The Long-term Effects of Increased General Education: evidence from the comprehensive Polish educational reform of 1999

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Abstract

This paper estimates the combined effect of increased length of general education and decreased tracking on labour market outcomes. Contrary to previous studies finding no effect of increased general education for vocational students, we show that increasing general education for the lowest educated improves their labour market prospects. The Polish education reform in 1999 replaced the previous 8 years of general and 4 years of tracked secondary education with 9 years of general and 3 years of tracked education. This change increased not only the length of finished years of schooling of the lowest educated school dropouts but also of students attending basic vocational tracks at secondary level. The identification of our methodology relies on a difference-in-differences approach using a quasi-panel of pooled year-of-survey and age-of-respondent observations from the Polish sample of the EU-SILC database. The results indicate that the reform has increased both the employment probability (around 3% points) and wages (around 4%) of young Polish people. This effect is likely to be driven by the lowest educated for whom the effect sizes are double.

Keywords: Education reform, returns to education, Poland, detracking, labour market, difference-in-difference

JEL codes: I21, I24, I26, J24

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1. INTRODUCTION

It is generally argued that more education is better. The rate of return to an additional year of education – while varying a lot by country and education level – on average can easily exceed 10% points per year (Psacharopoulos and Patrinos 2004), and this non-causal effect of education on earnings is not very far from the causal estimate (Angrist and Krueger 1991). It is also argued that general skills are valued more on the labour market than vocational specific skills. While vocational education might foster a closer link between the schools and the labour market thereby facilitating a smoother transition (Ryan 2001; Wolter and Ryan 2011), general education is argued to offer more flexible general skills and hence greater employment probabilities in the long run (Hanushek et al. 2016).

In recent studies, however, it has been shown that an additional year of education offered to vocational students did not help their labour market chances. Oosterbeek and Webbink (2007) in the Netherlands, Pischke and Von Wachter (2008) in Germany, Hall (2012, 2016) in Sweden, and Grenet (2013), in France have looked at separate reforms and found very vague or null effects on various later stage outcomes, as tertiary enrolment, wages or unemployment chances.

In this paper, we utilize a relatively new policy reform from 1999 in Poland. The reform increased the length of general education by one year while decreasing the length of tracked upper-secondary education by one year for most students. We show that the reform had a significant and substantial effect on the wages and employment chances of the low educated. We argue that these results differ from the null-results of the above-mentioned studies in that Polish general education was increased before students are tracked to vocational schools. Due to the nature of the reform, this change in general education was effective mainly for the lowest educated school dropouts and those opting for the basic vocational track.[1] We argue that the reason behind the positive difference in effects between the Polish reform and the other policy changes is that it is not only the length of education and the content of the curriculum that matters, but also the quality of the teachers and the peer composition of the classes as main inputs of the education production function.

[1] As a study by Jakubowski et al (2016) has recently shown that the apparent upsurge in general skills of Polish students in the OECD PISA studies is mainly driven by those in the basic vocational track and is due to the reform of 1999.

2. LITERATURE

Various studies have looked at the effect of increasing the length of education on wages, earnings or other labour market outcomes. While human capital theory would postulate that each additional year spent in education should have substantial returns, there are several studies that find no effects.

After World War II, German states started to increase the initial 8 years of compulsory education to 9. Utilizing the time and spatial variance in the introduction of these policies, Pischke and von Wachter (2008) show that an additional year of education has zero returns on earnings. In 1975 about half of the Dutch basic vocational school graduates saw their length of school increased from 3 to 4 years. Oosterbeek and Webbink (2007) studied the effect of this change on wages in 1995, and showed that the returns to an additional year of vocational schooling had not surpassed the returns to an additional year of labour market experience; that is, the net returns to education for vocational students are not positive. Malamud and Pop-Eleches (2010) argue that by shifting students from vocational to general tracks in Romania in 1973 had not affected the students' future earnings or employment prospects. Similarly, in the 1990's in Sweden, the academic content in all vocational tracks was increased. This change reduced curriculum differences between the academic and vocational tracks in upper secondary school. The increase in general academic content allowed vocational graduates to apply to universities. The reform was preceded by a pilot scheme, introducing the new system in selected schools throughout the country. Using the time and spatial variance in the pilot scheme Hall has tested the effect of the Swedish reform on tertiary enrolment and wage (Hall 2012) and unemployment chances (Hall 2016). The results of these analyses have shown no difference between pre- and post-treatment cohorts in outcomes. Hall argues that a potential reason for the non-effect is the increased dropout rate of the vocational track students induced by the academic content.

Grenet (2013) looked at the change in compulsory schooling age from 14 to 16 in France in 1967 and from 15 to 16 in England and Wales in 1972. Using a regression discontinuity framework, he showed that French students have not benefited from this increase, while effects for England and Wales were positive: one year increase in compulsory education resulted in 6-7% higher wages, due mainly to the decreasing number of early school dropouts.

Naturally, there are numerous studies showing that rates of return to education are not only significant but also very high when compared to any other capital investments. The seminal paper by Angrist and Krueger (1991) reports an IV estimate of around 7,5% points for a year of schooling in the US. Hamond and Walker (1995) or Vieira (1999) – among others - replicate their method on UK and Portuguese data, respectively, finding very similar results. Oreopoulos (2007) also shows for the US, Canada, and the UK that additional education has a positive impact on lifetime wealth as well as on employment, health, and happiness. Other studies report that comprehensive reforms, which increased the age of selection, had positive effects on the intergenerational income or wage mobility. Particularly the studies of Meghir and Palme (2005) for Sweden, Pekkarinen, Uusitalo, and Kerr (2009) for Finland have shown that decreasing selectivity and increasing general education help to decrease inequalities.

In short, while theory dictates that an additional year of education should have strong and positive effects on future labour market outcomes, and this is often underlined by empirical evidence, there are numerous studies that find no effect. What is the reason for this puzzle?

It is also commonly argued that general training helps to increase lifetime income more than vocational training, while vocational training is likely to speed up the school-to-work transition process as compared to general training (Hanushek et al. 2016). The usual argument for this difference is that general education offers general skills, which are transferable between firms, while vocational education offers specific skills, which shortens the initial training period for labour market entrants and hence are beneficial for the first employer. In some of the null-effect studies (e.g. Oosterbeek and Webbink 2007) vocational schooling was increased. In others, which used the increase in compulsory age of schooling in tracking countries, the additional year of schooling has increased vocational education for at least a fraction of the population (e.g. Pischke and von Wachter 2008; Grenet 2013). Thus the lack of long term outcomes might be due to the lack of increased general skills.

In other studies, the general content of education was emphasized over the vocational one (Malamud and Pop-Eleches 2010; Hall 2012, 2016). Thus, according to theory we should see a long term effect. Another likely explanation of this difference between general and vocational training, or more specifically between general academic tracks and vocational tracks, is the difference between their production inputs. In the education production process, the two crucial inputs are teachers and peers (besides the content). Offering increased general training to vocational students does not impact these two inputs, as teachers and peers are unlikely to change due to increased length of education or increased general content.[2] On the other hand, if the system is "de-tracked," i.e. age of selection is increased; the composition of the teachers and also the peer groups will change. This could have an additional, positive, impact on lifetime outcomes.

In 1999, the Polish education system was reformed significantly. The aim of this reform was to increase the level of education and to decrease inequalities The age of first selection was postponed from age 14 to 15 and thus, the number of years spent in general training was increased from eight to nine years. The former 8-year-long general school (primary and lower

[2] At least in the short run, and thus, the utilized regression-discontinuity approach shows no effects.

secondary level) was replaced by a 6-year-long primary and a 3-year-long lower secondary school. Conversely, years spent in upper secondary education were decreased by one year in all tracks except the basic vocational track. Besides this structural reform, several other changes were carried out: the curriculum, the examination, admission and assessment systems were all transformed.

In this paper, we take a look at the long run effects of this reform. There are a couple of studies that have looked at the long-run effects of similar reforms in the Scandinavian countries. Meghir and Palme (2005) demonstrated that a Swedish reform in the 1950's had increased both the attainment and the later earnings of children with lowly educated parents. At the same time, the reform also decreased the earnings of those with highly educated parents. Pekkarinen, Uusilato and Kerr tested the effects of the Finnish comprehensive reform on the income elasticity (2009) and also on the average test scores (2013) and concluded that it had only a small but an overall positive effect on both dimensions. The novelty of these studies is that they could test the causal effects of an educational reform using difference-in-difference estimates exploiting the fact that the reform was implemented gradually across the countries.

While the Polish reform of 1999 was similar in many aspects to the Scandinavian reforms, it was introduced at one point in time for the whole country. Thus, we will compare the employment chances and real wages of pre-reform (control) and post-reform (treatment) cohorts directly. Pooling several years of cross-sectional surveys we generate a quasi-panel of time of survey and age brackets, which we will use to estimate difference-in-differences models, but unlike in the Scandinavian studies, the variance comes not from the time of implementation but from the time (year) of observation.

Results suggest that the 1999 reform in Poland was successful in the long-run. The post-reform group is more likely to be employed, and they also earn higher wages, and this effect is likely to be driven by the lowest educated. These results suggest that the reform has reached its initial goals.

3. THE EDUCATIONAL REFORM OF 1999 IN POLAND

The educational reform of 1999 was one of the four reforms – of social security, health care, public administration and education – implemented by the government elected in 1997. The three main goals of the 1999 education reform were to increase the level of education in the society, to provide equal educational opportunities to everyone and to improve the quality of education (Bialecki, Johnson, and Thorpe 2002).

In sum, the reform of 1999 has

1. extended comprehensive basic education by one year,

2. changed the structure by dividing the previous 8-year-long general track to 6 years of primary and 3 years of lower secondary school, the gimnazjum.
3. shortened the years of the liceum and technikum by one year, but has not affected the length of the basic vocational school (it remained 3 years long).
4. introduced core curricula
5. gave teachers higher autonomy in determining their own syllabi
6. introduced a new testing structure

The 1999 educational reform changed the structure of the system from nursery school to higher education. It also reformed the curricula, gave greater autonomy for teachers and abolished decentralized entrance exams and introduced standardized final exams at the end of the 6th and 9th year, the latter being used as entrance exam to upper-secondary level. It has also standardized the maturity exam. It affected the qualification requirements for teachers and school administration and financing (see Jakubowski 2015 for a detailed description of the reform).

We only address the structural changes (points 1 to 3 above) in detail as we argue that our identification procedure benefits from these changes. All other parts of the reform (points 4 to 6 above) – while they might be beneficial in the long run – should not impact our results, as they affected both pre- and post-treatment cohorts, although in slightly different extent. The changes in the school administration and financing system or the higher autonomy for teachers affected all cohorts still in school. The new testing system was low stakes for children at the end of 6^{th} grade. The other, already existing tests at the end of 9^{th} grade and after upper-secondary remained high stakes but became standardized. But most importantly these were only introduced gradually after 2003. Thus, by comparing before and after treatment cohorts should only show differences if the structure of the system, affecting pre- and post-treatment cohorts differently, matter.

STRUCTURAL CHANGES IN 1999

Undoubtedly, the most important structural change of 1999 was the one year increase in the length of comprehensive education. While the school starting age did not change with this reform – it had been 7 for several decades and was only lowered to 6 in 2015 (Jakubowski 2015) – the length and structure of compulsory education changed (see Figure 1).

Before 1999, general education consisted of a one track primary and lower secondary level school, the general school. This school lasted for 8 years, usually till age 15. However, education was compulsory until age 17 with the possibility of part-time education.[3] The primary school was followed by upper secondary tracks: a 4-year long academic secondary track or liceum, a 5-year long secondary vocational track or technikum, and a 3-year long basic vocational track. The liceum and the technikum ended with a maturity examination.

In the new system, the 8-year general school was substituted by a 6-year primary school and a new institution: a 3-year lower secondary school. This was called *gimnazjum* and admitted students based on residence. The gimnazjum was introduced to provide the same quality education for one more year for all students. Fewer gimnazjums were established than primary schools, as they were only opened in larger settlements. In rural areas, one gimnazjum collected the children from neighbouring villages, as an important goal was to increase the level of education in these areas (Jakubowski et al. 2016). After a gimnazjum accepted all students from its catchment area, it could admit the best applicants from other areas to the remaining places.

The lower secondary track was followed by the same upper secondary tracks as before, except that liceum and technikum became one year shorter, 3-years and 4-years, respectively. However, basic vocational schools remained 3 years long. For a short time, a new institution was operating, the so-called profiled academic secondary school, but it was abolished after a few years.

It is important to mention that Poland signed the Bologna Declaration in 1999 along with 29 European countries, according to which the typical three-level system of tertiary education – bachelor, master, and doctorate – was introduced (Kwiek 2014).[4]

[3] Compulsory age of schooling was increased from 17 to 18 in 1997 in article 70 of the Polish Constitution. This change affected all cohorts already in school, i.e. both pre- and post-treatment cohorts (see Jung-Miklaszewska 2003).

[4] The three-cycle structure of higher education was implemented gradually. In 2003 Poland was already implementing the Bologna structure (Kwiek 2014, p. 153). By the school year 2004/2005 10% of state higher education institutions had already adopted the 2-cycle model (Bachelor and Master) in all fields of study and 50% of the institutions in at least 50% of the fields of study (see European Commission 2005). In 2008 all tertiary students enrolled in the Bologna system (see Kwiek 2014, p. 154) As a consequence, the first bachelor level graduates of the new system were entering the labour market around 2006. Therefore, the 1984 and 1985 cohorts also had the opportunity to study in the Bologna system, – depending on their institution and field of study – however, the first full „Bologna cohort" was the 1989 cohort (who finished in 2011). This coincidence makes it hard for us to study the effect of the reform on the upper end of the education distribution.

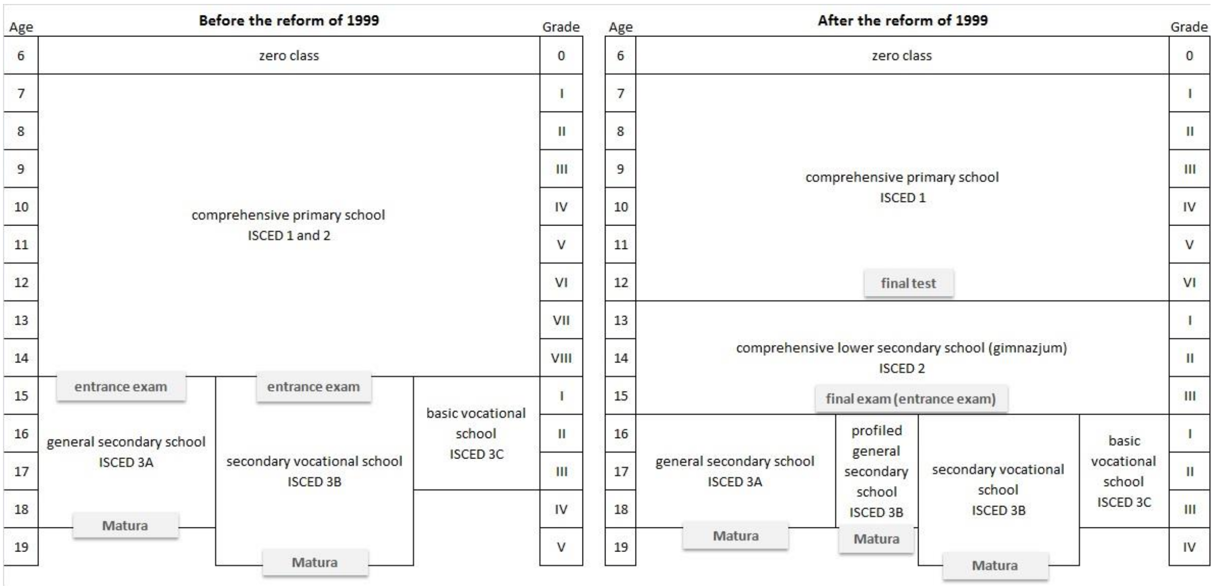

Figure 1. Pre- and post-reform structure of the Polish education system

Note: the width of the specific upper-secondary level tracks approximates the ratio of students attending these tracks in 2000 (pre-reform) and in 2004 (post-reform). Source of data: Polish Statistical Office

4. DATA

The EU Statistics on Income and Living Conditions (EU-SILC) consists of detailed data on income on personal and household level, as well as data concerning labour, education and health status. The population in the EU-SILC comprises private households with all household members surveyed but only over 16 years of age are people interviewed personally for income data.

In this paper, we utilize the cross-sectional database of EU-SILC. The data from Poland are available between 2005 and 2013. The sample selection is conducted in a two-stage process. The stratification is based on regions coded by NUTS 2. In the first stage, the population is divided into primary sampling units, from which a random sample of PSU-s is drawn. Then, in the second stage, every sampled PSU is divided into secondary sampling units and from every sampled PSU SSU-s are randomly drawn. Every household in a selected SSU is eligible for the sample (see Eurostat 2014).

To generate a balanced "quasi-panel," we pool the cross-sectional datasets between 2005 and 2013 and keep only those between ages 20-27. This allows us to compare pre-reform and post-reform participants: in the first survey year, 2005, the members of the youngest control group are 20 years old; and in the last survey year, 2013, the oldest treatment group members are 27 (see Table 1 below). This means we have 16 cohorts in the sample, eight in the treatment (T1 to T8) and eight in the control group (C1 to C8). These are people born between 1978 and 1993 (see also Table A2 and A3 in the appendix).

Table 1. Distribution of treatment and control group cohorts by age and year of survey[5]

year of survey age	2005	2006	2007	2008	2009	2010	2011	2012	2013
20	C1	T1	T2	T3	T4	T5	T6	T7	T8
21	C2	C1	T1	T2	T3	T4	T5	T6	T7
22	C3	C2	C1	T1	T2	T3	T4	T5	T6
23	C4	C3	C2	C1	T1	T2	T3	T4	T5
24	C5	C4	C3	C2	C1	T1	T2	T3	T4
25	C6	C5	C4	C3	C2	C1	T1	T2	T3
26	C7	C6	C5	C4	C3	C2	C1	T1	T2
27	C8	C7	C6	C5	C4	C3	C2	C1	T1

There are in total 48557 observations in the sample with 23471 in the control group and 25086 in the treatment group. In Poland, it is compulsory to start school in the year when the child turns 7. Thus, the threshold is 1 January 1986. In the sample, everyone born in 1986 or later is considered as treated – to have studied in the new system -, and everyone born until 31 December 1985 is considered as control.

Concerning the educational attainment, we rely on the ISCED classification: those with ISCED 2 or below are considered as low-educated, those with ISCED 3 or 4 are at the medium level, and those with ISCED 5 or above are highly educated. Unfortunately even these very rough categories are hard to compare before and after the reform (see descriptive statistics section below).[6]

Basic activity status in the EU-SILC classified into four categories: at work, unemployed, in retirement or early retirement, and other inactive. The first category covers those who work either full-time or part-time or are self-employed full-time or part-time. Students are considered inactive. When looking at employment chances, we will compare employed people to unemployed, as well as to the full population (inactive and unemployed merged). We will also run models on activity (active vs. inactive). We drop those in retirement or in early retirement as there are only 55 of these people in the full sample.

Data on income is collected as gross current monthly earnings, before the deduction of taxes and social insurance contributions. The income data is given in Euros. We converted this data to Polish Zloty, and to 2005 prices. The number of years spent at work, henceforth experience, is counted as the number of years spent as an employee or self-employed since the respondent first began a regular job.[7] The year of labour market entrance is equated with the year of finishing the highest education level (provided in the EU-SILC database), unless the

[5] For the number of observations, see the Appendix, Table A2
[6] Note also that the EU-SILC does not contain information on the upper-secondary track of the students.
[7] The summary of the variables can be found in the Appendix (Table A1).

year of starting the first job was earlier, in which case labour market entry is equated with the year of the first job.

5. DESCRIPTIVE STATISTICS

Before turning to the multivariate analysis, it is useful to look at the descriptive results. As expected the distribution of age when highest education was achieved is different before and after the reform. While the EU-SILC does not provide information on the age when each level of education was attained, we can look at the age when the highest level of education was attained. The median age of finishing education for those who have attained only ISCED 2 or below is 16 after the reform while it was 15 for those before the reform. This is most likely due to the one year longer general (primary and lower secondary) education. Similarly, there is a small but important difference between the control and the treated when we look at those who have obtained ISCED 3: post-reform cohorts tend to finish secondary education a bit later as there are fewer people who finish ISCED 3 at age 18 but more who finish at 19 or 20. This might be due to the relatively increased length of basic vocational education. However, most likely due to the Bologna system, the post-reform tertiary educated (ISCED 5) people are likely to finish education much earlier than the pre-reform cohort (see Figure 2).

When looking at the full population, it is also apparent that post-reform people tend to finish education a bit later than the pre-reform: as a result, they start their first job a bit later as well. This difference is the highest for the younger people. At age 20 and at 21 post-reform cohorts start their first job 4-5 months later, on average. This is probably due to those low-educated, who stay another year in school. The difference in first job starting age disappears at later ages (see Table 2).

Surprisingly, however, a later job starting does not go negatively together with experience. That is, treated people of the same age tend to have higher years of experience than the control group (see Table 3). This could be due to different employment chances, if post-reform cohorts have higher employment chances then – on average – they can gather more experience over a shorter period even if they start their first job at a later stage. Looking at the outcome measures, it is obvious that there are large differences between the control and the treatment cohorts. For instance, people at age 20 are 12% more likely to be employed after the reform than before the reform. This difference slowly evaporates as people get older, but remains significant till age 22 and on average it is positive for the full sample. Moreover, this positive difference is even more pronounced for the low-educated (see Table 4). The difference in employment chances, especially for the younger people, can explain the observed differences in experience.

Looking at the wages, similar differences can be found (see Figure 3 below). Treated people tend to earn a bit more on average, which is due mainly to the fact that there are fewer people in the treatment at the bottom of the wage distribution. That is, the earning distribution tilted to the right, moving those on the bottom of the distribution to the middle. This is apparent in the full cohort as well as in the sample of low-educated.

From these descriptive statistics and from the research before us, we suspect that the 1999 comprehensive education reform of Poland had a non-negligible and positive effect on the Polish labour market. We assume that all people after the reform benefited from it, but it was especially the young (where the level of education and skills gained matter most) and those on the bottom of the education distribution – the low–educated, low-skilled – who stayed one more year in school, who benefited most from the reform.

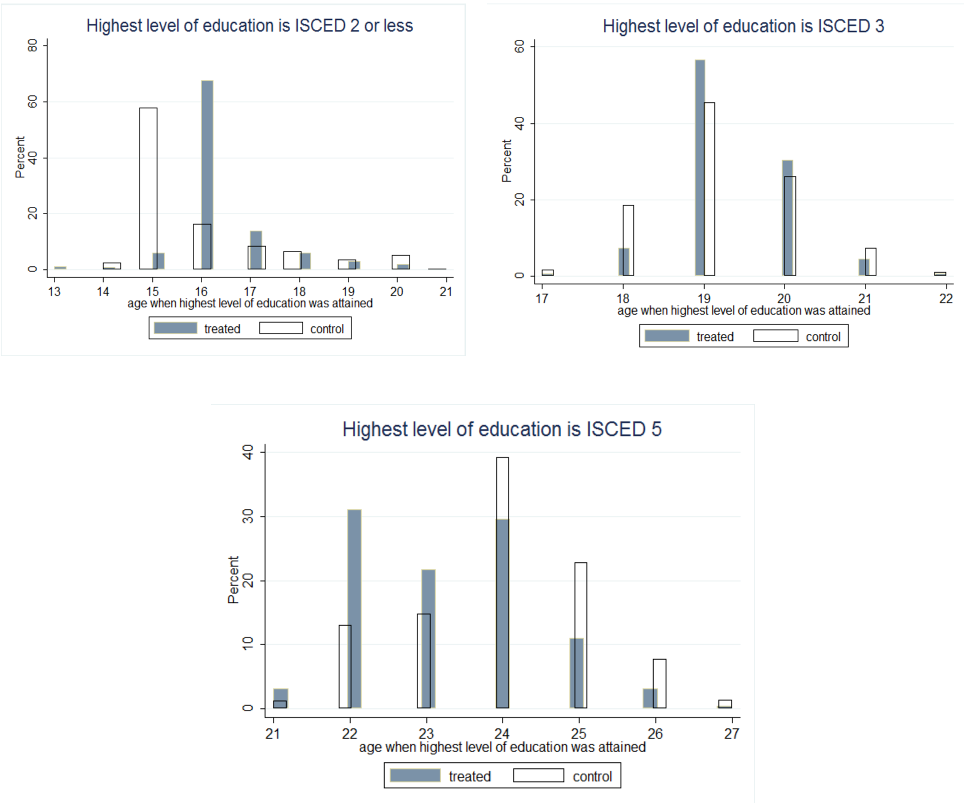

Figure 2. Distribution of ages when highest educational level was attained

Table 2. Ages when the control and treatment group members started their first job

age when the first job began			
age	control	treated	difference (st.err.)
20	18.14	18.49	0.346
			(0.136)**
21	18.87	19.31	0.444
			(0.096)***
22	19.52	19.70	0.176
			(0.083)**
23	20.00	20.02	0.020
			(0.082)
24	20.33	20.29	-0.041
			(0.088)
25	20.81	20.87	0.059
			(0.099)
26	21.12	21.21	0.084
			(0.121)
27	21.22	21.38	0.159
			(0.167)

*** p<0.01, ** p<0.05, * p<0.1

Table 3. Mean years of experience by age

mean experience in years			
age	control	treated	difference (st.err.)
20	0.79	0.85	0.064
			(0.093)
21	0.99	1.11	0.117
			(0.066)*
22	1.36	1.64	0.277
			(0.061)***
23	1.83	2.22	0.385
			(0.067)***
24	2.36	2.84	0.484
			(0.078)***
25	2.79	3.24	0.451
			(0.092)***
26	3.44	3.81	0.364
			(0.118)***
27	4.23	4.40	0.172
			(0.166)

*** p<0.01, ** p<0.05, * p<0.1

Table 4. Percentage of employed people among those who are active (employed or unemployed)

	Full sample			only ISCED 2 or below		
age	control	treated	difference (st.err.)	control	treated	difference (st.err.)
20	0.518	0.639	0.121	0.407	0.593	0.186
			(0.043)***			(0.098)*
21	0.603	0.700	0.097	0.440	0.590	0.150
			(0.025)***			(0.074)**
22	0.704	0.763	0.059	0.584	0.711	0.127
			(0.059)***			(0.069)*
23	0.768	0.791	0.024	0.542	0.680	0.138
			(0.017)			(0.077)*
24	0.797	0.810	0.013	0.621	0.647	0.025
			(0.017)			(0.076)
25	0.822	0.816	-0.006	0.597	0.581	-0.016
			(0.016)			(0.082)
26	0.835	0.866	0.031	0.646	0.672	0.026
			(0.016)**			(0.102)
27	0.852	0.818	-0.034	0.597	0.413	-0.184
			(0.024)			(0.136)

*** p<0.01, ** p<0.05, * p<0.1

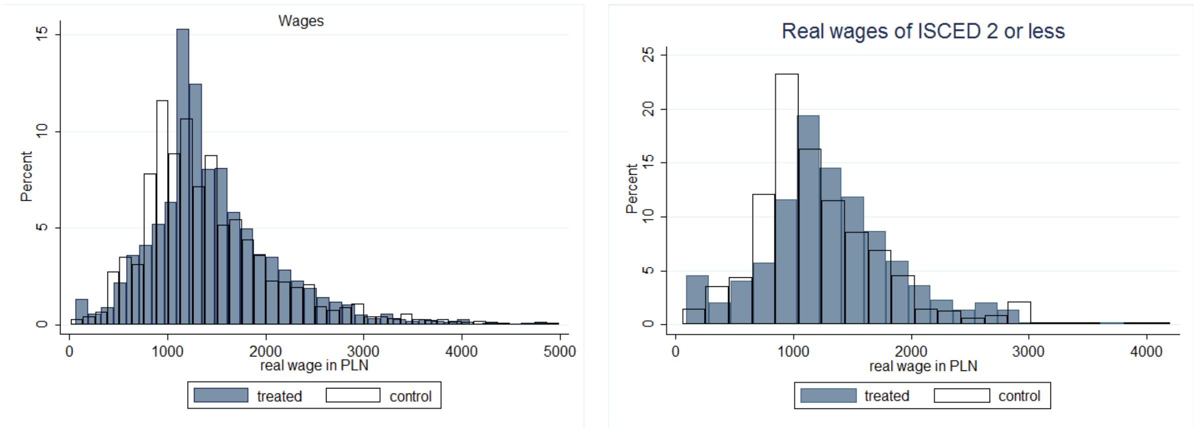

Figure 3. Distribution of real wages in 2005 PLN of treatment and control group

6. METHODOLOGY AND BASELINE RESULTS

Obviously, the descriptive statistics cannot uncover causal differences between the treated and the control groups, as the selection into treatment was not random. However, as selection to the treatment group was determined by the year of birth, we assume that there are no unobservable individual differences between the two groups: there must be cohort-specific differences, which can be taken into account, since every member of the treatment group was born in later years than the members of the control group. Similarly, every survey year is different from the other: for example, employment or wage outcomes recorded in the years of the great recession starting in 2008 must be different from the ones before. For this reason, as a baseline, we have opted for a difference-in-difference method where the age and the year of survey act as the two dimensions of the estimation (the first differences) and the treatment variable as the diff-in-diff (second difference) estimator.[8] We use age and year of survey fixed effects in every regression.

The baseline specification of the multivariate model is the following:

$$Y_{asi} = \alpha + \beta \mathrm{Treat}_{as} + \rho X_{asi} + \gamma_a + \delta_s + \varepsilon_{asi}$$

where Y is the outcome variable (education, employment or wage) for each individual (i). Treat is the treatment dummy, which can vary across cohorts (a) and year-of-survey (s). X is a factor of individual level variables (gender and level of education, in some specifications), and γ and δ are cohort and year of survey fixed effects, respectively. ε is the idiosyncratic error term, while α, β and ρ are parameters to be estimated.[9]

The results underline the pattern in the descriptive statistics. Treated cohorts, on average, tend to stay just as long in education as the control cohorts, but this average zero effect masks a significant composition effect (see Table 5): low educated stay about 0,89 years longer in school, while the average medium educated (ISCED 3 level) stay a little over 1 month (0.09 years) longer in school. This average effect for the medium educated is probably due to the cca. 15% of students in basic vocational tracks, who stay about one year longer in school. Higher educated, on the other hand, finish ISCED 5 level education about 0.7 years earlier; this is probably due to the previously nonexistent BA degree.[10]

When looking only at those who are currently not pursuing any education and thus they have finished their educational career (at least for a while), the pattern is similar, but effect

[8] The assumption behind this diff-in-diff approach is that changes between two consecutive years-of-observation should (in the absence of treatment) be the same for all ages, and also changes between two consecutive cohorts (ages) should be the same (in the absence of treatment) be the same for all years-of observation. For instance suppose the only available survey years are 2005-2006. Then the average change between 2005 and 2006 for cohorts ranging from 21 to 27 is a counter-factual change for cohort 20. Now, suppose we have only 2006 and 2007, the assumption is that average change between 2006 and 2007 for cohorts 22-27 and 20 is a counter-factual for change at age 21 etc. And similar logic applies for the other dimension.

[9] We have tested for potential differences in composition across regions (see Bukowski 2016). Including regional fixed-effects in the regressions do not change any of the results.

[10] Note that in the Bologna system BA and MA level is usually 3+2 years long, which means that students entering the Bologna should typically finish their first ISCED 5 level education 2years earlier, and yet treated students finish only 0.7 years earlier.

sizes are a bit different: the low educated stay in school half a year longer, medium level educated over 2 months longer and higher educated about a year less.

All in all, it seems that the reform has kept low and medium educated students in school longer, which could have long-run effects on their labour market success.

Table 5. The effect of the reform on the age of finishing highest level of education – linear models, full sample

	(1)	(2)	(3)	(4)
	Full sample		Currently not in education	
VARIABLES	age of finishing ed.	age of finishing ed.	age of finishing ed.	age of finishing ed.
Treated	0.0164	0.0900***	-0.0208	0.184***
	(0.0480)	(0.0285)	(0.0674)	(0.0426)
low ed.		-3.824***		-3.847***
		(0.0490)		(0.0518)
high ed.		3.946***		4.297***
		(0.0377)		(0.0369)
treat * low ed.		0.806***		0.494***
		(0.0590)		(0.0757)
treat * high ed		-0.785***		-0.956***
		(0.0654)		(0.0817)
Female	0.548***	-0.0444***	0.749***	-0.0142
	(0.0247)	(0.0148)	(0.0377)	(0.0232)
Constant	17.62***	18.95***	17.68***	18.77***
	(0.0502)	(0.0280)	(0.0817)	(0.0533)
Observations	37,569	37,569	21,510	21,510
R-squared	0.188	0.724	0.138	0.731
age fixed-effect	y	y	y	y
year-of-survey fixed-effect	y	y	y	y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Naturally, the reform also had an impact on the composition of the level of education. Unfortunately, the data does not allow for us to fully observe the finished level of schooling for the treated cohorts, as the oldest treated cohort was only 27 in 2013. Moreover, tertiary education qualification has changed significantly during these years (the Bologna process). Nevertheless, we can see that on average treated people are around 1,4% more likely to be low educated, 2% more likely to be high educated, and 3,4% less likely to be medium educated than the control group (see Table 6), in our sample. While the increased high level of education is certainly driven by the Bologna process, the increased low level of education is only due to the young cohorts (age 20), some of whom have not finished this lengthened level yet. However, for those between age 21 and 25, the ratio of people with the separate levels of school attainment have not changed due to the reform (see Figure 3).

Table 6. The effect of the reform on the (currently) finished highest level of education

	Multinomial probit		
VARIABLES	low ed.	medium ed.	high ed.
Treated	0.0139**	-0.0338***	0.0199**
	(0.00637)	(0.00900)	(0.00845)
Female	-0.0572***	-0.0463***	0.104***
	(0.00342)	(0.00598)	(0.00468)
Observations	37,605	37,605	37,605
age fixed-effect	y	y	y
year-of-survey fixed-effect	y	y	y

Standard errors in parentheses, marginal effects at the mean
*** p<0.01, ** p<0.05, * p<0.1

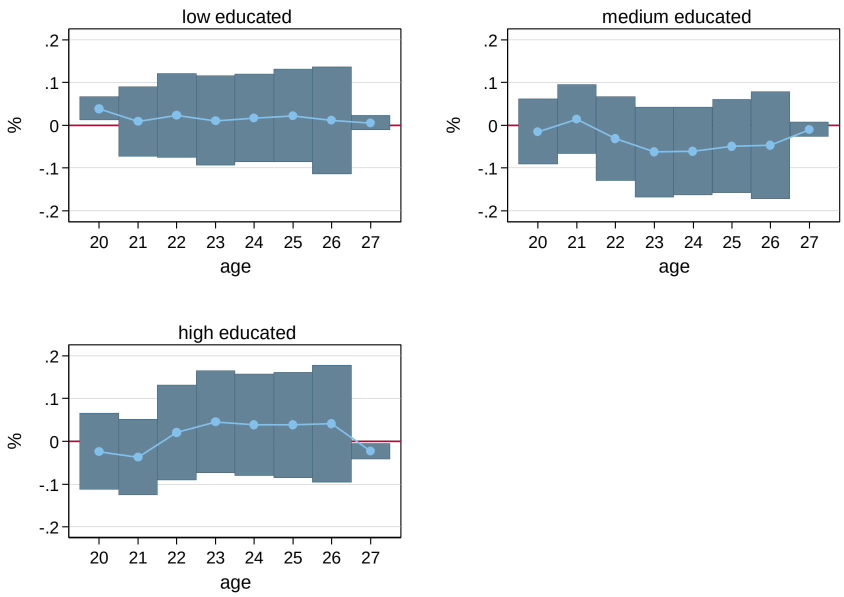

Figure 3. The effect of the reform on the (currently) finished highest level of education for the different ages

Although some students tend to stay longer in school, this does not mean that treated cohorts are more likely to study – on average – or be less (in)active in the labour market.

The EU-SILC database contains independent self-reported responses on studying as well as on the main labour market status. Being in education is correlated but not linearly dependent on inactive labour market status (i.e. employed and unemployed can also be enrolled in school, and inactivity includes other non-active responses than full-time education, see Table A4 in the appendix) we ran independent tests on studying as well as on inactivity status.

Models 1 and 2 in Table 7 below show the effect of the reform on studying (whether the respondent is currently in education or not). On average treated tend to be in education just as much as the control, but post-treatment respondents with a low level of attained education tend to study 15% more likely compared to pre-treatment low-educated (a difference which is driven by the younger age people). Similarly, treated highly educated tend to be in education 10% more than the control high-educated (which, again, is most likely an artifact of the Bologna system).

Models 3 and 4 in Table 7 look at the effect of treatment on the labour market outcomes. On average treated people are over 1% more likely to be employed (non-significant effect), or around 1,3% less likely to be unemployed (marginally significant effect) than the control group. Inactivity in the treatment and control group, on average, is the same.

Note however that the reform had a different effect on the differently educated people. Low educated are over 7,8% more likely to be employed after the treatment, which is due to their 5,1% decrease in unemployment and 2,7% decrease in inactivity. Highly educated, on the other hand, are both more likely to be employed (6,6%) and more likely to be unemployed (5.5%), which is due to their plummeting inactivity level (~12%) generated by the Bologna process. The labour market outcomes of medium educated are not affected by the reform.

CAVEATS

We have to note, however, that due to the compositional changes in educational levels, shown in Table 5 and Figure 4, education can be considered as a “bad control”(see Angrist and Pischke 2008), as the level of education is in itself an outcome of the reform. When comparing the labour market outcomes of low, medium or high educated before and after the treatment, we might compare apples with oranges (although note that the compositional changes for the low educated are probably much smaller than the changes for the high educated). Moreover using a large number of fixed effects in non-linear models might be problematic (Greene 2002), as well as using interaction effects in non-linear models (Ai and Norton 2003). And finally, while year of survey fixed effects and the age fixed effects should take out much of the unobserved heterogeneity across cohorts in educational outcomes, pre- and post-treatment cohorts might differ in their year of labour market (LM) entry (due to the changed educational outcomes), which can easily bias the effect of the treatment on longer-run labour market outcomes.[11] Consequently, we will include year of labour market entry fixed effects in the regressions below.[12]

As the level of inactivity seems to be unchanged for the full cohort (model 3 in Table 7), we will turn our attention to the active population only and estimate linear models with large number of fixed effects (year of survey, cohort and year of labour market entry) to see the effect of the reform on employment probabilities and real wage.

7. LABOUR MARKET OUTCOMES

The baseline multinomial probit models show that treated cohorts are over 1,3% less likely to be unemployed than the control cohorts, is the effect being significant only at the 10% level. Due to the incidental parameters problem, this result might be biased towards zero (Greene 2002).

In Table 8 below we estimate linear probability models for the sample of the active population. While linear models might be criticized for their functional form, they produce consistent estimates even with a large number of fixed-effects and interactions.

In Table 8 below, we have estimated all models with and without labour market entry fixed effects. On the one hand controlling for the year when one enters the labour market seems to be essential as different demand side factors can alter the employment probabilities as well as the initial wages of the entrants. On the other hand, the year of labour market entry might also be considered as a “bad control” as it correlates well with the years spent in schooling which depends on the reform as well. Moreover, the year of labour market entry correlates strongly with the age and the year of survey (their product) which inflates the variance of the model. Nevertheless, the substantial results of the models do not differ much with or without the year of labour market entry fixed effects.

Based on these results (in Table 8) we claim that the educational reform of 1999 increased the overall employment probability of the people by around 3%, and it also increased average wages by over 4%. This effect is larger for the younger people and declines with age (see Figure 4). People closer to their twenties benefited with a 5% increase from the reform as compared to the pre-treatment people with similar age. This effect is around 10% of the real wage. Both of these effects decline with age and disappear around age 23-24.

[11] For instance if treated cohorts stay in school one year longer, as it was the case for most ISCED 2 and some ISCED 3 level students, students born in 1986 might enter labour market not one year later compared to students in 1985, but two years later. Thus year of survey and age dummies might not fully control for the unobserved differences between cohorts.

[12] For people with no experience, we have imputed their year of labour market entry with their year of finishing highest education.

Table 9 below shows the same models with the level of education interacted with the treatment dummy. As in the multinomial probit models in Table 7 above (which were without labour market entry fixed effects), it seems that the low educated benefited the most from the reform. In fact, it is only the low educated treatment cohorts that are employed and earn significantly more than the control cohorts. Neither the medium educated nor the high educated treated cohorts differ significantly from their non-treated counterparts.

As robustness checks columns, 3 and 4 and columns 7 and 8 show the same models regressed on the 1985 and 1986 sample. That is cohorts born right before and right after the 1986 January 1 cutoff are compared. Results do not change significantly (although they became insignificant in places due to the lower number of cases).[13]

An alternative test of the mechanisms would be to use the reform as an instrument for the age of schooling. Unfortunately, the database does not contain data on the years spent in education, but only on the age when the highest educational level was attained. As shown above due mainly to the Bologna process, we cannot compare the average age for the full pre- and post-treatment cohort. However, we can compare the lowest educated sub-population, the school dropouts. Figure 5 below shows the age distribution for those with only ISCED 2 or less by for each pairs of cohorts: the median age of schooling was 15 before 1985 and has become 16 after 1986.

Table 10 below shows that the association of the age when the highest degree was obtained for this school-dropout population with log wage or with employment probability is null (OLS models). It is both insignificant and very close to zero in absolute terms as well. The reform can act as a strong instrument, as 1st stage estimates in Table 10 show that treated cohorts are 0.4 or 0.6 years older than the pre-treatment cohorts when they finish their highest degree of schooling. Unfortunately, the estimated 2sls parameters are not-significant, due most likely to the small number of cases. Nevertheless, both estimations show that school dropouts would have been more likely to earn more and had a higher probability of employment had they attended lower secondary schooling for an additional year. While the estimated parameters of 13,6% and 12% are insignificant, they are of very similar magnitude to the significant estimate of Table 9.[14]

[13] Note that this robustness check shows us that the 1997 change in the compulsory age of schooling does not drive our results. This is not to say that the increase from 17 to 18 did not have an effect, but it should not have affected the 1985 and the 1986 cohorts differently.

[14] As treated are only 0.4-0.6 years older than the pre-treatment cohorts, the causal estimate from the IV should be around 13,6*0.4=5,44 for wage and 12*.58=6.96 for employment.

Table 7. The effect of the reform on studying and labour market outcomes

	(1)	(2)		(3)			(4)	
	LPM		Multinomial probit[#]					
VARIABLES	currently in education		at_work	unemployed	inactive	at_work	unemployed	inactive
Treated	0.0125	-0.0189*	0.0114	-0.0131*	0.00164	0.00486	-0.00845	0.00358
	(0.00985)	(0.0111)	(0.0114)	(0.00720)	(0.0109)	(0.0125)	(0.00783)	(0.0118)
low ed.		-0.199***				-0.207***	0.117***	0.0896***
		(0.0104)				(0.0160)	(0.0120)	(0.0161)
high ed.		-0.000731				0.0657***	-0.0162	-0.0495***
		(0.00867)				(0.0191)	(0.0125)	(0.0190)
treat * low ed.		0.153***				0.0780***	-0.0507***	-0.0273
		(0.0175)				(0.0231)	(0.0101)	(0.0205)
treat * high ed		0.0980***				0.0662*	0.0557**	-0.122***
		(0.0185)				(0.0341)	(0.0278)	(0.0321)
Female	0.0892***	0.0794***	-0.152***	-0.00560	0.158***	-0.165***	-0.000383	0.166***
	(0.00576)	(0.00586)	(0.00606)	(0.00424)	(0.00608)	(0.00621)	(0.00448)	(0.00606)
Constant	0.683***	0.736***						
	(0.0112)	(0.0116)						
Observations	37,669	37,605	39,187	39,187	39,187	37,560	37,560	37,560
R-squared	0.195	0.203						
age fixed-effect	y	y	y	y	y	y	y	y
year-of-survey fixed-effect	y	y	y	y	y	y	y	y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[#] Marginal effects at the mean

Table 8. The effect of the reform on employment probability and wages – age interactions – linear models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES		employed vs unemployed				log wage		
Treated	0.0348***	0.0273	0.0278**	0.0815*	0.0447***	0.0860	0.0418**	0.116
	(0.0103)	(0.0421)	(0.0116)	(0.0434)	(0.0149)	(0.0994)	(0.0174)	(0.101)
* age 21		-0.00344		-0.0220		-0.0128		-0.0151
		(0.0496)		(0.0507)		(0.102)		(0.102)
* age 22		-0.00921		-0.0484		0.00364		-0.0147
		(0.0445)		(0.0457)		(0.104)		(0.105)
* age 23		0.00293		-0.0410		-0.0361		-0.0605
		(0.0444)		(0.0458)		(0.102)		(0.103)
* age 24		0.0179		-0.0590		-0.0529		-0.0926
		(0.0461)		(0.0473)		(0.101)		(0.103)
* age 25		0.00442		-0.0746		-0.0415		-0.0894
		(0.0444)		(0.0460)		(0.102)		(0.104)
* age 26		0.0442		-0.0557		-0.122		-0.170
		(0.0444)		(0.0467)		(0.102)		(0.104)
* age 27		-0.00780		-0.119**		-0.00942		-0.0897
		(0.0508)		(0.0551)		(0.107)		(0.110)
Female	-0.0422***	-0.0423***	-0.0478***	-0.0476***	-0.177***	-0.177***	-0.199***	-0.199***
	(0.00585)	(0.00584)	(0.00620)	(0.00621)	(0.00884)	(0.00882)	(0.00922)	(0.00919)
Constant	0.534***	0.540***	0.630***	0.577***	6.702***	6.667***	6.585***	6.518***
	(0.0202)	(0.0367)	(0.0702)	(0.0811)	(0.0310)	(0.0959)	(0.185)	(0.202)
Observations	24,393	24,393	23,274	23,274	16,254	16,254	15,499	15,499
R-squared	0.043	0.043	0.053	0.053	0.173	0.174	0.187	0.189
age fixed-effect	y	y	y	y	y	y	y	y
year-of-survey fixed-effect	y	y	y	y	y	y	y	y
year of LM entry fixed-effect	n	n	y	y	n	n	y	y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

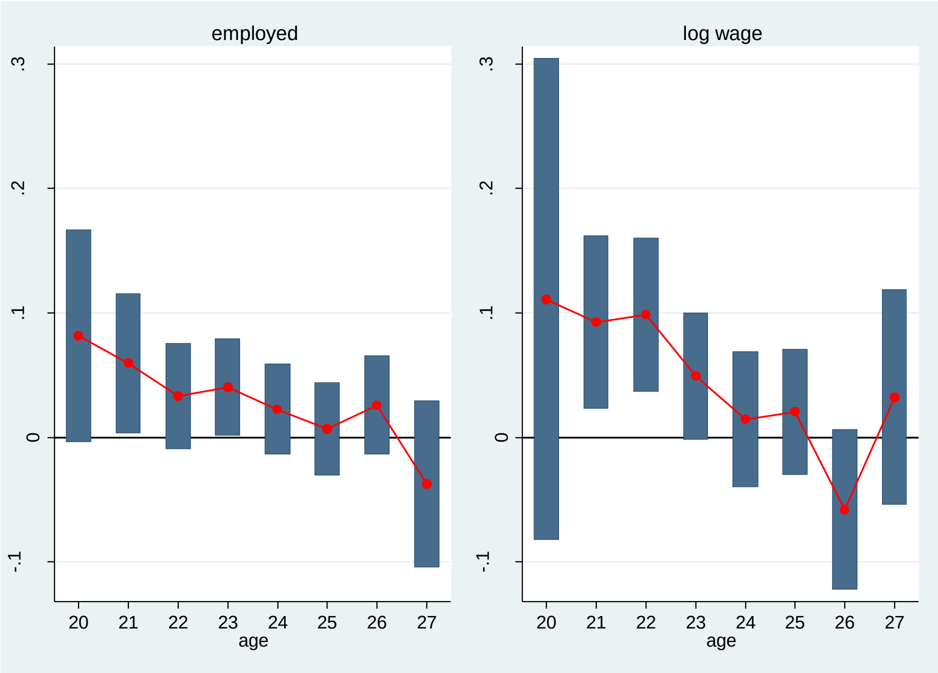

Figure 4. The effect of the reform on employment probability and wages – age interactions

note: models 4 and 8 in Table 8 above. Average effects with 95% confidence intervals.

Table 9. The effect of the reform on employment probability and wages – education interactions – linear models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	employed vs unemployed				log real wage			
VARIABLES	full sample		cohorts 1985/86		full sample		cohorts 1985/86	
Treated	0.0306***	0.0535*	0.0376**	0.0642	0.0390**	0.0448	0.0984***	0.164
	(0.0116)	(0.0276)	(0.0147)	(0.0578)	(0.0172)	(0.0457)	(0.0237)	(0.114)
medium ed.	0.205***	0.215***	0.154***	0.172***	0.135***	0.124***	-0.0185	0.0244
	(0.0149)	(0.0197)	(0.0343)	(0.0419)	(0.0234)	(0.0298)	(0.0509)	(0.0670)
high ed.	0.302***	0.316***	0.254***	0.259***	0.288***	0.316***	0.146***	0.160**
	(0.0185)	(0.0233)	(0.0388)	(0.0446)	(0.0269)	(0.0319)	(0.0540)	(0.0687)
treat * medium ed.		-0.0219		-0.0359		0.0340		-0.0869
		(0.0275)		(0.0579)		(0.0435)		(0.109)
treat * high ed.		-0.0332		-0.00926		-0.0871*		-0.0272
		(0.0294)		(0.0635)		(0.0472)		(0.111)
Female	-0.0616***	-0.0615***	-0.0580***	-0.0584***	-0.223***	-0.222***	-0.194***	-0.195***
	(0.00637)	(0.00638)	(0.0120)	(0.0120)	(0.00927)	(0.00926)	(0.0184)	(0.0183)
Constant	0.639***	0.628***	0.551***	0.537***	6.686***	6.682***	6.688***	6.650***
	(0.0758)	(0.0773)	(0.0547)	(0.0565)	(0.175)	(0.173)	(0.131)	(0.142)
Observations	23,267	23,267	5,082	5,082	15,496	15,496	3,439	3,439
R-squared	0.077	0.077	0.070	0.070	0.212	0.214	0.246	0.247
region fixed-effect	y	y	y	y	y	y	y	y
age fixed-effect	y	y	y	y	y	y	y	y
year-of-survey fixed-effect	y	y	y	y	y	y	y	y
year of LM entry fixed-effect	y	y	y	y	y	y	y	y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

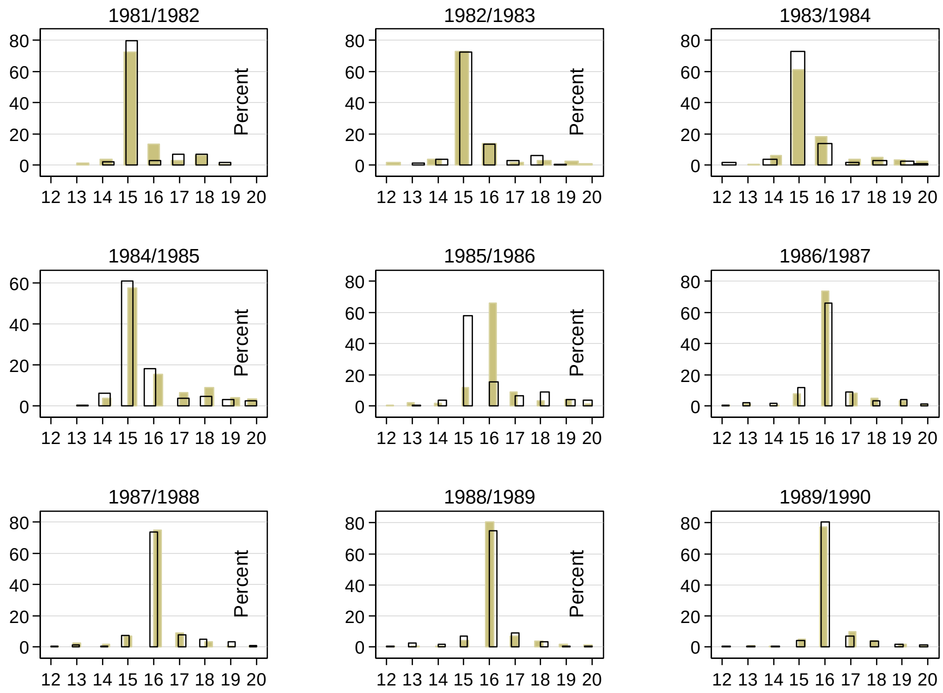

Figure 5. Distribution of ages when highest educational level was attained by pairs of cohorts for students with ISCED 2 or below

Table 10. The effect of the reform on employment probability and wages – IV estimation for people with ISCED2 or less

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		1st stage	2sls	1st stage	2sls
VARIABLES	log wage	employed	log real wage		employed	
age when highest education was attained	-0.00307	0.00633		0.136		0.120
	(0.0150)	(0.00923)		(0.194)		(0.0768)
Treated			0.401**		0.583***	
			(0.188)		(0.123)	
Female	-0.171***	-0.178***	0.186	-0.202***	0.166**	-0.201***
	(0.0615)	(0.0298)	(0.143)	(0.0649)	(0.0805)	(0.0362)
Constant	6.422***	0.361**	15.46***	4.829*	15.22***	-1.247
	(0.247)	(0.158)	(0.295)	(2.865)	(0.189)	(1.115)
Observations	914	1,917	914	914	1,917	1,917
R-squared	0.174	0.101	0.151	0.079	0.115	0.009
region fixed-effect	y	y	y	y	y	y
age fixed-effect	y	y	y	y	y	y
year-of-survey fixed-effect	y	y	y	y	y	y

Robust clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

8. CONCLUSION

While education is considered to be one of the highest return investments, some studies had shown that an additional year of education offered to vocational students did not help their labour market chances or increase their wages (e.g. Oosterbeek and Webbink 2007). In this paper, we utilized a relatively new policy reform from 1999 in Poland, and argued that it is important to increase the length of general education (that is, investment in the general skills of students) and not the length of vocational education. Contrary to several reforms, which added year(s) to the end of compulsory education, this Polish reform increased the length of general education by one year while decreasing the length of tracked upper-secondary education by one year for most students. The 1999 reform of the Polish education system has already been shown to increase the general skills of students, mainly through the increase of test-scores of students at the bottom end of the distribution (Jakubowski et al. 2016; Jakubowski 2015). However, no one had ever assessed its long-term labour market impacts. Similarly to two reforms during the 1950's in Sweden and the 1970's in Finland Poland has, among other things, decreased selection, lengthened compulsory schooling for some and imposed a national curriculum on schools. These changes in the education system of the Scandinavian countries have been shown to decrease inequality by increasing the earnings of the lower status people (see Meghir and Palme 2005; and Pekkarinen et al. 2009).

Our results are in line with the Scandinavian results. Using difference-in-difference estimates, we find that the 1999 reform in Poland was successful in the long-run. Post-reform cohorts are more likely to be employed, and they also earn higher wages. The reform had an impact on the composition of the highest educational attainment as well. However the overall average effects are likely to be driven by the young and by the lowest educated. This suggests that the reform has reached its initial goal of decreasing inequalities.

While this study can show little about the potential mechanisms driving the results, we speculate that the increased general education along with decreased tracking caused the effects. After 1999 students in Poland were forced to sit an additional year in less selected classes than before and be taught by teachers that were less selected. This change was likely to be beneficial for the (previously) low-track children, as their composition of peers and teachers was substantially improved.

All in all we speculate that increasing general education in exchange for vocational education was the crucial step in this reform, and this is why we see 3% differences in employment changes and real wages between the pre- and post-treatment cohorts for the full sample of respondents, and a higher than 5% increase in employment chance and wage for the lowest educated.

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10. APPENDIX

Table A1.

Summary of independent variables

Variable	Obs	Mean	Std. Dev.	Min	Max
year of birth	48557	1985.7	3.60	1978	1994
female	48557	0.48	0.50	0	1
experience	26697	2.27	2.26	0	12
age when the first job began	23186	20.28	2.47	8	27
age	48557	22.87	2.57	19	27
level of educ.: low	43057	0.18	0.39	0	1
level of educ.: medium	43057	0.65	0.48	0	1
level of educ.: high	43057	0.17	0.37	0	1
treated	48557	0.52	0.50	0	1
gross real wage (PLZ)	16762	1473	795	15	16939
Labour market status					
at work	48557	0.44	0.50	0	1
unemployed	48557	0.12	0.33	0	1
retired	48557	0.00	0.03	0	1
inactive	48557	0.43	0.50	0	1

Table A2

Number of observations in each age/year-of-survey cell

	year of survey									
age	2005	2006	2007	2008	2009	2010	2011	2012	2013	Total
19	929	814	679	657	626	586	532	497	529	5,849
20	843	821	732	623	572	569	538	499	485	5,682
21	860	730	732	655	523	528	498	511	464	5,501
22	926	773	687	655	546	539	518	496	486	5,626
23	812	801	665	658	573	491	503	469	424	5,396
24	746	708	732	618	582	523	495	489	420	5,313
25	790	644	618	640	558	515	494	479	467	5,205
26	708	676	587	555	567	534	499	495	465	5,086
27	675	599	603	531	488	527	515	501	460	4,899
Total	7,289	6,566	6,035	5,592	5,035	4,812	4,592	4,436	4,200	48,557

Table A3

Number of observations in each year-of-birth/year-of-survey cell

	year of survey									
year of birth	2005	2006	2007	2008	2009	2010	2011	2012	2013	Total
1978	675	0	0	0	0	0	0	0	0	675
1979	708	599	0	0	0	0	0	0	0	1,307
1980	790	676	603	0	0	0	0	0	0	2,069
1981	746	644	587	531	0	0	0	0	0	2,508
1982	812	708	618	555	488	0	0	0	0	3,181
1983	926	801	732	640	567	527	0	0	0	4,193
1984	860	773	665	618	558	534	515	0	0	4,523
1985	843	730	687	658	582	515	499	501	0	5,015
1986	929	821	732	655	573	523	494	495	460	5,682
1987	0	814	732	655	546	491	495	479	465	4,677
1988	0	0	679	623	523	539	503	489	467	3,823
1989	0	0	0	657	572	528	518	469	420	3,164
1990	0	0	0	0	626	569	498	496	424	2,613
1991	0	0	0	0	0	586	538	511	486	2,121
1992	0	0	0	0	0	0	532	499	464	1,495
1993	0	0	0	0	0	0	0	497	485	982
1994	0	0	0	0	0	0	0	0	529	529
Total	7,289	6,566	6,035	5,592	5,035	4,812	4,592	4,436	4,200	48,557

Table A4

Number of observations and row percentage of activity status and current education activity

	Current education activity			
basic activity status	not in education	in education	missing	Total
at work	13,958	4,450	3,047	21,455
%	65	21	14	100
unemployed	5,027	424	549	6,000
%	84	7	9	100
in retirement or early retirement	18	36	1	55
%	32.73	65.45	1.82	100
other inactive	3,219	15,996	1,832	21,047
	15.29	76	8.7	100
Total	22,222	20,906	5,429	48,557
	45.76	43.05	11.18	100