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Liwiński, Jacek

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The Impact of Compulsory Schooling on Earnings. Evidence from the 1999 Education Reform in Poland

Jacek Liwiński[∀]

Abstract

In 1999, a reform of education was implemented in Poland, which added one year to the shortest available educational path, leading to the acquisition of basic vocational education. In the new system, students choosing this path acquire one more year of general education, which, according to the authors of the reform, should improve the student's position in the labor market, as the inadequate general skills were identified as the main deficit of basic vocational education prior to the reform. Using the regression discontinuity design and data from the Polish LFS, we find that an additional year of general education has led to an increase in hourly wages of men, but not of women, who completed basic vocational schools.

Keywords: education; schooling; earnings; regression discontinuity design

JEL Classification: 121, 126, J24

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[∀] Corresponding author. Assistant Professor of Economics at University of Warsaw, Faculty of Economic Sciences, research fellow of the GLO. Address: ul. Dluga 44/50, 00-241, Warszawa, Poland. Mobile: +48.501321473; Email: jliwinski@wne.uw.edu.pl

Introduction

In recent years, the regression discontinuity design (RDD) has been used as a method of estimating the wage premium from formal education, with respect to changes in the compulsory schooling age. This method allows for estimating the local average treatment effect (LATE) resulting from the lengthening of compulsory schooling. Some empirical studies using this method find a fairly high wage premium from an additional year of education, of about 10% (Oreopoulos (2008) for the UK), but there are also studies showing a zero premium (Devereux and Hart (2010) for the UK, or Grenet (2013) for France). So far, the empirical evidence has been coming mainly from the Western European countries, the United States and Asia. The RDD method has not been used with respect to education reforms implemented in the Central and Eastern European countries (CEEC), yet.

The reform of education instituted in Poland in 1999 seems to be a good example of a reform of education in a CEEC, which can be used to estimate the wage premium from compulsory education. The basic change was a shift from a two-tiered education system (primary + secondary school) to a three-tiered one (primary + lower secondary + secondary school). The introduction of a an entirely new school type: the lower secondary school called "gymnasium" - to the education system in Poland was most visible to the public, therefore gymnasiums have became a symbol of the education reform of 1999. For pupils wishing to pursue secondary education, the number of years of schooling throughout the education path did not change. At the same time, the shortest available education path, leading to the basic vocational education, was extended from 11 to 12 years. In the new education system, those who choose this shortest path have to study for one year longer in comprehensive schools, which, according to the reformers, should improve their general skills and thus have a positive impact on their position in the labor market. The reformers claimed that in the old system those leaving basic vocational schools were missing general skills which was considered to be the key obstacle in their further career development.

We find that the additional year of general education resulting from the 1999 reform has led to a 15-percent increase in hourly wages for men with basic vocational education. The effect is higher than reported in similar studies for other countries. This may be due to the fact that this study is focused on individuals with relatively low abilities and general skills, who, had the reform not occurred, would have studied less.

The paper is structured as follows. The first section describes the reform of education introduced in Poland in 1999. In the second section we briefly review the empirical literature on the wage premium from compulsory education, with a focus on the methodologies and the findings. In sections 3 and 4 we present the method and data used in our analysis. Finally, in the last two sections we present the results and the most important conclusions.

1. The 1999 education reform in Poland

The education reform was implemented in Poland in the period 1999-2006. The reform changed the structure of the education system by replacing the two-tiered system (8 + 4), which existed in Poland since 1966, with a three-tiered one (6 + 3 + 3) (see Figure 1). In the old system, children aged 7 were obliged to start education in an eight-year primary school, after which they could continue at a secondary school (4 years of general secondary school or 5 years of vocational secondary school) or at a basic vocational school (3 years). In the new system, compulsory general education was extended from 8 to 9 years and divided into two stages - 6 years of primary school and 3 years of lower secondary school, called *gymnasium*. Those who complete gymnasiums may choose to attend secondary school or basic vocational school. The education cycle in secondary schools is one year shorter than in the old system (3 years in a general secondary school and 4 years in a vocational secondary school). The basic vocational school remained a three-year school (Jakubowski, 2015).

[Figure 1 about here]

In the new education system, the length of education paths leading to the completion of a secondary general or secondary vocational school remained the same as before the reform - 12 and 13 years, respectively. But at the same time, the shortest available education path, leading to the basic vocational education, has been extended from 11 to 12 years. Those who choose the shortest path have to study for one year longer in comprehensive schools, which, according to the reformers, should improve their position in the labour market, where general competencies are required as those enabling one to acquire new knowledge and skills throughout the professional career. Previously, the relatively low level of general skills of basic vocational school leavers was considered to be the main obstacle in the development of their professional career.

One could expect that the change in the structure of education system will encourage some young people to attend secondary vocational schools instead of basic vocational ones. The reason is that before the reform getting a secondary vocational certificate required two years of schooling more than getting a basic vocational certificate, while the reform reduced this difference in years of schooling to one year only. Thus, the alternative cost of obtaining a secondary vocational certificate dropped, which could have induced some of the gymnasium certificate holders to choose secondary vocational schools instead of basic vocational ones.

However, a quick look at the data do not provide evidence of any impact of the reform on educational decisions of young people. Figure 2 shows that the net enrollment ratio in basic vocational schools was gradually decreasing prior to the reform (1990-1998) and continued its downward trend during the implementation of the reform (1999-2004), to

stabilize at the level of 12-14% in the subsequent years. At the same time, the percentage of young people attending secondary vocational and secondary general schools was gradually increasing before and during the implementation of the reform, and it stabilized afterwards.

[Figure 2 about here]

The reform was launched on September 1, 1999. The students who completed the sixth grade of primary school began their first grade of gymnasium (instead of going to the seventh grade of primary school). This means that the reform affected children born on or after 1 January 1986, while children born until 31 December 1985 continued their education in the old system. The first cohort affected by the reform entered secondary schools in 2002 and completed them in 2005 (general secondary schools and basic vocational schools) or 2006 (vocational secondary schools). The first students who undertook university education having completed the new secondary schools could have graduated from the first degree in 2008 at the earliest, while from the second degree or unified master's degree – in 2010 at the earliest.

An important obstacle in estimating the impact of the 1999 reform on earnings is the fact that the first cohort covered by the 1999 reform was also the first cohort covered by the 3+2 reform in higher education, which was implemented in Poland starting from the academic year 2005/2006 (Kwiek, 2014). Hence, earnings of HEI graduates may have been impacted by both the 1999 reform and 3+2 reform and it does not seem possible to separate the two effects. Similarly, as the 3+2 reform may have affected the educational choices of secondary school leavers, one may argue that earnings of secondary school leavers may be impacted by both the 1999 reform and 3+2 reform.

This is why we restrict our analysis of the impact of the 1999 reform on earnings to the population of young people who completed formal education after obtaining a basic vocational school certificate. We argue that this group has never considered studying at HEIs, so their earnings are not affected by the 3+2 reform. We expect a positive impact of the additional year of general education resulting from the 1999 reform on their earnings.

We are aware that as we restrict the analysis to basic vocational school leavers our results might suffer from selection bias, had the reform have an impact on the share of young people choosing to study at this type of school. We address this issue below by conducting a formal analysis of the impact of the 1999 reform on the percentage of people holding a basic vocational school certificate.

2. Review of literature

The research of the wage effects of compulsory education was originated over 20 years ago and the methods used have undergone some evolution since then. Initially, the instrumental variable (IV) method was used, following Harmon and Walker (1995). The method boils down to estimating the Mincer wage equation, in which the years of schooling are instrumented by the compulsory school age. The average effect of extending the compulsory schooling is estimated as the ratio of the increase in earnings to the increase in years of schooling between the cohorts before and after the reform. In fact, this is an effect that applies to those only who, as a result of the reform, learn longer than they would have learned in the absence of reform, which is called the local average treatment effect (LATE) (Imbens and Angrist 1994).

The disadvantage of the above method is that it does not take into account the wage effect of changes in years of schooling which resulted from other reasons than the educational reform that increased the minimum school-leaving age. The positive wage premium may be due to the fact that regardless of the reform, the years of schooling are growing in successive cohorts of young people and so are the wages. So, two other approaches are used that eliminate this problem.

The first approach is the DID method, which can be used when the educational reform is not implemented simultaneously across the country, but gradually, i.e. at different time in different administrative units (Oosterbeek and Webbink 2004; Meghir and Palme 2005; Pischke and von Wachter 2008; Pekkarinen et al. 2009; Fischer et al. 2016). Pupils from units where the reform was implemented first are the treatment group, while those from units where the reform was implemented later are a control group. The average causal effect of the reform is estimated as the difference between wage growth rates in the treatment and control groups. In order for LATE to be consistent, the implementation of the education reform cannot be correlated with the implementation of other reforms or with any of the characteristics of the administrative units which influence the explanatory variable. So if, for example, the education reform will be implemented first in dynamically growing regions where wages are rising rapidly, the estimator of the wage premium will be biased.

The second approach is to estimate the regression discontinuity design (RDD) (Oreopoulos 2006, Devereux and Hart 2010, Grenet 2013, Ou 2013, Fuwa and Korwatanasakul 2015, Chib et al. 2016). This involves comparing the years of schooling in formal education and the earnings of the first cohort affected by the reform and the last cohort preceding the reform. LATE is estimated as the ratio of the earnings growth to the increase in the years of schooling.

The results of the studies vary considerably, depending on the method used and the reform case being analyzed. The studies using the IV method usually show a positive effect of compulsory schooling on earnings. For example, Harmon and Walker (1995), analyzing the education reforms introduced in England and Wales in 1947 and 1972, report that both reforms led to a significant increase in earnings (15%). In later studies using the same method, the positive effect of compulsory schooling on earnings was also found in other

countries (Levine and Plug (1999) for the Netherlands; Vieira (1999) for Portugal; Callan and Harmon (1999) for Ireland; Brunello and Miniaci (1999) for Italy, Pons and Gonzalo (2002) for Spain).

The studies using the DID method show little or no overall wage premium from compulsory schooling. Meghir and Palme (2005), analyzing the effects of extending the compulsory schooling in Sweden from 7-8 to 9 years in the 1949-1962 period, show that in general the reform did not affect earnings but it only led to a 3.4% increase in the earnings of individuals with ungualified fathers. Pischke and von Wachter (2008) report that the extension of compulsory schooling from 8 to 9 years, which was gradually implemented in Germany between 1949 and 1970, had no impact on earnings. Oosterbeek and Webbink (2007) show no wage effects of mandatory extension of years of schooling at secondary vocational schools from 3 to 4 years, implemented in the Netherlands in 1975. Pekkarinen et al. (2009) analyzed the effects of the education reform implemented in Finland between 1972 and 1975. They found that the extension of mandatory comprehensive education by three years had brought the wage benefits to individuals coming from families of low social status. In turn, Fischer et al. (2016) studied the effects of two educational reforms implemented in Sweden in the years 1930-1950. The first reform extended compulsory education from 6 to 7 years, while the second one extended the school year from 34.5 / 36.5 to 39 weeks. These reforms were not implemented at the same time in all schools, which allowed for separating the effect of each of them. It was found that both reforms brought a wage premium, but only to women - an additional year of schooling increased their wages in the initial period of their careers by 2%, and the extending of the school year led to a 4-5% increase in wages.

The studies using the RDD method also find that the effects of compulsory schooling are lower than the studies using the IV method show. Three of them concern the wage benefits of raising the compulsory schooling age in the UK from 14 to 15 years, which is the same reform that Harmon and Walker (1995) analyzed. Oreopoulos (2008) shows that this reform led to a 10% increase in earnings, while Devereux and Hart (2010) indicate that the earnings growth was much lower - only 3% - and only for men. In turn, Chib and Jacobi (2016) using a fuzzy regression discontinuity design model estimate the increase in earnings at 5-6%. In all the three studies, the wage premium is lower than that found by Harmon and Walker (1995). In turn, Grenet (2013) conducted a comparative study of the effects of raising the compulsory school age from 15 to 16 years in France and in England and Wales. He found that in France the reform did not affect wages, while in England and Wales the hourly rate increased by 6-7%. The author links the positive impact of the reforms in England and Wales to the fact that it led to an increase in the proportion of young people completing formal education with a diploma confirming their competences, which was not the case in France. Eble and Hu (2016) show that the extension of compulsory primary education by one year in China in 1980 led to an increase in wages on average by 2%, with a slightly higher wage premium obtained by individuals coming from low-income families. Finally, Fuwa and Korwatanasakul (2015) report that the extension of compulsory primary schooling from 4 to 6 years, which was implemented in 1978 in Thailand, resulted in a ca. 8% increase in wages.

To sum it up, the results obtained differ, depending on the method used. In particular, the wage premium estimated using the DID and RDD is lower than the one estimated using instrumental variables. Many studies using these first two methods, however, show a positive impact of compulsory schooling on wages. Obviously, the nature of reforms themselves might have affected the results, too.

As for the 1999 education reform in Poland, it has not been evaluated so far in terms of its impact on wages. But Jakubowski et al. (2010) argue that the reform had a positive impact on the PISA test scores of 15-year-old Poles between 2000 and 2006. In 2000, the PISA test was taken by pupils attending the first grade of post-primary schools (general secondary, vocational secondary and basic vocational schools) who were educated in the old system, while in 2003 and 2006 the test was taken by pupils learning in the new system, who were attending the third grade of gymnasium. The results of the math test were 470, 490 and 495 points in 2000, 2003 and 2006, respectively, while the results from the reading test were 479, 490 and 508, respectively. While the reading test score obtained in 2000 was below the OECD average, in 2003 it reached the OECD average level, and in 2006 it exceeded the OECD average, giving Poland the ninth place in the world. Jakubowski et al. (2010) report that this improvement was mainly owing to better results obtained by students of vocational schools (basic and secondary). The authors argue, that the improvement demonstrated by vocational school students may be a result of the additional year of general education introduced by the reform, which translated into more teaching hours of Polish language and mathematics.

3. Method

As the 1999 reform of education was implemented simultaneously across the country, the RDD is the most appropriate method for analyzing its effects.

Our analysis of the impact of the 1999 reform on earnings is restricted to the population of young people who completed formal education after obtaining a basic vocational school certificate. The discontinuity lies in the fact that individuals born until 31 December 1985 were able to obtain this certificate after 11 years of schooling, while those born on 1 January 1986 or later had to study for 12 years to obtain it, as the reform extended comprehensive education by one year. Based on the theory of human capital, we expect that the additional year of education will lead to an increase in general skills of students completing basic vocational schools and, consequently, to an increase in their productivity and earnings.

Based on previous studies performed using the fuzzy RDD (Oreopoulos 2006, Devereux and Hart 2010, Grenet 2013, Fuwa and Korwatanasakul 2015) we estimated the first stage equation and the reduced-form effect of the raising of the minimum school-leaving age by means of a global polynomial approximation. This methodology involves using the whole sample and choosing a high-order polynomial to fit the relationship between the outcome variable Y_i (number of years of education, earnings) and the forcing variable X_i (school cohort), allowing for an intercept shift at the cut-off, i.e. at the first cohort affected by the reform.

The first stage equation takes the form:

$$SCH_i = \alpha_0 + \alpha_1 REF_i + g (X_i - 1986) + \alpha_2 SVY + \varepsilon_i$$
(1)

where: dependent variable *SCH*_i is the number of years of formal education, *REF*_i represents the fact of being covered by the education reform (it takes value 0 for individuals born up to 1985 and 1 for individuals born in 1986 or later), g(.) is a quartic polynomial function, X_i - the respondent's year of birth, SVY - the survey year, and ε_i is a random error. In equation (1), the key parameter is α_1 , which measures the average treatment effect (ATT) of the increase in compulsory school-leaving age on the years of schooling of individuals born in 1986.

The reduced form of the wage equation takes the form:

$$\ln W_i = \beta_0 + \beta_1 REF_i + g (X_i - 1986) + \beta_2 SVY + v_i$$
(2)

where: the dependent variable $\ln W_i$ is the logarithm of hourly earnings, *REF_i* represents the fact of being covered by the education reform, g(.) is a quartic polynomial function, X_i - the respondent's year of birth, SVY - the survey year, and v_i is a random error. In equation (2), the key parameter is β_1 , which shows the average treatment effect (ATT) of the increase in compulsory school-leaving age on the hourly earnings of individuals born in 1986.

Then, in order to identify the effect of the increase in compulsory school-leaving age on hourly earnings, the following equation was estimated using 2SLS:

$$\ln W_i = \gamma_0 + \gamma_1 SCH_i + g (X_i - 1986) + \gamma_2 SVY + \mu_i$$
(3)

where the variable which represents the fact of being covered by the education reform (*REF_i*) was used to instrument the years of formal schooling (*SCH_i*). In equation (3), the key parameter is γ_1 , which can be interpreted as LATE, i.e. it represents the wage premium from an additional year of comprehensive education obtained by individuals born in 1986 who studied a year longer because of the reform. Such an interpretation requires the monotonicity assumption to be met (Imbens and Angrist 1994), which means that the increase of compulsory school-leaving age should prompt some of the individuals covered by the reform to study longer, but at the same time it should not induce anyone to shorten

their schooling. It seems that in the case of individuals deciding to study in basic vocational schools this condition is met.

Since the year of birth is a discrete variable, it needs to be taken into account when estimating standard errors, that observations within individual cohorts are not independent of each other. Therefore, as suggested by Lee and Card (2008), robust standard errors were obtained by clustering at the cohort level.

Finally, we wanted to make sure that the 1999 reform had no impact on the fraction of people holding a basic vocational school certificate. Otherwise, one could argue that the identified wage effect may be biased due to the impact of the reform on the decisions of young people whether to study or not to study at basic vocational schools. Had the reform affected in this way unobservable characteristics of individuals holding a basic vocational school certificate, e.g. by encouraging the more able individuals to choose secondary vocational schools instead of basic vocational ones, our results would actually suffer from a sample selection bias. To address this issue formally, we used the RDD and estimated the following equation:

$$BASIC_i = \delta_0 + \delta_1 REF_i + g \left(X_i - 1986 \right) + \delta_2 SVY + \varepsilon_i$$
(4)

where: dependent variable $BASIC_i$ is a dummy variable equal to 1 for holders of a basic vocational school certificate, and 0 otherwise. The other independent variables are the same as in equation (1). We argue that insignificant δ_1 can be regarded as evidence of no selection.

4. Data

The analysis of the 1999 education reform impact is based on individual data from the Polish Labor Force Survey (LFS) for years 2001-2016, carried out by the Central Statistical Office (GUS).

The sample consists of graduates only, who are defined as individuals who achieved their highest level of education at least 12 months prior to the survey and were not attending any school at the moment of the survey. In addition, the sample is limited to those who have completed basic vocational school at the most. Therefore, the sample includes also those who were continuing their education in a three-year secondary vocational school for graduates of basic vocational schools provided that they have dropped out of the secondary vocational school and thus their highest education level completed is the basic vocational one. The years of schooling were computed by subtracting seven years, that is the age at which children are obliged to start primary education in Poland, from the age at which the respondent completed his/her last school. Thus, the years of schooling may be underestimated for individuals who dropped out of the three-year secondary vocational school. The sample is limited to individuals born between 1978 and 1993 who completed formal education at the age of 16-25. Besides, the sample is limited to the employed who reported the amount of earnings and working hours they had on the month prior to the survey.¹ Therefore, the analysis of wage premium does not cover the self-employed, as they are not asked to report the amount of income in the LFS. To avoid using several observations for the same individual, we further restrict the sample to respondents who were interviewed in the first of the four survey waves. The descriptive statistics of the sample are shown in Table 1.

[Table 1 about here]

5. Results

First, we estimated equation (4) to make sure that the 1999 reform had no impact on the fraction of individuals holding a basic vocational school certificate. Figure A1 in the appendix shows a downward trend of the fraction, both below cutoff cohort (1986) and 2-3 years above it, and an upward trend in subsequent cohorts. There seems to be no discontinuity of the fraction at the cutoff, neither for men, nor for women. This visual impression is confirmed by the estimation results provided in Table A1 in appendix. We estimated four specifications of the model, using different forms of local polynomial function of cohort. The basic specification, using quartic local polynomial, shows no impact of the reform on the fraction, which holds for men and women. For men the result is the same regardless of the degree of the polynomial function of cohort. For women we find only weak evidence of negative impact of the reform on the fraction, when we use linear polynomial. However, when we look at Figure A1 it seems that using the quartic or square polynomial function is superior. So we argue that our results regarding the wage premium from compulsory schooling should not be subject to a selection bias.

As a starting point of the analysis of the impact of the education reform on earnings, we estimated the wage equation using OLS on a full sample of the basic vocational school certificate holders. The hourly earnings were regressed on the years of schooling, the quartic polynomial of age and the dummy variables for each survey year. The obtained naive estimator of the wage premium from a year of schooling was used as a benchmark for later estimations of the causal effect of the reform. The results for men and women separately are presented in column 1 of Table 2. They show that hourly earnings are not correlated to the years of schooling, both for men and women.

¹ The earnings were corrected for inflation using CPI.

[Table 2 about here]

Column 2 in Table 2 presents the results of estimating the first-stage equation. They show that the 1999 education reform, which added one year to the education path leading to the basic vocational school certificate, had in fact a positive impact on the years of schooling of those completing this path. Men covered by the reform studied on average 0.26 years more to get the certificate, while women 0.30 years more.

The positive impact of the reform on years of schooling is also evidenced on Figure 3. The solid lines show the fitted values from a local quadratic polynomial on each side of the cut-off. The visual inspection suggests that the average basic vocational school leaving age exhibits a substantial jump at the cut-off point.

[Figure 3 about here]

Figure 4 plots the evolution of the log of hourly earnings across school cohorts, separately for men and women. It shows that there is a positive jump in the hourly earnings of males at the cut-off. In case of females the picture is not so clear - there seems to be an upward jump but one year after the cut-off, not right after the cut-off as expected, so we cannot argue that 1999 reform had an impact on earnings of women. The econometric analysis confirms this visual impression. The results of the reduced-form equation, presented in column 3 of Table 2, show a positive impact of the reform on the hourly earnings of men only. Those covered by the reform earn 3.9% more than those who studied in the old system.

[Figure 4 about here]

One could argue that a jump in hourly earnings may result from a potential impact of the reform on employment rate. To address this potential criticism we estimated the reduced form (equation 2) with employment rate as the dependent variable. The results presented in column 4 in Table 2 show that the reform had no impact on employment rate of men and women. The same conclusion can be drawn from visual inspection of Figure 5, which shows the evolution of employment rate across school cohorts, separately for men and women. Clearly, there is no discontinuity in employment rates at the 1986 school cohort.

Finally, column 4 in Table 2 presents the 2SLS estimates, showing a positive impact of an additional year of schooling on the hourly earnings of men, amounting to 14.9 logarithm points, and no impact on earnings of women. This result can be interpreted as LATE, that is as an increase in hourly wages obtained by individuals who extended their formal education by one year as a result of the reform. The robustness of the results obtained was checked in several ways. Firstly, their sensitivity to the functional form of the model was tested. In the related literature, the RDD method is usually used with a global quartic polynomial (Oreopoulos 2006, Devereux and Hart 2010, Grenet 2013, Fuwa and Korwatanasakul 2015), and we used the same functional form for the baseline specification. However, Gelman and Imbens (2016) claim that high-order polynomials should not be used in the RDD method and they recommend the use of linear local functions and quadratic polynomials. Therefore, we estimated the model using global and local functions, both linear and polynomial of the second and third order. For almost all the functional forms tested, the results did not change qualitatively (see Table A2 in the appendix). In most specifications the wage premium is between 12 and 15%. The premium is much higher only when we use local polynomial of the third and fourth degree. But based on the suggestion of Gelman and Imbens (2016), we argue that the wage premium from an additional year of schooling amounts to 12-15% for men.

Secondly, the analysis was performed for two other bandwidths of the forcing variable, that is for a broader bandwidth than the baseline, covering 1976-1995 cohorts, and for a narrower one, covering 1980-1991 cohorts. In both cases, the results are not qualitatively different from those obtained for the baseline bandwidth, i.e. 1978-1993 cohorts (see Table A3 in the appendix).

Thirdly, the so-called placebo tests were done, that is we tested for the discontinuity of changes in the years of schooling at other cohorts than 1986. As suggested by Imbens and Lemieux (2007), the occurrence of discontinuity was tested on both sides of the cutoff. We tested for discontinuity at 1981 and 1991 cohort. Eight functional forms of polynomials were tested. The results show that in most cases there is no discontinuity in wages at both cutoffs tested (see Tables A4 and A5 in the appendix). These results are also confirmed by visual inspection of Figures A3 and A5 in the appendix. So we conclude that our results are rather robust.

6. Conclusions

The aim of our study was to determine, whether the 1999 education reform, which added one year to the shortest available education path, had any impact on the hourly earnings of those completing this path. Using the RDD method and the Polish LFS data, we find that an additional year of comprehensive education has resulted in a 12-15% increase in the hourly earnings of men holding basic vocational school certificates, but at the same time it had no impact on earnings of women. The effect for men is higher than those reported in similar studies in other countries, where the wage premium from an additional year of formal education is typically found to remain under 10% percent. The higher wage premium identified in our analysis may be due to the fact that our study does not refer to all the young people covered by the reform, but it focuses on the holders of basic vocational school certificates only, i.e. those who, in order to complete formal education as soon as possible, had to acquire one year more of general education as a result of the reform. It seems that for the individuals choosing this educational path, which provides the least amount of general skills when compared to other paths, an additional year of general education is particularly valuable.

What is common for this study and the studies conducted for Western European countries, is the higher value of the wage premium estimated using 2SLS than with OLS. Such a result is usually interpreted as evidence of the compensatory role of increased compulsory schooling, as it shows benefits from compulsory schooling obtained by those who would not extend their years of schooling in the absence of such an obligation.

The results obtained are also consistent with those reported by Jakubowski *et al.* (2010) on the educational effects of the 1999 reform. The authors argue that the spectacular improvement of the PISA test scores of Polish students between 2000 and 2006 is primarily due to the improvement of performance of the least able students. In the light of these findings, the reform seems to be of a compensatory nature, as it leads to a reduction in the stratification of educational outcomes and, as a consequence, a decrease in wage differentials.

Obviously, it is puzzling why, with such a high wage premium from an additional year of schooling, basic vocational students are willing to finish formal education as soon as possible. There are a few potential explanations. Firstly, they may have no information on the magnitude of potential economic benefits from education. Secondly, obtaining these benefits in the future is not certain. Thirdly, for individuals with relatively low abilities, formal education may involve high mental costs.

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Annex of Figures and Tables

| Age | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | |
|--------|---|---------|---|----|----|----|---------------------|-----------------------------------|--------------|----|---------|----|-----------|-----------|--|
| Before | | Primary | | | | | | General sec Hig Vocational sec | | | | | Hig ec | her du | |
| 1999 | | | | | | | В | Basic voc | | | | | | | |
| After | | | | | | | | | General sec | | | | Hig | her | |
| 1000 | | Primary | | | | | | Lower | Vocational s | | onal se | ec | ec | du | |
| 1999 | | | | | | | secondary Basic voc | | | | | | | | |

Figure 1. Changes to the Polish education system over time

Source: adapted from Jakubowski (2015).

Figure 2. Net enrolment ratio in post-primary and post-gymnasium schools in Poland in 1990-2015



Source: author's analyses based on data from GUS (1992,...,2015).

| Table 1. | Descriptive | statistics | of the | sample |
|----------|-------------|------------|--------|--------|
| | | | | |

| Variable | Number of observations | Mean | Standard deviation | Min | Max | | | |
|--|--|--|--------------------|------|-------|--|--|--|
| | School certificate holders, total | | | | | | | |
| Hourly earnings | | | | | | | | |
| (PLN from 1995) (<i>W_i</i>) | 7193 | 13,56 | 6,36 | 0,63 | 83,68 | | | |
| Employment rate (<i>E_i</i>) | 28861 | 0,638 | 0,480 | 0 | 1 | | | |
| Covered by the education | | | | | | | | |
| reform (<i>REF_i</i>) | 28861 | 0,221 | 0,415 | 0 | 1 | | | |
| Year of birth (X _i) | 28861 | 1983 | 4 | 1978 | 1993 | | | |
| Years of schooling (SCH _i) | 28861 | 11,23 | 0,76 | 9 | 18 | | | |
| Age | 28861 | 27,28 | 4,66 | 18 | 38 | | | |
| Female | 28861 | 0,359 | 0,480 | 0 | 1 | | | |
| Survey year | 28861 | 2010 | 4 | 2001 | 2016 | | | |
| | School c | School certificate holders covered by the reform | | | | | | |
| Hourly earnings | | | | | | | | |
| (PLN from 1995) (<i>W_i</i>) | 5791 | 13,25 | 6,46 | 0,63 | 83,68 | | | |
| Employment rate (<i>E_i</i>) | 22475 | 0,642 | 0,479 | 0 | 1 | | | |
| Covered by the education | | | | | | | | |
| reform (<i>REF</i> _i) | 22475 | 0 | 0 | 0 | 0 | | | |
| Year of birth (X_i) | 22475 | 1981 | 2 | 1978 | 1985 | | | |
| Years of schooling (SCH _i) | 22475 | 11,14 | 0,71 | 9 | 18 | | | |
| Age | 22475 | 28,25 | 4,68 | 18 | 38 | | | |
| Female | 22475 | 0,372 | 0,483 | 0 | 1 | | | |
| Survey year | 22475 | 2009 | 4 | 2001 | 2016 | | | |
| | School certificate holders not covered by the reform | | | | | | | |
| Hourly earnings | | | | | | | | |
| (PLN from 1995) (<i>W_i</i>) | 1402 | 14,83 | 5,75 | 4,81 | 64,85 | | | |
| Employment rate (<i>E_i</i>) | 6386 | 0,627 | 0,484 | 0 | 1 | | | |
| Covered by the education | | | | | | | | |
| reform (<i>REF</i> _i) | 6386 | 1 | 0 | 1 | 1 | | | |
| Year of birth (X_i) | 6386 | 1989 | 2 | 1986 | 1993 | | | |
| Years of schooling (SCH _i) | 6386 | 11,55 | 0,85 | 9 | 18 | | | |
| Age | 6386 | 23,88 | 2,51 | 18 | 30 | | | |
| Female | 6386 | 0,312 | 0,463 | 0 | 1 | | | |
| Survey year | 6386 | 2013 | 2 | 2004 | 2016 | | | |

Table 2. The estimation of the 1999 education reform impact on the years of schooling and the hourly earnings of graduates with basic vocational education

| | | OLS | First stage | Reduce | 2SLS | |
|--------|--------------|----------|-------------|----------|------------|----------|
| Gender | | (1) | (2) | (3) | (4) | (5) |
| | | Hourly | Years of | Hourly | Employment | Hourly |
| | | earnings | schooling | earnings | rate | earnings |
| Male | Coefficient | -0.008 | 0,261*** | 0,039** | -0,003 | 0,149** |
| | (std. error) | (0.008) | (0,047) | (0,011) | (0,019) | (0,052) |
| | N | 5234 | 5234 | 5234 | 18511 | 5234 |
| Female | Coefficient | 0.006 | 0,300*** | 0,019 | -0,003 | 0,062 |
| | (std. error) | (0.005) | (0,061) | (0,044) | (0,013) | (0,142) |
| | N | 1959 | 1959 | 1959 | 10350 | 1959 |

Notes: The age and age square were additionally included in specification 1. Specifications 2-4 include the global quartic polynomial in school cohort, allowing for an intercept shift at the cut-off point (1986 school cohort). *** / ** / ** denote 0.1%, 1% and 5% significance level, respectively.



Figure 3. The impact of the 1999 reform on the average years of schooling, calculated separately for males and females (school cohorts 1978-1993)

Note: The dots show the average years of schooling grouped at the school cohort cell for female and male wage earners who were born between 1978 and 1993. The solid lines represent the fitted values from a local quadratic polynomial regression, allowing for an intercept shift at the 1986 school cohort.



Figure 4. The impact of the 1999 reform on the log of hourly earnings, calculated separately for males and females (school cohorts 1978-1993)

Note: The dots show the average log of hourly earnings grouped at the school cohort cell for female and male wage earners who were born between 1978 and 1993. The solid lines represent the fitted values from a local quadratic polynomial regression, allowing for an intercept shift at the 1986 school cohort. *Source: author's own analysis based on unit data from the Polish LFS for 2001-2016.*





Note: The dots show the average employment rate grouped at the school cohort cell for female and male wage earners who were born between 1978 and 1993. The solid lines represent the fitted values from a local quadratic polynomial regression, allowing for an intercept shift at the 1986 school cohort. *Source: author's own analysis based on unit data from the Polish LFS for 2001-2016.*

Appendix

Table A1.

The estimation of the impact of the 1999 reform on the fraction of individuals holding a basic vocational school certificate

| Birth cohort | | Male | Female |
|---------------|--------------|---------|----------|
| controls | | (1) | (2) |
| Local quartic | Coefficient | 0,012 | 0,022 |
| • | (std. error) | (0,018) | (0,022) |
| Local cubic | Coefficient | -0,019 | -0,020 |
| | (std. error) | (0,010) | (0,012) |
| Local square | Coefficient | 0,009 | -0,011 |
| | (std. error) | (0,015) | (0,009) |
| Local linear | Coefficient | -0,021 | -0,025** |
| | (std. error) | (0,014) | (0,007) |
| | N | 62831 | 55890 |

Note: *** / ** / * denote 0,1%, 1% and 5% significance level, respectively.

Figure A1. The impact of the 1999 reform on the fraction of individuals holding a basic vocational school certificate (school cohorts 1978-1993)



Note: The dots show the fraction of individuals holding a basic vocational school certificate grouped at the school cohort cell for female and male wage earners who were born between 1978 and 1993. The solid lines represent the fitted values from a local quadratic polynomial regression, allowing for an intercept shift at the 1986 school cohort.

Table A2.

The estimation of impact of the 1999 reform on the years of schooling, the hourly earnings of individuals holding a basic vocational school certificate (school cohorts 1978-1993)

| | | | Male | | Female | | | |
|----------------|--------------|-------------|----------|----------|-------------|----------|----------|--|
| | | First stage | Reduced | 2515 | First stage | Reduced | 2010 | |
| School cohort | | FIIST STARE | form | 2313 | FIRST STABE | form | 25L5 | |
| controls | | (1) | (2) | (3) | (4) | (5) | (6) | |
| | | Years of | Hourly | Hourly | Years of | Hourly | Hourly | |
| | | schooling | earnings | earnings | schooling | earnings | earnings | |
| Global quartic | Coefficient | 0,261*** | 0,039** | 0,149** | 0,300*** | 0,019 | 0,062 | |
| • | (std. error) | (0,047) | (0,011) | (0,052) | (0,061) | (0,044) | (0,142) | |
| Global cubic | Coefficient | 0,280*** | 0,033* | 0,119** | 0,270** | 0,034 | 0,128 | |
| | (std. error) | (0,034) | (0,012) | (0,044) | (0,078) | (0,040) | (0,155) | |
| Global square | Coefficient | 0,370*** | 0,052*** | 0,140*** | 0,266*** | 0,033 | 0,124 | |
| • | (std. error) | (0,042) | (0,012) | (0,030) | (0,067) | (0,037) | (0,142) | |
| Global linear | Coefficient | 0,378*** | 0,048** | 0,126*** | 0,239** | 0,038 | 0,158 | |
| | (std. error) | (0,044) | (0,012) | (0,032) | (0,075) | (0,029) | (0,126) | |
| Local guartic | Coefficient | 0.324*** | 0.135*** | 0.416*** | 0.797*** | -0.020 | -0.025 | |
| | (std. error) | (0.037) | (0.033) | (0.061) | (0.148) | (0.038) | (0.047) | |
| Local cubic | Coefficient | 0,179*** | 0,057** | 0,319*** | 0,259 | -0,029 | -0,111 | |
| | (std. error) | (0,025) | (0,015) | (0,055) | (0,145) | (0,025) | (0,135) | |
| Local square | Coefficient | 0,240*** | 0,029 | 0,120 | 0,288** | 0,021 | 0,074 | |
| | (std. error) | (0,032) | (0,014) | (0,062) | (0,083) | (0,032) | (0,107) | |
| Local linear | Coefficient | 0,378*** | 0,051*** | 0,136*** | 0,264*** | 0,032 | 0,121 | |
| | (std. error) | (0,039) | (0,012) | (0,029) | (0,064) | (0,034) | (0,126) | |
| | N | | 5234 | | | 1959 | | |

Note: *** / ** / * denote 0.1%, 1% and 5% significance level, respectively.

Table A3.

The estimation of the impact of the 1999 reform on the years of schooling and hourly earnings of individuals holding a basic vocational school certificate

| | | | Male | | Female | | | |
|---------------|--------------|-------------|----------|----------|-------------|----------|----------|--|
| | | First stage | Reduced | 2616 | First stage | Reduced | 2515 | |
| School cohort | | riist stage | form | 2313 | Filst stage | form | 2363 | |
| range | | (1) | (2) | (3) | (4) | (5) | (6) | |
| | | Years of | Hourly | Hourly | Years of | Hourly | Hourly | |
| | | schooling | earnings | earnings | schooling | earnings | earnings | |
| 1978-1993 | Coefficient | 0,261*** | 0,039** | 0,149** | 0,300*** | 0,019 | 0,062 | |
| | (std. error) | (0,047) | (0,011) | (0,052) | (0,061) | (0,044) | (0,142) | |
| | Ν | | 5234 | | 1959 | | | |
| 1976-1995 | Coefficient | 0,311*** | 0,042** | 0,136*** | 0,276*** | 0,036 | 0,131 | |
| | (std. error) | (0,048) | (0,011) | (0,035) | (0,074) | (0,046) | (0,172) | |
| | Ν | | 6847 | | | 2694 | | |
| 1980-1991 | Coefficient | 0,218*** | 0,040* | 0,186* | 0,332** | 0,001 | 0,003 | |
| | (std. error) | (0,045) | (0,014) | (0,077) | (0,093) | (0,046) | (0,133) | |
| | N | | 3685 | | | 1332 | | |

Notes: Each specification includes also controls for global quartic polynomial in school cohort, allowing for an intercept shift at the cut-off point (1986 school cohort). *** / ** / * denote 0.1%, 1% and 5% significance level, respectively.

Table A4.

The results of placebo tests (school cohorts 1976-1985; cutoff cohort = 1981)

| | | | Male | | Female | | | |
|----------------|--------------|-------------|-----------------|----------|-------------|-----------------|----------|--|
| School cohort | | First stage | Reduced form | 2SLS | First stage | Reduced form | 2SLS | |
| controls | | (1) | (2) | (3) | (4) | (5) | (6) | |
| | | Years of | Hourly | Hourly | Years of | Hourly | Hourly | |
| | | schooling | earnings | earnings | schooling | earnings | earnings | |
| Global quartic | Coefficient | -0.056** | -0.014 | 0.251 | -0.194* | -0.012 | 0.064 | |
| • | (std. error) | (0.015) | (0.007) | (0.143) | (0.062) | (0.015) | (0.060) | |
| Global cubic | Coefficient | -0.056** | -0.004 | 0.081 | -0.156* | 0.100 | -0.011 | |
| | (std. error) | (0.016) | (0.021) | (0.355) | (0.061) | (0.117) | (0.070) | |
| Global square | Coefficient | -0.076*** | -0.010 | 0.140 | -0.115* | -0.008 | 0.073 | |
| | (std. error) | (0.014) | (0.013) | (0.159) | (0.040) | (0.014) | (0.119) | |
| Global linear | Coefficient | -0.058 | -0.007 | 0.120 | -0.093 | -0.009 | 0.103 | |
| | (std. error) | (0.039) | (0.017) | (0.224) | (0.050) | (0.013) | (0.154) | |
| Local quartic | Coefficient | 0,035* | 0.139*** | 4.008* | 0.530*** | -0.018 | -0,033 | |
| • | (std. error) | (0.015) | (0.010) | (1.758) | (0.052) | (0.015) | (0.024) | |
| Local cubic | Coefficient | 0,037*** | -0.058 | -1.561* | -0.553** | -0.011 | 0.019 | |
| | (std. error) | (0.007) | (0.028) | (0.779) | (0.041) | (0.007) | (0.016) | |
| Local square | Coefficient | -0.039* | 0.012 | -0.319 | -0.070 | -0.024* | 0.155 | |
| • | (std. error) | (0.014) | (0.016) | (0.331) | (0.057) | (0.008) | (0.104) | |
| Local linear | Coefficient | -0.058*** | -0.007 | 0.121 | -0.096* | -0.009 | 0.097 | |
| | (std. error) | (0.012) | (0.009) | (0.158) | (0.042) | (0.012) | (0.110) | |
| | N | | 5740 | | | 2342 | | |

Notes: *** / ** / * denote 0.1%, 1% and 5% significance level, respectively.



Figure A2. The results of placebo tests - impact on years of schooling (school cohorts 1976-1985; cutoff cohort = 1981)

Source: author's own analysis based on unit data from the Polish LFS for 2001-2016.

Figure A3. The results of placebo tests - impact on hourly earnings (school cohorts 1976-1985; cutoff cohort = 1981)



Source: author's own analysis based on unit data from the Polish LFS for 2001-2016.

Table A5.

The results of placebo tests (school cohorts 1986-1995; cut-off cohort = 1991)

| | | | Male | | Female | | | |
|----------------|--------------|-------------|-----------------|----------|-------------|-----------------|----------|--|
| Birth cohort | | First stage | Reduced form | 2SLS | First stage | Reduced form | 2SLS | |
| controls | | (1) | (2) | (3) | (4) | (5) | (6) | |
| | | Years of | Hourly | Hourly | Years of | Hourly | Hourly | |
| | | schooling | earnings | earnings | schooling | earnings | earnings | |
| Global quartic | Coefficient | -0.226* | -0.052 | 0.232 | 0.148 | 0.025 | 0.173 | |
| • | (std. error) | (0.096) | (0.031) | (0.083) | (0.098) | (0.027) | (0.110) | |
| Global cubic | Coefficient | -0.286 | -0.044 | 0.156 | -0.004 | 0.017 | -3.822 | |
| | (std. error) | (0.137) | (0.028) | (0.081) | (0.137) | (0.032) | (126.5) | |
| Global square | Coefficient | -0.261 | -0.051 | 0.195* | -0.035 | -0.059 | 1.693 | |
| | (std. error) | (0.122) | (0.025) | (0.091) | (0.143) | (0.040) | (5.581) | |
| Global linear | Coefficient | -0.199* | -0.037 | 0.189 | 0.105 | -0.056 | -0.534 | |
| | (std. error) | (0.079) | (0.026) | (0.123) | (0.075) | (0.039) | (0.522) | |
| Local quartic | Coefficient | 1.285*** | 0.485*** | 0.377*** | 1.701*** | 0.287* | 0.169* | |
| • | (std. error) | (0.033) | (0.033) | (0.032) | (0.207) | (0.123) | (0.075) | |
| Local cubic | Coefficient | -0.405 | -0.025 | 0.061 | 0.131 | -0.037 | -0.289 | |
| | (std. error) | (0.228) | (0.075) | (0.141) | (0.220) | (0.065) | (0.843) | |
| Local square | Coefficient | -0.353** | -0.036 | 0.103 | -0.049 | 0.052 | -1.063 | |
| · | (std. error) | (0.085) | (0.032) | (0.065) | (0.102) | (0.034) | (2.219) | |
| Local linear | Coefficient | -0.210 | -0.042 | -0.199 | 0.074 | -0.058 | -0.788 | |
| | (std. error) | (0.100) | (0.025) | (0.103) | (0.111) | (0.041) | (1.431) | |
| | N | | 987 | | | 309 | | |

Notes: *** / ** / * denote 0.1%, 1% and 5% significance level, respectively.





Source: author's own analysis based on unit data from the Polish LFS for 2001-2016.

Figure A5. The results of placebo tests - impact on hourly earnings (school cohorts 1986-1995; cutoff cohort = 1991)



Source: author's own analysis based on unit data from the Polish LFS for 2001-2016.