NATIONAL RESEARCH UNIVERSITY
HIGHER SCHOOL OF ECONOMICS

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ZERO RETURNS TO HIGHER EDUCATION: EVIDENCE FROM A NATURAL EXPERIMENT

BASIC RESEARCH PROGRAM
WORKING PAPERS

SERIES: ECONOMICS
WP BRP 236/EC/2020

This Working Paper is an output of a research project implemented within NRU HSE's Annual Thematic Plan for Basic and Applied Research. Any opinions or claims contained in this Working Paper do not necessarily reflect the views of HSE

Zero Returns to Higher Education: Evidence from a Natural Experiment *

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September 18, 2020

Abstract

Although many papers estimate returns to education, little causal evidence has been found for low– and middle–income countries. This paper estimates the causal effect of one year of university education on wages and employment in Russia. In 2011, the Bologna reform shortened the university study period by one year and reduced the content of the curricula but did not change the quality of admitted students. I exploit this reform as a natural experiment and use a difference–in–differences design. I find no adverse effect of a one–year reduction on wages and on the probability of being employed. This suggests that the reform lowered the opportunity costs of education but did not affect the accumulation of specific skills relevant for the labour market.

Keywords: difference–in–differences, returns to education, human capital, higher education, employment, wages, Bologna reform, Russia

JEL Codes: I23, I26, J24

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*I would like to thank Bernardo Pincheira and Victor Rudakov for extensive feedback and guidance, Vladimir Gimpelson, Anna Lukiyanova, Aleksey Oshchepkov, Harry Patrinos, Dmitrii Shchetinin, colleagues at the Center for Institutional Studies, many seminar and conference participants at the Higher School of Economics, the 2020 Annual Congress of the European Economic Association, the 2020 SASE Session on Educational Reforms, the GEEZ Seminar on the Economics of Education, and an anonymous referee for valuable comments and suggestions. Any errors are those of the author. The paper was prepared within the framework of the Higher School of Economics Basic Research Programme.

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1 Introduction

Labour market outcomes are important, given the consensus that the primary goal of university education is to prepare students for labour market entry. While the causal effects of higher education for high–income countries are estimated in the literature (Webbink, 2007; Maurin and McNally, 2008; Heckman et al., 2018; Gunderson and Oreopolous, 2020), papers on the causal estimates of economic returns to education for low– and middle–income countries are scarce due to the unavailability of appropriate data (Arteaga, 2018; Patrinos and Psacharopoulos, 2020). Natural experiments are a useful tool to evaluate such causal effects (Card, 1999; Webbink, 2005; Schwerdt and Woessmann, 2020). In Russia, the Bologna reform transformed higher education programmes from 5 to 4 years, while some programmes were not affected by the reform, i.e. remained as 5–year degrees. Vocational education programmes did not change in length. When students entered the labour market together after the reform, it became possible to estimate the impact of one year of university education on early labour market outcomes. Although the reform was implemented in 2011, there is no empirical literature that evaluates how this reform affected the graduate labour market in Russia, particularly wages and employment.

This paper estimates the short–term causal effect of one year of university education on wages and employment in Russia. There are two main reasons that make the case of Russia particularly interesting. First, the implementation of the Bologna reform was compulsory for all universities for specific programmes of study.[1] The reform was exogenously implemented in the university sector in 2011, thus, universities and students could not affect their decision to participate in the reform or not. I consider the reform as a source of an exogenous variation in the length of the university study period by programme. This reform generates the exogenous assignment of students into the 4–year treatment or 5–year control groups. As an alternative control group I use 3–year vocational education students. Since the year of application to university is a function of the year of graduation from school, this policy change provides variation in the length of the university study period, which is exogenous from the perspective of students. This reform allows me to use a difference–in–differences design to estimate the causal effect of one year of uni-

[1] The terms "university" and "higher education institution" are used interchangeably.

versity education (Angrist and Pischke, 2009). Second, the duration of programmes is strictly limited to the nominal study period. In other words, almost all students graduate within the study limit period. Thus, I assume that any changes in the differences between the treated and control groups can be attributed to the effect of one year of university education.

The results suggest that a one–year reduction of the university study period does not have the detrimental effect on the wages of affected graduates. Nor does a one–year reduction decrease the probability of being employed. The findings are consistent for both males and females and for graduates, who did and did not combine study and work. The reform has lowered the opportunity costs of education, while it has not decreased the labour market outcomes of graduates. In line with my findings, previous papers do not find a significant impact of the Bologna reform on wages and employment in European countries (Bosio and Leonardi, 2011; Garra, 2013). Although several papers find a significant short–term effect of the Bologna reform (Farčnik and Domadenik, 2012; Neugebauera and Weiss, 2018), there is no effect for recent graduates in the Russian labour market.

The OLS estimates of log hourly wages yield returns of about 4% to one year of university education. These estimates are similar to the results found in the literature based on the Russian labour market. However, my findings suggest that returns to one year of university education are negligible. This suggests that the OLS results are biased and one should use quasi–experimental frameworks to solve the endogeneity problem. The results are robust to choosing an alternative control group. My results are consistent with papers that estimate the causal effect of changing the length of education and find no effect on labour market outcomes (Meghir and Palme, 2005; Oosterbeek and Webbink, 2007; Pischke, 2007; Pischke and von Wachter, 2008; Devereux and Hart, 2010; Grenet, 2013; Eble and Hu, 2019).

The most credible explanation of the results is that a one–year reduction of university education did not lead to a reduction in the accumulation of relevant human capital. The reform mostly reduced specialised courses, which led to a reduction of specific skills for 4–year graduates. As the results suggest, specific skills turned out to be irrelevant for the labour market, as the effect of the reduction is zero. To support the skill–based explanation, I offer suggestive evidence and show that 4– and 5–year graduates do not differ in terms of their on–the–job training, the level of competence, and job-education mismatch. This paper argues that specific skills are irrelevant for the labour market, leading to zero returns to the additional year of university education in Russia.

Finally, I consider several potential confounding factors that might affect the results and the identification strategy. First, as 5–year graduates are older and have more years of experience, the results might capture an effect of age and experience. To account for that, I estimate two specifications, where I first exclude experience variables and secondly replace experience with age variables. The results are similar to the baseline model. Second, in 2015, there was a crisis in Russia, which could have influenced labour market conditions for graduates. To address this concern, I include students, who graduated in 2016–2018, to estimate the effect of one year of university education on employment. My results are robust to several additional checks explained in the robustness section. Lastly, I check whether the reform changed the pool of admitted students to universities. For instance, as earning a higher education degree is easier, lower–ability individuals could be motivated to enrol in 4–year programmes. I show that the reform did not change the quality of admitted students.

This paper offers several important contributions to the literature. First, it contributes to the emerging literature on the causal estimates of economic returns to education in low– and middle–income countries (Arteaga, 2018; Eble and Hu, 2019; Elsayed and Marie, 2020). Second, it contributes to the literature focusing on evaluating the results of the Bologna reform in European countries (Bosio and Leonardi, 2011; Farčnik and Domadenik, 2012; Garra, 2013; Neugebauera and Weiss, 2018). This paper is the first to study the impact of the Bologna reform on the labour market outcomes of university graduates in Russia. Third, it enriches the literature by suggesting a possible mechanism that explains the zero effect of shortening the length of university education on the labour market.

The paper is structured as follows. Section 2 reviews the causal estimates of returns to education, the results of the Bologna reform in European countries, and the institutional background of the Russian higher education system. Section 3 reports the data and Section 4 describes the identification strategy. I present the results, robustness checks, and discuss possible mechanisms in Section 5. In Section 6, I present concluding remarks.

2 Literature review

2.1 Causal estimates of returns to education

The economic literature has established a firm correlation between education and wages (Psacharopoulos and Patrinos, 2018).[2] Causal inferences, from estimates of returns to higher education, are scarcer (Webbink, 2007; Maurin and McNally, 2008; Arteaga, 2018; Heckman et al., 2018; Gunderson and Oreopolous, 2020; Patrinos and Psacharopoulos, 2020). It is not clear whether the higher earnings for better–educated workers are caused by their higher education, or whether individuals with greater earning capacity chose to acquire more education. Hence, this concern leads to the problem of self–selection. To solve the endogeneity problem and estimate the causal effects of education, a common approach has been to use randomised controlled trials (RCT) or to exploit natural experiments and use quasi–experimental methods, such as instrumental–variable (IV), regression discontinuity (RD), or difference–in–differences (DD) approaches (Card, 1999; Webbink, 2005; Schwerdt and Woessmann, 2020). The former approach has ethical issues and is costly to conduct in educational research, thus, papers focus on the latter quasi–experimental methods. Policy changes, that cause one group to obtain more education than another, are a useful tool to make causal inferences from acquiring more years of education.

On the one hand, many papers estimate the causal effect of an extra year of compulsory education and find positive substantial returns to education in high–income countries (Angrist and Krueger, 1991 for the US; Levin and Plug, 1999 for the Netherlands; Brunello and Miniaci, 1999 for Italy; Vieira, 1999 for Portugal; Oreopoulos, 2006 for the UK; Aakvik et al., 2010 for Norway). Hence, one extra year allows institutions to expand their curricula and increase the funding of education, suggesting increased wages and employment rate of graduates. On the other hand, there is rich literature with credibly identified estimates of a zero effect of compulsory schooling on wages and employment in high–income countries. Meghir and Palme (2005) use the social experiment, where a school reform was implemented gradually across municipalities in Sweden in the late 1940s—1950s. The authors, using a

[2]Hereinafter by returns to education are meant returns to higher education unless explicitly stated otherwise.

DD design, find that increased compulsory schooling has no significant impact on average earnings. Pischke and von Wachter (2008) exploit the reform of compulsory schooling in West Germany during the period from 1947 to 1969. The authors, using an IV approach, find a zero effect of the additional year on earnings. Devereux and Hart (2010) exploit the reform that increased the duration of compulsory schooling by one year in Britain in 1947. The authors, using a RD design, find no evidence of any positive returns to an extra year for female earnings and modest returns for male earnings. Grenet (2013) uses the reform that raised the minimum school–leaving age by two years in France in 1967. The author, using a RD design, finds that the impact of the change on hourly wages is close to zero.

The causal estimates of returns to the additional year of other levels of education are sparse and inconclusive. Oosterbeek and Webbink (2007) exploit the reform of vocational education in the Netherlands in 1975. Until the reform, about half of all graduates from Dutch basic vocational schools finished 3–year programmes, the other half finished 4–year programmes. In 1975, the reform increased the length of all programmes from 3 to 4 years and extended the general content of the curricula. The authors, using a DD design, find no long–term effect of the policy change on labour market outcomes. Maurin and McNally (2008) exploit student riots in 1968 as a natural experiment and use birth in an affected cohort as an IV. Due to the events in 1968 and the relaxation of examinations, it became easier to progress to the next stage of higher education and to obtain more years of higher education than would otherwise have been possible. The authors find that each additional year spent at university increases wages by about 14%.

Few papers identify a direct causal effect of a reduction in the years of education on labour market outcomes. Webbink (2007) exploits the institutional reform in the Netherlands in 1982, which reduced the duration of all programmes at research universities from 5 to 4 years, while it did not change the length of programmes at universities of professional education, which were already 4 years. This reform allows him to use a DD design to identify the wage effect of one year of university education. The author finds that wages for those who graduated from 4–year programmes are substantially lower than wages of graduates completing 5–year programmes. Pischke (2007), using variation in the length of the school year in West Germany in 1967, estimates the causal effect of education on earnings and employment. The reform reduced two–thirds of a year in secondary schools across German states, while it did not change the nominal curricula. The author, using an IV approach, finds that shortening the length of education has no adverse effect on earnings and employment later in life.

To the best of my knowledge, only three papers with a credible identification strategy estimate the causal effect of education on labour market outcomes in low– and middle–income countries. Arteaga (2018) exploits a reform at Colombia's top university, which reduced the amount of coursework and the university study period by half a year but did not change the quality of incoming or graduating students. The author, using a DD design, finds that this reduction led to lower wages and employment, suggesting that human capital plays an important role in the labour market, and rejects a pure signalling model. Eble and Hu (2019) study the policy change in China in the late 1980s–2000s which extended the duration of primary school by one year while holding the national curricula unchanged. The authors, using a RD design, find that the additional year of education has a zero effect on employment and a modest impact on income. Elsayed and Marie (2020) study the policy reform that reduced compulsory schooling by one year in Egypt in the late 1980s. The authors find that this reform led to an increase in the total amount of education attained by the treated students. As a result, the policy has a positive significant effect on earnings and employment.

Ultimately, one part of the literature finds a positive effect of an extra year of education on wages and employment (Angrist and Krueger, 1991; Levin and Plug, 1999; Brunello and Miniaci, 1999; Vieira, 1999; Oreopoulos, 2006; Webbink, 2007; Maurin and McNally, 2008; Aakvik et al., 2010; Arteaga, 2018; Eble and Hu, 2019; Elsayed and Marie, 2020), while another part fails to find an effect of the length change of education on labour market outcomes (Meghir and Palme, 2005; Oosterbeek and Webbink, 2007; Pischke, 2007; Pischke and von Wachter, 2008; Devereux and Hart, 2010; Grenet, 2013; Eble and Hu, 2019). These puzzling results suggest that institutional features of the education and labour markets play a significant role in the formation of returns to education in different economies.

Arcidiacono et al. (2010) show that returns to education in the US are large for college graduates immediately upon entering the labour market and do not significantly change with labour market experience. These findings suggest that employers know fully the skills of college graduates as soon as they enter the job market. Gimpelson (2019) shows that workers with higher education earn significantly more upon entering the labour market, compared to all other workers in Russia. Thus, short–term labour market outcomes are good measures of workers' accumulated human capital, which allow me to evaluate graduate experiences in the school–to–work transition in Russia (Ryan, 2001).

2.2 The Bologna reform in European countries

One of the main ideas of the Bologna reform is to enhance the employability of graduates (Teichler, 2011). The core step, in order to implement the reform, is to shorten the length of university education and create a two–cycle degree structure (bachelor's and master's). A unitary higher education system was common among many European countries. The implementation of the Bologna reform, i.e. creating new types of degrees, with different lengths of study, had mixed effects on the labour market outcomes of graduates in European countries. One should keep in mind two important characteristics of research design that might affect the results and the interpretation of the results. First, the Bologna reform was implemented independently in each country. In some countries, the reform was implemented by the Ministry of Education, while other countries allowed regions or universities to decide when and how to implement the new programmes. Second, the authors use databases with different time gaps between graduation and the measured outcomes of the employment rate and wages.

On the one hand, some papers find positive or insignificant results of the Bologna reform. Bosio and Leonardi (2011), using DD and IV models, estimate the employment rate of college graduates relative to non–graduates 5 years after the reform in Italy. The authors find that the reform increased significantly the relative employment of Bologna male graduates, while the effect is not significant for female graduates in most specifications. Garra (2013), using a DD design, finds evidence of the impact of the Bologna reform in the Portuguese labour market for recent graduates. The author finds statistically significant results for females, while the results for male graduates are not significant. Female graduates whose courses were affected by the Bologna reform have wages 2,5–3% higher than those in the control group. Neugebauera and Weiss (2018) compared wages of Bologna bachelors' graduates with other labour market entrants within 4 years after graduation in Germany. They find that bachelors' graduates are associated with higher wages than initial vocational education graduates, and with similar wages to further vocational degrees.

On the other hand, others find a negative effect of the Bologna reform. Bosio and Leonardi (2011) find that Bologna graduates in Italy have a significantly lower college premium 3 years after graduation. Farčnik and Domadenik (2012), using propensity score matching, estimate the probability of the employment of recent Bologna graduates in Slovenia. They find that Bologna graduates exhibit a lower probability of employment than their pre–Bologna counterparts. Neugebauera and Weiss (2018) find that Bologna bachelors' degrees are related to higher risks of unemployment than vocational degrees in Germany. Glauser et al. (2019) investigate the impact of the Bologna reform on the graduate labour market 1 and 5 years after graduation in Switzerland. They find that the newly introduced bachelors' degrees serve as a signal of graduate productivity in the first years of the Bologna reform. However, there is no empirical literature that evaluates the impact of the Bologna reform on the labour market outcomes of university graduates in Russia.

2.3 Institutional background and the reform

The majority of students have state–funded places in higher education institutions (HEIs) in Russia.[3] Therefore, the number of available places is exogenous to applicants. About 90% of university applicants are students who apply to university in the same year as they graduate from school. The Ministry of Science and Higher Education determines the number of state–funded places and the curricula in the majority of universities. Some HEIs, which are under the supervision of sectoral ministries, such as the Ministry of Agriculture, the Ministry of Culture, and the Ministry of Health, set their own regulations for universities, although they have little power to influence the curricula and the quality of programmes.

The two–cycle degree structure has been optional for HEIs, parallel to 5–year

[3] In this paper I describe the system of higher education that is relevant for full–time students in public and private HEIs.

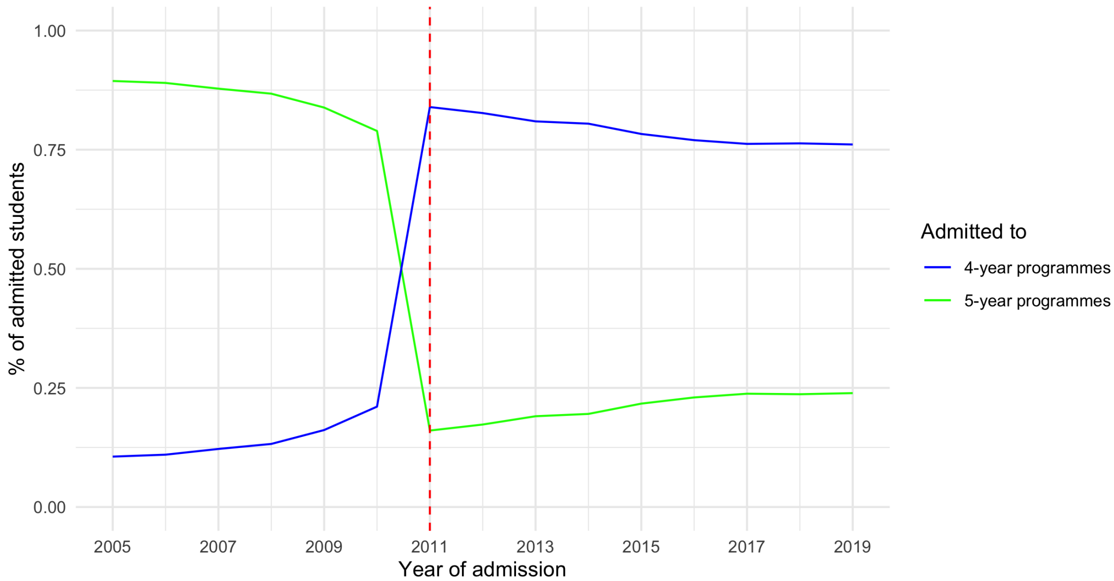
degrees, since 1992. In 2003, Russia signed the Bologna Declaration and some universities started implementing two–cycle degree programmes. In 2011, universities, which were under the supervision of the Ministry of Science and Higher Education and several others, transformed all programmes from 5– to 4–year degrees, while other ministries did not require universities to follow this reform.[4] It was a politically–oriented decision to keep programmes at 5 years and it did not relate to labour market conditions. Thus, if applicants applied to a specific programme, they could only apply to either 4– or 5–year programmes. Such an institutional design allows me to use those programmes that were transformed from 5 to 4 years as a treatment group, and those that remained as 5 years as a control group.

The Bologna reform was only implemented in the higher education sector. Vocational education programmes were not affected by the reform, thus, all programmes remained the same length. The majority of vocational educational programmes last for 3 years. Thus, 3–year vocational educational programmes can be used as an alternative control group to estimate the effect of one year of university education. There was also no change in the high school curricula to adjust for the reduction in the university study period, so university applicants are homogeneous over time (see Section 5.3 for more details).

The Bologna reform transformed Russian higher education from a unitary system, with 5–year specialists' degrees, to the two–cycle system, with 4–year bachelors' and 2–year masters' programmes. In general, some parts of the course structure and curricula were reduced, especially optional elements. A one–year reduction of the curricula was mostly accompanied by the reduction of specialised courses, so the so–called "specialisation" of university education was reduced. This is one of the most fundamental institutional reforms of the Russian higher education system in the last decade and one of the most debated ones. The mass introduction of two–cycle higher education degrees happened in the academic year 2010–2011. In 2010, 79% students were admitted to 5–year programmes and 21% to 4–year programmes, and the opposite pattern in 2011: 16% were 5-year students and 84% were 4-year students (see Figure 1). This creates a natural experiment for the 2011 intake.

[4]The ministries that did not require HEIs to reduce the length of programmes followed a Decree of the Government of the Russian Federation N1136, which specified certain programmes within broad fields of study to keep at 5 years.

Figure 1: The share of 4– and 5–year full–time admitted students in public and private HEIs in 2005–2019



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

The Bologna reform generated at least two possible effects in the labour market in Russia. First, it shortened the study period of university graduates. Employers could see the new 4–year degrees as a compressed version of the old 5–year programmes, with a downgrade in the quality of degrees. Second, it created an excess supply of graduates in 2015 (see Figure 3 in the appendix). Two cohorts of students, who completed 4– and 5–year programmes, started looking for a job simultaneously. This could potentially affect the wages and employment rate of both cohorts. Besides, 4– and 5– year graduates might continue studying to get masters' degrees. About 30% of 4–year and 20% of 5–year graduates studied at the master's level after the reform. In a nutshell, in 2015, there was a situation in the labour market with an excess number of job–seekers having 4– and 5–year degrees.

Lukiyanova (2010) and Roshchin and Rudakov (2015) summarise a wide variety of papers based on Russian labour market data, which are dedicated to estimating returns to education. All in all, studies for Russia rely on OLS estimates. Lukiyanova (2010), using meta–analysis, finds that returns to one year of education are 7.1%.

Belskaya et al. (2014) show that returns to one year of university education are 7.3% in Russia. Ultimately, there are no causal estimates of economic returns to education in the Russian labour market.

3 Data

The data are obtained from the National Survey of Graduate Employment carried out by the Russian Federal State Statistics Service (Rosstat) in 2016. This is a cross–section sample of students who graduated in 2010–2015. The sample is representative at the national level. The data were collected from April to September 2016, i.e. all outcomes are relevant for the year 2016. The survey provides extensive information on educational backgrounds and labour market experiences during the first years after graduation. I restrict data to the following groups of students: 4– and 5–year university graduates and 3–year vocational education graduates. The education variable refers to the highest level of education attained. When respondents were asked to specify their higher education degree, they could only choose between "bachelor's" or "specialist's/master's". Although it may potentially create a bias of the estimates of the wages and employment of the group "specialist's/master's", the share of masters' graduates in 2010–2015 is negligible.[5] Such a bias would only strengthen the results. According to the definition of youth by Rosstat, I further restrict the dataset to 20–30 year–old graduates who are not currently students. Only graduates from full–time programmes are included. In total, there are 18,285 observations. I use net hourly wages, calculated from net monthly wages, the number of working days, and working hours per day. 36% of employed university graduates and 33% of employed vocational education graduates did not report their wages. There are no statistically significant differences between graduates who did and did not report their wages, except for the gender variable for vocational education graduates. The unemployed category captures both the registered unemployed and the economically inactive.

Table 1 shows the descriptive statistics of the variables for university graduates. The average age is 25.8. 42% of graduates are males. 15% of graduates finished

[5]The share of masters' graduates is about 4–8% out of all graduates in 2010–2015.

4–year programmes. 95% of graduates studied at public universities and 59% of students had state–funded places. Almost a quarter of graduates combined study and work. 86% of graduates were employed in the year of the survey while the mean work experience is 3.8 years. The mean monthly wage in 2016 was 24,359 RUB (375 USD, 1 USD = 65 RUB).

Table 1: The descriptive statistics of the variables for university graduates

Variable	N	Mean	St. Dev.	Min	Max
Age	11,332	25.81	2.09	20	30
Male	11,332	0.42	0.49	0	1
4–year graduate	11,332	0.15	0.36	0	1
Public university	11,332	0.95	0.21	0	1
State–funded	11,332	0.59	0.49	0	1
Combined study and work	11,332	0.24	0.42	0	1
Employed	11,332	0.86	0.35	0	1
Work experience	11,332	3.82	1.65	0	7
Monthly wage, RUB	6,207	24,359	11,890	5,000	130,000

Table 2 shows the descriptive statistics of the variables for vocational education graduates. The average age is 23.8. Almost half of graduates are males. 97% of vocational education students graduated from public institutions while 78% had state–funded places. 15% of vocational education graduates combined study and work. 81% of graduates were employed in the year of the survey and the mean work experience is 3.5 years. The mean monthly wage in 2016 was 20,223 RUB (310 USD).

Tables 9 and 10 in the appendix show the descriptive statistics for university and vocational education graduates by the year of graduation. As expected, younger cohorts of graduates have lower wages and employment rates, as all outcomes were measured in 2016. The share of graduates from public universities, the share of students who had state–funded places, the share of graduates who combined study and work, is consistent throughout all years. Overall, the observed characteristics of the cohorts of graduates are stable over time, as it is also evident and discussed in details in Section 5.3.

Table 11 in the appendix shows the descriptive statistics separately for 4– and 5–

Table 2: The descriptive statistics of the variables for vocational education graduates

Variable	N	Mean	St. Dev.	Min	Max
Age	6,809	23.77	2.32	20	30
Male	6,809	0.49	0.50	0	1
Public institution	6,809	0.97	0.16	0	1
State–funded	6,809	0.78	0.42	0	1
Combined study and work	6,809	0.15	0.36	0	1
Employed	6,809	0.81	0.39	0	1
Work experience	6,809	3.52	1.75	0	7
Monthly wage, RUB	3,759	20,223	9,749	5,600	150,000

year university graduates in 2015. The means and standard deviations are displayed in column (1) for 4–year graduates and in column (2) for 5–year graduates. Column (3) displays the differences in the means and p–value in a two–sample t–test for the difference of means. 4– and 5–year graduates do not differ in most observable characteristics. 5–year graduates more often have state–funded places. As 5–year graduates study one year more, they are older and have more work experience. As a result, they are more likely to get a job and receive higher wages. The differences between rural residence or Moscow and Saint Petersburg residence are not significant at the 0.05 level. This indicates that the implementation of the reform has not led to a shift in the composition of student cohorts (see Section 5.3 for more details).

4 Empirical strategy

4.1 Identification strategy

This paper estimates the short–term causal effect of one year of university education on wages and employment in Russia. I do so by using the Bologna reform as a source of an exogenous variation in the length of university education that is orthogonal to the potential outcomes. I use the Mincer earnings function to estimate returns to education (Mincer, 1974). Using two cohorts of students who graduated in the same year, I can estimate the differences between the level of wages and the probability of being employed of 4–and 5–year degree holders under similar labour market conditions. However, the simple estimates of returns to education for these two groups are confounded by the effect of other changes that had impacts on students enrolled before and after the reform. To correct for that I use a control group, i.e. graduates from programmes that did not change in length. Thus, the changes in the differences between the treated and control groups under certain assumptions are explained by the effect of one year of university education.[6]

The implicit assumption is that the treatment (shortened length of university education) is as good as randomly assigned from the perspective of students. As described in the institutional background, most programmes transformed from 5 to 4 years. The share of 4–year admitted students by field of study is in Table 12 in the appendix. The unit of treatment is a programme, i.e. all students follow the nominal study duration of a specific programme. Thus, the treatment group consists of graduates from programmes that changed in length, the control group consists of graduates from programmes that did not change in length. The decision to keep some programmes at 5 years was politically oriented and it did not relate to labour market conditions. In 2010, almost all programmes lasted for 5 years and the policy change occurred in 2011. As the year of application to university is a function of school graduation year, university applicants could not affect their decision to start 4– or 5–year programmes. As a result, I argue that the Bologna reform offers an exogenous variation in the length of university education. In other words, variation in the length of the university study period is assumed to be uncorrelated with other factors that might affect labour market outcomes, such as ability or self–selection. As an alternative control group, I use graduates from 3–year vocational education programmes. As the Bologna reform transformed only the university sector, vocational education programmes were not affected by the reform. Thus, these graduates constitute a relevant control group.

I conduct a DD analysis to estimate the effect of one year of university education on labour market outcomes. Although labour market conditions faced by graduates from different fields can differ, this difference is captured by field fixed–effects, i.e. fixed–effects are captured on the level of 9 fields of study (see Table 12 in the

[6]Although the DD design is used to estimate the causal effect of a policy intervention, when some individuals are subject to a 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).

appendix). Additionally, I include year and region fixed–effects, as outcomes can vary significantly by the year of graduation and regions. The results of a DD analysis are obtained from Model 1:

$$Y_{ipr} = \beta_0 + \beta_1 Treated_p + \beta_2 Treated_p * Post_y + \beta_3 X_i + \xi_y + \mu_f + \eta_r + \varepsilon_i \qquad (1)$$

where Y_{ipr} is the labour market outcome of graduate i from programme p in region r (I look at log hourly wages and a dummy variable that equals to 1 if a graduate is employed and 0 otherwise); $Treated_p$ is a dummy variable that equals to 1 if a student has graduated from 4–year programme p and 0 otherwise; $Treated_p * Post_y$ is the interaction of $Treated_p$ and a dummy variable for the year of graduation 2015; X_i are observable individual controls of graduate i (gender, experience, experience squared, type of institution (public/private), funding (state–funded, fee–based), combining study and work); ξ_y are the year of graduation fixed–effects; μ_f are field fixed–effects; η_r are region fixed–effects; ε_i is the error term that captures the unobserved effects that determine graduate labour market outcomes. β_2 is the parameter of interest that gives the causal estimate of the effect of a one–year reduction of university education. To estimate the probability of being employed, I use a linear probability model. As a robustness check, I estimate a logistic model that yields identical results.

4.2 Parallel trend assumption

Figure 5 in the appendix plots log hourly wages for the 4–year treatment and 5–year control groups controlling for individual characteristics. Figure 6 in the appendix plots the employment rate for both groups controlling for individual characteristics. The general trend for wages is going down, reflecting that younger cohorts of graduates have lower wages. The employment rate for both groups is decreasing, as younger cohorts of graduates have less time in the labour market to find a job.

On the one hand, Figure 5 provides evidence that the wages of the 4–year treatment group closely follow the pattern of the 5–year control group, and there seems to be no deviation from this trend in 2015. On the other hand, the employment rate of the 4–year treatment group is lower, compared to the 5–year control group. Figure 6 provides visual evidence of the treatment and control groups having a common underlying trend, and the treatment effect in 2015 also does not seem to induce a deviation from this trend.

Figures 7 and 8 in the appendix show log hourly wages and the employment rate for the 4–year treatment and 3–year control groups. The wages and the employment rate of 3–year vocational education graduates are lower than that of 4–year university graduates. All in all, there also seems to be no effect of the reform at 2015, using the alternative control group.

Ultimately, the parallel trend assumption for both wages and employment seems plausible (Angrist and Pischke, 2009). Figures 5, 6, 7, and 8 suggest that prior to 2015 labour market conditions for the treated and control groups followed similar patterns: wages were relatively low and the employment rate was high. There were no significant differences between graduates in the treated and control groups. In 2015, the policy change might have affected human capital accumulation of 4–year graduates, due to less time spent at university, thus, it might have influenced the labour market outcomes of graduates later in life.

5 Results

5.1 Main results

Table 3 shows the OLS estimates as baseline results of returns to one year of university education in Russia. Columns (1), (2), and (3) show the effect of a one–year reduction of university education on log hourly wages for all graduates, and separately for males and females. Columns (4), (5), and (6) indicate the results of the linear probability model for employment. The results for log hourly wages show that a one–year reduction leads to lower wages, suggesting that returns to one year of university education are 3.8%. Returns are higher for females than for males, which is consistent with the current literature. The overall results are driven by returns to education for females. Returns to one year of university education are 4.8% for females and are statistically significant, while returns to education for males are 2.1% and are not statistically significant. The results for employment indicate that a one–year reduction does not induce a reduction in the probability of being employed for the full sample or for males and females separately. Nonetheless, the OLS results are most likely biased, thus, I estimate the causal effect of one year of university education, using a DD design.

Table 3: The OLS estimates of the effect of a one–year reduction of university education on log hourly wages and employment for all and separately for males and females

	log hourly wages			*employed*		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	All	Male	Female
OLS	−0.038***	−0.021	−0.048**	0.005	−0.005	0.011
	(0.014)	(0.022)	(0.019)	(0.005)	(0.006)	(0.008)
Ind. controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,207	2,771	3,436	11,332	4,783	6,549
Adjusted R^2	0.357	0.332	0.359	0.656	0.686	0.653

Robust standard errors in parentheses. Columns (1) and (4) include individual controls for gender, experience, experience squared, type of institution (public/private), funding (state–funded, fee–based), combining study and work. Columns (2), (3), (5), and (6) include the same individual controls as columns (1) and (4), excluding gender.

*p<0.1; **p<0.05; ***p<0.01

Table 4 shows the results for log hourly wages, using a DD design. The main variable is *Treated_Post*, which shows the causal effect of a one–year reduction of university education. The variable *Treated_Post* with the reversed sign can be interpreted analogously to returns to one year of university education. Columns (1), (2), (3), (4), and (5) yield the results for 5–year university graduates as a control group while columns (6), (7), and (8) indicate the results for 3–year vocational education graduates as a control group. Column (1) shows that returns to one year of university education are 2.2% but they are not statistically significant. Columns (2) and (3) show the results separately for males and females. The coefficient has a negative sign for male graduates but it is not statistically significant. Female graduates seem to benefit from the policy change but the effect remains statistically insignificant. A one–year reduction of university education does not have the statistically significant effect on wages for graduates, whether or not they combined study and work (see columns (4) and (5)). Column (6) supports the previous findings, using 3–year vocational education graduates as a control group, and shows the zero effect of one year of university education on wages. Columns (7) and (8) show identical results for male and female graduates. All in all, the results suggest that the OLS estimates of log hourly wages in Table 3 are biased and returns to one year of university education are close to zero and not significantly different from it.

Table 5 shows the results of the linear probability model for employment. The marginal effects of the logistic regression are presented in Table 13 in the appendix and yield similar results. Columns (1), (2), (3), (4), and (5) indicate the results, using 5–year university graduates as a control group while columns (6), (7), and (8) indicate the results for 3–year vocational education graduates as an alternative control group. Column (1) shows that 4–year graduates are as likely as 5–year graduates to have a job, as the effect is not significant. Columns (2) and (3) show the results separately for male and female graduates. The effect is zero and not statistically significant either for males or females. Columns (4) and (5) show the results separately for graduates, who did and did not combine study and work. In line with the previous results, the employment status of both groups of graduates is not affected by a one–year reduction of university education, the effect is not statistically significant. Ultimately, I do not find a significantly negative effect of a one–year reduction on the employment rate, using 5–year graduates as a control group. Columns (6), (7), and (8) show the results for employment, using the alternative control group, i.e. 3–year vocational education graduates. Column (6) shows the zero effect of one year of university education on employment. The results are consistent for males and females, the effect is not statistically significant (see columns (7) and (8)). Thus, the results are robust to using an alternative control group. These findings confirm that a one–year reduction does not affect the labour market outcomes of university graduates. These findings support the results of the OLS estimates of the linear probability model and suggest that there is no adverse effect on the employment rate.

Table 4: The effect of a one–year reduction of university education on log hourly wages for all, separately for males and females, and by combining study and work

	5–year control group					*3–year control group*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Male	Female	Yes	No	All	Male	Female
Treated_Post	−0.022	−0.050	0.019	0.046	−0.048	−0.002	−0.029	0.019
	(0.030)	(0.047)	(0.039)	(0.065)	(0.032)	(0.031)	(0.049)	(0.041)
Ind. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,207	2,771	3,436	1,528	4,679	4,695	2,314	2,381
Adjusted R^2	0.357	0.332	0.359	0.280	0.382	0.324	0.269	0.330

Robust standard errors in parentheses. Columns (1) and (6) include individual controls for gender, experience, experience squared, type of institution (public/private), funding (state–funded, fee–based), combining study and work. Columns (2), (3), (7), and (8) include the same individual controls as columns (1) and (6), excluding gender. Columns (4) and (5) include the same individual controls as columns (1) and (6), excluding combining study and work.

*p<0.1; **p<0.05; ***p<0.01

Table 5: The effect of a one–year reduction of university education on employment for all, separately for males and females, and by combining study and work

	5–year control group					*3–year control group*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Male	Female	Yes	No	All	Male	Female
Treated_Post	0.005	0.006	0.000	0.007	0.004	0.014	0.004	0.004
	(0.011)	(0.014)	(0.015)	(0.028)	(0.012)	(0.012)	(0.016)	(0.017)
Ind. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,332	4,783	6,549	2,667	8,665	8,675	4,105	4,570
Adjusted R^2	0.656	0.686	0.653	0.442	0.702	0.661	0.654	0.672

Robust standard errors in parentheses. Columns (1) and (6) include individual controls for gender, experience, experience squared, type of institution (public/private), funding (state–funded, fee–based), combining study and work. Columns (2), (3), (7), and (8) include the same individual controls as columns (1) and (6), excluding gender. Columns (4) and (5) include the same individual controls as columns (1) and (6), excluding combining study and work.

*p<0.1; **p<0.05; ***p<0.01

5.2 Mechanism

Why are returns to one year of university education zero in Russia, while the literature shows relatively high estimates in low–, middle–, and high–income countries (Webbink, 2007; Maurin and McNally, 2008; Arteaga, 2018; Heckman et al., 2018; Gunderson and Oreopolous, 2020; Patrinos and Psacharopoulos, 2020)? Although some papers find a zero effect of one year of education, only a few suggest mechanisms explaining the results.

The most common explanation of zero returns to one year of education is that estimated returns may reflect, mostly, a signalling effect of the highest earned degree, rather than actual human capital accumulation, i.e. time spent on education. In other words, schooling does not enhance productivity but rather signals it. The literature aims to set human capital and signalling theories apart, although the theories are not mutually exclusive (Layard and Psacharopoulos, 1974; Groot and Oosterbeek, 1994; Riley, 2001; Chevalier et al., 2004). On the one hand, as human capital theory suggests, shortening the length of university study period reduces the direct and indirect costs of obtaining a degree. The reduction in the length of study leads to the decreased productivity of a graduate, which will result in lower wages and unfavourable labour market opportunities (Schultz, 1961; Becker, 1962). On the other hand, employers may hire and pay higher wages to more educated workers, not only because education makes a worker more productive, but because educational credentials signal a certain level of abilities (Spence, 1973; Arrow, 1973). The main assumption is that the signalling costs are negatively correlated with productivity. Thus, the marginal cost of signalling is lower for the more able than it is for the less able. Perhaps, shortening the length of university study period did not change the highest level of education completed in Russia, thus, it did not affect the signalling effect of a degree for 4– and 5–year graduates. Nevertheless, this seems to be too extreme and it is hard to disentangle a signalling effect from a human capital effect.

The second explanation, which partly relates to the first one, is that the additional year of education does not provide the necessary skills for the labour market. Although the former signalling explanation is more common, I favour the latter and the more credible skill–based explanation and provide suggestive evidence for this. Pischke and von Wachter (2008) explain zero returns to compulsory schooling in

Germany by the fact that the general skills most relevant for the labour market were already learnt before the extension of the length of education. Oosterbeek and Webbink (2007) suggest that zero returns to the additional year of vocational education in the Netherlands are because individuals do not benefit from the additional year of general education in the labour market. I argue that the Bologna reform mostly reduced specialised courses and affected the accumulation of specific skills, which turned out to be irrelevant for the Russian labour market.

I present a few pieces of suggestive evidence in support of the skill–based explanation. Suppose that employers did not discriminate graduates with fewer years of university education. Thus, the probability of being hired for 4–year graduates did not fall, compared to 5–year graduates. Further, employers offered similar wages to 4– and 5–year graduates, as they seemed to be of similar productivity because they had a higher education degree. My claim is that zero returns to the additional year are explained by the fact that specific skills, accumulated during the additional year by 5–year graduates, turned out to be irrelevant for the Russian labour market. To support this claim, one should expect that after 4–year graduates have gained work experience and showed their productivity, they would not need to possess more on–the–job training, compared to 5–year graduates. If 4–year graduates had a lack of specific knowledge, they should have invested more in the acquisition of on–the–job training, compared to 5–year graduates.

There are some questions in the National Survey of Graduate Employment that help to defend this claim. First, graduates were asked whether they had to get on–the–job training. 33.3% of 4–year and 29.6% of 5–year graduates said that they did. The difference between the shares of graduates, who possessed on–the–job training, is not statistically significant (see Table 11). This suggests that both 4– and 5–year graduates have similar levels of accumulated specific skills. Further, graduates specified the reasons why they had to get on–the–job training. The most relevant question for the paper is whether the need to get such training was due to the insufficient competence of a graduate. 14.5% of 4–year and 12.8% of 5–year graduates said that they got on–the–job training due to the insufficient competence (see Table 11). The difference is not statistically significant, suggesting that both groups of graduates have similar levels of accumulated human capital.

Graduates were also asked how matched they felt their field of study was to their current job. If the level of education is not optimal, a job–education mismatch occurs. Graduates with a lack of the relevant specific skills are more likely to work in a job which is not related to their field of study. 29.6% of 4–year and 26.2% of 5–year graduates work in a job which is not related to their field of study. The difference is not statistically significant, suggesting that both groups of graduates experience similar levels of job–education mismatch (see Table 11). As discussed in the institutional background, the content of the curricula for 4–year graduates was reduced mostly by the elimination of specialised courses or hours for these courses. Hence, I conclude that a possible explanation for the zero effect of shortening the length of university education is the irrelevance of specific skills, accumulated during the additional year by 5–year graduates, for the Russian labour market.

5.3 Robustness checks

Several possible confounding factors might influence my findings. As 5–year graduates have more work experience, while at university, than 4–year graduates do, the experience and experience squared variables might be endogenous, i.e. "bad" controls (Angrist and Pischke, 2009). The longer the study period, the more work experience a graduate has. This might bias the results of Model 1. Thus, I estimate Model 1, excluding experience variables, to test the robustness of the results. The results of the restricted model are in Table 6 for log hourly wages and in Table 7 for employment. Columns (1) and (4) of Table 6 indicate the results for log hourly wages for the 5– and 3–year control groups, respectively. Having excluded the experience variables, a one–year reduction does not cause any adverse changes in wages. Column (1) of Table 7 shows the results for employment for 5–year graduates as a control group. Column (4) of Table 7 shows the results for employment for 3–year graduates as a control group. In line with the previous findings, the effect of a one–year reduction on employment remains statically insignificant.

An additional concern is that students graduating at different ages would face different labour market conditions, as older cohorts of students can be preferred due to life experience. 5–year graduates are older than 4–year graduates on average, thus, they might face different labour market opportunities. In order to check this, I estimate Model 1, including age and age squared variables instead of experience variables. The results are presented in columns (2) and (5) of Table 6 for log hourly wages and in columns (2) and (5) of Table 7 for employment for the 5– and 3–year control groups, respectively. The estimates again show the zero effect of shortening the university study period on labour market outcomes.

The previous findings use log hourly wages to estimate the effect of one year of university education. One might expect that individuals with more years of education not only receive higher hourly wages but also work longer per month. Thus, I estimate Model 1, using log monthly wages as a dependent variable. Columns (3) and (6) of Table 6 show the results for the 5– and 3–year control groups, respectively. The results yield identical results for both control groups, suggesting that a one–year reduction does not cause changes in the working hours of graduates.

Another confounding factor is that there was a crisis in Russia in 2015. The literature shows that students, who graduate in a recession, suffer lower wages and experience unfavourable labour market conditions (Kahn, 2010; Oreopoulos et al., 2012). Russian graduates could have suffered due to the crisis in 2015, which is the year of interest. To check the robustness of my findings, I use data from the same source for students who graduated in 2014–2018.[7] The drawbacks of this dataset are that there is no information on whether a student graduated from a part– or full–time programme, and there is no information on wages. Although there might arise a bias, due to the inclusion of part–time graduates, I expect this bias to be consistent for the treatment and control groups. I estimate Model 1 including students who graduated in 2014–2018. Thus, there are 4 cohorts of university students who graduated after the reform (2015–2018). Column (3) of Table 7 shows the results for 5–year university graduates as a control group. Column (6) demonstrates the estimates for 3–year vocational education graduates as a control group. I fail to reject the zero effect of one year of university education on employment for graduates in 2015–2018 at the 0.05 level.

Finally, in order to check the results for robustness, I use the specification of Model 1 with leads (2012–2014). The rationale is that there should be no effect of a one–year reduction before the reform, as the policy change occurred in 2015. Table

[7] The data are obtained from the Russian Labour Force Survey carried out by the Russian Federal State Statistics Service (Rosstat) in 2019. The restrictions described in Section 3 are applied to this dataset if possible. More detailed information about this dataset is available upon request.

Table 6: The effect of a one–year reduction of university education on log wages, using different specifications

	5–year control group			*3–year control group*		
	(1)	(2)	(3)	(4)	(5)	(6)
	No exp	Age	Monthly	No exp	Age	Monthly
Treated_Post	−0.025	−0.013	−0.021	−0.003	−0.013	−0.002
	(0.030)	(0.030)	(0.028)	(0.031)	(0.032)	(0.030)
Ind. controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,207	6,207	6,207	4,695	4,695	4,695
Adjusted R^2	0.347	0.348	0.376	0.313	0.315	0.343

Robust standard errors in parentheses. In columns (1), (2), (4), and (5) the dependent variable is log hourly wages. In columns (3) and (6) the dependent variable is log monthly wages. Columns (1) and (4) include individual controls for gender, type of institution (public/private), funding (state–funded, fee–based), combining study and work. Columns (2) and (5) include the same individual controls as columns (1) and (4) and add age and age squared. Columns (3) and (6) include the same individual controls as columns (1) and (4) and add experience and experience squared.

*p<0.1; **p<0.05; ***p<0.01

Table 7: The effect of a one–year reduction of university education on employment, using different specifications

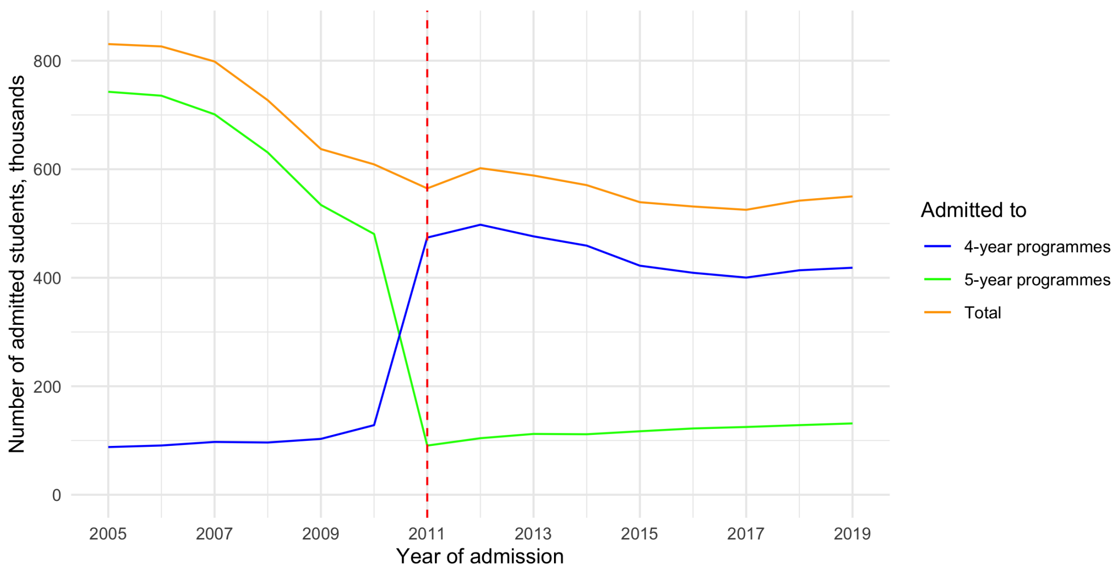
	5–year control group			*3–year control group*		
	(1)	(2)	(3)	(4)	(5)	(6)
	No exp	Age	16–18	No exp	Age	16–18
Treated_Post	−0.030	−0.025	−0.006	0.005	−0.024	−0.018*
	(0.022)	(0.023)	(0.010)	(0.024)	(0.025)	(0.011)
Ind. controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Field FE	Yes	Yes	No	Yes	Yes	No
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,332	11,332	37,591	8,675	8,675	30,172
Adjusted R^2	0.064	0.065	0.095	0.072	0.076	0.093

Robust standard errors in parentheses. Columns (1) and (4) include individual controls for gender, type of institution (public/private), funding (state–funded, fee–based), combining study and work. Columns (2) and (5) include the same individual controls as columns (1) and (4) and add age and age squared. Columns (3) and (6) include individual controls for gender, age, and age squared.

*p<0.1; **p<0.05; ***p<0.01

14 in the appendix shows the results of the DD model with three leads included, as suggested by Angrist and Pischke (2009). I fail to reject the zero effect on log hourly wages and employment in the 3 years before the reform. All other variables show similar magnitude and significance. A one–year reduction does not decrease wages nor does it affect the probability of being employed. The results are then more convincing, as pre–treatment data show no effects on the outcomes.

Figure 2: The number of 4– and 5–year full–time admitted students in public and private HEIs in 2005–2019, thousands



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

As one might expect, the reduction in the length of university study period could attract more students, as earning a degree has reduced the direct and indirect costs. Moreover, this reform could attract more students into HEIs, who would not go to university otherwise. Given the shortening of the university study period, lower–ability individuals should be motivated to enrol in these programmes. This might potentially create a bias, if more students were admitted to universities in 2011 and later on, compared to previous years. Figure 2 shows the total inflow of 4– and 5–year full–time students into public and private HEIs in 2005–2019. The total number of admitted students is decreasing over time, due to demographic changes. As evident in Figure 2, there is no increased inflow of students in the year of the reform and the following years, suggesting that the reform did not change the pool of admitted students.

Table 8 shows the observable characteristics of graduates when they were admitted to university. There are no differences in the ages of students at which they were admitted to university; the mean age is 17.6. The share of students from the most developed cities, Moscow and Saint Petersburg, remained stable over the period. The share of students from rural areas is a bit higher in the period 2013–2015 than in the period 2010–2012. This is most likely due to the reform of the standardised exam for admission to universities – the Unified State Exam (similar to the SAT). The Unified State Exam was implemented in some regions in 2003 and was introduced nationwide in 2009. Thus, it increased the accessibility of HEIs for students with different backgrounds. Although this reform influenced the quality of students, those, who graduated in 2013–2015, were equally affected by this reform. Thus, this reform does not influence the results of this paper.

Table 8: The observable characteristics of graduates of 2010–2015 when they were admitted to university

Variable	2010	2011	2012	2013	2014	2015
Age at the year of application	17.38	17.45	17.62	17.66	17.82	17.73
Rural residence	0.21	0.23	0.24	0.28	0.27	0.28
Moscow and Saint Petersburg residence	0.13	0.13	0.13	0.12	0.10	0.12

It is unlikely that university applicants could affect the year of their application. About 90% of school graduates apply to full–time university programmes in the same year as they graduate from school. As a result, the first cohort of students, who were admitted to 4–year programmes in 2011, graduated in 2015, and the last cohort of students, who were admitted to 5–year degrees in 2010, also graduated in 2015. Thus, the proportion of 4–year graduates to 5–year graduates was 1:1 in 2015 (see Figure 4 in the appendix).

6 Concluding remarks

This paper contributes to both empirical and methodological topics. The main result of the paper is that it provides more convincing evidence of the causal effect of one year of university education on labour market outcomes in low– and middle–income countries, i.e. Russia. As I exploit an exogenous variation in the university study period, my findings do not suffer from the endogeneity problem, which the previous literature, based on Russian labour market data, has difficulties addressing. I show that a natural experiment, such as the Bologna reform, provides researchers with more robust evidence of the causal effects of higher education, thus, one should use this reform in a quasi–experimental design as a source of identification.

The Bologna reform has affected the higher education system in Russia, by lowering the length of university education from 5 to 4 years in 2011. The first cohort of 4–year students and the last cohort of 5–year students graduated in 2015, while some programmes did not change the length of study. Importantly, the reform did not alter the quality of admitted students. These institutional settings allow me to use a DD design to estimate the causal effect of one year of university education on labour market outcomes. As forgone earnings and tuition fees are the real costs of going to university instead of working, 4–year graduates have lower costs for earning a higher education degree. This suggests that the reform decreased the university study period, forgone earnings, and tuition fees without negatively affecting labour market outcomes. The reform has shortened the university study period, allowing 4–year graduates to have the additional year of labour market experience compared to those who completed 5–year programmes. As the literature suggests, additional work experience has an impact on lifetime earnings, meaning the reform has increased the lifetime wealth of 4–year graduates (Topel, 1991; Dustmann and Meghir, 2005).

Ultimately, the results shed light on the causal effects of higher education on the labour market in low– and middle–income countries. Further research can use the Bologna reform as a source of an exogenous variation in the length of education and estimate long–term labour market outcomes. The reform can be used to study other effects of higher education, such as lifetime wealth, health, fertility, smoking, voting, as it offers a rare exogenous variation for producing convincing estimates.

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7 Appendix

Table 9: The descriptive statistics of the variables for university graduates by the year of graduation

Variable	2010	2011	2012	2013	2014	2015
N	1,741	1,723	1,847	1,817	1,788	2,416
Share	0.15	0.15	0.16	0.16	0.16	0.21
Age	28.30	27.35	26.51	25.52	24.65	23.47
Male	0.44	0.44	0.45	0.42	0.40	0.39
4–year graduate	0.08	0.10	0.12	0.14	0.16	0.26
Public university	0.94	0.96	0.95	0.95	0.96	0.95
State–funded	0.59	0.60	0.59	0.59	0.59	0.58
Combined study and work	0.22	0.23	0.24	0.24	0.26	0.22
Employed	0.89	0.89	0.88	0.88	0.85	0.78
Work experience	4.82	4.59	4.21	3.88	3.32	2.60
Monthly wage, RUB	26,670	25,715	25,233	24,198	22,568	22,351

Table 10: The descriptive statistics of the variables for vocational education graduates by the year of graduation

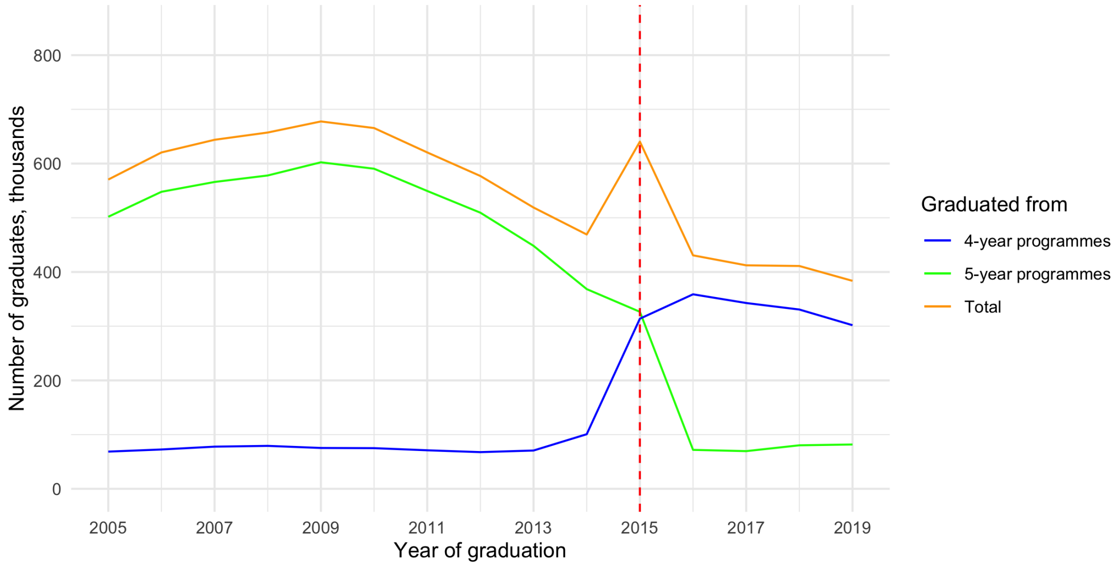
Variable	2010	2011	2012	2013	2014	2015
N	1,330	962	1,035	1,135	1,083	1,264
Share	0.20	0.14	0.15	0.17	0.16	0.19
Age	26.17	25.22	24.26	23.22	22.34	21.47
Male	0.50	0.50	0.46	0.49	0.48	0.48
Public institution	0.97	0.97	0.97	0.98	0.98	0.97
State–funded	0.79	0.78	0.80	0.78	0.76	0.76
Combined study and work	0.15	0.15	0.16	0.17	0.16	0.14
Employed	0.85	0.86	0.83	0.81	0.80	0.73
Work experience	4.39	4.28	3.85	3.43	2.95	2.34
Monthly wage, RUB	21,447	21,131	21,067	20,052	19,472	18,155

Table 11: The descriptive statistics of the variables separately for the 4– and 5–year graduates in 2015

	4–year	5–year	t–test
	(1)	(2)	(3)
Age	23.06	23.62	0.56
	(1.31)	(1.34)	(<0.001)
Male	0.40	0.39	0.00
	(0.49)	(0.49)	(0.890)
Public university	0.94	0.96	0.02
	(0.24)	(0.20)	(0.113)
State–funded	0.54	0.60	0.06
	(0.50)	(0.49)	(0.008)
Combined study and work	0.21	0.23	0.02
	(0.41)	(0.42)	(0.234)
Employed	0.74	0.79	0.05
	(0.44)	(0.41)	(0.021)
Work experience	2.43	2.66	0.23
	(1.53)	(1.47)	(0.001)
Monthly wage, RUB	21,401	22,673	1272
	(9,664)	(10,938)	(0.052)
Rural residence	0.28	0.28	0.00
	(0.45)	(0.45)	(0.914)
Moscow and Saint Petersburg residence	0.14	0.11	−0.03
	(0.35)	(0.32)	(0.058)
On–the–job training	0.33	0.30	−0.04
	(0.47)	(0.46)	(0.128)
Insufficient competence	0.14	0.13	−0.02
	(0.35)	(0.33)	(0.609)
Job-education mismatch	0.30	0.26	0.03
	0.46	0.44	(0.154)
Observations	639	1,777	

Columns (1) and (2) display the means and standard deviations. Column (3) displays the differences in the means and p–value in a two–sample t–test for the difference of means.

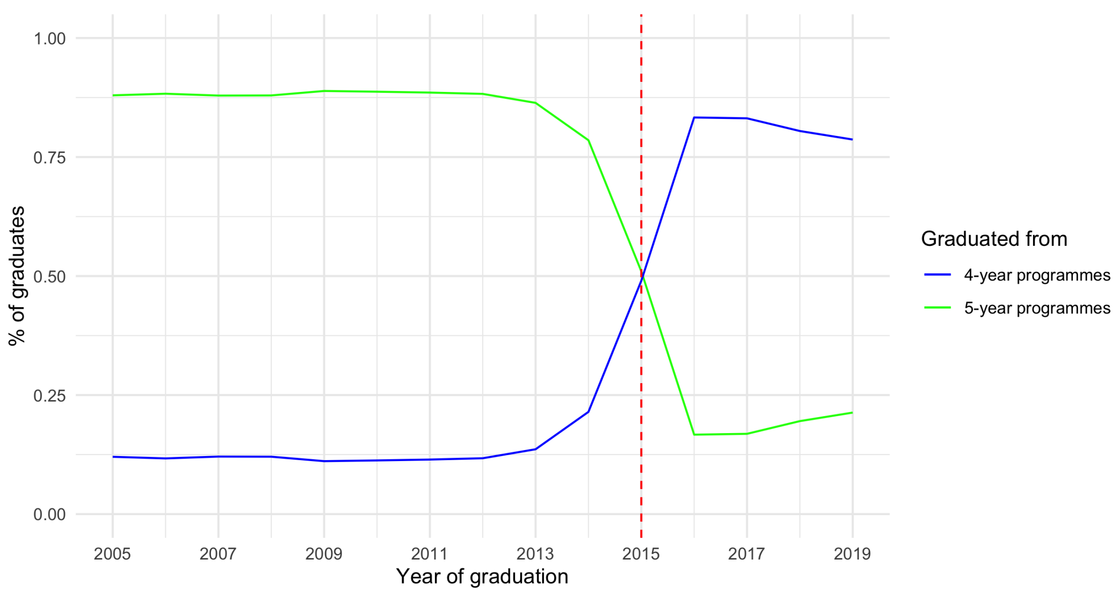
Figure 3: The number of 4– and 5–year full–time graduates in public and private HEIs in 2005–2019, thousands



Source: Ministry of Science and Higher Education.

The red dashed line shows the year of a double flow of graduates.

Figure 4: The share of 4– and 5–year full–time graduates in public and private HEIs in 2005–2019



Source: Ministry of Science and Higher Education.

The red dashed line shows the year of a double flow of graduates.

Table 12: The share of 4–year full–time admitted students in public and private HEIs by field of study in 2010–2011

Field of study	Share of 4–year admitted students	
	2010	2011
Education	0.31	1.00
Services	0.20	1.00
Social Sciences	0.26	0.99
Natural Sciences, Mathematics and Statistics	0.51	0.95
Information and Communication Technologies	0.30	0.92
Arts and Humanities	0.27	0.85
Agriculture, Forestry, Fisheries and Veterinary	0.24	0.85
Engineering, Manufacturing and Construction	0.27	0.83
Health and Welfare	0.00	0.00
Total	0.21	0.84

Source: Ministry of Science and Higher Education.

Table 13: The marginal effects of the logistic model of a one–year reduction of university education on employment for all, separately for males and females, and by combining study and work

	5–year control group					*3–year control group*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Male	Female	Yes	No	All	Male	Female
Treated_Post	−0.001	0.000	−0.006	−0.001	−0.002	0.010	0.000	0.009
	(0.005)	(0.007)	(0.012)	(0.014)	(0.006)	(0.007)	(0.005)	(0.017)
Ind. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,332	4,783	6,549	2,667	8,665	8,675	4,105	4,570

Robust standard errors in parentheses. Columns (1) and (6) include individual controls for gender, experience, experience squared, type of institution (public/private), funding (state–funded, fee–based), combining study and work. Columns (2), (3), (7), and (8) include the same individual controls as columns (1) and (6), excluding gender. Columns (4) and (5) include the same individual controls as columns (1) and (6), excluding combining study and work.

*p<0.1; **p<0.05; ***p<0.01

Figure 5: The log hourly wages of graduates in the 4–year treatment and 5–year control groups

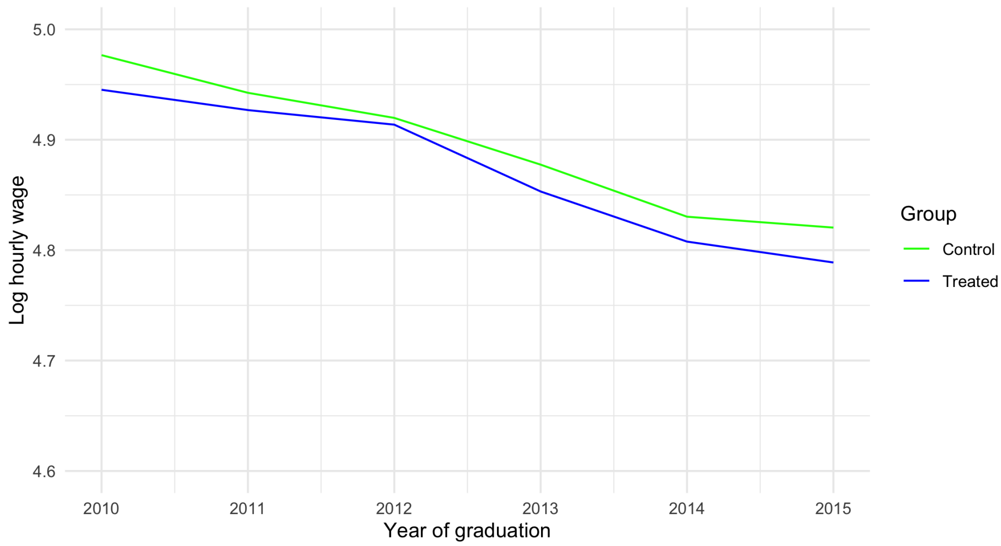


Figure 6: The employment rate of graduates in the 4–year treatment and 5–year control groups

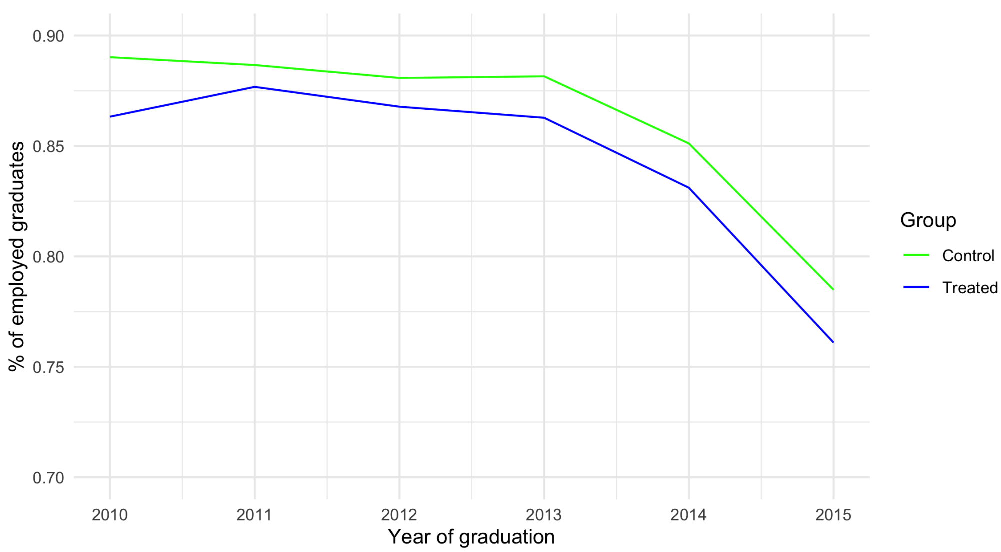


Figure 7: The log hourly wages of graduates in the 4–year treatment and 3–year control groups

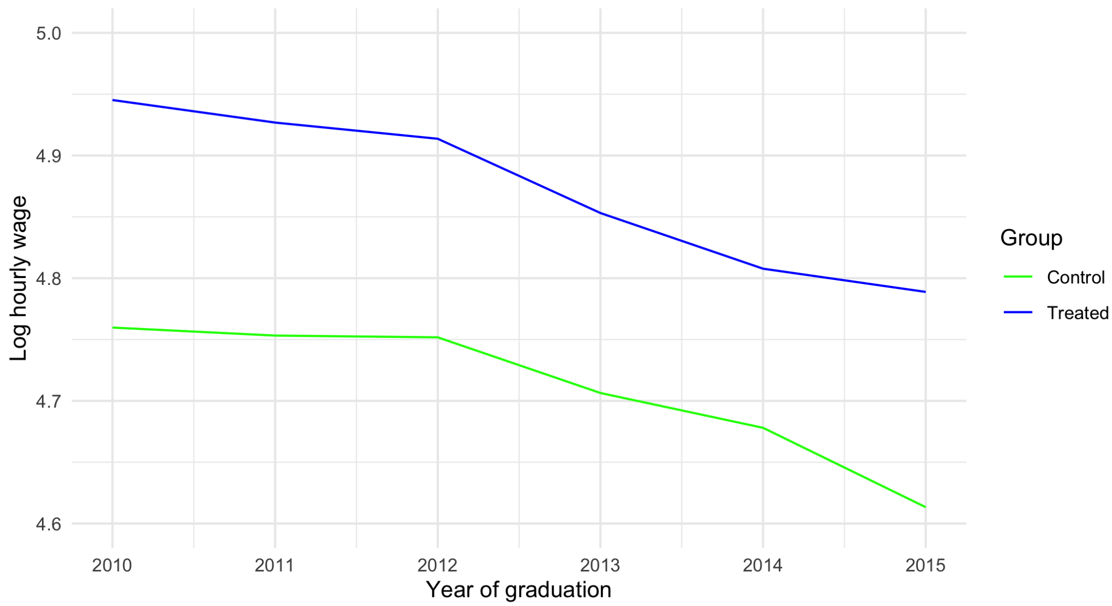


Figure 8: The employment rate of graduates in the 4–year treatment and 3–year control groups

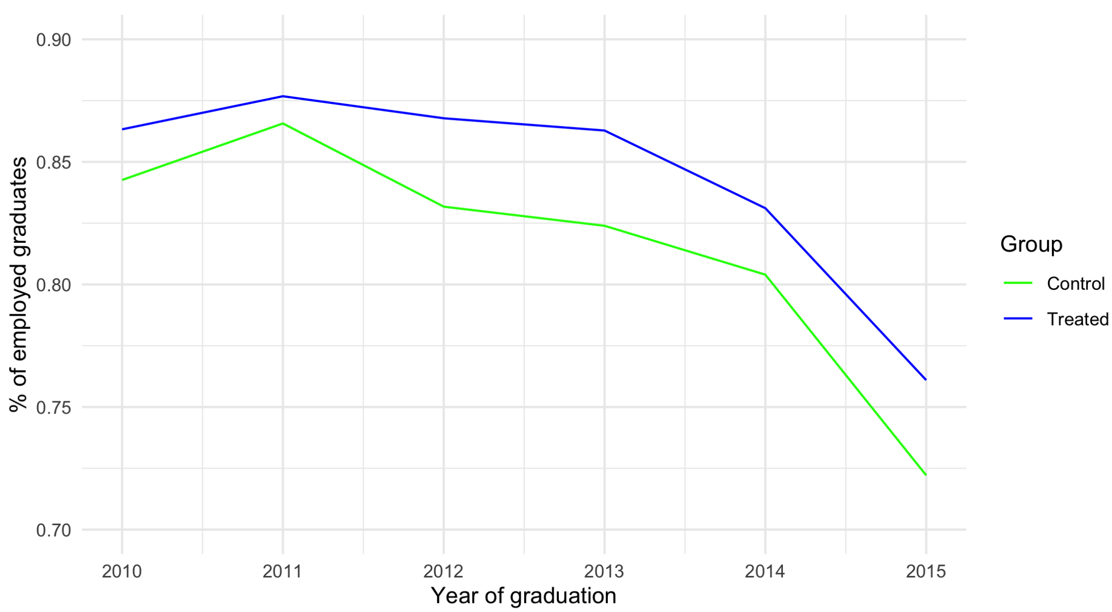


Table 14: The effect of a one–year reduction of university education on log hourly wages and employment with leads

	5–year control group		*3–year control group*	
	(1)	(2)	(3)	(4)
	log hourly wages	employed	log hourly wages	employed
Treated_Post	−0.016	0.015	−0.038	0.013
	(0.038)	(0.015)	(0.040)	(0.016)
Lead 2014	0.018	0.025	−0.050	0.004
	(0.043)	(0.017)	(0.045)	(0.019)
Lead 2013	−0.022	0.026	−0.067	0.019
	(0.045)	(0.016)	(0.046)	(0.017)
Lead 2012	0.037	−0.009	−0.003	−0.017
	(0.053)	(0.019)	(0.051)	(0.021)
Individual controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Field FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	6,207	11,332	4,646	8,531
Adjusted R^2	0.356	0.656	0.324	0.656

Robust standard errors in parentheses. All columns include individual controls for gender, experience, experience squared, type of institution (public/private), funding (state–funded, fee–based), combining study and work.

*p<0.1; **p<0.05; ***p<0.01

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