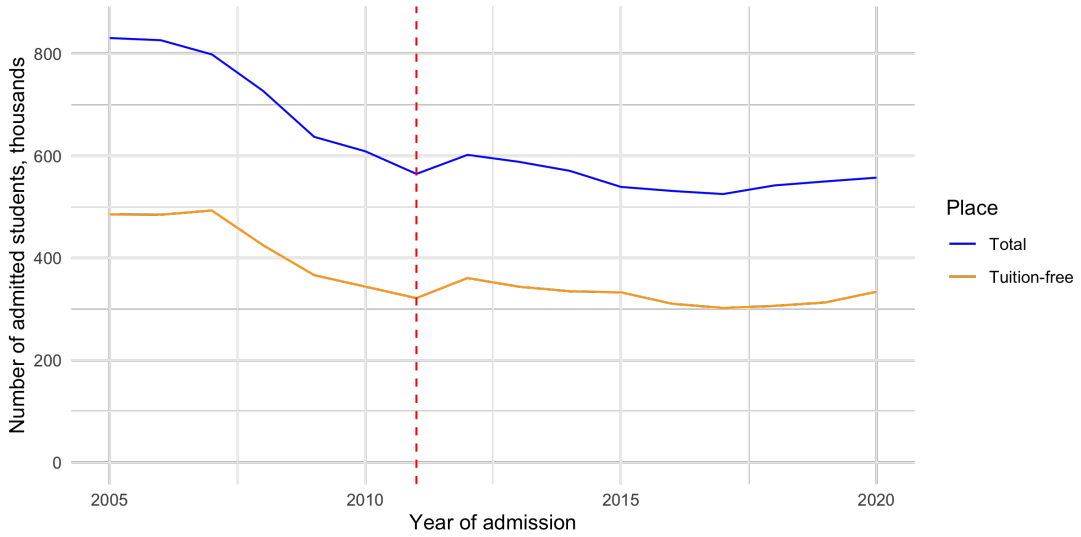
Appendix

Figure A1: The number of bachelor's and specialist's full–time students admitted in 2005–2020, thousands



Notes: The red dashed line shows the period of the implementation of the reform.
Source: The Ministry of Science and Higher Education.

Table A1: The share of applicants to bachelor's programs in 2010 and 2011 by the field of study

	Field	2010	2011
1	Services	0.20	1.00
2	Education	0.31	0.99
3	Social Sciences	0.26	0.99
4	Natural Sciences, Mathematics and Statistics	0.51	0.95
5	Information and Communication Technologies	0.31	0.92
6	Arts and Humanities	0.27	0.85
7	Agriculture, Forestry, Fisheries and Veterinary	0.24	0.85
8	Engineering, Manufacturing and Construction	0.27	0.83
9	Health and Welfare	0.00	0.03
	Total	0.21	0.84

Source: The Ministry of Science and Higher Education.

Table A2: Summary statistics of characteristics of graduates

	Observations	Mean	SD	Min	Max
Panel A: Graduated in 2010–2015					
Bachelor's graduates					
Male	1,760	0.41	0.49	0	1
Urban	1,760	0.76	0.43	0	1
Citizen	1,760	0.99	0.02	0	1
Public university	1,760	0.95	0.23	0	1
Tuition–free	1,760	0.53	0.50	0	1
Employed	1,760	0.82	0.38	0	1
Monthly wage, RUB	906	23,190	11,031	5,000	130,000
Specialist's graduates					
Male	10,008	0.42	0.49	0	1
Urban	10,008	0.74	0.44	0	1
Citizen	10,008	0.99	0.04	0	1
Public university	10,008	0.96	0.21	0	1
Tuition–free	10,008	0.60	0.49	0	1
Employed	10,008	0.86	0.34	0	1
Monthly wage, RUB	5,542	24,544	11,937	5,000	120,000
Panel B: Graduated in 2010–2020					
Bachelor's graduates					
Male	74,884	0.39	0.49	0	1
Urban	74,884	0.69	0.46	0	1
Citizen	74,884	0.99	0.05	0	1
Employed	74,884	0.83	0.37	0	1
Specialist's graduates					
Male	225,696	0.41	0.49	0	1
Urban	225,696	0.75	0.43	0	1
Citizen	225,696	0.99	0.05	0	1
Employed	225,696	0.87	0.33	0	1

Source: Panel A is based on National Survey of Graduate Employment carried out by the Russian Federal State Statistics Service. Panel B is based on Russian Labor Force Survey carried out by the Russian Federal State Statistics Service.

Table A3: The characteristics of students by whether they reported their wages

	Reported	Not reported	p–value
Male	0.42	0.42	0.23
	(0.50)	(0.49)	
Urban	0.76	0.73	0.55
	(0.43)	(0.44)	
Citizen	0.99	0.99	0.32
	(0.04)	(0.05)	
Public university	0.96	0.95	0.90
	(0.20)	(0.22)	
Tuition–free	0.60	0.58	0.59
	(0.49)	(0.49)	

Notes: This table shows sample means and standard deviations from the National Survey of Graduate Employment carried out by the Russian Federal State Statistics Service. p–values are from the t–test.

Table A4: DiD estimates of the effect of the Bologna reform on employment and wages separately by sex and finding

Panel A: Sex	*Employed*		*Log monthly wages*	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
Bologna reform	0.003	–0.009	–0.044**	0.023
	(0.028)	(0.026)	(0.016)	(0.014)
Pre–reform mean	0.95	0.83	10.13	9.92
Observations	4,958	6,810	2,869	3,579
Panel B: Funding	*Employed*		*Log monthly wages*	
	Tuition–free	Fee–based	Tuition–free	Fee–based
	(1)	(2)	(3)	(4)
Bologna reform	0.001	−0.007	−0.008	0.009
	(0.011)	(0.016)	(0.018)	(0.010)
Pre–reform mean	0.89	0.86	10.02	10.01
Observations	6,947	4,821	3,877	2,571

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. Bologna reform indicates whether a student graduated from a bachelor's program after 2015. All columns include program, region, and year fixed effects, as well as individual controls. *p<0.1; **p<0.05; ***p<0.01

Table A5: OLS estimates of the association between years of education and the probability to combine study and work with wages

	Log monthly wages			
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
Years of education	0.031*	0.060***		
	(0.014)	(0.016)		
Combining study and work			0.036**	0.039***
			(0.012)	(0.010)
Observations	2,869	3,579	2,869	3,579

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. All columns include program, region, and year fixed effects, as well as individual controls. *p<0.1; **p<0.05; ***p<0.01

Table A6: DiD estimates of the effect of the Bologna reform on the probability to combine study and work, the level of on–the–job training, and the quality of a job

Panel A	*Combining study and work*		*On–the–job training*	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
Bologna reform	0.093***	−0.054***	0.073**	−0.049**
	(0.012)	(0.012)	(0.026)	(0.015)
Pre–reform mean	0.29	0.21	0.14	0.13
Observations	4,958	6,810	1,422	1,800
Panel B	*Full–time job*		*Permanent contract*	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
Bologna reform	−0.011***	0.012**	−0.027	0.011
	(0.001)	(0.003)	(0.029)	(0.014)
Pre–reform mean	0.89	0.80	0.85	0.78
Observations	4,320	5,392	4,339	5,415

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. Bologna reform indicates whether a student graduated from a bachelor's program after 2015. All columns include program, region, and year fixed effects, as well as individual controls. *p<0.1; **p<0.05; ***p<0.01

Table A7: DiD estimates of the effect of the Bologna reform on employment and wages using a sample of full– and part–time students

	Employed in 2015		*Log monthly wages*	
	Full–time	Full– and part–time	Full–time	Full– and part–time
	(1)	(2)	(3)	(4)
Bologna reform	−0.004	−0.004	0.000	−0.003
	(0.006)	(0.005)	(0.012)	(0.016)
Pre–reform mean	0.88	0.88	10.02	10.01
Observations	11,768	13,439	6,448	7,342

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. Bologna reform indicates whether a student graduated from a bachelor's program after 2015. All columns include program, region, and year fixed effects, as well as individual controls. *p<0.1; **p<0.05; ***p<0.01

Table A8: DiD estimates of the effect of the Bologna reform on wages including occupation and industry fixed effects

	Log monthly wages					
	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Bologna reform	−0.044**	0.023	−0.045**	0.021	−0.045**	0.018
	(0.016)	(0.014)	(0.018)	(0.013)	(0.017)	(0.013)
Occupation FEs			✓	✓		
Industry FEs					✓	✓
Observations	2,869	3,579	2,869	3,579	2,869	3,579

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. Bologna reform indicates whether a student graduated from a bachelor's program after 2015. All columns include program, region, and year fixed effects, as well as individual controls. *p<0.1; **p<0.05; ***p<0.01

Table A9: DiD estimates of the effect of the Bologna reform on employment and wages using different model specifications

Panel A: Employment	(1)	(2)	(3)	(4)
Bologna reform	−0.001	−0.002	0.001	0.000
	(0.005)	(0.005)	(0.004)	(0.005)
Program, region, year FEs	✓			
Program*year FEs		✓		
Region*year FEs			✓	
Program*region FEs				✓
Observations	300,580	300,580	300,580	300,580
Panel B: Wages	(1)	(2)	(3)	(4)
Bologna reform	0.000	−0.002	0.005	0.003
	(0.012)	(0.006)	(0.012)	(0.010)
Program, region, year FEs	✓			
Program*year FEs		✓		
Region*year FEs			✓	
Program*region FEs				✓
Observations	6,448	6,448	6,448	6,448

Notes: Robust standard errors clustered at the program and graduation year level in parentheses. Bologna reform indicates whether a student graduated from a bachelor's program after 2015. All columns include individual controls. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A10: The characteristics of students by the year of graduation in 2010–2015

	2010	2011	2012	2013	2014	2015
Male	0.44	0.43	0.45	0.42	0.40	0.39
	(0.50)	(0.50)	(0.50)	(0.49)	(0.49)	(0.49)
Urban	0.79	0.77	0.75	0.72	0.73	0.72
	(0.41)	(0.42)	(0.43)	(0.45)	(0.44)	(0.45)
Citizen	0.99	0.99	0.99	0.99	0.99	0.99
	(0.03)	(0.03)	(0.02)	(0.03)	(0.06)	(0.05)
Public university	0.95	0.96	0.95	0.95	0.96	0.95
	(0.22)	(0.20)	(0.22)	(0.22)	(0.19)	(0.21)
Tuition–free	0.59	0.60	0.59	0.59	0.59	0.58
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)

Notes: This table shows sample means and standard deviations from the National Survey of Graduate Employment carried out by the Russian Federal State Statistics Service. To check whether characteristics of students changed due to the reform, I run Model 2 using observable characteristics as the dependent variable, i.e. sex, the type of settlement, citizenship status, the type of university, and funding. p–values of the treatment coefficient γ_2 are as follows: 0.65, 0.97, 0.17, 0.11, and 0.38.

Table A11: The characteristics of students by the year of graduation in 2010–2020

	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020
Male	0.41	0.41	0.40	0.40	0.41	0.41	0.41	0.40	0.41	0.42	0.41
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)
Urban	0.73	0.73	0.74	0.73	0.73	0.73	0.72	0.73	0.72	0.74	0.73
	(0.43)	(0.43)	(0.43)	(0.44)	(0.44)	(0.45)	(0.45)	(0.45)	(0.46)	(0.46)	(0.45)
Citizen	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)

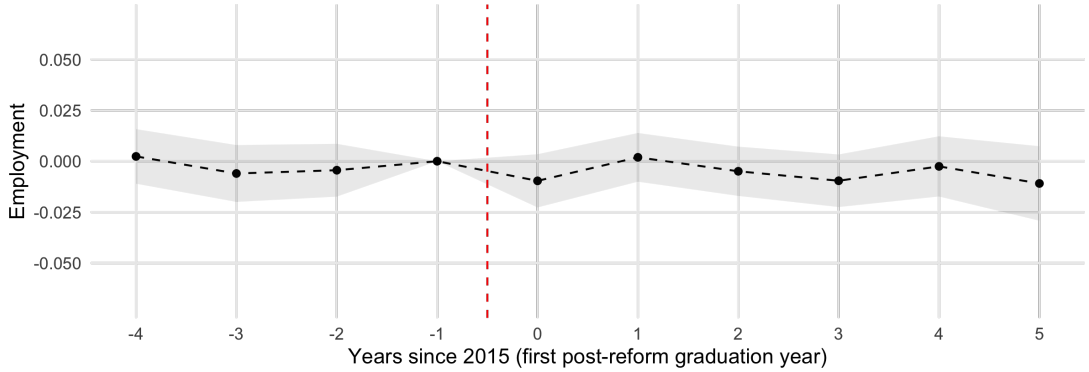
Notes: This table shows sample means and standard deviations from the Russian Labor Force Survey carried out by the Russian Federal State Statistics Service. To check whether characteristics of students changed due to the reform, I run Model 2 using observable characteristics as the dependent variable, i.e. sex, the type of settlement, and citizenship status. p–values of the treatment coefficient γ_2 are as follows: 0.70, 0.44, and 0.27.

Table A12: The effect of the Bologna reform on graduation from a bachelor's program, employment, and wages

	Graduated from a bachelor's program		*Employment*		*Wages*	
	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Bologna reform	0.171***	0.201***	−0.003	−0.009	−0.068***	0.008
	(0.041)	(0.030)	(0.005)	(0.007)	(0.006)	(0.008)
Pre–reform mean	0.25	0.23	0.80	0.79	10.02	9.86
Observations	859	1,417	859	1,417	442	752

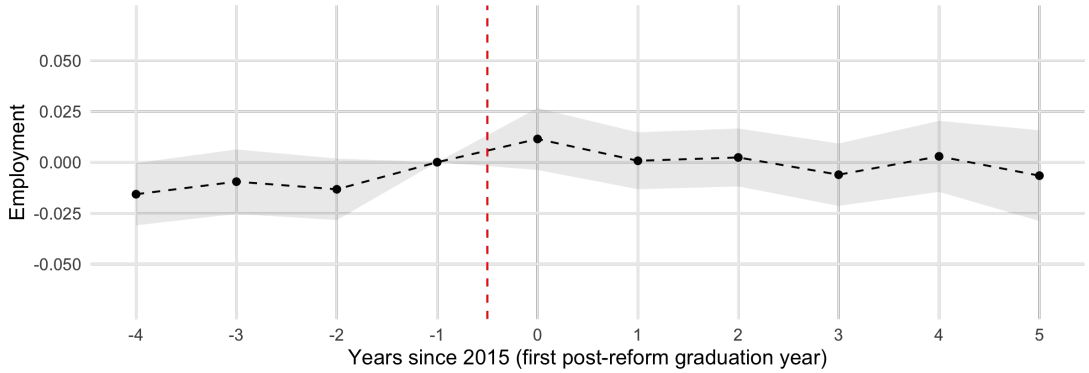
Notes: Robust standard errors in parentheses. Bologna reform indicates whether a person is born in 1993 or 1994. *p<0.1; **p<0.05; ***p<0.01

Figure A2: Event study estimates of the effect of the Bologna reform on employment of males



Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure A3: Event study estimates of the effect of the Bologna reform on employment of females



Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

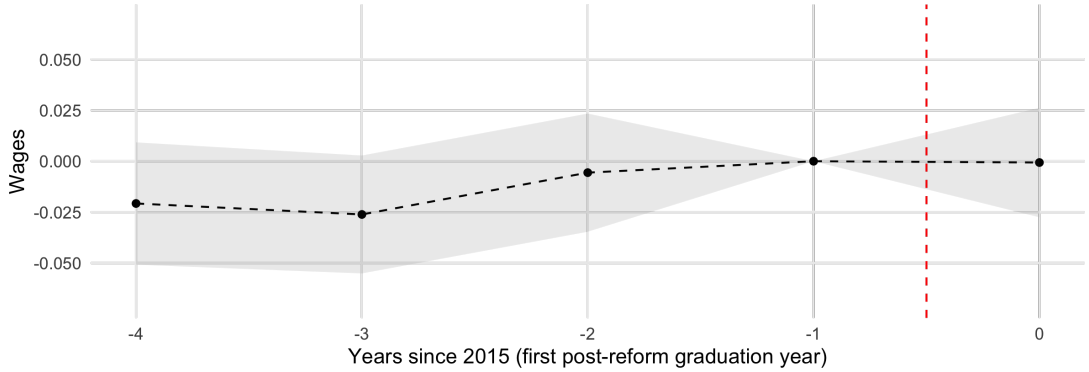
Figure A4: Event study estimates of the effect of the Bologna reform on wages of males

Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure A5: Event study estimates of the effect of the Bologna reform on wages of females

Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure A6: Event study estimates of the effect of the Bologna reform on wages of graduates studied at tuition-free places



Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

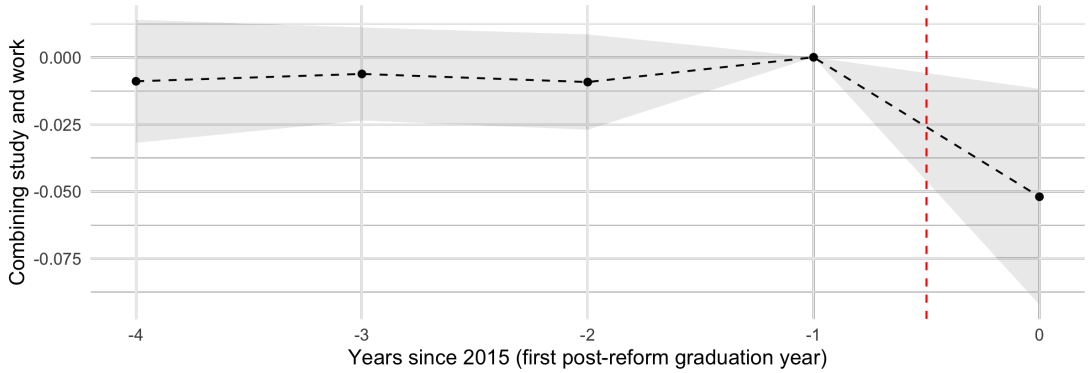
Figure A7: Event study estimates of the effect of the Bologna reform on wages of graduates studied at fee-based places

Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure A8: Event study estimates of the effect of the Bologna reform on the probability to combine study and work of males

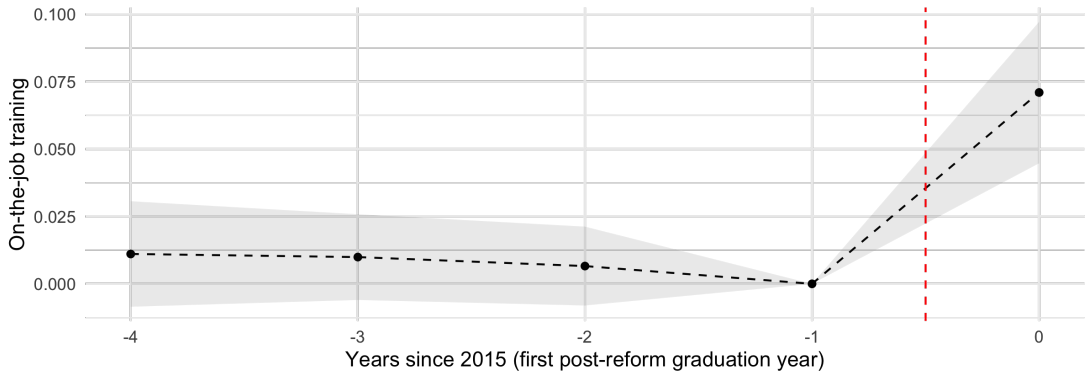
Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure A9: Event study estimates of the effect of the Bologna reform on the probability to combine study and work of females



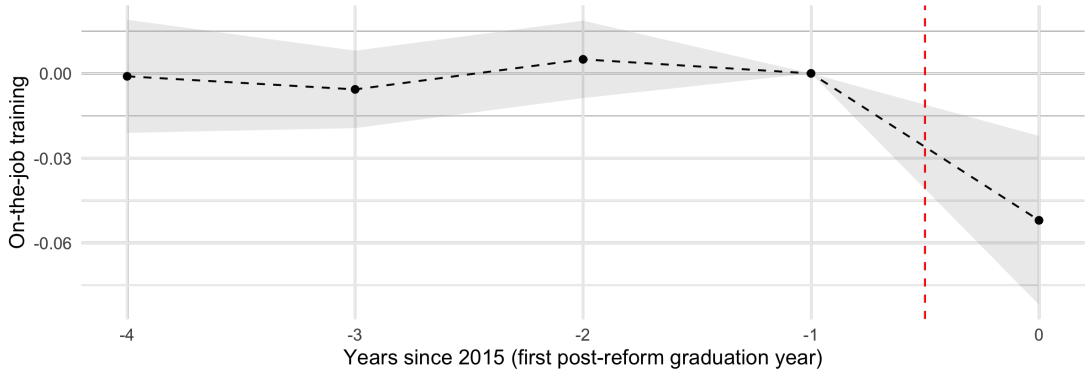
Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure A10: Event study estimates of the effect of the Bologna reform on the level of on–the–job training of males



Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.

Figure A11: Event study estimates of the effect of the Bologna reform on the level of on–the–job training of females



Notes: The figure shows the regression coefficients of interaction between the treatment variable and time period dummies with 95% confidence intervals. The omitted period is −1 – year 2014. The red dashed line shows the first post–reform graduation year. Robust standard errors are clustered at the program and graduation year level. The model includes program, region, and year fixed effects, as well as individual controls.