Opće i strukovno obrazovanje: pouke iz kvazi-eksperimenta u Hrvatskoj

Žilić, Ivan

Ivan Žilić

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Listopad . October 2016

Radni materijali EIZ-a . EIZ Working Papers

ekonomski institut, zagreb

Br . No EIZ-WP-1608

Radni materijali EIZ-a
EIZ Working Papers
EIZ-WP-1608

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Ivan Žilić
Research Assistant
The Institute of Economics, Zagreb
Trg J. F. Kennedyja 7
10000 Zagreb, Croatia
T. 385 1 2362238
F. 385 1 2335165
E. izilic@eizg.hr

www.eizg.hr

Zagreb, October 2016

IZDAVAČ / PUBLISHER:
Ekonomski institut, Zagreb / The Institute of Economics, Zagreb
Trg J. F. Kennedyja 7
10000 Zagreb
Croatia
T. 385 1 2362 200
F. 385 1 2335 165
E. eizagreb@eizg.hr
www.eizg.hr

ZA IZDAVAČA / FOR THE PUBLISHER:
Maruška Vizek, ravnateljica / director

Tiskano u 80 primjeraka
Printed in 80 copies

ISSN 1846-4238
e-ISSN 1847-7844

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General versus Vocational Education: Lessons from a Quasi-Experiment in Croatia

Ivan Zilic*

Abstract

This paper identifies the causal effect of an educational reform implemented in Croatia in 1975/76 and 1977/78 on educational and labor market outcomes. High-school education was split into two phases which resulted in reduced tracking and extended general curriculum for pupils attending vocational training. Exploiting the rules on elementary school entry and timing of the reform, we use a regression discontinuity design and pooled Labor Force Surveys 2000–2012 to analyze the effect of the reform on educational attainment and labor market outcomes. We find that the reform, on average, reduced the probability of having university education, which we contribute to attaching professional context to once purely academic and general high-school programs. We also observe heterogeneity of the effects across gender, as for males we find that the probability of finishing high school decreased, while for the females we do not observe any adverse effects, only an increase in the probability of having some university education. We explain this heterogeneity with different selection into schooling for males and females. Reform did not positively affect individuals' labor market perspectives; therefore, we conclude that the observed general-vocational wage differential is mainly driven by self-selection into the type of high school.

Keywords: general education, vocational training, reform.

JEL classification: I21, J24, P20.

Sažetak

U radu se analizira učinak obrazovne reforme implementirane u Hrvatskoj 1975./76. i 1977./78. godine na obrazovne ishode i ishode tržišta rada. Reforma je srednjoškolsko obrazovanje podijelila u dvije faze, što je rezultiralo kasnijim odvajanjem u strukovna odjeljenja te proširenjem općeg kurikuluma za učenike koji pohađaju strukovno obrazovanje. Koristeći pravila o dobi kretanja u osnovnu školu, vrijeme implementacije reforme te Anketu o radnoj snazi 2000.–2012., pomoću regresije diskontinuiteta ispitujemo efekte reforme na obrazovne ishode i ishode na tržištu rada. Rezultati ukazuju kako je reforma, u prosjeku, smanjila vjerojatnost da osoba ima završeno visoko obrazovanje, što objašnjavamo dodavanjem para-profesionalnog konteksta na nekoć opće srednjoškolske programe (gimnazije). Također, primjećujemo heterogenost učinaka prema spolu. Dok je vjerojatnost završavanja srednje škole za muškarce smanjena, što objašnjavamo visokom stopom nezavršavanja prve faze, za žene ne pronalazimo nikakve negativne učinke, samo povećanje vjerojatnosti nastavka obrazovanja nakon srednje škole. Ovu heterogenost s obzirom na spol objašnjavamo drugačijom selekcijom u obrazovanje za dječake i djevojčice. Reforma nije pozitivno utjecala na perspektivu pojedinca na tržištu rada pa zaključujemo da su razlike u ishodima na tržištu rada uvjetovane neopazivim karakteristima koje utječu na odabir vrste obrazovanja.

Ključne riječi: opće obrazovanje, strukovno obrazovanje, reforma.

JEL klasifikacija: I21, J24, P20.

*Research assistant at The Institute of Economics, Zagreb and a PhD student at the Johannes Kepler University in Linz. E-mail: zilicivan@gmail.com. I thank Rudolf Winter-Ebmer for numerous comments and guidance. I also thank Nicole Schneeweis, seminar participant at EIZ Zagreb, JKU Linz, ZEW Mannheim, NTNU Trondheim, WU Vienna, YES Dubrovnik, CERGE-EI Prague and EALE Ghent. This research was supported by a grant from the CERGE-EI Foundation under a program of the Global Development Network. All opinions expressed are those of the author and have not been endorsed by CERGE-EI or the GDN.

1 Introduction

The debate on general versus vocational education has been an important part of policy makers' and academics' agenda. As both educational systems have their benefits, there exists a well-known general-vocational trade-off. In particular, skills acquired by vocational training may ease the transition into the labor market, but may become obsolete at a faster rate; while general education gives access to broader knowledge that can serve as a sound basis for subsequent learning and specialization (Hanushek et al., 2016). Verhaest and Baert (2015) characterize general versus vocational schooling as a trade-off between lower risk of bad match persistence later on, and higher employment chance and better match at the start of the career.

Some authors claim that general education is especially important for the fast-changing economy, as individuals can change occupations and adapt new technologies more quickly (Goldin, 2001; Hanushek et al., 2016). Adopting this view suggests that a more general education should pay a labor market premium in transition and post-transition countries. With the fall of socialism and the establishment of market-oriented economies in the 1990s, countries of the Eastern Bloc went through profound institutional and political changes. The economy was affected drastically as business activities turned to different sectors and technologies which translated into different sets of skills required on the labor market. Was a more general education beneficial for individuals in this changing age?

Answering these questions is not an easy task as educational choice suffers from self-selection – comparing labor market outcomes of individuals with general and vocational education would reflect unobserved differences across individuals making the estimates biased (Ryan, 2001).

To shed some light on this matter, in this paper we identify the causal effect of a comprehensive high-school reform implemented in 1975/76 and 1977/78. In particular, high-school education was split into two phases – the first phase, two years of general curriculum common to all students regardless of the school enrolled, and a second phase, which prepared students for a particular profession. This introduced two novelties. Firstly, extra-educational decision as to where to continue second phase was introduced; therefore, separation into vocational tracks was postponed, i.e. tracking was reduced. Secondly, individuals could not enter a vocational school directly after an eight-year compulsory elementary school – instead, they needed to attend two additional years of general education. By exploiting elementary school age entry rules and the timing of the implementation of reform we are able to use regression discontinuity design on pooled Labor Force Surveys

2000–2012.

We test whether reduced tracking affected the highest educational attainment, years of schooling and the field of study. We also analyze if two extra years of general curriculum affected labor market prospects of nongymnasium high-school graduates in terms of wages, years of employment, probability of being unemployed as well as the probability of being inactive.

Results indicate that the reform, on average, reduced the probability of having university education. The estimated negative effect is varying from 2.7 to 5.5 percentage points. We argue that this effect came from attaching paraprofessional and vocational context to once general programs. In the old system, gymnasiums were perceived as a preparation for university education, while in the reformed system, gymnasiums *de facto* existed, but they were associated with some vocation, making graduates of general programs employable. This interpretation is supported by the drop in university enrollment rates.

We also observe different effects across gender. For male pupils we find that the probability of finishing only elementary school increased, which indicates a high incidence of first-phase dropouts. The first phase was mostly general curriculum, which might have been a challenge for low-ability pupils who would otherwise be able to finish a three-year vocation school. Also, like in the whole sample, we observe a drop in the probability of having a university education.

On the other hand, we do not find any adverse effects for females. The only significant effect is an increase in the probability of attending some university education. We argue that this heterogeneity in the reform effects is driven by a different selection into schooling across genders. While most of the males could go to school, due to informal barriers, such as gender and family roles, a significantly lower portion of females enrolled secondary schooling. We argue that these informal barriers selected more-able females into schooling who had no problems finishing the first phase, and were actually motivated to continue education after high school. We also observe that a portion of females shifted from teacher and health care education into social sciences.

Restricting our sample on nongymnasium high-school graduates, we find that the two additional years of general education did not positively affect individuals' labor market prospects. This lack of premium on more general education is surprising, given the potential upward bias of the estimates. In particular, as the reform caused a drop in the probability of finishing a university, the nongymnasium high school sample contains different ability distributions before and after the reform. We conclude that the observed general vocational wage differential is mainly driven by self-selection into the type of high school.

This paper contributes to the empirical literature on nexus between more years of general education and labor market outcomes. For example, Hanushek et al. (2016), using difference-in-differences approach and pooling individuals from 11 countries, provide results that support the general-vocational education trade-off as they find that individuals with general education do initially have worse employment outcomes, but their perspective improves as they get older. On the other hand, papers that rely on quasi-experimental evidence contrast these results. Using educational reform in the 1970s in Romania, Malamud and Pop-Eleches (2010) find that more years of general education did not affect labor market participation and earnings. Oosterbeek and Webbink (2007), analyzing the reform of the Dutch vocational schools, also find no evidence on premium on more general years of schooling. Analyzing a pilot scheme administrated in Sweden that introduced more comprehensive upper secondary education, Hall (2012) finds no effect of more general education on university enrollment and earnings, as well as no evidence that attending general education reduces unemployment risk during the 2008-2010 crisis (Hall, 2013).

The rest of the paper is organized as follows: section 2 explains the educational reform in Croatia, section 3 explains the methodology and data, section 4 presents the results, while section 5 the conclusion.

2 Educational reform in Croatia

Prior to the reform in the 1970s, education in Yugoslavia, and hence Croatia, was regulated at the federal level by the General Law on Education from 1958. Children enrolled an eight-year compulsory elementary school, on average, at the age of seven. Upon the completion of elementary school, depending on their performance and aptitude, they could continue in one of the following secondary schools: gymnasium, art school, technical school, trade or vocational school, teacher's school and military secondary school. Duration of the secondary school depended on the type of the school, ranging from three years for trade or vocational schools for skilled workers to five years for teachers, but averaging around four years. After successfully finishing high school and earning a diploma, pupils could enroll into a higher educational institution or enter the labor market (Georgeoff, 1982).

On the tenth Congress of the League of Yugoslav Communists in 1974 the basis for the so-called “directed” education was established. The reform redesigned high-school education abolishing general secondary schooling (gymnasiums), making all secondary education vocation-oriented. In words of Stipe Šuvar, then Secretary of State for Education in Croatia, the educational system was flawed as: "*Homo faber* and *homo sapiens* are socially separated, alienated, opposed in the existence of different classes; and the primary purpose of education is to perpetuate these divisions... it has, in fact, been developed as a specific ritual which selects a small proportion of the population for the social elites, and places them on a pedestal which is inaccessible to the vast majority of the population." (Bacevic, 2016).

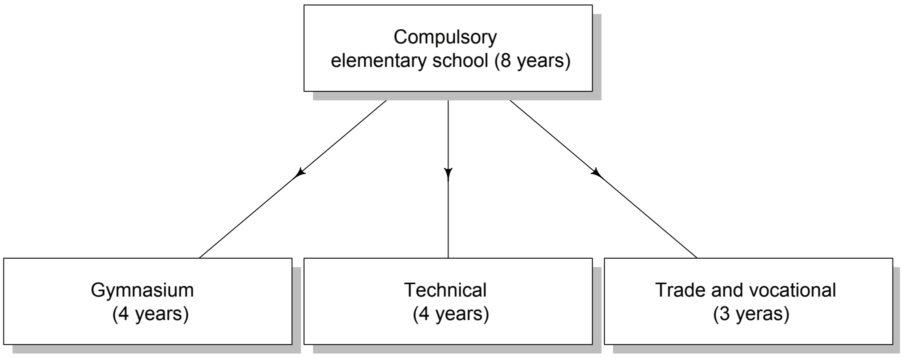
Therefore, the objectives of the reform were: (i) a more equal distribution of students from various socio-economic backgrounds enrolled in secondary schools of various types; (ii) a greater emphasis on the development of specific occupational skills with the goal of easier school to work transition; (iii) a promotion of greater equality of access to education and employment opportunities; and (iv) a closer integration of the schooling system with the needs of the social system and self-management (Obradović, 1986).

Under the new educational system, the high school was split into two phases, both administered at the so-called school centers. The first phase, which lasted for two years, was common for all students irrespective of the type of the secondary school they enrolled. The majority of the first-phase curriculum was general (85 percent, Obradović (1986)): official language, chemistry, biology, physics, geography, mathematics and history. Selection into the first phase was based on elementary school performance. Upon the completion of the first phase, students could enter the labor market or continue to the second phase. The second phase was designed to provide vocational preparation. In total, programs for 36 professions and more than 350 occupations were available (UNESCO, 1984), and programs lasted for one or two years. All students who completed the first-phase could apply for any of the second phase programs, but selection was based on the grades from the first phase. All high schools were renamed as school centers associated with some vocation. For example, mathematical gymnasium was renamed the school center for mathematics and informatics, so programs for general education were still *de facto* available but were given a vocational or paraprofessional context. For example, upon finishing the school center for mathematics and informatics a person would get a vocation titled "technician for mathematics and natural sciences".

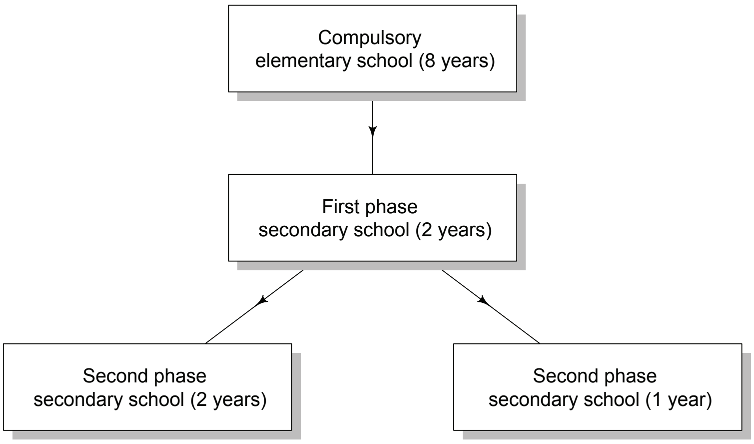
The first phase of the new high-school system was implemented in all secondary education in Croatia in the school year 1975/76, for the high-school freshmen, while the second phase was implemented for the same cohort in the school year 1977/78 (UNESCO, 1977). Stylized representation of the reform is depicted in Figure 1.

Figure 1: Changes in high-school education in Croatia during the 1975/76 and 1977/78 reform

(a) Before the 1975/76 and 1977/78 reform



(b) After the 1975/76 and 1977/78 reform



Before highlighting the differences between the reformed and the old schooling, we stress the things that did not change. Firstly, elementary schools remained the only compulsory education. Secondly, selection procedure into the next phase of education remained the same – it was based on performance in the last two years of schooling. This implies that pupils in the first phase, like in the prereform high schools, were homogeneous in ability. And lastly, all the educational resources, including teachers and buildings were the same as the new high schools were merely renamed school centers.

The reform did introduce a few important changes. Firstly, an additional educational decision was introduced into the schooling system – a decision of where to continue schooling after the first phase. Both phases could have been attended at the same school center, but pupils could also change the school center after the first phase. Since the first phase consisted of a general curriculum, pupils were able to make their educational choice two years later, which implies later separation into vocational tracks. For example, an individual who set their mind on becoming a carpenter would, in a old educational system, make that decision after eight years of elementary school by enrolling a three-year vocational school. In the reformed system, an individual could decide to become a carpenter, enroll the first phase, but could then, after being exposed to general subjects, change his/her mind and apply for a different vocational program.

The second change was that the pupils were prevented from entering vocational training straight after elementary school. Instead, they needed to go through two additional years of general education before specializing for a particular vocation. This implied that, for example, an individual who would have enrolled a three-year vocational program before the reform, would have had eight years of general education, while the same person in the same vocational program after the reform would have had ten years of general education (the discontinuity in the years of general education is depicted in Figure 2).

3 Methodology and data

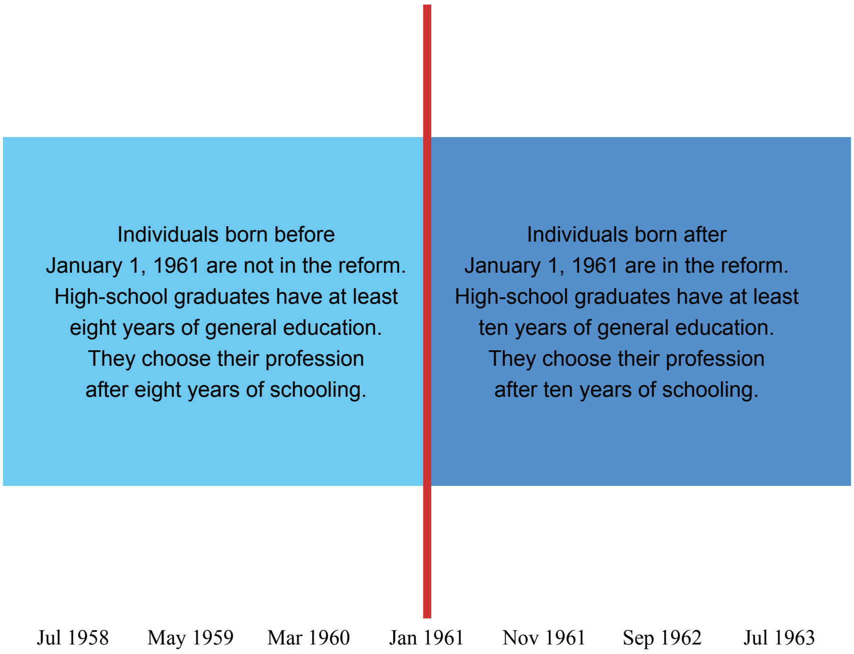
3.1 Methodology

Bennell (1996) claims that the majority of studies, which assess general and vocational education in developing countries disregard the issues of selection bias. To circumvent the self-selection nature of an educational choice, and hence bias ordinary least squares estimates, we exploit the high-school educational reform. The first stage of the reform was implemented in the academic year 1975/76 for high-school freshmen. We combine the timing of the reform, the date of birth and rules for elementary school entry to construct an indicator if the person was included in the reform. In particular, we identify an individual born on January 1, 1961 as an individual who was marginally included in the reform. Figure 2 depicts discontinuity in the reform inclusion.

This framework enables us to use regression discontinuity design (RDD), introduced into the economics literature by Thistlethwaite and Campbell (1960), where the date of birth of each individual is used to construct an assignment variable that discontinuously determines the reform inclusion. Suppose c_i is the distance, in weeks, between individual's i birth date and January 1, 1961, and let $AFTER_i = \mathbb{1}\left[c_i \geq 0\right]$, i.e. an indicator taking value 1 if individual i was born after January 1, 1961. In order to estimate the effects of the reform on educational attainment and labor market outcome y_i, we estimate:

$$y_i = \beta' X_i + f(c_i) + \delta AFTER_i + \nu_i \tag{1}$$

Figure 2: Discontinuity in the reform inclusion



where X_i is a vector of controls (intercept and predetermined variables, such as gender and nationality), $f(c_i)$ a function of an assignment variable and δ is a causal parameter of interest.

We analyze the effects of the reform with two sets of outcome variables. First, by using all individuals born within a certain time frame, we analyze the effects of the reform on the highest educational attainment, years of schooling and field of education. We do so to explore whether the additional educational decision and hence the reduced tracking affected schooling outcomes. Next, by using only nongymnasium high-school graduates, we explore whether more years of general education provided labor market premium in terms of wages, years of work, the probability of being employed and the probability of being inactive. We do so as the reform can be interpreted as an extension of the general part of curriculum in vocational schools.

In order to avoid misinterpreting nonlinearities around the cutoff as discontinuities, caution regarding functional form of $f(c_i)$ is advised (Angrist and Pischke, 2008). Following Lee and Lemieux (2010), we estimate equation (1) semi-parametrically using different *ad hoc* bandwidths around the cutoff date and modeling $f(c_i)$ using polynomials of different order.

Regression discontinuity designs rely on the assumption that individuals cannot precisely manipulate their assignment variable and thus completely control their inclusion into the treatment (Lee and Lemieux, 2010). As the educational reform was announced in 1974 and implemented in the academic year 1975/76, and the assignment variable is predetermined, it seems rather implausible that individuals could manipulate the inclusion into the reform. Nevertheless, we perform the sorting test from McCrary (2008) to see whether grouping of individuals one side of the cutoff is present. Results indicate no sorting, so we conclude that the reform did randomly split the population and thus can be viewed as a quasi-experiment.

Relationship between the assignment variable and treatment status might not be deterministic, there might be noncompliers – individuals who should have been, based on the date of birth, included in the reform, but were not, and *vice versa*. Given that we do not have access to information of whether an individual was indeed included in the reform, we cannot exploit the assignment variable as an instrument for reform participation so our analysis should be viewed as intention-to-treat effect. Noncompliance is a threat to our identification only if the pattern of noncompliance discontinuously changes with the threshold, which we, given the short lag between announcement of the reform and actual implementation, view implausible.

3.2 Data

Data are obtained by pooling 2000–2012 versions of the Croatian Labor Force Survey (LFS), which contains basic demographic characteristics, labor market outcomes, education variables, and, importantly, date of birth which we use to construct an assignment variable for the regression discontinuity design. We do not observe if an individual was actually included in the reform, so we cannot resort to instrumental variable estimation. We also do not capture information on the school center an individual attended and whether the individual changed the school center between the two phases.

Table 1 presents descriptive statistics of pooled data. Note that we restrict the sample to individuals born within three years around the cutoff date of January 1, 1961. We do so to restrict sample to cohorts that cope with similar labor market conditions upon finishing education. The left panel of Table 1 displays cohort-restricted data on the individuals with all educational attainments (N=22,374), which we use to explore whether the reform changed educational decisions and outcomes. Right panel of Table 1 displays cohort-restricted data on individuals with nongymnasium secondary education (N=12,787), which we use to analyze the effect of more years of general education on labor market outcomes.

Table 1: Descriptive statistics

	Whole sample (N=22,374)		High-school graduates (N=12,787)	
	Mean	Std. dev.	Mean	Std. dev.
Predetermined variables				
Female	0.460	0.498	0.423	0.494
Non-Croatian	0.081	0.273	0.082	0.274
Years of schooling				
$<$ 8 years	0.023	0.151	0	0
8 years	0.171	0.376	0	0
9 years	0.004	0.060	0	0
10 years	0.011	0.103	0	0
11 years	0.211	0.408	0.368	0.482
12 years	0.384	0.486	0.623	0.485
13 years	0.009	0.094	0	0
14 years	0.068	0.252	0	0
15 years	0.008	0.087	0	0
16 years	0.089	0.285	0	0
$>$ 16 years	0.023	0.150	0	0
Education level				
No elementary	0.023	0.150	0	0
Elementary	0.180	0.384	0	0
Vocational (3 years)	0.275	0.446	0.472	0.499
Vocational (4 years)	0.310	0.463	0.528	0.499
Gymnasium	0.029	0.169	0	0
Some university	0.075	0.263	0	0
University and more	0.108	0.311	0	0
Field of education*				
General programs	0.232	0.422	0	0
Teacher training	0.036	0.186	0.006	0.080
Humanities	0.010	0.098	0.006	0.074
Foreign languages	0.001	0.032	0.000	0.015
Social sciences	0.202	0.402	0.243	0.429
Life sciences	0.018	0.134	0.020	0.140
Biological sciences	0.001	0.030	0.000	0.018
Physical sciences	0.010	0.099	0.013	0.115
Mathematics	0.001	0.035	0.001	0.027
Computer science	0.003	0.055	0.003	0.050
Computer use	0.000	0.019	0.001	0.023
Engineering	0.318	0.466	0.478	0.500
Agriculture	0.028	0.165	0.034	0.181
Health care	0.053	0.223	0.059	0.235
Services	0.103	0.303	0.154	0.361
Labor market outcomes				
Log hourly wage	2.950	0.771	2.980	0.626
Years of work	23.000	6.030	23.400	5.630
Employed	0.790	0.407	0.824	0.380
Nonactive	0.014	0.118	0.014	0.115

Note: Both samples are restricted to individuals born between January 1, 1958 and January 1, 1964. Secondary education sample is restricted to nongymnasium high-school graduates. * question regarding the field of finished education is available in Labor Force Surveys 2004 onwards; sample size of the whole sample is N=16,629, while for the secondary education sample is N=9,460.

4 Results

4.1 Reduced tracking and educational outcomes

In this section we present the effects of reduced tracking on the highest educational attainment, years of schooling, and the field of education. As can been seen from Figure 3, results indicate stable portions of different educational attainments before and after the reform. The biggest change was in the portion of people having some university education, and university education and more.

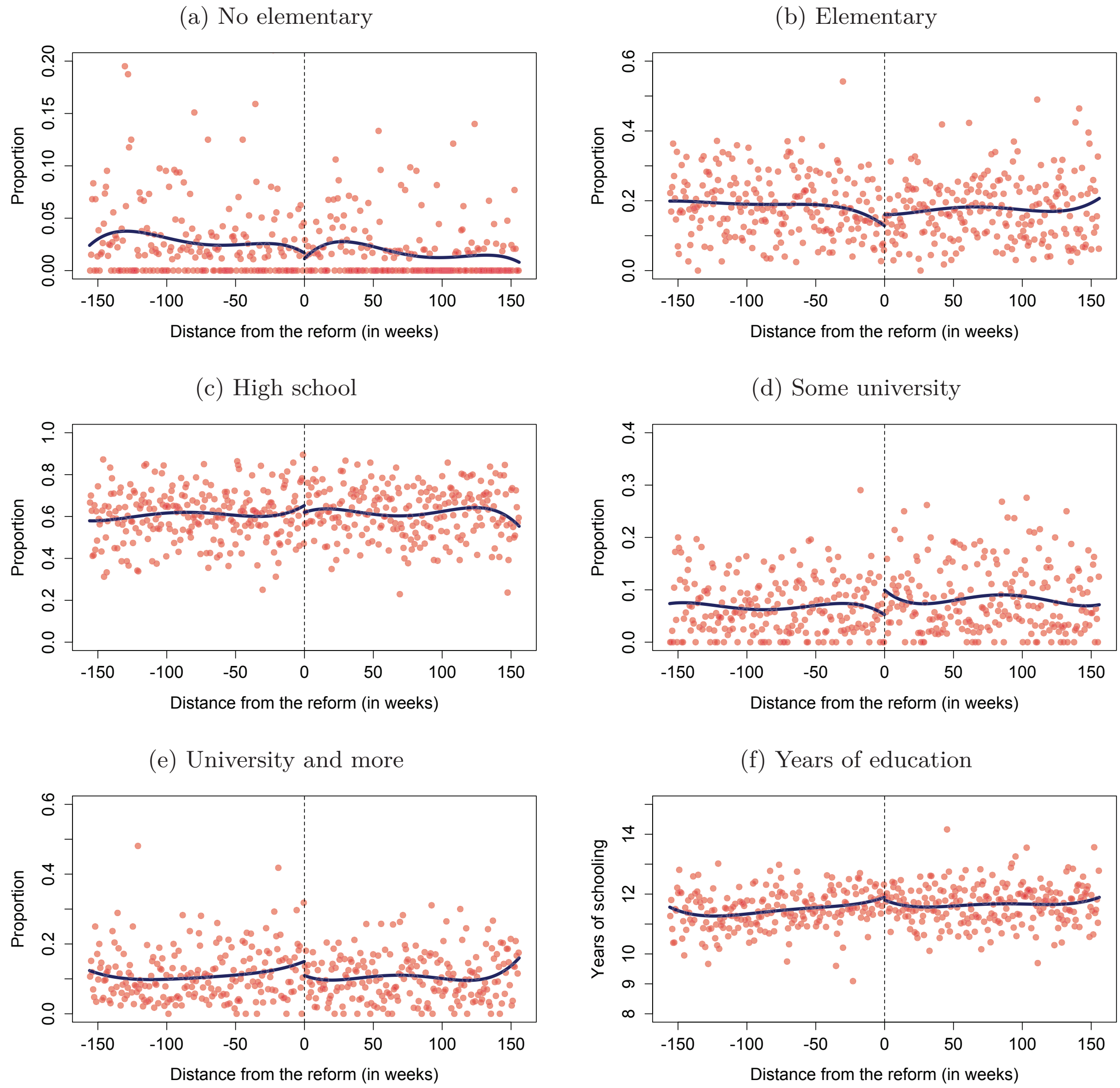
Table 2 provides a more comprehensive picture as it reports the results using different windows of observations, different specifications of $f(c_i)$ as well as statistical significance of the effects. The first column, where the indicator—if a person has no elementary school—is taken as an outcome, should be considered as a placebo test. The reform redesigned only high-school education so no effect should be found in this outcome. Therefore, the absence of a statistically significant effect in our results reinforces our identification strategy.

The second column shows no effect on the probability of finishing only elementary school implying that the introduction of general-curriculum first phase did not increase the incidence of high-school dropouts. For example, pupils that enrolled carpenter programs had to cope with the same general subjects as pupils in the physics programs so some first-phase dropouts should be expected. Indeed, Obradović (1986) reports that 27 percent pupils failed to complete the first phase, but it seems that the probability of finishing only elementary school did not change with the reform, at least not while analyzing all the pupils. By analyzing the results for the probability of finishing high school, we reach the same conclusion – the reform did not significantly change the portion of people with secondary education as the highest educational attainment. This holds also for the distribution of the types of high school (results omitted for brevity).

While the outcome of some university education is unaffected by the reform, the effect on finishing university education and more is negative and significant. The negative effect is varying from 2.7 and 5.5 percentage points, which corresponds to 2.5 percent and 5.1 percent of the sample mean. So why did the reform disincentivize pursuing university education? One explanation could be in attaching paraprofessional context to general programs and recognizing this profession on the labor market. In the old system, general high-school programs were perceived as preparation for universities, disregarding employability concerns. In the reformed system, general programs *de facto* existed but were associated with some profession, allowing them to be a final educational stop, not only a link between elementary and university education. This interpretation is supported by the drop of university enrollment rates – in the academic year 1975/76, 21.37 percent of age group 20–24 in the 1971 census was enrolled in the university, while in the academic year 1979/80, 19.25 percent of age group 20–24 in the 1981 census was attending university. In absolute terms, the number of students in the prereform 1975/76 fell from 78,511 to 69,858 in the postreform 1979/80 (Croatian Bureau of Statistics, 1980, 1993). These numbers support our interpretation that an observed drop in the probability of having is not caused by the inability to finish university, but lower university enrollment rates. The effect of the reform on years of education is negative but insignificant.

These conclusions are reinforced with the results presented in Table 3, where outcomes are years of schooling. The third column indicates that the reform reduced the probability of having 16 years of education between 2.5 and 5.2 percentage points, which corresponds to 2.8 percent and 5.8 percent of the sample mean. The outcome of more than 16 years of education is not affected, which implies that the probability of finishing postgraduate studies did not change. The probability of having years of schooling associated with elementary and high school did not change. We also tested for every schooling year separately and found no significant change (results omitted for brevity).

Figure 3: Regression discontinuity graphs for the highest educational attainment



Note: Sample is restricted to individuals born from January 1, 1958 to January 1, 1964. Solid blue line represents the fourth order polynomial estimation of $f(c_i)$. Number of bins is chosen using an evenly-spaced mimicking variance method from Calonico et al. (2015).

Table 2: Results for the highest educational attainment

	Finished education					
	No elementary	Elementary	High school	Some university	University and more	Years of education
3 year window (N=22,374)						
Linear spline	0.003	0.002	0.004	0.018	-0.027^{**}	-0.111
	(0.006)	(0.016)	(0.021)	(0.012)	(0.014)	(0.116)
Quadratic spline	0.004	0.012	0.018	0.007	-0.040^{*}	-0.194
	(0.009)	(0.024)	(0.031)	(0.017)	(0.021)	(0.162)
Cubic spline	-0.006	0.012	0.024	0.025	-0.055^{*}	-0.102
	(0.009)	(0.031)	(0.040)	(0.022)	(0.028)	(0.206)
Quartic spline	-0.005	0.030	-0.029	0.047^{*}	-0.042	-0.109
	(0.011)	(0.037)	(0.050)	(0.027)	(0.035)	(0.260)
2 year window (N=15,065)						
Linear spline	0.005	-0.001	0.029	0.006	-0.040^{**}	-0.177
	(0.008)	(0.019)	(0.025)	(0.014)	(0.017)	(0.134)
Quadratic spline	-0.004	0.019	0.0003	0.028	-0.043^{*}	-0.083
	(0.009)	(0.028)	(0.037)	(0.021)	(0.026)	(0.188)
Cubic spline	-0.006	0.029	-0.001	0.034	-0.056	-0.214
	(0.011)	(0.037)	(0.049)	(0.026)	(0.034)	(0.254)
Quartic spline	-0.002	-0.002	0.006	0.025	-0.026	0.051
	(0.016)	(0.043)	(0.064)	(0.030)	(0.045)	(0.321)
1 year window (N=7,480)						
Linear spline	0.002	0.016	0.013	0.020	-0.052^{**}	-0.214
	(0.009)	(0.027)	(0.035)	(0.019)	(0.024)	(0.176)
Quadratic spline	-0.009	0.006	-0.001	0.035	-0.030	0.044
	(0.012)	(0.038)	(0.052)	(0.028)	(0.037)	(0.267)
Cubic spline	-0.015	0.038	-0.007	0.024	-0.041	-0.045
	(0.017)	(0.044)	(0.074)	(0.031)	(0.055)	(0.372)
Quartic spline	-0.027	0.056	-0.024	0.043	-0.049	-0.053
	(0.018)	(0.047)	(0.094)	(0.037)	(0.072)	(0.447)

Note: Standard errors clustered at the week of birth are in the brackets. Each cell represents different regression and presents the coefficient on variable AFTER which takes value 1 if the individual was born after January 1, 1961, and 0 otherwise. Window width denotes ± years around the cutoff date. Covariates include female and non-Croatian dummy as well as dummies for the survey years.

Significance levels: *p<0.1; **p<0.05; ***p<0.01

Table 3: Results for the years of schooling

	Years of schooling			
	8 years or less	10, 11 or 12 years	16 years	More than 16
3 year window (N=22,374)				
Linear spline	0.002	0.012	−0.025*	−0.001
	(0.017)	(0.021)	(0.013)	(0.007)
Quadratic spline	0.015	0.021	−0.036*	0.0003
	(0.025)	(0.031)	(0.020)	(0.010)
Cubic spline	−0.009	0.040	−0.052**	0.003
	(0.031)	(0.040)	(0.027)	(0.014)
Quartic spline	0.019	−0.020	−0.043	0.008
	(0.038)	(0.050)	(0.033)	(0.017)
2 year window (N=15,065)				
Linear spline	0.004	0.034	−0.035**	−0.002
	(0.020)	(0.025)	(0.016)	(0.008)
Quadratic spline	0.001	0.015	−0.042*	0.004
	(0.028)	(0.036)	(0.024)	(0.013)
Cubic spline	0.018	0.002	−0.055*	0.004
	(0.038)	(0.049)	(0.033)	(0.017)
Quartic spline	−0.007	0.005	−0.027	0.012
	(0.044)	(0.063)	(0.044)	(0.022)
1 year window (N=7,480)				
Linear spline	0.012	0.019	−0.049**	0.001
	(0.027)	(0.035)	(0.023)	(0.012)
Quadratic spline	−0.012	0.010	−0.032	0.009
	(0.039)	(0.052)	(0.035)	(0.018)
Cubic spline	0.017	−0.017	−0.042	0.014
	(0.047)	(0.072)	(0.053)	(0.027)
Quartic spline	0.029	−0.034	−0.058	0.031
	(0.053)	(0.093)	(0.072)	(0.034)

Note: Standard errors clustered at the week of birth are in the brackets. Each cell represents different regression and presents the coefficient on variable AFTER which takes value 1 if the individual was born after January 1, 1961, and 0 otherwise. Window width denotes ± years around the cutoff date. Covariates include female and non-Croatian dummy as well as dummies for the survey years.

Significance levels: *p<0.1; **p<0.05; ***p<0.01

4.1.1 Heterogeneous effects

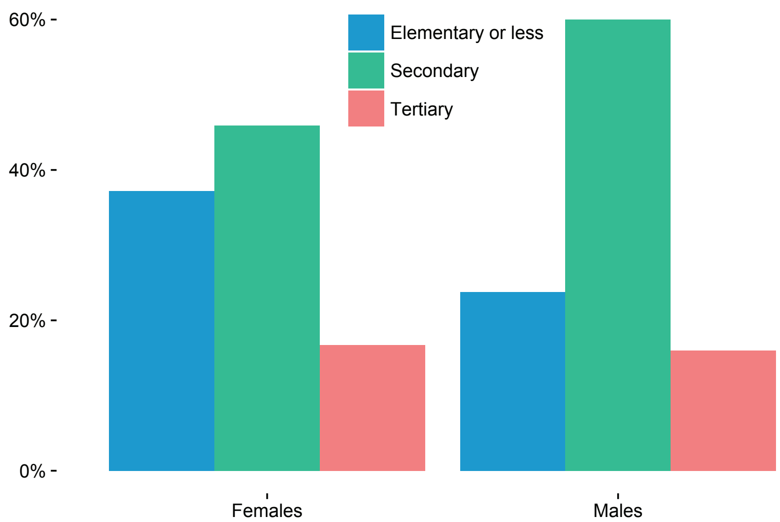
So far we have established that the reform, when analyzing all individuals, reduced the probability of having university level education. Next, we turn to differences of these effects across gender. In terms of predetermined variables we only have access to two – nationality and gender. Everything else could be affected by the reform so we avoid conditioning on potentially endogenous covariates. Given that in the sample we have only 8.1 percent of non-Croatians, we turn to gender-heterogeneous effects.

Table 4 presents the results for males. Results indicate that the probability of finishing only elementary school significantly increased as most of the specifications turn up with the significant results. This adverse effect is in line with the interpretation of the high rate of first-phase dropouts. Pupils who could have finished vocational school in the old system had to pass gymnasium-like first phase in the reformed one, which resulted in a high rate of first-phase dropouts. This is supported by the fact that the ratio of pupils continuing education after elementary school is fixed before and after the reform at around 92 percent (Croatian Bureau of Statistics, 1978). There is also evidence that male pupils were disincentivized to attend university as the negative effect on the probability of having university education is significant in few specifications. These two effects result in the drop in total years of education for males, and the adverse effect is ranging from 0.303 to 0.496 years.

Empirical evidence in Table 5, where we present the results for females, is in quite a contrast with the overall results and the results for males. The reform did not change the probability of finishing elementary, high school or university or more. It did, however, positively affect the probability of having some university education.

What drives these gender heterogeneous effects? Males had problems finishing the first phase and did not enroll university to such an extent, while for the females we actually observe an increase in the attendance of some university education. We provide a different selection into high school as a cause of this gender heterogeneity in the reform effects. As it can be seen from Figure 4, which displays distribution of education in 2011 for individuals older than 15 years of age (therefore including cohorts born from roughly 1930 to 1996), in the years prior to the reform, the distribution of education was different for males and females. More than a third of females had finished only an elementary school – 37.2 percent, while 45.9 percent and 16.7 percent had finished secondary and university education, respectively. On the other hand, 23.8 percent of males had finished only elementary school, while 60.0 percent and 16.0 percent had finished secondary and university education (Croatian Bureau of Statistics, 2015).[1] In 1971, 13.74 percent of females aged 15–19 were high-school freshmen, while 15.84 percent of males aged 15–19 were high-school freshmen (Croatian Bureau of Statistics, 1978, 1993). This clearly indicates unequal access to secondary education across gender prior to the reform. While this is due to a number of socio-economic reasons, it does showcase that self-selection into secondary school was different for females. We argue that informal barriers in access to education, such as gender roles and family planning, actually filtered more-able females into secondary education. Asymmetric barriers to secondary education across genders resulted in different ability distributions – while almost all males could enroll high school, females were informally selected so only more relatively able ones continued. This could explain the heterogeneous reform effect – for males we observe an increase of elementary school as the highest attainment as males across the whole ability distribution could continue education, so for a portion of them general-curriculum first phase was problematic. On the other hand, only more-able females could continue education, so not only did they not have problems with the general first phase, it actually motivated them into pursuing further education.

Figure 4: Distribution of education by gender in 2011 for 15+ individuals



Analyzing the changes in the finished field of education supports this interpretation.[2] For males, we observe an increase of probability of finishing the general program, which is consistent with the increased probability of finishing only elementary school since elementary school is coded as

[1]The numbers do not sum up to 100 percent due to unknown educational attainment.
[2]Tables are omitted for brevity.

general education in the Croatian Labor Force Surveys. For females, we observe a significant drop in the probability of finishing teacher education and health care education and an increase in the probability of finishing social science programs. Therefore, extended exposure to general curriculum shifted a portion of females from teacher and nurse profession into social sciences.

Table 4: Results for the highest educational attainment – males

	Finished education					
	No elementary	Elementary	High school	Some university	University and more	Years of education
3 year window (N=12,080)						
Linear spline	0.001	0.003	0.020	0.013	-0.038^{**}	-0.175
	(0.008)	(0.020)	(0.028)	(0.014)	(0.018)	(0.139)
Quadratic spline	0.009	0.049^{*}	-0.018	0.012	-0.051^{*}	-0.440^{**}
	(0.011)	(0.029)	(0.045)	(0.021)	(0.030)	(0.206)
Cubic spline	0.0003	0.066^{*}	-0.028	0.016	-0.055	-0.424
	(0.012)	(0.039)	(0.064)	(0.028)	(0.041)	(0.283)
Quartic spline	-0.007	0.097^{**}	-0.055	0.025	-0.060	-0.547
	(0.016)	(0.047)	(0.083)	(0.035)	(0.054)	(0.383)
2 year window (N=8,078)						
Linear spline	0.008	0.017	0.012	0.011	-0.048^{**}	-0.303^{*}
	(0.010)	(0.023)	(0.036)	(0.017)	(0.024)	(0.169)
Quadratic spline	0.003	0.073^{**}	-0.047	0.021	-0.050	-0.461^{*}
	(0.012)	(0.036)	(0.059)	(0.026)	(0.037)	(0.256)
Cubic spline	-0.008	0.074	-0.024	0.017	-0.059	-0.459
	(0.016)	(0.047)	(0.080)	(0.034)	(0.053)	(0.375)
Quartic spline	-0.021	0.043	0.038	-0.012	-0.048	-0.219
	(0.020)	(0.056)	(0.103)	(0.041)	(0.074)	(0.525)
1 year window (N=4,045)						
Linear spline	0.007	0.069^{**}	-0.047	0.027	-0.056	-0.496^{**}
	(0.011)	(0.034)	(0.054)	(0.025)	(0.034)	(0.233)
Quadratic spline	-0.014	0.051	0.013	-0.004	-0.046	-0.301
	(0.016)	(0.049)	(0.084)	(0.035)	(0.058)	(0.408)
Cubic spline	-0.046^{**}	0.046	0.093	-0.026	-0.066	-0.056
	(0.022)	(0.055)	(0.121)	(0.047)	(0.084)	(0.600)
Quartic spline	-0.029	0.139^{**}	-0.032	-0.029	-0.048	-0.452
	(0.029)	(0.054)	(0.136)	(0.062)	(0.103)	(0.769)

Note: Standard errors clustered at the week of birth are in the brackets. Each cell represents different regression and presents the coefficient on variable AFTER which takes value 1 if the individual was born after January 1, 1961, and 0 otherwise. Window width denotes ± years around the cutoff date. Covariates include female and non-Croatian dummy as well as dummies for the survey years.

Significance levels: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 5: Results for the highest educational attainment – females

	Finished education					
	No elementary	Elementary	High school	Some university	University and more	Years of education
3 year window (N=10,294)						
Linear spline	0.006	−0.001	−0.014	0.025	−0.016	−0.041
	(0.010)	(0.026)	(0.034)	(0.017)	(0.021)	(0.190)
Quadratic spline	−0.001	−0.032	0.062	0.001	−0.030	0.078
	(0.014)	(0.034)	(0.049)	(0.025)	(0.030)	(0.246)
Cubic spline	−0.014	−0.058	0.092	0.035	−0.055	0.297
	(0.014)	(0.043)	(0.064)	(0.030)	(0.042)	(0.294)
Quartic spline	−0.003	−0.055	0.008	0.073^{**}	−0.023	0.432
	(0.015)	(0.048)	(0.076)	(0.035)	(0.052)	(0.364)
2 year window (N=6,987)						
Linear spline	0.003	−0.023	0.049	0.001	−0.031	−0.037
	(0.013)	(0.029)	(0.040)	(0.021)	(0.025)	(0.216)
Quadratic spline	−0.012	−0.054	0.063	0.037	−0.033	0.390
	(0.014)	(0.039)	(0.058)	(0.028)	(0.038)	(0.270)
Cubic spline	−0.004	−0.032	0.034	0.055	−0.052	0.103
	(0.014)	(0.047)	(0.074)	(0.035)	(0.052)	(0.353)
Quartic spline	0.023	−0.060	−0.029	0.068^{*}	−0.003	0.332
	(0.022)	(0.051)	(0.089)	(0.037)	(0.066)	(0.436)
1 year window (N=3,435)						
Linear spline	−0.003	−0.052	0.089	0.014	−0.047	0.150
	(0.014)	(0.039)	(0.057)	(0.027)	(0.036)	(0.266)
Quadratic spline	−0.002	−0.051	−0.015	0.083^{**}	−0.015	0.440
	(0.015)	(0.050)	(0.079)	(0.035)	(0.053)	(0.370)
Cubic spline	0.024	0.025	−0.121	0.084^{**}	−0.012	−0.054
	(0.025)	(0.054)	(0.102)	(0.035)	(0.078)	(0.471)
Quartic spline	−0.024	−0.060	−0.003	0.133^{***}	−0.047	0.499
	(0.024)	(0.078)	(0.133)	(0.045)	(0.098)	(0.596)

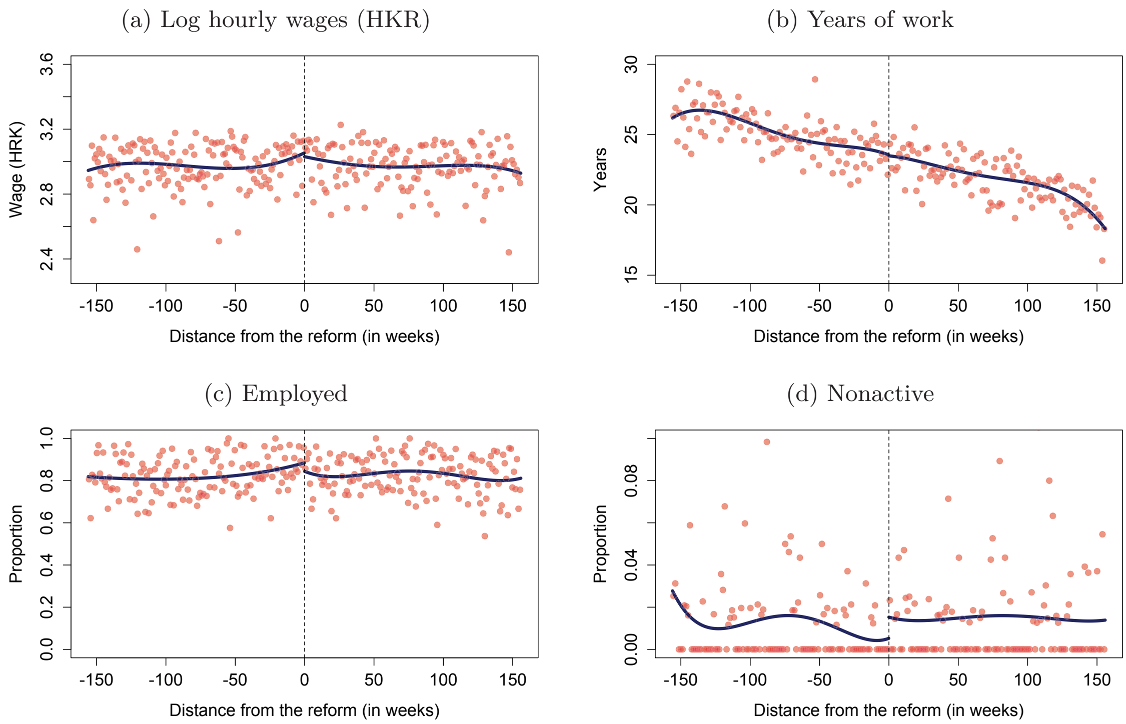
Note: Standard errors clustered at the week of birth are in the brackets. Each cell represents different regression and presents the coefficient on variable AFTER which takes value 1 if the individual was born after January 1, 1961, and 0 otherwise. Window width denotes ± years around the cutoff date. Covariates include female and non-Croatian dummy as well as dummies for the survey years.

Significance levels: *p<0.1; **p<0.05; ***p<0.01

4.2 Extended general curriculum and labor market outcomes

In this section we restrict the sample to nongymnasium high-school graduates and analyze the effect of more years of general education on labor market outcomes for both genders. Figure 5 and Table 6 contain the results. Note that we actually condition on an endogenous covariate. As results in Table 2 suggested that, as the probability of finishing university education reduced, the sample of high-school graduates changed with the reform. Our estimates are most likely upward biased, as high-school graduates sample contains individuals who would have finished university in the old system. We therefore interpret these results as the upper bound of the effect of extended general curriculum on the labor market outcomes.

Figure 5: RDD graphs for the labor market outcomes



Note: Sample is restricted to individuals born from September 1, 1958 to September 1, 1964. Solid blue line represents the fourth order polynomial estimation of $f(c_i)$. Number of bins is chosen using an evenly-spaced mimicking variance method from Calonico et al. (2015).

Even having this in mind, results reveal no labor market premium on more years of general education. Wages and years of work are not affected by the reform, while there is a significant adverse effect on the probability of being employed and nonactive. This lack of premium on more general education is surprising, given the potential upward bias of the estimates. This absence of positive effect of general curriculum is in line with other research which relies on quasi-experimental evidence [3], so we reinforce their interpretation that the observed general vocational wage differential is mainly driven by self-selection into the type of high school.

[3] For example Malamud and Pop-Eleches (2010), Oosterbeek and Webbink (2007), Hall (2012) and Hall (2013).

Table 6: Labor market outcomes

	Labor market outcomes			
	Log hourly wages	Years of work	Employed	Nonactive
3 year window (N=12,677)				
Linear spline	0.010	0.337	−0.016	0.008
	(0.030)	(0.234)	(0.020)	(0.005)
Quadratic spline	0.009	−0.230	−0.063**	0.009
	(0.038)	(0.346)	(0.028)	(0.006)
Cubic spline	−0.008	−0.298	−0.067*	0.016**
	(0.048)	(0.444)	(0.037)	(0.008)
Quartic spline	−0.013	−0.089	−0.049	0.010
	(0.054)	(0.551)	(0.044)	(0.009)
2 year window (N=8,576)				
Linear spline	0.009	−0.080	−0.042*	0.011**
	(0.034)	(0.278)	(0.023)	(0.005)
Quadratic spline	0.007	−0.203	−0.063*	0.010
	(0.045)	(0.402)	(0.034)	(0.007)
Cubic spline	−0.035	−0.095	−0.059	0.017*
	(0.055)	(0.529)	(0.043)	(0.009)
Quartic spline	−0.005	−0.552	−0.063	0.012
	(0.064)	(0.728)	(0.049)	(0.011)
1 year window (N=4,358)				
Linear spline	−0.014	−0.288	−0.068**	0.012*
	(0.045)	(0.370)	(0.032)	(0.006)
Quadratic spline	−0.021	−0.205	−0.044	0.011
	(0.055)	(0.560)	(0.045)	(0.009)
Cubic spline	0.080	−0.164	−0.081*	0.020*
	(0.063)	(0.772)	(0.049)	(0.011)
Quartic spline	0.167**	0.815	−0.095*	0.018
	(0.071)	(0.854)	(0.052)	(0.015)

Note: Standard errors clustered at the week of birth are in the brackets. Each cell represents different regression and presents the coefficient on variable AFTER which takes value 1 if the individual was born after January 1, 1960, and 0 otherwise. In all specifications the sample is restricted to nongymnasium high school graduates. Window width denotes ± years around the cutoff date. Covariates include female and non-Croatian dummy as well as dummies for the survey years.

Significance levels: *p<0.1; **p<0.05; ***p<0.01

5 Conclusion

In this paper we identify the causal effect of an educational reform implemented in Croatia in 1975/76 and 1977/78 on educational and labor market outcomes. The reform redesigned mostly secondary education as the high school was split into two phases. The first phase, which lasted for two years, was common to all students irrespective of the type of secondary school they enrolled, and contained mostly general curriculum. Upon the completion of the first phase, students could enter the labor market or continue to the second phase, which was designed to provide vocational preparation. Depending on the profession and occupation, the duration of the program was one or two years. General gymnasium-like programs were still available, but they were associated with some vocation or profession. The reform established few important changes – tracking was reduced and the general part of the curriculum was extended as individuals could not enter a vocational school directly after an eight-year compulsory elementary school, instead they needed to attend two additional years of general education.

We find that the reform, on average, reduced the probability of finishing university education. We argue the reason for this lies in the attachment of paraprofessional context to general programs, thus making graduates of once general programs employable after high school. When analyzing the effects across gender, we observe significant heterogeneity. For males we record an increase in the probability of finishing only elementary school which is driven by high first-phase dropout rates; we also observe a drop in the probability of having university as the highest educational attainment. For females, we do not observe any adverse effects. In fact, the probability of attending some university significantly increased. We argue that this heterogeneity in the reform effects is caused by a different selection into high school across genders. While high-school education was available for most of the males, informal barriers in access to education were still present, so more-able females were selected into high-school education. Therefore, exposing this selected sample of female pupils to more general subjects and the opportunity to change profession after two years, shifted a portion of them to some university education, and also shifted a portion of them form teacher and health care education to social sciences.

Restricting our sample to nongymnasium high-school graduates, we find that two additional years of general education did not significantly affect individuals' labor market prospects. This lack of premium on more general education is surprising, given the potential upward bias of the estimates. In particular, as the reform decreased the probability of finishing university, nongymnasium high-school graduates sample contains different ability distributions before and after the reform. These findings are in line with other studies that rely on quasi-experimental evidence on the effects of more general education.

From the policy perspective, this research is relevant as it displays unintended reform effects. One of the most important objectives of the reform was to give broader access to general and academic education. However, it resulted in increased incidence of high-school dropouts for males which is clearly opposed to its proclaimed objectives. Also, it showcases that general high-school curriculum itself is not explaining the long run labor market performance and that the observed general-vocational wage differential is mainly driven by self-selection into the type of high school. Therefore, it perpetuates the debate on not only how to combine academic and specific parts of education, but also how to implement such an optimal mix.

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ISSN 1846-4238
9 771846 423001