

The Impact of Compulsory Schooling on Hourly Wage: Evidence From the 1999 Education Reform in Poland

Evaluation Review
2020, Vol. 44(5-6) 437-470
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DOI: 10.1177/0193841X20987104
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Abstract

Background: In 1999, an education reform was implemented in Poland, which added 1 year to the shortest available educational path—that of basic vocational education. Under the new system, students choosing this path receive one additional year of general education. According to the authors of the reform, this should improve the students' position in the labor market, as a shortage of general skills was identified as the main deficit of basic vocational school leavers prior to the reform. **Purpose:** The study assesses the impact of the 1999 reform on the hourly wage of individuals who completed formal education after obtaining a basic vocational school certificate. **Methods:** The analysis is based on individual data from the Polish Labor Force Survey for the years 2001–2016. As the education reform was implemented simultaneously across the country, a regression discontinuity design is used to assess its impact. **Results and Conclusions:** The study

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finds that one additional year of general education has led to a 13% increase in the hourly wages of individuals who completed basic vocational schools. This effect is higher than those reported in studies for other countries where the wage premium is typically found to be under 10%. This may be because the extension of compulsory schooling in Poland affected individuals with relatively low general skills and abilities.

Keywords

education, schooling, wages, regression discontinuity design

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 of about 10% from an additional year of education (Oreopoulos, 2008, for the United Kingdom); however, there are also studies showing a zero premium (Devereux & Hart, 2010, for the United Kingdom; Grenet, 2013, for France). Thus far, the empirical evidence has largely originated from Western European countries, the United States, and Asia. The RDD method has yet to be used with respect to the education reforms implemented in Central and Eastern European countries.

The education reform instituted in Poland in 1999 appears to be a good example of an education reform from a Central or Eastern European country that 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 new school type—lower secondary school, which is referred to as “gymnasium”—to the education system in Poland was the most publicly visible; consequently, the gymnasium has become a symbol of the 1999 education reform. For pupils wishing to pursue secondary education, the total number of years of schooling did not change. However, total schooling was extended from 11 to 12 years for the majority of basic vocational education students. Under the new education system, those who choose basic vocational education remain in comprehensive school for one additional year, 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, under the old system, those leaving basic vocational schools lacked general skills. This was considered to be the key obstacle in their career development.

I find that the additional year of general education resulting from the 1999 education reform has led to a 13% increase in hourly wages for individuals with basic vocational education. This 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.

This article is structured as follows. In the first section, I briefly review the empirical literature on the wage premium from compulsory education, with a focus on the methodologies and findings. The second section describes the education reform introduced in Poland in 1999. In the third and fourth sections, I present the method and data used in my analysis. Finally, in the last two sections, I present the results and the most important conclusions.

Review of Literature

Research on the wage effects of extended compulsory education began over 20 years ago, and the methods used to identify the causal effect have evolved since then. Initially, the instrumental variable (IV) method was used, following Harmon and Walker (1995).¹ The method can be thought of as estimating the Mincer's (1974) wage equation in which the years of schooling are instrumented by the compulsory school age. The average effect of extending 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. This effect applies only to those who, as a result of the reform, learn longer than they would have in the absence of the reform, and is referred to as the LATE (Imbens & 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 reasons other than the education reform. The positive wage premium may be due to the fact that regardless of the reform, the years of schooling are increasing in successive cohorts of young people and so are the wages. Thus, two other approaches are used that eliminate this problem.

The first approach is the difference-in-differences (DID) method, which can be used when an education reform is not implemented simultaneously across the country, but rather, is implemented gradually, at different times in different administrative units (Fischer et al., 2016; Meghir & Palme,

2005; Oosterbeek & Webbink, 2007; Pekkarinen et al., 2009; Pischke & von Wachter, 2008). Pupils from units where the reform was implemented first are the treatment group, while those from units where the reform was implemented later are the control group. The average causal effect of the reform is estimated as the difference between the wage growth rates in the treatment and control groups.

The second approach is the RDD (Chib & Jacobi, 2016; Devereux & Hart, 2010; Fuwa & Korwatanasakul, 2017; Grenet, 2013; Oreopoulos, 2006; Ou, 2013), which is in fact a special case of the IV method. This involves regressing years of schooling in formal education and earnings on the birth cohort separately for those not affected by the reform (below the cutoff) and those affected by the reform (above the cutoff). The idea is to test for a discontinuity of the fitted line in years of schooling and earnings at the cutoff. LATE is estimated as the ratio of the earnings growth to the increase in 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%) of the treated (treatment-on-the-treated [TOT] estimate). In later studies using the same method, the positive effect of compulsory schooling on earnings was also found in other countries (Brunello & Miniaci, 1999, for Italy; Callan & Harmon, 1999, for Ireland; Levine & Plug, 1999, for the Netherlands; Pons & Gonzalo, 2002, for Spain; Vieira, 1999, for Portugal).

The studies using the DID method show that compulsory schooling has no overall effect on earnings. Meghir and Palme (2005), who analyze the effects of extending compulsory schooling in Sweden from 7 or 8 to 9 years in the 1949–1962 period, show that in general, the reform did not affect earnings and only led to a 3.4% increase in the earnings of individuals with unqualified fathers (intention-to-treat [ITT] estimate). Pischke and von Wachter (2008) report that the extension of compulsory schooling from 8 to 9 years, which was implemented gradually in Germany between 1949 and 1970, had no impact on earnings. Oosterbeek and Webbink (2007) show no wage effects from the mandatory extension of years of schooling from 3 to 4 years at secondary vocational schools in the Netherlands in 1975. Pekkarinen et al. (2009) analyze the effects of the education reform implemented in Finland between 1972 and 1975. They find that the extension of mandatory comprehensive education by 3 years had brought wage

benefits to individuals from families of low social status. In turn, Fischer et al. (2016) analyze the effects of two education reforms implemented in Sweden during 1930–1950. The first reform extended compulsory education from 6 to 7 years, while the second extended the school year from 34.5 or 36.5 to 39 weeks. These reforms were not implemented at the same time, which allowed for separating the effect of each of them. They find that both reforms brought a wage premium, but only to women—increasing compulsory schooling by 1 year increased their wages in the first period of their careers by 2% (ITT) and extending the school year led to a 4%–5% increase in wages (ITT).

The studies using the RDD find that the wage effects of compulsory schooling are small but positive. Three studies examine the wage benefits of raising the compulsory schooling age in the United Kingdom from 14 to 15 years, which is the same reform that Harmon and Walker (1995) analyze. Oreopoulos (2008) shows that this reform led to a 10% increase in earnings for the treated (TOT), while Devereux and Hart (2010) find that the increase in earnings of the treated (TOT) was much smaller—only 4%—and only for men. In turn, Chib and Jacobi (2016), using a fuzzy RDD, estimate the overall increase in earnings of the treated (TOT) at 5%–6%. In all three studies, the wage premium is smaller than that found by Harmon and Walker (1995). Grenet (2013) conducts 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 finds that the reform did not affect wages in France, while the hourly rate of the treated increased by 6%–7% (TOT) in England and Wales. The author attributes the positive impact of the reforms in England and Wales to the fact that the reform led to an increase in the proportion of young people completing formal education with a diploma confirming their competencies, which was not the case in France. Eble and Hu (2019) show that the extension of compulsory primary education by 1 year in China in 1980 led to an average increase in wages of 2% (ITT), with a slightly higher wage premium obtained by individuals from low-income families. Finally, Fuwa and Korwatanasakul (2017) report that the extension of compulsory primary schooling from 4 to 6 years in Thailand in 1978 resulted in an approximate 8% increase in the wages of the treated (TOT).

Thus, the empirical evidence is not consistent. Some studies show a substantial wage premium from extended years of schooling, while others show no overall effect, with only small effects for specific subgroups (women, low social and economic background). This may also be a result of the different natures of the education reforms under analysis in these

studies. In particular, impacts may vary with the year of schooling that is added (adding lower level grades may have a larger impact than adding higher level grades) and with the proportion of people who actually extended their schooling as a result of the reform.

The 1999 Education Reform in Poland

Poland's 1999 education reform had three main objectives: (1) to improve the quality of education, (2) to raise the level of education of society by increasing the percentage of young people pursuing secondary and tertiary education, and (3) to ensure equal educational opportunities, especially by improving the opportunities of young people in rural areas.

The reform was comprehensive in its nature, as it introduced changes in many areas including the structure of the education system, the curricula, the examination system, and the professional promotion and remuneration system of teachers. For my analysis, the first of these changes—the structural reform—is crucial; thus, I describe it in detail below. The other changes are described in Box 1 in the Online Appendix.

The structural 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 system (6 + 3 + 3; see Figure 1). Under the old system, children were obliged to attend an 8-year primary school from the age of 7 to the age of 15. After, they could continue their education at a secondary school (4 years of general secondary school or 5 years of vocational secondary school) or basic vocational school (3 years). Thus, children who pursued the secondary general track were required to study for 12 years while those in the basic vocational track for 11 years.

Under the new system, compulsory general education was extended from 8 to 9 years and was divided into two stages:

1. Six years of primary school (as primary education was shortened from 8 to 6 years) and
2. Three years of lower secondary school, called gymnasium (a new type of school in the Polish system).

After gymnasium, children may choose to attend one of the same three types of schools as in the old system: secondary general school, secondary vocational school, or basic vocational school. Now, the education cycle in secondary schools is 1 year shorter than in the old system (3 years in a

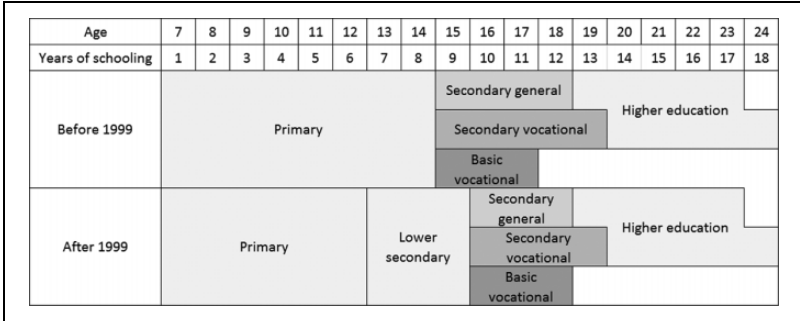


Figure 1. Changes to the Polish education system over time.
 Source: Adapted from Jakubowski (2015).

secondary general school and 4 years in a secondary vocational school). Thus, the total duration of the secondary general track remained at 12 years of required education—organized as 8 + 4 years before the reform and 6 + 3 + 3 years after the reform. Similarly, the total duration of the secondary vocational track remained at 13 years of required education; it was structured as 8 + 5 years before the reform and 6 + 3 + 4 years afterward.

The change in required years of education occurred in basic vocational education. The majority of basic vocational schools remained 3-year schools (as they were in the old system). Thus, the total required education in the basic vocational track was extended from 11 years before the reform (organized as 8 + 3 years) to 12 years after the reform (6 + 3 + 3 years). However, in case of some professions taught at basic vocational schools, the education cycle was shortened to 2 years.² Thus, the total duration of the basic vocational track in these professions remained at 11 years, which was structured as 8 + 3 years before the reform and 6 + 3 + 2 years afterward.

Consequently, a portion of students in the basic vocational track were affected by the reform because they had to study 1 year longer than before to obtain a basic vocational school certificate. Additionally, all students in this track were affected by the change in the share of general education in their schooling. They were required to study 1 year longer at comprehensive schools (primary school + gymnasium), which, according to the reformers, should improve their positions in the labor market because general competencies are viewed as enabling one to acquire new knowledge and skills throughout the professional career. Previously, the relatively low level of

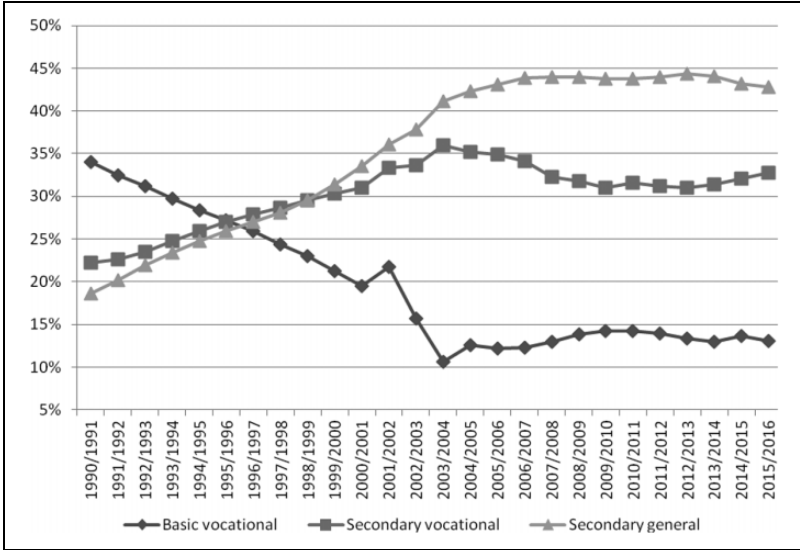


Figure 2. Net enrollment ratio in postprimary and postgymnasium schools in Poland in 1990–2015. *Note.* Net enrollment rate is the ratio of the number of pupils (at the beginning of the school year, in age group) at a given level of education to the population (as of December 31) in the age group identified as corresponding to this level of education. The net enrollment rates in the graph do not total to 100% because of participation in nonschool forms of education. *Source:* Author’s own analyses based on data from Central Statistical Office (1992, . . . , 2016).

the general skills of basic vocational school leavers was considered to be the main obstacle in the development of their professional careers.

One could expect that the change in the structure of education system would encourage some young people to attend secondary vocational schools instead of basic vocational ones. Previously, two additional years of schooling were required to obtain a secondary vocational certificate as opposed to a basic vocational certificate; however, the reform reduced the additional schooling required to just 1 year. 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 does not provide evidence of any impact of the reform on the 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), continued its downward trend during the implementation of the reform (1999–2004), and then stabilized at a level of 12%–14% in 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 also stabilized afterward.

The reform was launched on September 1, 1999, for the 1999/2000 school year. Thus, students who completed the sixth grade of primary school in the 1998/1999 school year began their first grade of gymnasium instead of going to the seventh grade of primary school. This group of students included those born from January 1, 1986, to December 31, 1986, as in Poland, children start their education in primary school on September 1 during the year of their seventh birthday. Thus, in general, the reform affected children born on or after January 1, 1986, while children born until December 31, 1985, continued their education under the old system. The first cohort affected by the reform entered secondary schools in 2002 and completed them in 2005 (secondary general schools and basic vocational schools) or 2006 (secondary vocational 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 and 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 wages 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, the wages of higher education institution graduates may have been impacted by both the 1999 reform and the 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 the wages of secondary school leavers may be impacted by both the 1999 reform and the 3 + 2 reform.

This is why I restrict my analysis of the impact of the 1999 reform on wages to the population of young people who completed formal education after obtaining a basic vocational school certificate. I argue that this group has never considered studying at higher education institutions, so their hourly wage is not affected by the 3 + 2 reform.

We are aware that as I restrict the analysis to basic vocational school leavers, my results might suffer from selection bias if the reform had an impact on the share of young people choosing to study at this type of school. I 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.

The 1999 education reform has not been evaluated thus far in terms of its impact on wages. However, Jakubowski et al. (2010) argue that the reform had a positive impact on the Programme for International Student Assessment (PISA) test scores of 15-year-old Poles between 2000 and 2006. In 2000, the PISA test was taken by pupils attending the first year of post-primary schools (secondary general, secondary vocational, 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 year of gymnasium. The math test scores were 470, 490, and 495 points in 2000, 2003, and 2006, respectively, and the reading test scores were 479, 490, and 508, respectively. While the reading test score obtained in 2000 was below the Organization for Economic Co-operation and Development (OECD) average, it reached the OECD average in 2003 and exceeded it in 2006, ranking Poland ninth in the world. Jakubowski et al. (2010) report that this improvement was mainly due to better scores obtained by students of basic vocational schools: After controlling for observables, their average reading test score increased by 28% between 2000 (the last but one cohort before the reform) and 2003 (the second cohort affected by the reform).³ The authors argue that this improvement may be a result of the additional year of general education introduced by the reform, which translated into more teaching hours of the Polish language and mathematics. Therefore, I expect to find a positive impact of the reform on the hourly wage of individuals who completed basic vocational schools.

Although in the above description of the reform, I focus on the fact that another year of general education was added to the basic vocational track, I am aware that the reform itself had several components aimed at a higher quality of education and that many of these reforms could have had a positive impact on general skills and, hence, on hourly wage. First, new curricula were created not only for gymnasiums but also for primary and basic vocational schools. Second, the reform established a system of uniform external examinations at the end of each educational stage (primary, lower secondary, and secondary school) and external vocational examinations at the end of basic vocational and secondary vocational schools. Third, teachers were motivated to improve their competences thanks to the new system of professional promotion. It seems that at least the first two of these changes could have had an impact on the general and vocational skills of the first cohort covered by the reform and, in that sense, could add to the wage effect of “another year of schooling.” In other words, as the 1999 education

reform was in fact a bundle of reforms implemented simultaneously, I am not able to identify the effect of extended schooling alone, but rather, I estimate the joint effect of the whole bundle. I argue, though, that the extended schooling was the most important of these reforms and, hence, that it contributes the most to the joint effect.

Data

To analyze the effects of the 1999 education reform, I use individual data from the Polish Labor Force Survey (LFS) for the years 2001–2016. The LFS is a representative sample survey of Polish residents that is conducted quarterly by the Central Statistical Office. Approximately 50,000 individuals aged 15 years or more are surveyed every quarter. They provide detailed information on their economic activity and on a large set of background characteristics. Importantly for this study, respondents report the year they completed their last school and their net earnings at their present job.

The Polish LFS is a rotating panel with a 2-(2)-2 rotation scheme. Each individual is surveyed four times: The first and second observations occur in two consecutive quarters; next, there is a two-quarter break; and then, the third and fourth observations occur in the next two consecutive quarters. Thus, the LFS provides cross-sectional data for each quarter, but when the data from several consecutive quarters are appended, there may be up to four observations from the same respondent in the pooled sample. Therefore, while using the pooled LFS data for the period 2001–2016, I restricted my sample to the first observation of each respondent.

Although I was interested in the impact of the reform on individuals who completed basic vocational school regardless of the highest level of education achieved, I had to restrict my analysis to those with basic vocational school as the highest level of education achieved. The reason is that my database contains information only on the last school completed on one's education track. So, two groups of individuals who studied at basic vocational schools are missing from my sample:

1. Those who dropped out of basic vocational schools⁴ and
2. Those who continued their education after completing basic vocational school and achieved a higher level of education (secondary or tertiary).⁵

Thus, my sample includes:

1. Those who finished their education immediately after completing basic vocational school and
2. Those who continued their education in a 3-year secondary vocational school (a special type of school for basic vocational school leavers) provided that they dropped out of the secondary vocational school and thus their highest education level completed is the basic vocational one.

I also restricted my analysis to 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 this way, I wanted to eliminate individuals still considering whether or not to continue their education, as this might affect their employment decisions and wages.

The sample was also limited to individuals born between 1978 and 1993—up to 8 years before and after the cutoff—who completed their formal education at the ages of 16–25. The years of schooling were computed by subtracting 7 years—the age when children are obliged to start primary education in Poland—from the age when the respondent achieved his present level of education.

The sample size, subject to the above restrictions, is 28,861 observations. As this sample includes both employed and nonemployed individuals, it was used for the analysis of the impact of the reform on employment only.

For the purpose of identifying the impact of the reform on hourly wage, I further restricted my sample to the employed individuals who reported the amount of earnings and working hours they had on the month prior to the survey.⁶ In other words, I dropped all individuals who were not employed or did not report their earnings or working hours. Importantly, I also dropped the self-employed and those helping family members, as they are not asked to report their income in the LFS. However, when I estimated a probit model of the determinants of revealing information on earnings, I found that the fact of revealing this information is not correlated with the 1999 education reform, neither for men nor for women.⁷ The sample size, subject to all the above restrictions, is 7,193 observations.

The descriptive statistics of the sample are shown in Table 1.

Method

We used the RDD to identify the effects of the 1999 education reform because I have a sharp cutoff in the school cohort (only individuals born

Table 1. Descriptive Statistics of the Sample.

| Variable | Number of Observations | Mean | SD | Minimum | Maximum |
|--|------------------------|-------|-------|---------|---------|
| A. Full sample | | | | | |
| All cohorts (1978–1993) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 7,193 | 13.56 | 6.36 | 0.63 | 83.68 |
| Employment rate (E_i) | 28,861 | 0.638 | 0.480 | 0 | 1 |
| Covered by the education reform (REF_i) | 28,861 | 0.221 | 0.415 | 0 | 1 |
| Year of birth (X_i) | 28,861 | 1983 | 4 | 1978 | 1993 |
| Years of schooling (SCH_i) | 28,861 | 11.23 | 0.76 | 9 | 18 |
| Age | 28,861 | 27.28 | 4.66 | 18 | 38 |
| Women | 28,861 | 0.359 | 0.480 | 0 | 1 |
| Survey year | 28,861 | 2010 | 4 | 2001 | 2016 |
| Cohorts not covered by the reform (1978–1985) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 5,791 | 13.25 | 6.46 | 0.63 | 83.68 |
| Employment rate (E_i) | 22,475 | 0.642 | 0.479 | 0 | 1 |
| Covered by the education reform (REF_i) | 22,475 | 0 | 0 | 0 | 0 |
| Year of birth (X_i) | 22,475 | 1981 | 2 | 1978 | 1985 |
| Years of schooling (SCH_i) | 22,475 | 11.14 | 0.71 | 9 | 18 |
| Age | 22,475 | 28.25 | 4.68 | 18 | 38 |
| Women | 22,475 | 0.372 | 0.483 | 0 | 1 |
| Survey year | 22,475 | 2009 | 4 | 2001 | 2016 |
| Cohorts covered by the reform (1986–1993) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 1,402 | 14.83 | 5.75 | 4.81 | 64.85 |
| Employment rate (E_i) | 6,386 | 0.627 | 0.484 | 0 | 1 |
| Covered by the education reform (REF_i) | 6,386 | 1 | 0 | 1 | 1 |
| Year of birth (X_i) | 6,386 | 1989 | 2 | 1986 | 1993 |
| Years of schooling (SCH_i) | 6,386 | 11.55 | 0.85 | 9 | 18 |
| Age | 6,386 | 23.88 | 2.51 | 18 | 30 |
| Women | 6,386 | 0.312 | 0.463 | 0 | 1 |
| Survey year | 6,386 | 2013 | 2 | 2004 | 2016 |

(continued)

Table 1. (continued)

| Variable | Number of Observations | Mean | SD | Minimum | Maximum |
|---|------------------------|-------|-------|---------|---------|
| B. Men | | | | | |
| All cohorts (1978–1993) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 5,234 | 14.47 | 6.81 | 0.63 | 83.68 |
| Employment rate (E_i) | 18,511 | 0.730 | 0.444 | 0 | 1 |
| Covered by the education reform (REF_i) | 18,511 | 0.237 | 0.426 | 0 | 1 |
| Year of birth (X_i) | 18,511 | 1983 | 4 | 1978 | 1993 |
| Years of schooling (SCH_i) | 18,511 | 11.3 | 0.8 | 9 | 18 |
| Age | 18,511 | 27.2 | 4.6 | 18 | 38 |
| Survey year | 18,511 | 2010 | 4 | 2001 | 2016 |
| Cohorts not covered by the reform (1978–1985) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 4,169 | 14.16 | 6.98 | 0.63 | 83.68 |
| Employment rate (E_i) | 14,115 | 0.737 | 0.441 | 0 | 1 |
| Covered by the education reform (REF_i) | 14,115 | 0.000 | 0.000 | 0 | 0 |
| Year of birth (X_i) | 14,115 | 1981 | 2 | 1978 | 1985 |
| Years of schooling (SCH_i) | 14,115 | 11.1 | 0.7 | 9 | 18 |
| Age | 14,115 | 28.2 | 4.7 | 18 | 38 |
| Survey year | 14,115 | 2009 | 4 | 2001 | 2016 |
| Cohorts covered by the reform (1986–1993) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 1,065 | 15.67 | 5.98 | 4.81 | 64.85 |
| Employment rate (E_i) | 4,396 | 0.711 | 0.454 | 0 | 1 |
| Covered by the education reform (REF_i) | 4,396 | 1.000 | 0.000 | 1 | 1 |
| Year of birth (X_i) | 4,396 | 1989 | 2 | 1986 | 1993 |
| Years of schooling (SCH_i) | 4,396 | 11.6 | 0.8 | 9 | 17 |
| Age | 4,396 | 23.9 | 2.5 | 18 | 30 |
| Survey year | 4,396 | 2013 | 2 | 2004 | 2016 |
| C. Women | | | | | |
| All cohorts (1978–1993) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 1,959 | 11.12 | 4.03 | 3.33 | 58.14 |
| Employment rate (E_i) | 10,350 | 0.474 | 0.499 | 0 | 1 |

(continued)

Table 1. (continued)

| Variable | Number of Observations | Mean | SD | Minimum | Maximum |
|---|------------------------|-------|-------|---------|---------|
| Covered by the education reform (REF_i) | 10,350 | 0.192 | 0.394 | 0 | 1 |
| Year of birth (X_i) | 10,350 | 1982 | 4 | 1978 | 1993 |
| Years of schooling (SCH_i) | 10,350 | 11.2 | 0.7 | 9 | 18 |
| Age | 10,350 | 27.5 | 4.7 | 18 | 38 |
| Survey year | 10,350 | 2010 | 4 | 2001 | 2016 |
| Cohorts not covered by the reform (1978–1985) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 1,622 | 10.90 | 4.02 | 3.33 | 58.14 |
| Employment rate (E_i) | 8,360 | 0.482 | 0.500 | 0 | 1 |
| Covered by the education reform (REF_i) | 8,360 | 0.000 | 0.000 | 0 | 0 |
| Year of birth (X_i) | 8,360 | 1981 | 2 | 1978 | 1985 |
| Years of schooling (SCH_i) | 8,360 | 11.1 | 0.7 | 9 | 18 |
| Age | 8,360 | 28.4 | 4.7 | 18 | 38 |
| Survey year | 8,360 | 2009 | 4 | 2001 | 2016 |
| Cohorts covered by the reform (1986–1993) | | | | | |
| Hourly wage (PLN from 1995; W_i) | 337 | 12.20 | 3.93 | 5.17 | 49.36 |
| Employment rate (E_i) | 1,990 | 0.441 | 0.497 | 0 | 1 |
| Covered by the education reform (REF_i) | 1,990 | 1.000 | 0.000 | 1 | 1 |
| Year of birth (X_i) | 1,990 | 1989 | 2 | 1986 | 1993 |
| Years of schooling (SCH_i) | 1,990 | 11.5 | 0.9 | 9 | 18 |
| Age | 1,990 | 23.9 | 2.6 | 18 | 30 |
| Survey year | 1,990 | 2013 | 2 | 2004 | 2016 |

Source: Author's own analysis based on unit data from the Polish Labor Force Survey for 2001–2016.

in 1986 or later were covered by the reform). Furthermore, as the reform was implemented simultaneously across the country, I was not able to employ the DID method.

Our analysis of the impact of the 1999 reform on hourly wage 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 up to December 31, 1985, were able to obtain this certificate after 11 years of schooling, whereas some of those born on January 1, 1986, or later were required to study for 12 years to

obtain it, as the reform extended comprehensive education by 1 year. Based on the theory of human capital, I expect that the additional year of education will lead to an increase in the general skills of students completing basic vocational schools and, consequently, an increase in their productivity and hourly wage.

Following other similar studies (Devereux & Hart, 2010; Fuwa & Korwatanasakul, 2017; Grenet, 2013; Oreopoulos, 2006), I employed the fuzzy RDD as the assignment to treatment group is not a deterministic but rather a stochastic function of the running variable. The reason is that for some students in the basic vocational track (54 professions), the reform extended required education from 11 to 12 years, while for other students in this track (22 professions), required education remained at 11 years.

A few previous studies analyzed the effects of similar reforms for men and women separately. While some of these studies show similar effects for men and women (Fuwa & Korwatanasakul, 2017; Grenet, 2013), the others find a positive wage effect only for men (Devereux & Hart, 2010) or women (Fischer et al., 2016). Additionally, I can expect a different effect of extended schooling on men's and women's employment rates because of the different traditional family roles of men and women.⁸ Therefore, I conduct my analysis not only for the full sample but also for men and women separately.

I employed the RDD to estimate the impact of the reform on years of schooling and hourly wage by means of a local polynomial approximation. This methodology involves using the whole sample and choosing a polynomial to fit the relationship between the outcome variable Y_i (number of years of education, hourly wage) and the forcing variable X_i (school cohort), allowing for an intercept and slope shift at the cutoff, that is, at the first cohort affected by the reform.⁹ Following the recommendation by Gelman and Imbens (2018), I use a quadratic polynomial instead of using high-order polynomials.

First, I wanted to find out whether the reform had a positive impact on years of schooling. Thus, I estimated the first-stage equation that takes the form:

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

where the 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 the value 0 for individuals born up to 1985 and 1 for individuals born in 1986 or later), $g(\cdot)$ is a local quadratic polynomial function, X_i —the respondent's year of birth, SVY —dummies for each survey year, and ϵ_i is a random error. In Equation 1, the key parameter is

α_1 , which is the regression discontinuity (RD) estimate of the average causal effect of the reform on years of schooling.

Second, to identify the average impact on the hourly wage of individuals covered by the reform, I estimated my main RD equation, which is also the reduced form of the wage equation, that takes the form:

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

where the dependent variable, $\ln W_i$, is the logarithm of hourly wage, REF_i represents the fact of being covered by the education reform, $g(\cdot)$ is a local quadratic polynomial function, X_i —the respondent's year of birth, SVY —dummies for each survey year, and v_i is a random error. In Equation 2, the key parameter is β_1 , which is the RD estimate of the average causal effect of the reform on the hourly wages of individuals covered by the reform (ITT estimate).

Then, in order to identify the impact on the hourly wage of compliers (those who studied 1 year more as a result of the reform), the following equation was estimated using the two-stage least squares (2SLS) method:

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

where the variable that represents the fact of being covered by the education reform (REF_i) was used to instrument the years of schooling (SCH_i). In Equation 3, the key parameter is γ_1 , which can be interpreted as LATE, that is, it represents the wage premium from the additional year of comprehensive education obtained by individuals born in 1986 who studied 1 year more because of the reform (TOT estimate). Such an interpretation requires the monotonicity assumption to be met (Imbens & Angrist, 1994), which means that the increase of the 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 at basic vocational schools, this condition is met.

Since the year of birth is a discrete variable shared by observations within individual cohorts, it needs to be taken into account when estimating standard errors. Therefore, as suggested by Lee and Card (2008), robust standard errors were obtained by clustering at the cohort level.

Finally, I wanted to make sure that the 1999 reform had no impact on the proportion 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 regarding whether or not to study at basic vocational schools. Had the reform affected

the unobservable characteristics of individuals holding a basic vocational school certificate in this way, for example, by encouraging the more able individuals to choose secondary vocational schools instead of basic vocational ones, my results might suffer from a sample selection bias. To address this issue formally, I used the RDD and estimated the following equation:

$$BASIC_i = \delta_0 + \delta_1 REF_i + g(X_i - 1986) + \delta_2 SVY + \epsilon_i, \quad (4)$$

where the 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. I argue that an insignificant δ_1 can be regarded as evidence of no selection.

Results

First, I estimated Equation 4 to make sure that the 1999 reform had no impact on the proportion of individuals holding a basic vocational school certificate. Figure 3 shows a downward trend for the proportion, both below the cutoff cohort (1986) and 2–3 years above it, and an upward trend in subsequent cohorts. The downward trend is the effect of the gradually declining net enrollment rate for basic vocational schools in the period 1990–2003, which was shown in Figure 2. To understand where the upward trend comes from, one needs to bear in mind that the analysis is based on data from the period 2001–2016. Thus, individuals born in 1978 were aged 23–38 years during the survey, while those born in 1993 were only aged 16–23 years. As one moves to the right in Figure 3, the cohorts are younger and younger and include fewer and fewer individuals who decided to study at a university because those who have not finished their education were dropped from the sample. Consequently, the proportion of individuals holding a basic vocational certificate increases. Importantly, there seems to be no discontinuity of the proportion at the cutoff birth cohort for the full sample (Panel A) and similarly for men and women (Panels B–C).

This visual impression is confirmed by the estimation results provided in Table 2. I estimated four specifications of the model using different forms of the local polynomial function of birth cohort. The basic specification, using a quadratic local polynomial, shows no impact of the reform on the proportion of basic vocational school certificate holders, and this also holds for men and women. For men, the results are the same regardless of the degree of the polynomial function of the cohort. For the full sample and for women, I find weak evidence of a negative impact of the reform on the proportion but only when I use a linear polynomial. However, when one

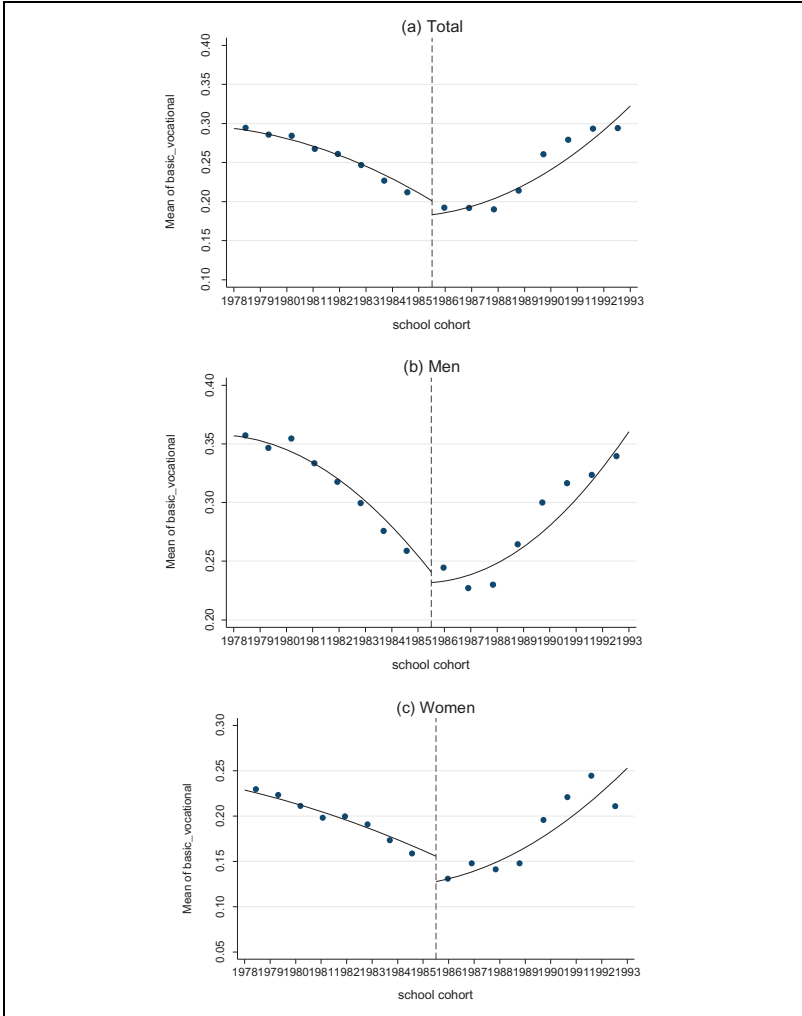


Figure 3. Impact of the 1999 reform on the proportion of individuals holding a basic vocational school certificate (school cohorts 1978–1993). *Note.* The dots show the proportion of individuals holding a basic vocational school certificate grouped at the school cohort cell for female and male wage earners who were born between 1978 and 1993. The solid lines represent the fitted values from a local quadratic polynomial regression, allowing for an intercept and slope shift at the 1986 school cohort. *Source:* Author’s own analysis based on unit data from the Polish Labor Force Survey for 2001–2016.

Table 2. Estimation of the Impact of the 1999 Reform on the Proportion of Individuals Holding a Basic Vocational School Certificate.

| | Total | Men | Women |
|-----------------------|---------------|--------------|----------------|
| Birth Cohort Controls | (1) | (1) | (2) |
| Local quartic | | | |
| Coefficient (SE) | .014 (.019) | .012 (.018) | .022 (.022) |
| Local cubic | | | |
| Coefficient (SE) | -.018 (.009) | -.019 (.010) | -.020 (.012) |
| Local quadratic | | | |
| Coefficient (SE) | -.001 (.010) | .009 (.015) | -.011 (.009) |
| Local linear | | | |
| Coefficient (SE) | -.022* (.010) | -.021 (.014) | -.025** (.007) |
| N | 118,721 | 62,831 | 55,890 |

Source: Author’s own analysis based on unit data from the Polish Labor Force Survey for 2001–2016.

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

looks at Figure 3 (Panels A and C), it seems that using the quadratic polynomial function is superior. Thus, I argue that my results regarding the wage premium from compulsory schooling should not be subject to a selection bias, which might be caused by a possible change in the proportion of individuals that choose to study at basic vocational schools.

As a starting point to the analysis of the impact of the education reform on hourly wage, I estimated the wage equation using ordinary least squares (OLS) and the full sample of basic vocational school certificate holders. Hourly wage was regressed on years of schooling, the quadratic polynomial of age, and 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 further estimations of the causal effect of the reform. The results are presented in Column 1 of Table 3. They show that hourly wage is not correlated with years of schooling. This also holds for the samples of men and women. However, since OLS estimates represent the ATE rather than LATE, this result does not necessarily mean that the year of schooling added by the 1999 reform has no impact on wages.

Column 2 in Table 3 presents the results of estimating the first-stage equation. It shows that the 1999 education reform, which added 1 year to the educational path leading to a basic vocational school certificate, had in fact a positive impact on the years of schooling of those completing this path.

Table 3. Estimation of the Impact of the 1999 Reform on the Years of Schooling and Hourly Wage of Graduates With Basic Vocational Education.

| Gender | OLS | | First Stage | | Reduced Form | | 2SLS | |
|------------------|--------------|--------------------|--------------|-----------------|----------------|-------------|-------------|--|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | |
| | Hourly Wage | Years of Schooling | Hourly Wage | Employment Rate | Hourly Wage | Hourly Wage | Hourly Wage | |
| Total | | | | | | | | |
| Coefficient (SE) | .000 (.005) | .254*** (.034) | .033* (.012) | .007 (.009) | .131*** (.047) | | | |
| N | 7,193 | 7,193 | 7,193 | 28,861 | 7,193 | | | |
| Men | | | | | | | | |
| Coefficient (SE) | -.007 (.009) | .240*** (.032) | .029† (.014) | -.010 (.013) | .120† (.062) | | | |
| N | 5,234 | 5,234 | 5,234 | 18,511 | 5,234 | | | |
| Women | | | | | | | | |
| Coefficient (SE) | .007 (.005) | .288** (.083) | .021 (.032) | .011 (.014) | .074 (.107) | | | |
| N | 1,959 | 1,959 | 1,959 | 10,350 | 1,959 | | | |

Source: Author's own analysis based on unit data from the Polish Labor Force Survey for 2001–2016. Note. Age and age² were additionally included in Specification 1. Specifications 2–5 include the local quadratic polynomial in school cohort, allowing for an intercept and slope shift at the cutoff point (1986 school cohort).

***, **, *, and † denote 0.1%, 1%, 5%, and 10% significance level, respectively.

Individuals covered by the reform studied on average 0.25 years (3 months) more to get the certificate. The increase in years of schooling was found for men (0.24 years) and women (0.29 years).

The positive impact of the reform on years of schooling is also apparent in Figure 4. The solid lines show the fitted values from a local quadratic polynomial on each side of the cutoff. One can see that before the reform, young people studied for about 11.2 years to obtain the basic vocational school certificate and that there is a clear jump in years of schooling at the cutoff birth cohort—up to about 11.5 years. A similar pattern can be observed for men and women.

It is apparent that the increase in the years of schooling caused by the reform is smaller than 1 year, both for men and women. This may be due to the fact that despite the original intentions of the reformers, the education cycle for certain professions at basic vocational schools was shortened from 3 to 2 years when the reform was implemented. In 2002, when the first cohort of students covered by the reform entered basic vocational schools, there were 22 professions taught in a 2-year cycle and 54 professions taught in a 3-year cycle. Thus, the total length of the education track leading to a basic vocational certificate was 11 years in the former case and 12 years in the latter case.

Another potential explanation is that the reform reduced the proportion of individuals repeating grades. Unfortunately, the LFS database does not include information on grade repetition or on the years of schooling in each school attended, so I cannot check whether this effect actually took place or correct for it. But if the grade repetition was actually decreased, then the impact of the reform on years of schooling will be underestimated, and consequently, the wage effect estimated for compliers (LATE) will be overestimated.

Figure 5 plots the evolution of the log of hourly wage across school cohorts. It shows that there is an upward jump in hourly wage at the cutoff. The graphs for the full sample (Panel A) and men (Panel B) are similar—the fitted line goes up by about 5% at the cutoff. However, the picture is not as clear for women (Panel C)—there is an upward jump in hourly wage at the second cohort after the cutoff but not at the first as expected. Thus, I cannot argue that the 1999 reform had an impact on the hourly wage of women, or at least I cannot argue that the impact was immediate.

In fact, the result for women is puzzling. The reform seems to have had a positive impact on the hourly wage of women, but for some reason, this effect is delayed by one cohort. Although the first and consecutive cohorts of women covered by the reform studied longer as a result of the reform, it

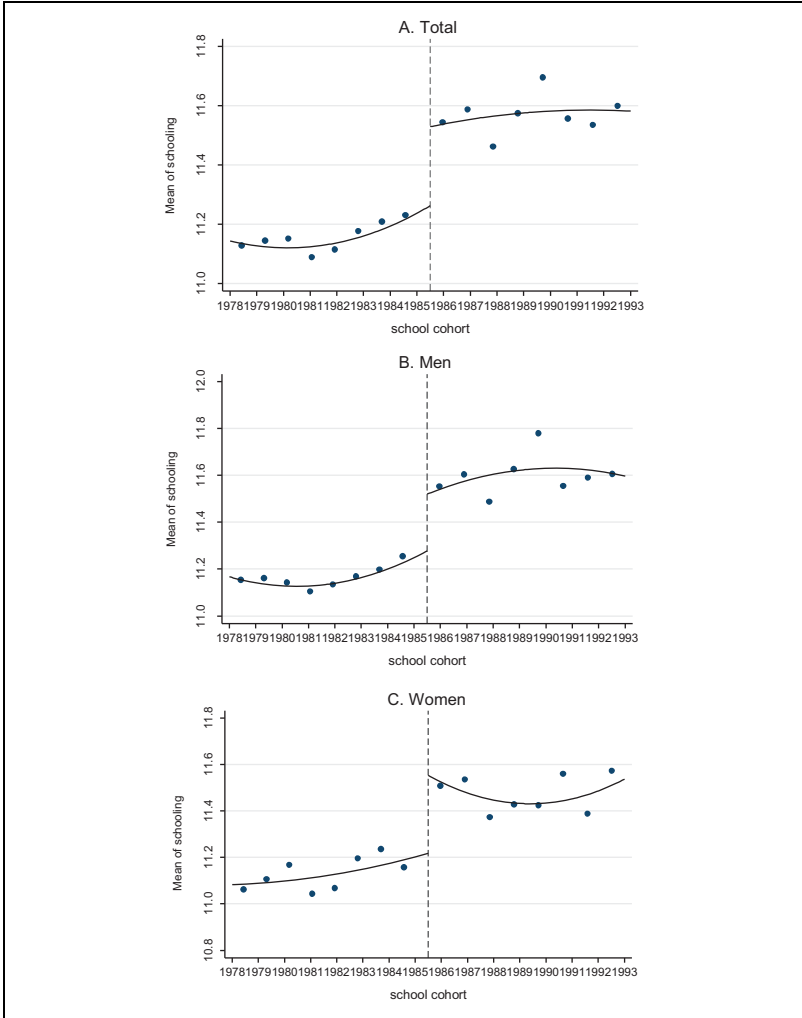


Figure 4. Impact of the 1999 reform on average years of schooling, calculated for the total population, men, and women (school cohorts 1978–1993).
Note. The dots show the average years of schooling grouped at the school cohort cell for female and male wage earners who were born between 1978 and 1993. The solid lines represent the fitted values from a local quadratic polynomial regression, allowing for an intercept and slope shift at the 1986 school cohort.
Source: Author’s own analysis based on unit data from the Polish Labor Force Survey for 2001–2016.

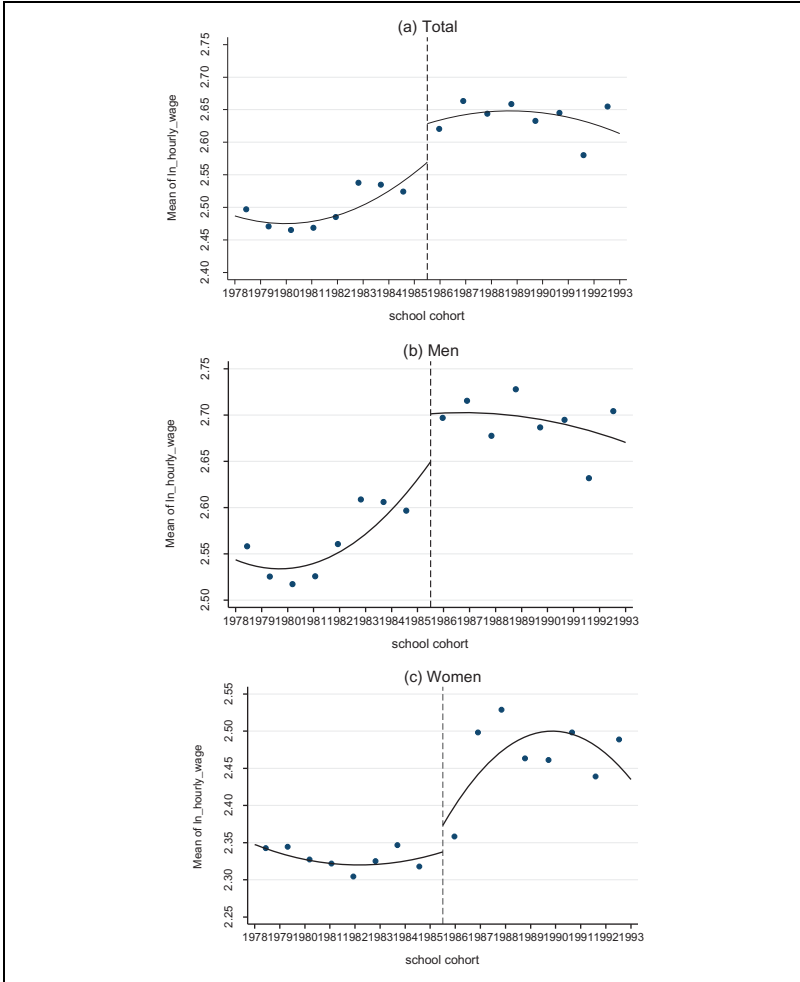


Figure 5. Impact of the 1999 reform on the log of hourly wage, calculated for the total population, men, and women (school cohorts 1978–1993).

Note. The dots show the average log of hourly wage 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 and slope shift at the 1986 school cohort.

Source: Author’s own analysis based on unit data from the Polish Labor Force Survey for 2001–2016.

appears that the first cohort did not benefit from the reform in terms of the knowledge and skills acquired, which could explain no impact on hourly wage. This result is puzzling because the reform had a positive impact on the hourly wage of the first cohort of men covered by the reform. To the best knowledge of the author, the reform was implemented in the same way with respect to male and female students and with respect to the professions where men or women are the vast majority of students.

The econometric analysis confirms the conclusions drawn from the visual inspection of Figure 5. The RDD results presented in Column 3 of Table 3 show a positive impact of the reform on hourly wage. Individuals covered by the reform earn 3.3% more than those who studied under the old system, and this result represents the causal effect of studying for 0.25 years longer, on average. The effect for men (2.9%) is weakly significant (only at the 10% level), while the effect for women (2.1%) is not significant. These two effects are not statistically different from each other, so I cannot argue that the reform had a bigger wage effect on men than on women.

One could argue that the jump in hourly wage may result from the potential impact of the reform on the employment rate. To address this potential criticism, I estimated the reduced-form equation (Equation 2) with the employment rate as the dependent variable. The results presented in Column 4 of Table 3 show that the reform had no impact on the employment rate—this holds not only for the full sample but also for men and women separately. The same conclusion can be drawn from the visual inspection of Figure 6, which shows the evolution of the employment rate across school cohorts. Clearly, there is no discontinuity in the employment rate at the 1986 school cohort.

Finally, Column 5 in Table 3 presents the 2SLS estimates, which by construction are equal to the ratio of the reduced-form estimate (representing the wage effect of being subject to the reform) to the first-stage estimate (representing the schooling effect of the reform). The 2SLS estimates can be interpreted as LATE estimates—they show the causal effect of the reform on hourly wage for compliers, that is, for the individuals who extended their formal education by 1 year as a result of the reform. For the full sample, this causal effect is positive and significant (at the 5% level)—individuals who studied 1 year longer because of the reform receive higher hourly wages by 13%, on average. The effect for men (12%) is weakly significant (only at the 10% level), while the effect for women (7%) is not significant. Again, I cannot argue that the wage effect of the reform was higher for men than for women.

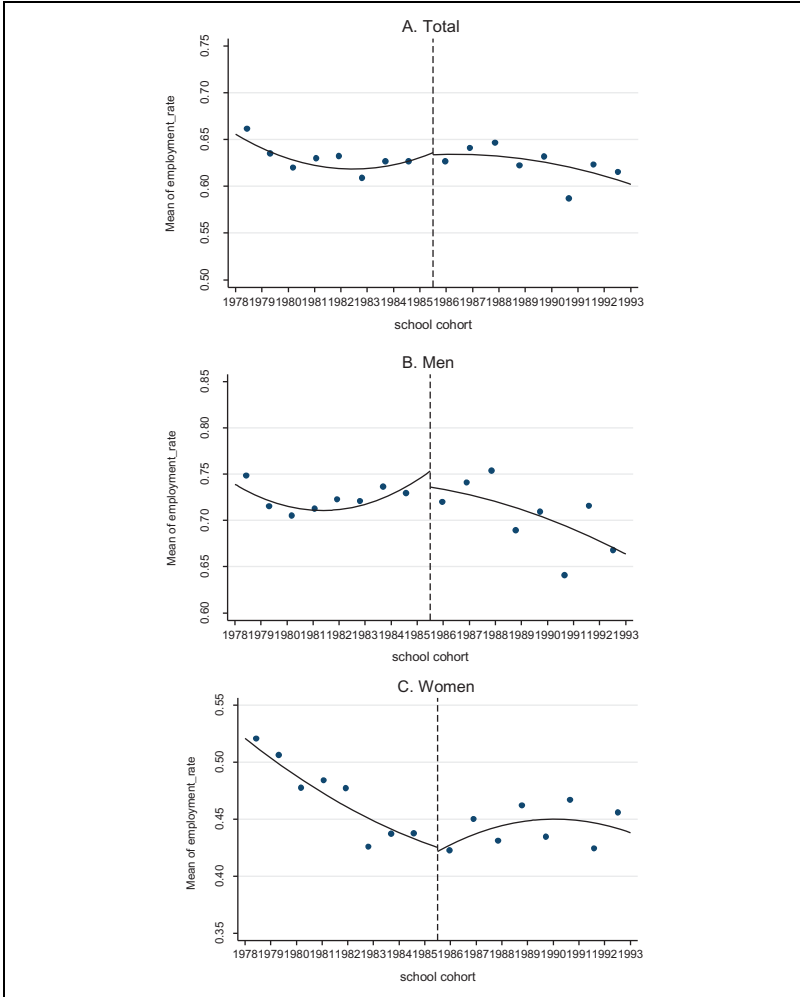


Figure 6. Impact of the 1999 reform on the employment rate, calculated for the total population, men, and women (school cohorts 1978–1993).
Note. The dots show the average employment rate grouped at the school cohort cell for female and male wage earners who were born between 1978 and 1993. The solid lines represent the fitted values from a local quadratic polynomial regression, allowing for an intercept and slope shift at the 1986 school cohort.
Source: Author’s own analysis based on unit data from the Polish Labor Force Survey for 2001–2016.

The robustness of the results obtained was checked in several ways. First, I tested the sensitivity of estimates to the functional form of the model. Following the recommendation of Gelman and Imbens (2018), I used the local quadratic polynomial function of the school cohort in the baseline specification, but I also decided to check whether the results are affected by using other functional forms. This resulted in estimations of the model using global and local functions, both linear and polynomials of the second, third, and fourth order. For almost all the functional forms tested, the results did not change qualitatively (see Table A1 in the Online Appendix). In most specifications, the wage premium from an additional year of education is between 13% and 15% (Column 3). The premium is much higher only when I use local polynomials of the third and fourth degree, but based on the recommendation of Gelman and Imbens (2018), I argue that the wage premium from an additional year of schooling is more likely in the range of 13%–14%.

Second, I performed the analysis for four other bandwidths of the forcing variable: for one bandwidth broader than the baseline, covering the 1976–1995 cohorts, and for three narrower ones, covering the 1980–1991, 1981–1990, and 1982–1989 cohorts. In all cases, the results are not qualitatively different from those obtained for the baseline bandwidth, that is, the 1978–1993 cohorts (see Table A2 in the Online Appendix).

Third, I conducted placebo tests. I tested for discontinuity in the years of schooling for cohorts other than 1986. As suggested by Imbens and Lemieux (2008), the occurrence of discontinuity was tested on both sides of the cutoff. I set the artificial cutoffs both far from the actual cutoff (at the 1981 and 1991 cohorts) and close to it (at the 1985 and 1987 cohorts). I used the baseline specification, which was the specification with the quadratic polynomial function of school cohort. The results presented in Table A3 in the Online Appendix show that in most cases, there is no discontinuity in wages at the placebo cutoffs that were tested. The only exception is the positive reduced-form estimate for women when the cutoff is set at the 1987 cohort. However, the 2SLS estimate is not significant in this case because of the insignificant first-stage estimate.

Fourth, as the expected length of the education track was 11–12 years, I treated individuals who studied for less than 11 years or more than 12 years as outliers and dropped them from my sample.¹⁰ Table A4 in the Online Appendix shows that after imposing this restriction, my results have not changed qualitatively.

Fifth, while the baseline results refer 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, I also extended the sample to all individuals with basic vocational education regardless of when the school was completed. Table A5 in the Online Appendix shows that this change does not affect my results.

Based on these additional tests, I may conclude that my results are robust to a variety of specifications.

I also wanted to find out if the reform had an impact on hourly wage only or if it also affected earnings of the employed individuals—that is, the product of hourly wage and working time. Thus, I estimated the wage regressions again using the natural logarithm of earnings as a dependent variable. The sample size is now slightly larger ($N = 7,216$) than in the baseline hourly wage model ($N = 7,193$), as now it additionally includes individuals who reported their earnings and did not report their working time. Table A6 in the Online Appendix shows that the reform had a positive effect on earnings of the employed. This result refers to all the employed individuals in the sample on average but not to men or women separately. Thus, it seems that the additional year of schooling increased not only the productivity of individuals, as represented by hourly wage, but also the earnings of the employed. As for men, whose hourly wage increased as a result of the reform, it seems that they must have reduced their working time, which is why their earnings were not affected.

Finally, I wanted to know the overall impact of the reform on individuals' economic situation, which includes its effect on hourly wage, working time, and employment. Therefore, I estimated the wage regression for the whole sample of individuals with basic vocational education, regardless of their employment status. This time my dependent variables were earnings (not log earnings), as they were set to zero for nonemployed individuals. With this approach, my outcome variable is a measure of the overall impact of the reform, as the level of earnings is a product of being employed, number of hours worked, and the level of hourly wage. I find that the reform had no overall impact on individuals' economic situation on average (see Table A7 in the Online Appendix).

Conclusions

The aim of my study was to determine whether the 1999 education reform, which added 1 year to the shortest available education track, had any impact on the hourly wage of those completing this track. Using the RDD and

Polish LFS data, I find that the hourly wage of individuals who completed the basic vocational track increased by 3.3% on average (ITT estimate). However, those who actually studied one additional year as a result of the reform (compliers) earn 13%–14% more per hour (TOT estimate) than those who studied under the old system.

The wage effect of this reform is higher than the wage effects reported in similar studies in other countries, where the wage premium from an additional year of formal education obtained by compliers is typically found to be under 10% (TOT estimate). The higher wage premium identified in my analysis may be due to the fact that my study does not refer to all the young people covered by the reform, but rather it focuses on those who completed the basic vocational track—in other words, on individuals with a relatively low socioeconomic background. This result is consistent with those of Meghir and Palme (2005) and Pekkarinen et al. (2009), who also found that the extension of mandatory comprehensive education in Sweden and Finland, respectively, had a positive wage effect but only on individuals coming from families of low social status. Interestingly, Meghir and Palme (2005) identified a wage effect of the same size as I did—they found that the wages of individuals with unqualified fathers increased by 3.4% on average (ITT estimate) as a result of extending compulsory schooling in Sweden from 7 or 8 to 9 years.

It seems that for the individuals choosing the basic vocational track, which provides the least amount of general skills when compared to other tracks, an additional year of general education is particularly valuable. Education experts who supported the implementation of the 1999 reform argued that individuals in the basic vocational track under the old system lacked general skills and, consequently, that it was difficult for them to acquire vocational skills and adapt to changes in the labor market. Thus, it seems that the positive wage effect may be attributed to the extra year of general education that was added to the basic vocational track.

This explanation is also consistent with the results obtained by Jakubowski et al. (2010), who studied the impact of the 1999 reform on students' educational outcomes. The authors argue that the spectacular improvement of the PISA test scores of Polish students between 2000 and 2006 is primarily due to the improved performance of the least able students and, in particular, those attending basic vocational schools. In light of this finding, 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, to a decrease in wage differentials.

What is also common between my study and the studies conducted for Western European countries is the higher value of the wage premium estimated with 2SLS than with OLS. This type of 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.

Obviously, my results are subject to a number of limitations. First, one should remember that the 1999 education reform in Poland—like similar reforms in other countries—was in fact a bundle of reforms (including new curricula and a new system of uniform external examinations). Thus, my estimates represent the joint effect of all of these reforms. I argue, though, that extended schooling was the most important component of the bundle and that the identified wage effect can be attributed primarily to it. The reason is that the other components could be expected to have an effect on the quality of education in the longer run, not immediately.¹¹ Second, my analysis of the impact of the reform on hourly wage is restricted to the employed individuals who reported their earnings and working hours. Additionally, the data on earnings and working hours may suffer from misreporting.

Finally, it is worth mentioning here that the 1999 education reform was canceled in 2017 and was replaced by the previous system; consequently, individuals who now choose the basic vocational track are required to study for only 11 years instead of 12.¹² In particular, general education in this track has been shortened from 9 (6 + 3) to 8 years.¹³ Based on the results of my study, one can expect that the hourly wage of individuals who choose the basic vocational track after the 2017 education reform will decrease when compared to those who studied in the system that existed in the period 1999–2016. Thus, it seems that from an economic point of view, the 2017 education reform may be detrimental to individuals who are going to finish their education after basic vocational school. Although they will study 1 year less and thus will be able to work and earn for 1 year more, their hourly wage may be approximately 10% less, so the total income earned throughout their career will likely be less than before the 2017 reform.

Author' Note

This article has been presented on several occasions: XXXII AIEL Conference (September 14–15, 2017, Cosenza, Italy); fifth Workshop on Efficiency in Education “Efficiency in education and performance measurement in public sector”

(October 19–20, 2017, Budapest, Hungary); and AASLE 2017 Conference (December 07–09, 2017, Canberra, Australia).

Acknowledgments

I thank Julien Grenet, Francesco Pastore, all seminar participants, the editor, and three anonymous referees for useful comments. Nonetheless, the usual disclaimer applies.


Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

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Supplemental Material

Supplemental material for this article is available online.

Notes

1. Actually, the first manuscript on the impact of compulsory schooling on earnings was by Angrist and Krueger (1991), but they analyzed the effect of school start age policy and compulsory school attendance laws rather than the effect of a reform increasing the school-leaving age.
2. In fact, there were 54 professions taught in a 3-year cycle and 22 professions in a 2-year cycle in the first school year after the reform.
3. At the same time, the reading test scores of students in the secondary vocational track increased by only 2%, while the scores of those in the secondary general track decreased by 3%.
4. Data from the System of Educational Information show that the rate of dropout from basic vocational schools in 2011 was 27.5%.
5. Data from the 2007 Graduate Tracer Study, a representative survey of Polish graduates conducted in 2007 by the Central Statistical Office, show that 68% of individuals who completed basic vocational schools finished their education at this level, 18% achieved secondary vocational education, 6%—secondary general education, and 5%—tertiary education. Unfortunately, this survey does not provide information on dropouts from basic vocational or secondary schools.

6. The earnings were corrected for inflation using the consumer price index.
7. The results of this estimation are available from the author upon request.
8. The author is grateful to the editor for pointing this out.
9. In some previous studies, the global polynomial function was used (e.g., Grenet, 2013). This method only allows for an intercept shift at the cutoff, not for a change of the slope.
10. I compute years of schooling as the age when an individual obtained a basic vocational school certificate minus 7 years. The extreme cases in my sample are individuals having 9 and 18 years of schooling (these groups constitute 0.96% and 0.08% of the sample, respectively). The former case may be a result of misreporting. These individuals could have started working as an apprentice after gymnasium while attending evening basic vocational school at the same time. As they spent most of their time on learning a profession at a workplace, they did not report this period as years of schooling. The latter extreme case may be due to repeating grades or breaks in education.
11. This refers to the system of uniform external examinations (the first two cohorts of individuals covered by the reform were obliged to take only one exam at the age of 16, while the third and consecutive cohorts—two exams at the age of 13 and 16) and the new system of the professional promotion of teachers, which was aimed at motivating teachers to increase their qualifications in the long run.
12. This refers to individuals born in 2004 or later.
13. The 6-year primary school and 3-year gymnasium were replaced by an 8-year primary school.

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