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Institute of Economic Studies

Jáchym Hercher

Impact of Educational Reforms in (former) Czechoslovakia

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Author: Jáchym Hercher
Supervisor: Doc. Daniel Münich, Ph.D.
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Autor: Jáchym Hercher
Institut: Institut ekonomických studií
Vedoucí práce: Doc. Daniel Münich, Ph.D.
E-mail vedoucího: daniel.munich@cerge-ei.cz

Abstrakt:
Ve své práci jsem se zabýval dopadem školských reforem v Československu, respektive České republice, mezi léty 1948 a 1996, které zaváděly a rušily devátou třídu. Dopad reforem jsem zkoumal prostřednictvím srovnávání příjmů mezi těmi, kteří reformou zasaženi byli, a těmi, kteří nebyli. K tomu jsem použil regresní nespojitost, která vznikla uplatňováním nových zákonů, a Mincerovu funkci lidského kapitálu. Pracoval jsem s daty ze šetření Životní podmínky (EU-SILC) pro roky 2006 až 2009 a také prostřednictvím vzdáleného přístupu s Informačním systémem o průměrném výdělku (SES). V závislosti na použité metodě jsem nalezl nulové, či mírně kladné rozdíly ve výnosech z nejvyššího dokončeného stupně vzdělání mezi absolventy devítiletého a osmiletého zákaldního vzdělání. Další výsledky potvrzovaly klesající mezní užitek z dalšího roku vzdělání.

Klíčová slova: vzdělávací reformy, Mincerova funkce, Česká republika, regresní nespojitost

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Author: Jáchym Hercher
Department: Institute of Economic Studies
Supervisor: Doc. Daniel Münich, Ph.D.
Supervisor's email address: daniel.munich@cerge-ei.cz

Abstract:
In this thesis I examine the impact of educational reforms in the former Czechoslovakia and the Czech Republic, which took place between 1948 and 1996, and changed the length of primary and lower secondary education. I do this by quantifying the effect the reforms had on the incomes of individuals using a regression-discontinuity design and the Mincer human capital function. I used EU SILC (Statistics on Income and Living Conditions) data for 2006-2009, and via remote access also the Structure of Earning Survey. Depending on the method used, I found zero or small positive changes in the returns to the highest achieved certificate. Furthermore, I found evidence of diminishing returns to an extra year of education.

Keywords: educational reforms, Mincer function, Czech Republic, regression discontinuity, EU-SILC

Thesis length: 77,468 characters

Declaration:

I hereby declare I have elaborated the Bachelor's Thesis on my own, and I have used only listed sources and references. Furthemore, I have written this thesis only for the purpose of achieving the Bachelor's degree at IES FSV UK. I agree with the lending and publishing of my thesis.

In Prague, 17^{th} May 2011 signature

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Contents

1 Introduction

During the last one hundred years, the lands of the Czech crown have several times undergone significant political turmoil. The extent to which these periods have been analyzed differs, but for no area is there still a larger gap in the literature than for the socialist period between 1948 and 1989. This is a huge missed opportunity to learn from recent history. Furthermore, in analyzing this period it is hard to find a more overlooked field than the educational policy of the socialist governments, as can be illustrated by the following excerpt from the comprehensive Czech publication in the field of pedagogy, the Encyclopedia of Pedagogy:

> "The current Czech educational historiography deals with the educational system before 1938 and earlier, but avoids evaluating education under the socialist rule. (...) The lack of an impartial scientific evaluation of the state and quality of the socialist education between 1948 and 1989 today often results (most significantly in the media) in only its black and white presentation." (Průcha, 2009)

This is understandable. Yet, as the lessons that can be learned are potentially of huge value for current policy decisions, I think more attention is needed.

In my work, I would like to address this deficiency, and by examining the impact of reforms in educational policies evaluate, what inference we can make concerning present policies. The reforms in question are the changes in length of primary and secondary education from eight to nine years (and vice versa)[1], which occurred several times during the socialist period.

The reasons for choosing this question as the topic of my thesis are twofold. The first reason is that it is an evergreen topic in the Czech educational policy debate. It is in the back of people's mind, policy makers and the general public alike, and then surfaces every now and then in the media. Most recently it was proposed as an anti-crisis measure by one of the advisors to the Czech Minister of Education. From the media, it is easy to get the impression that the debate lacks a certain degree of empirical, or quantitative, background, and it seems that this is not just an impression. While this work does by no means offer a clear answer to the question of what impact the extra year of education had, it does try to provide some of the above mentioned background important for the debate.

The qualitative discussion on the length of education has a great number of aspects. For illustration and context, I shall mention a few. From the pedagogical

[1]Because of terminological variability(Průcha, 1999, p. 67-68) it is useful to note that when referring to "primary and secondary education" in this text I mean primary and lower secondary education, i.e. the education taking part between the sixth and 14th or 15th year of age. According to the International Standard Classification of Education (ISCED) these are the levels ISCED 1 and ISCED 2A(Kalous, 2006, p. 18).

point of view the main topic is the impact another year has on the child's development - knowledge-wise and in psychological terms. Part of the discussion in this context is also how old the child should be before it is able to decide on the future course of its life, including choice of high school. From the financial perspective, the question is how much money can be saved by shortening education. Very rough estimates in the media talk about a reduction in number of students at one moment by up to 1/14 which would mean annual savings of several billion korunas. This is the idea behind the reduced length of education as an anti-crisis measure. From the social perspective this question is related to the current trends of child misbehavior. The growing age in which children are in school (combined with the current popular yearly delay in entering school) also leads to the issues of legal sex for pupils in lower secondary schools and legal alcohol consumption and driving during high school. Possible implication for other areas of life of the society, such as the impact on fertility, are also intriguing.

It must be noted that the lack of empirical grounding which manifests itself in public debate, while partially present in the academic sphere, does definitely not hold there. It seems, that estimating the impact of returns to different lengths of education is a topic which is not being given too much attention in the science of pedagogy (Průcha, 1999, p. 72), but in the field of educational economics (to which this thesis belongs, dealing with the financial returns to education) there exists a body of research addressing it. This is the second reason I chose this topic.

Estimation of returns to education is a classical topic of educational economics. Methodologically, various approaches and datasets have been used to try to estimate it, and with my thesis I add another item to this collection. My approach falls into the methodological group of natural experiments. According to Leigh and Ryan (Leigh and Ryan, 2008) we can further divide these approaches broadly into three categories. Either we use month of birth as an instrumental variable for education, utilizing its discontinuity on the basis of local legislation concerning mandatory schooling duration, or we make use of a change in educational laws which creates a discontinuity for the cohorts before and after the change, or we can have a sample consisting of identical twins. My work falls into the second category. By comparing data on cohorts, which were not hit by the reforms, and those which were hit, I can quantify the effects of the above mentioned reforms. The variable I evaluate was chosen because of its availability in the data, and the established relationship with expected general educational goals. Other possible promising approaches are mentioned in the Conclusion.

It is not necessary to stress, that investments into education are a very important policy topic. The 2011 budget of the Ministry of Education, Youth and Sports of the Czech Republic represents (at approximately 127 billion Korunas) almost eleven percent of the entire Czech state budget. Furthermore, based on the size of the Czech Republic and its lack of natural resources combined with its history of quality education it would seem that investing into human capital as our main asset is a natural course of action. Czech Republic as a “knowledge economy” is a frequent political catchphrase. However, attaining top educational results in international competition is getting more difficult by the day, the crisis of public funds in the developed world means that budgets are tight, and so knowing where the state gets the most “bang for the buck” from its investments into education is absolutely crucial.

It must also be noted that it is hard to be inspired by international examples, as the situation differs from country to country. The countries with eight years of primary and lower secondary education include Austria, Belgium, Hungary, and Italy, those with nine France, Ireland, Sweden, and Poland, and then there are also countries with ten years of education, for instance Germany and Denmark(Eurydice, 2010).

The structure of the thesis is as follows: chapter two offers a brief introduction to the returns to education literature, as education economics is not a classical field of economics taught in mandatory courses, and thus prior knowledge of the field by all readers cannot be assumed, chapter three deals with the historical background concerning changes in educational laws, chapter four describes the datasets used (EU-SILC and Structure of Earning Survey), in chapter five the various regressions and methods used are explained, chapter six discussed the results and their implications, chapter seven concludes.

2 Returns to Education Literature

2.1 Concepts

In my thesis I use several concepts well established in Educational Economics literature, in this section I would like to briefly introduce them. I draw from several Educational Economics schoolbooks, articles from the Economics of Education Review, and other papers, including the excellent literature overview concerning returns to education by Psacharopoulos and Patrinos (2004) for the World Bank, more specifically on his latest review paper written jointly with Patrino.

Firstly, it is important to set the economic context in which my research is taking place. When discussing the types of public investments into education, they can be divided into two broad categories: investments aiming at changing the quality of education, and those aiming at changing its quantity. According to Fitzpatrick et al. (2011) more attention has historically been given in educational research to the qualitative aspects of investing into education (quality of teachers, size of class, etc.), than to the quantitative aspects (duration of school year, number of school years). However, in the same article they also state that this trend has been changing lately.

The educational economics concept at which I begin my analytical journey is the attempt to estimate the differences in returns to a year of education between cohorts with different lengths of primary and secondary school using the Mincer function, the exact form of which is described later in the chapter Methodology. It is a well established function often used for estimating the returns to education, most often used on cross-sectional data. It is based on the papers Human Capital: A Theoretical and Empirical Analysis by Gary Stanley Becker from 1964, who introduced the concept of internal rate of return to education, and Schooling, Experience and Earnings by Jacob Mincer from 1974 (Hanushek, 2006, p.311). The following excerpt from the Handbook of the Economics of Education gives a good idea of its usage:

> [The Mincer] model is widely used as a vehicle for estimating "returns" to schooling quality, for measuring the impact of work experience on male–female wage gaps, and as a basis for economic studies of returns to education in developing countries. It has been estimated using data from a variety of countries and time periods. Recent studies in growth economics use the Mincer model to analyze the relationship between growth and average schooling levels across countries. (Hanushek, 2006, p. 311)

It is important to note that since its creation, drawbacks of the Mincer function have appeared, as discussed extensively in the above mentioned Handbook. However, these problems are related to the interpretation of the coefficient in the Mincer equation as rate of return to investment into education (Hanushek, 2006, p. 350). As my goal is not to asses the specific financial returns to an investment in the examined periods, but to describe the direction in which these returns shift if we change the length of education, I have a simpler task. I do not have to deal with the topics of hard to measure psychic costs of schooling, impact of taxes, length of life, etc., but rather I compare the differences in the "returns" for individual ways in which primary and secondary education can be organized, and for this the Mincer function is sufficient. Its limitations are not relevant to my thesis. With the danger of oversimplification I could say that I use the returns to education as an "index" of the results that the setting of the educational system has on its pupil, which is consistent with the interpretation of the returns as the "growth rate of market earnings with years of schooling" (Hanushek, 2006, p. 311).

Concerning the general trends, which apply for the rate of return, Psacharopoulos states in his overview that "the pattern of falling returns to education by level of economic development and level of education are maintained", and he also computes that the average rate of return for another year of completed education is approximately 10% for the world, 7.5% for OECD (Psacharopoulos and Patrinos, 2004). For the Czech Republic in 1996 the return for an extra year was 5.8% for males and growing rapidly, according to Munich et al. (2005). It is also useful to look at the returns per level of education, specifically the equivalent to the Czech "základní škola", which is primary and lower secondary education. The total (social and private) returns for primary and secondary education are 45.5% and 30.1% for the World and 21.9% and 20.7% for OECD, respectively. My goal however, is to look for changes in the returns to education rather than in their absolute values.

As a side note, it would be also possible to criticize the measurement of the returns on education by stating that education during socialism has had no effect on earnings during capitalism because of different skills needed in each period. However, in Filer et al. (1999)we read, that

> "Studies (summarized in [Švejnar, J., "Labor markets in the transitional central and east European economies", Handbook of Labor Economics, Volume 3, Part 2, 1999, Pages 2809-2857, North Holland, Amsterdam]) investigating changes in the value of education as communist countries converted into market economies have typically found increases, especially among men."

which does not agree with this hypothesis.

Another interpretation of the outcome of my work is to shed more light on the so called "sheepskin effect" hypothesis, as discussed for instance by Hungerford and

Solon. To use their phrasing, this hypothesis states that "wages will rise faster with extra years of education when the extra year also conveys a certificate" (Hungerford and Solon, 1987). (The fact that an elementary school certificate is a normal certificate can be found for instance in Card (1999))

The testing of this hypothesis is a part of the discussion between the screening and human capital theories, which disagree on what is the underlying reason for returns to education. Screening theory, based on asymmetric information, states that the extra return to education may not necessarily be from the change in the pupil's skills, but because of the screening function of education. The title is a symbol of someone inherently capable, and so leads to higher income in later years, but the person's capability itself may have not changed.

In our case, because the title that basic school conveys does not change with the changes in law, we are able to discern whether the predictions of the screening theory of education (its the certificate that matters, no extra returns to education from an extra year of schooling) are right, or if the human capital theory is right (extra year of schooling enhances the pupil's abilities, and thus increases the returns to education). A further asset of the method used in this thesis is that it overcomes limitation in interpretation that often occur in other designs, as explained in Hungerford and Solon, which are caused by comparing dropouts and successful students, who may have different inherent traits. Thus, while comparing dropouts and graduates limits the interpretation of the sheepskin effect as supporting the screening theory, in my work this is possible.

An interesting aspect of the screening vs. human capital theories is whether it matters, and if yes, for whom. From the individual's point of view it does not - clearly, if you manage to get a higher education, you tend to make more money, no matter the reason. However, from the societal welfare point of view the answer is much less clear. As the question is put in Johnes (2004) "If [schooling's] only purpose is to sort prospective workers, then questions arise as to the appropriateness of investing in the expansion and/or qualitative upgrading of schooling: does it really need three years of undergraduate education to sort young adults effectively?"

2.2 Past Studies

Now I would like to comment on two approaches used in factual researches dealing with the amount of time spent by students in school, and the impact this has on their achievement, give several examples, and compare their results. Generally, because of higher incidence, it is more common that differences in the amount of time spent in school within a single year between different groups of students are examined. For instance Fitzpatrick et al. (2011) is based on a different length of periods between taking the same tests among kindergarten children, Marcotte (2007) uses the differences in the number of schooldays among individual schools caused by snow calamities, and assess their impact throughout primary and lower secondary school, Pischke (2003) estimates the results of changing lengths of school year during 1966 and 1997 in Western Germany, etc. These studies uniformly report positive impact of a higher amount of educational time on educational achievement.

However, their topic is different from my thesis, and that is so for several reasons: obviously the difference in magnitude (and resulting topics such as what is the role of curricula, what is the role of the disturbed structure of education during a usual school year), less obviously the changing returns to education with time. It is usually assumed that the returns to education are homogenous throughout the educational process (in cases where years of education are used), or at least implicitly per year spent at the same educational institution (when they are represented by qualitative variables).

This is a very simplifying assumption, and lifting it means that we are suddenly severely limited in extrapolating the results from an "extra day" to an "extra year". Lifting this assumption makes sense however, because, taking into account the curriculum, there is no reason why returns to learning to read or sum in the first grade should be equal to learning to solve quadratic equations). This notion is also supported in Marcotte (2007), where one of the results is that "the performance of students in lower grades was more affected by snowfall than students in higher grades". These types of studies do not directly answer my question, and it is necessary to look at studies which are closer in nature.

The following studies have addressed the question of the impact of an extra year of education. The first intriguing study is Oosterbeek and Webbink (2007a), which examines the impact of a change in educational laws in the Netherlands in 1975, which lengthened a portion of the basic vocational education from three to four years, and had an impact on the 15 and 16 year olds. The main difference from my thesis is that they do not estimate an extra year of education for whole cohorts, but only for a part of the group that went to vocational schools (which is a more partial information, but also allows for comparison between groups from the same cohorts). They do not find any statistically significant improvement in average wage as a result of the change. Another used approach is the combination of examining mandatory education and some sort of discontinuity. Angrist and Krueger (1990) utilize a discontinuity caused by the quarter of birth in the United States, *Aakvik et al. (2003)* uses a change in mandatory education laws in Norway, and so does Oreopoulos (2003), this time in Britain and Ireland.

All three of these studies find a substantial positive result of the lengthening of mandatory education, and so I would not be afraid to conclude that the benefit of longer education for those who would have left school was it not for the law, is well established. The contribution of my study is that I deal with the effect that the changes have on the entire cohort. This is related on one hand to the issue of heterogeneity of returns to schooling between individuals in the sample, but on the other hand also means that my conclusions are of significantly higher policy relevance.

3 Overview of Chosen Socialist Educational Reforms

Educational reform in the former Czechoslovakia took place in the form of „educational laws“ („školský zákon“) which altered the institutional framework of the Czech educational systems. The reformatory laws dealing with the length of school enrollment took place in 1948, 1953, 1960, 1984, and, again after the Velvet Revolution, in 1996. Furthermore, I will briefly discuss the reform of 1968, which ended the six day school week. Two comments must be made concerning these reforms in general.

Firstly (and regrettably) these laws dealt with the whole educational system, and so the identification of the most important factor which resulted in the perceived change is hindered. However, it can be expected that while many changes were “subtle” and time was required for their implementation to take place after their formal acceptance, the binary nature of the length of education means that the impact was immediate. This is one of the assumptions on which I base that the change in primary and secondary school enrollment was the main driving factor in the reforms. The only other factor of similar binary nature was the change in length of general compulsory education, which, as in most countries Průcha (1999, p. 67-68), was tied to the length of primary and secondary education, as shown in the table Time Periods and Basic / Mandatory Education.

Secondly, the laws themselves were not always executed at once over the whole Czechoslovakia or Czech Republic. In some cases the schools could close the extra class already before the law came into effect. The extent to which the socialist bureaucracy complied is illustrated by table Number of Pupils in Ninth Grade around the Cutoff Years. This means that my study is not free of noise in the data, making quantitative analysis more difficult. However, that is the burden of a social scientist, and the degree of the noise can be estimated from the changes in numbers of students in the ninth grade. Furthermore, this means that if we are able to discern an effect on the current standing of the cohorts even though the reforms are somewhat diluted, our conclusion is not endangered. If we are not able to find an effect, further discussion is necessary.

The laws in question were the following

- Law no. 95/1948 of the Code, implemented nine year primary and secondary education.
- Law no. 31/1953 of the Code, implemented eight year primary and secondary education.
- Law no. 186 / 1960 of the Code, implemented nine year primary and secondary education.

- Law no. 29/1984 of the Code, implemented eight year primary and secondary education.
- Law no. 258/1996 of the Code, implemented nine year primary and secondary education.

The following table summarizes these laws and compares them to the length of mandatory education:

Table 1: Time Periods and Basic / Mandatory Education

Period	-1948	1948-1953	1953-1960	1960-1984	**1984-1990**	**1990-1996**	1996-
Years to complete primary and secondary education	8	9	8	9	8	8	9
Years of mandatory education	8	9	8	9	10	9	9

As we can see, because primary and secondary education was considered as the mandatory level, the development of the mandatory number of years closely follows - except for one period - the development in the first table. This means that any effect we discern "per cohort" consists of both of these effects. However, when we compare the magnitude of each (by comparing the number of people who finish their education just after completing lower secondary education and the rest of the population) we see that the effect of an extra year of education is much larger. To be specific, in the SILC data, an extra year of education influences more than 99.5% of the population, while the change in mandatory education at most 15% (the number of people in the sample who have completed lower secondary education as their highest level). Based on this I assume that the effect I measure was caused more by a global change in the length of primary and secondary education than in its mandatory length. Nevertheless, these numbers do not allow us to definitely identify the cause of the effect, and this must be kept in mind when interpreting the results.

I think it is a good idea to document the extent to which the change was immediate and significant. I do this by examining the data from the Statistical Yearbook of Czechoslovakia and the Statistical Yearbook of the Czech Republic[2] for the respective years, skipping the first two changes in the law in 1948 and 1953, because between 1949 and 1956 the Statistical Yearbooks were not published. The table shows the number of pupils who went to ninth grade in a given year (1996 stands for the school year 1996/1997). In years when the ninth grade was implemented

[2]Specifically, the Statistical Yearbook of Czechoslovakia for 1960, 1963, 1965, 1981, 1984, 1987, 1990 and the Statistical Yearbook of the Czech Republic for 1996, 2000, 2001.

(1960, 1996) we expect to see a sharp increase, and on the other hand, when the ninth grade was scrapped we would expect a rapid fall. We can see that the growth is immediate and sharp, the fall is visible also, but a little more gradual - thus we know that the implementation of the change was also more gradual. However, it still a very significant number.

Table 2: Number of Pupils in Ninth Grade around the Cutoff Years

Year	1956	1957	1958	1959	**1960**	1961	1962	1963	1964
Number of pupils in ninth grade	0	0	0	40,408	***113,380***	205,868	208,849	202,398	185,158
Year	1980	1981	1982	1983	***1984***	1985	1986	1987	1988
Number of pupils in ninth grade	101,469	64,043	22,581	5,016	***0***	0	0	0	0
Year	1992	1993	1994	1995	***1996***	1997	1998	1999	2000
Number of pupils in ninth grade	30,570	20,270	10,183	5,811	***112,689***	108,641	109,662	108,797	108,507

I think it is also worth mentioning that originally I planned to examined the impact of the reduction in number of schooldays on the returns to a year of education. As discussed in Returns to Education Literature there is a significant body of literature indicating that the effect would be possible to find and significantly positive. However, the change in the length of the school week did not take place at once. As it was a part of the endeavor for a shorter school year, it came in waves, as the political situation changed in favor of it. Firstly, starting in 1964 every fourth Saturday was without school, then from 1966 every second one, and finally from 1968 it was abolished altogether. The possibility of finding any effect as a result of this gradual implementation is then totally undermined by the events of 1968, which caused serious change all over the society, and much less importantly the discretionary implementation in individual regions. For these reasons I omitted the analysis of this educational reform.

4 Household Data

4.1 SILC

The main dataset I shall work with is the Czech EU-SILC (Statistics on Income and Living Conditions) from 2006 to 2009 (before 2006 individual income, my explanatory variable, was not reported). SILC is collected by Eurostat via the Czech Statistical Office, and provides reliable data on household and individual income in the Czech Republic. From these datasets I take those respondents, who were not affected by the change in policy by a maximum of three years, and compare them to the first three cohorts which were affected.

As is stated in the respective laws, mandatory enrollment takes place in the September of the first school year after the child has attained six years of age, and is followed by eight or nine years of education. Thus, for example when assessing the impact of the 1953 policy, I compare the cohorts born between the beginning of September 1935 and the end of August 1938, which experienced nine years of primary and lower secondary education, with the cohorts born between the beginning of September 1938 and the end of August 1941, which experienced nine years of primary and lower secondary education. As we can see, the middle of this group, those born in September 1938, were those who were first to experience only eight years of primary and secondary education. (Being born in September 1938, they went to first grade on 1 September 1945, finished eighth grade in June 1953 and did not go into ninth grade, because the law was already in effect. Those born just a month earlier were not affected.)[3]

However, the SILC dataset does not provide the date of birth of its respondents, but only their age. Since the SILC data collection, as stated in the documentation, takes place approximately on the 1 May of each year, we must furthermore leave out from our data those who are of age, not allowing us to recognize whether they were affected by the change. For example, a person whose reported age on 1 May 2009 is 70[4] was born between 2 May 1938 and 1 May 1939, thus we cannot determine whether or not he was affected. To overcome this, we leave out the middle year in each case.

The cohorts we are interested in and amount of corresponding data provided are summed up in tables Relevant Cohorts and Ages and *Sample Size.*

[3]An objection brought up (and refuted inMarcotte (2007)) in cases of mildly similar studies which make use of monthly cutoff points for entering first grade is that parents may be taking this into account when deciding when to have a child. This problem may be ignored in my study as the time horizon makes any prediction impossible.

[4]When referring to a certain age in the data throughout the text, I calculate it in relation to the SILC 2009 dataset, i.e. data collected around 1^{st} of May 2009. When working with older datasets I adjusted the ages accordingly, see below.

Table 3: Relevant Cohorts and Ages

Date when law came into effect	Pre-change cohorts	Post-change cohort	Relevant age, SILC 2009	Excluded age
1.9.1948	Sep. 1930 - Aug. 1933	Sep. 1933 - Aug. 1936	78-72	75
7. 5. 1953	Sep. 1935 - Aug. 1938	Sep. 1938 - Aug. 1941	73-67	70
28. 12. 1960	Sep. 1943 - Aug. 1946	Sep. 1946 - Aug. 1949	65-59	62
1. 9. 1984	Sep. 1966 - Aug. 1969	Sep. 1969 - Aug. 1972	42-36	39
1. 9. 1996	Sep. 1978 - Aug. 1981	Sep. 1981 - Aug. 1984	30-24	27

The size of the data useful for regressions is the following:

Table 4: Sample Size

Age\ SILC Year	2009	2008	2007	2006	Total size by cohort
78-72 in 2009, excluding 75	1,216	1,401	1,181	851	4,649
73-67 in 2009, excluding 70	1,435	1,718	1,392	1,052	5,597
65-59 in 2009, excluding 62	2,157	2,437	1,935	1,470	7,999
42-36 in 2009, excluding 39	1,802	1,965	1,729	1,270	6,766
30-24 in 2009, excluding 27	1,628	1,979	1,778	1,368	6,753

When choosing the dependent variable from the SILC dataset I chose the individual's income. It must be noted that for a policy-relevant topic this is of course a strongly reductionist approach. It would be very easy, and true, to object that I ignore many results of education, in this context many parts of human capital, both by choosing only this one variable, and by the pure fact that a large portion of the outcomes of education are inherently unmeasurable. I fully acknowledge that. My thesis is a partial view of the topic, which must necessarily be reductionist, if it is to be empirical, and thus complement the otherwise prevailing qualitative approach to the topic. I will not dive into the discussion of what are the relationships between an empirical approach, a qualitative approach, the controversial noun "objectivity", and policy relevance, as that is discussed elsewhere. This is to establish, that the offered viewpoint is only partial.

The table 5 is a list of chosen variables I took from the SILC dataset and the ones I generated in the process of preparing the data for the regressions together with their descriptions. From now on and throughout the thesis, I will use the *Names* of the variables used in this table when I need to refer to them.

Information about what was done with the data can be found in detail in the .do list of commands, which is available on request. Here I will try to briefly sum it up. The mechanical data changes included renaming the variables, removing observations lacking *Income*, removing observations of individuals of other than Czech and Slovak nationality, generating dummy variables from qualitative variables, and

Table 5: List of SILC Variables

Variables	Description
Income (cprijmy)	The net income per year in korunas
Education (vzd)	The highest attained educational certificate. Starts at 0 (unfinished elementary school) and ends at 9 (Ph.D.). When relevant in the regressions i removed less than 0,5% of the observations which were those who did not complete even lower secondary education
EducYrs (VZDROKY)	The number of years an individual spent in school. An imputed variable, see discussion in text
Workedweekly (odprac)	Number of working hours per week
*Worked*yrs (odprac_let)	Number of years employed. The variable X in the Mincer function
Sex (pohl)	Sex (1=male)
MStatus (stav)	Marital status (single, married, divorced, widowed)
AgeOriginal (vek)	Age
Age (VEK_ALT)	Age of respondent in 2009
LnIncome (LNCPRIJMY)	The logarithm of annual net income. (For a discussion why to use the log see Card (1999)) Used as variable lnY in the Mincer function
LnHourlyIncome (LNCPRIJMYHOUR)	$\equiv \ln(cprijmy/odprac)$. The logarithm of the ratio of annual wage and number of worked hours per week. Thanks to the same number of workweeks across professions it functions similarly to "hourly wage"
Workedyrs2 (ODPRAC_LET_SQR)	$\equiv$(odprac_let)^2
Collected (POZOROVANI)	The year of data collection
Prague (PRAGUE)	Resident in Prague, a dummy variable
Case (PRIPAD)	To which discontinuity the observation is related and whether it preceded it or came after it. Starts at 1, which are those between 24 and 26 of age, and ends at 10 which are those between 76 and 78. For further information see table Age, Treatment and Case

separating the observations into individual groups (*Case*). However, then there are two modifications of the data, which were more major and which require deeper explanation.

The first problem is that for calculating the returns to a year of education it was necessary to recode the variable *Education* from "highest attained certificate" to "number of schooling years", so that it could then be used as the first variable of the classical Mincer equation. This is very problematic, but it can be defended on three levels. First of all, the irregularities that this step causes can be expected to be symmetrically split between the cohorts before and after the change, not biasing the "per cohort" data in any direction. Furthermore, the magnitude of these irregularities should not be too great. According to the EU Youth Report (2009) the Czech Republic has a dramatically low number of dropouts, and even though data is lacking, it seems that this is for historical reasons - the trend of very low dropout rate was similar in the past. From this we can assume that the number of students repeating years of education was low, and thus the fluctuations in this low number might not have had a drastic effect on the data. Third, the *Education* variable is reasonably well defined: it has 10 different levels of education most of which can be assigned a definite number of years. However, the largest exception to this, category three - lower vocational education, where it is unsure whether the length of study is two or three years - includes the largest number of observations, around 40 percent of them.

Second serious step I took was that I put together data from different SILC data collections. In the process I created the variable *age* which shows the age of the individual in year 2009. This step seems benign, but it does add an certain amount of uncertainty. However, as is shown in section 5, the effect of this step is largely predictable and does not seriously influence the educational estimates.

4.2 Structure of Earning Survey

Structure of Earnings Survey (SES) is a large data source that I have used to replicate and complement the regressions tested on SILC. The Czech version of SES is collected quarterly for the Czech Ministry of Labor and Social Affairs by TREX-IMA, and contains data from businesses and the government on employee pay. For my research it is useful that the data includes average hourly pay, and a number of hours worked per quarter, the combination of which I use as the dependent variable. As I did not have direct access to the data[5] the regressions run will be practically the same as for the SILC dataset. However, the size of the same dataset is significantly higher, which allows for more precise estimates. On the other hand, the number of

[5]Remote access to a 1% sample of the 2005 Structure of Earning Survey (the Information system on average earnings) was provided by Stepan Jurajda.

years of work experience is unavailable, so only age was used. The dataset used was from 2005.

Table 6: List of SES Variables

Variables	Description
hwage	average hourly wage
wagequart	average quarterly wage (
age	Age
age2	$\equiv$age^2
Education	Vector of dummies for highest attained education, starts at primary end at Ph.D. level.
educyrs	Number of schooling years, imputed

5 Methodology

5.1 Introduction

As has been explained in the section Overview of Chosen Socialist Educational Reforms, we have several independent cases of prolonging and shortening education. When trying to asses these, I would like to deal with each of them separately, which means that I would be dealing with five independent cases. However, the situation is not so simple. In *Case 1&2* the individuals are still too young for proper income data to be available. In case *Case 5&6* the income data is not reliable, because the retirement age comes into effect precisely at that time (differently for different individuals depending on their number of children, information which is sadly not included in the individual SILC dataset). And finally, in *Case 7&8* and *Case 9&10* the incomes are the pensions, so the differences are significantly smaller than in the previous cases. It is true that pensions are income dependent, albeit weakly, so the change should in theory be detectable, but finding them among the various noise in the model makes the task quite hard. Furthermore, the case *Case 9&10* deals with individuals who "were" in primary and lower secondary school during (and shortly after) World War II. Because of the chaos (Kalous, 2006, p. 18), which was commonplace at the time in Czech schools, comparisons from that period are very hard to make. As we see, our main case is primarily the *Case 3&4* and secondarily *Case 7&8*, i.e. the cohorts of those who were around 39 and those around 70 (± 3 years) in 2009.

I will test two hypotheses, a stronger one and a weaker one, both concerning the impact of policy changes on the returns to education. The stronger one is that "Overall returns to an elementary school certificate do not change with a change of length of basic school education", the weaker one, "Returns to an extra year of education grow after shortening basic school education."

Unsurprisingly, the weaker hypothesis is implied by the stronger one. The aim of the weaker hypotheses is to find out whether an additional year of schooling has added less to the individual's income than the previous years, and the aim of the stronger one is to find out, if it had any effect at all, or if only the certificate matters. The weaker hypothesis is more important for public investment decisions, the stronger hypothesis is a variant of the above mentioned "sheepskin effect", where we find out the importance of a certificate in the returns to education.

However, there is still an interpretational problem with these hypotheses. This is because I am actually testing simultaneously two unknown characteristics: the sheepskin effect and the human capital impact of an extra year of education. Thus, in the extreme case, if I find no beneficial effect of an extra year of education, I do not know whether this is because the ninth year of education has no impact, or whether because in general no years of education have impact, as it is the certificate that matters. This is further discussed in the section Results.

5.2 Regression discontinuity & The Mincer Function

I shall use a sharp regression discontinuity design to compare the individual traits of the cohorts with and without the ninth grade. The basis for this method is taken from Imbens and Lemieux (2008). As argued in the study, for the approach to be applicable, it must be possible to describe all individual data available in the form (Y_i, W_i, X_i, Z_i) where Y_i is a certain outcome, W_i is a binary variable showing whether a treatment was administered, and X_i, Z_i are pretreatment variables. While Z_i is a matrix of covariates, X_i is a number. The crucial role here is played by the variable X_i, the values of which determine W_i.

This notation holds for my data. The values Y_i are the regression dependent variables, i.e. *Income,* the variable W_i shows whether or not the individual received a treatment in the form of a 9^{th} grade of education. The determining variable X_i is the age of the person (as the administration of the treatment is age dependent) and Z_i represents the remaining available data. The principle of the regression discontinuity design lies in our expectation of a smooth relationships between X_i and Y_i. I examine, if this smooth relationships holds even in cases where there might be a reason for it to not do so, i.e. I compare the relationships around the ages where cutoff points occurred - theoretically 27, 39, 62, 70 and 75. Out of these, we are interested mainly in those around 39 and those around 70. The historical relationship between X_i and W_i, (including *Case*), is summed up in table Age, Treatment and Case.

Table 7: Age, Treatment and Case

X_i	<24,26>	<28,30>	**<36,38>**	**<40,42>**	<59,61>
W_i	1	0	**0**	**1**	1
Case	1	2	**3**	**4**	5
X_i	<63,65>	**<67,69>**	**<71,73>**	<72,74>	<76,78>
W_i	0	**0**	**1**	1	0
Case	6	**7**	**8**	9	10

As the regression on which we measure the discontinuity we will use the human capital earning function based on Mincer, introduced in the section Returns to Education Literature and as taken from Ferrer and Riddell (2001):

$$\log y = \alpha + \beta S + \gamma X + \boldsymbol{\delta X}^2 + \boldsymbol{\theta Z} + \varepsilon \tag{5.1}$$

where y_i is an individuals income, S_i is the number of years of completed education, X_i is the number of years an individual has worked since completing his education (which is a proxy for experience), $\mathbf{Z}$ is a vector of other possibly related variables (for instance sex or marital status), $\boldsymbol{\theta}$ is a vector of parameters for individual variables from $\mathbf{Z}$, ε_i is the residual and $\alpha, ..., \delta$ are the appropriate coefficients in the regressions (out of these the coefficient β we call "the returns to education").

As the income variable, we have the opportunity to choose between the ratio of the net income per year and the number of workhours per week, or net income per year. While net income per year grows with both the average wage (per hour) and the workload, the ratio controls for the different amount of time spent at work (the number of working weeks per year is the same for nearly every profession). It has been established, that higher average income per hour represents more than 2/3 Card (1999) of the returns to education (see the same source for further discussion). Thus, the ratio of annual income and workweeks would theoretically be a better dependent variable.

However, in SILC there is a large amount of missing observations for the number of workhours per week (the *Workedweekly* variable), and so I will rather use just the net income. A good point is also brought up in Ashenfelter and Ham (1979) that the extra time spent working thanks to higher education may either be caused by a higher inclination to work caused by higher education (a growth in voluntary amount of employment), or by being less in danger of not being able to find a job (a reduction in involuntary unemployment).

As I will return to it later, I also mention the extended Mincer function which includes the effect of receiving a certificate. It is in the form of

$$\log y = \alpha + \beta S + \gamma X + \delta X^2 + \boldsymbol{\theta}\mathbf{Z} + \boldsymbol{\zeta}\mathbf{C} + \varepsilon, \tag{5.2}$$

Ferrer and Riddell (2001) identical to (5.1), except for the vector $\mathbf{C}$ representing dummy variables for individual earned credentials and the vector $\boldsymbol{\zeta}$ representing parameters the for the individual certificates. We will return to this second function shortly. Now let us proceed to the regressions themselves. We shall start with the SILC dataset, then replicate all the steps for the SES dataset.

5.3 The Regressions

Firstly we examine the standing of the weaker proposed hypothesis. Because we are interested in the returns to an extra year of grade school education while the elementary school certificate remains present with all individuals, we will use equation (5.1)[6]. As the vector $\mathbf{Z}$ I introduced a set of dummy variables: *Sex*, ***MStatus***, *Prague* (life in Prague is associated with significantly higher income), and a vector of dummies for years of SILC origin ***Collected***. We compare these dummies to the estimated value of the intercept α which stands for the returns of a single male not living in Prague on whom data was collected in 2009.

We start with running a usual OLS regression in the form 5.3 for three cohorts before and after the cut-off point

$$\begin{aligned}\log \text{Income} = \alpha + \beta \text{EducYrs} + \gamma \text{Workedyrs} \\ + \delta \text{Workedyrs2} + \theta_1 \text{Sex} + \boldsymbol{\theta_2 MStatus} \\ + \boldsymbol{\theta_3} \textbf{Collected} + \theta_4 \text{Prague} + \varepsilon. \quad (5.3)\end{aligned}$$

and we specify the model by removing the least significant variables as long as this raises the Adjusted R-square, while staying in the form of the model proposed by the Mincer equation. This leads to very slightly different models for both cases. However, after testing for the assumptions of the OLS model[7] , I discovered that the residual homoskedasticity does not hold, that the residuals are not from the normal distribution, and that the data is on the verge of being multicollinear. For this reason I applied the robust regression[8], which is less dependent on met assumptions, in the form (5.3), and again specified the model, this time using R-square [9]. Even though the coefficients themselves vary between the OLS and robust estimates, the trend is the same in both cases, which is a good sign (Kennedy, 2001).

To be able to compare the results before and after the cut-off, I now multiply the data from before the cut-off point with the dummy *Eighth* and the data after with a dummy variable *Ninth*. These dummies represent the sides of the cutoff point which had the respective length of education. (Throughout the text, I will denote other variables multiplied by these dummies by an E or an N at their end, for instance *EducYrsE* for a variable denoting the total number of schooling years of those who did not have a ninth grade.)

I then ran an OLS regression, and after testing for the assumptions and finding

[6]According to Card (1999) it is also possible to use the cubic and quartic transformations of the "experience" proxy. I tried it and the corresponding coefficients were not significant at the one percent level, so I use only the quadratic transformation.

[7]I test for heteroskedasticity using the Breusch-Pagan, Cook-Weiseberg, and White's tests, for normality using the Shapiro-Wilk, Shapiro-Francia, and Skewness-Curtosis tests, and for multicollinearity using the Variance Inflation Factors.

[8]The violation of the assumption of homoskedasticity in classical OLS means that the vector of estimates is not Best (it does not have the lowest possible variance), non-normal distribution of residuals means we cannot use t-tests and F-tests reliably, and multicollinearity also causes high variance. (Kennedy, 2001)

[9]A "measure of fit" for robust regression is not a default part of Stata, so we use the rregfit plug-in by Ender and Chen, UCLA. I specify models according to these criteria also in future regressions, with no significant differences.

out that the OLS conditions are not met, I ran also the robust regression. The results I got were very different from those of the OLS regression. For cohorts around 39, I cannot reject the hypothesis of equality of returns to education at a 30% level of significance between both sides of the cutoff point (with a p-value of 0.322). For those around 70, the coefficients are less than half those of OLS, but they are significantly different at the 1% level, and they are lower in case of a presence of the ninth grade (estimate of 0.019 with a standard error of 0.001 and an estimate 0.024 with a standard error of 0.001 for those with and without a ninth grade, respectively). The R-squared of the regressions was between 0.3 and 0.4 in both cases, the standard in similar studies. As in one case the returns did not grow with the shortening of education, and in the second they did not, our results are inconclusive. The results for 39 disagree with the weaker proposed hypothesis, while those for case 70 agree with it. Further examination is needed.

Before I continue, I must mention another important result from these regressions. The dummies ***Collected*** are always significant and they have a negative coefficient - the earlier the observations of income, the higher the magnitude of the effect. This is unsurprising, if for no other reason than for annual inflation. To make sure that the year of observations has no effect, I run the regressions also for each year independently. These regressions, made on a lower number of observations and with a lower coefficient of determination, show the same trends as the previous regressions, and so I will not discuss them further. This softens one of the concerns I had about the usage of data that I mentioned before.

Next I shall do the same regressions as before, but this time I will not look for the significance of the change in law using the change in the returns to education coefficient, but I will test for the significance of the *Ninth* dummy variable itself. It would be tempting to compare the coefficient of *Ninth* to the *EducYrs* coefficient and expect them to be similar, as both provide an extra year of education. However, both of these variables are also very different in nature. As the years of education were imputed without differentiation, the coefficient for *EducYrs* represents an extra year of "any" education. On the other hand, the coefficient for *Ninth* represents an extra year of lower secondary education (because it is a separate dummy), but it will also capture any other effects which happened over the same time period. The regression will have the form (5.4)

$$\begin{aligned}\log \text{Income} = \alpha + \beta\text{EducYrs} + \gamma\text{Workedyrs} \\ + \delta\text{Workedyrs2} + \theta_1\text{Sex} + \boldsymbol{\theta_2 MStatus} \\ + \boldsymbol{\theta_3}\textbf{Collected} + \theta_4\text{Prague} + \lambda\text{Ninth} + \varepsilon. \quad (5.4)\end{aligned}$$

After running the regressions I got results which were in trend similar to the those before. The coefficient λ is not too significant (p-value of 0.0613) for 39, and it is significant for 70 (p-value close to zero), and in both cases it is negative ($\lambda_{39} = -0.021$ and $\lambda_{70} = -0.024$ with standard errors 0.011 and 0.004, respectively).[10] From these results, it seems that an extra year of education had a negative impact on the earnings of the individuals.

The negative results of the dummy variable *Ninth* are somewhat puzzling. If the extra grade had no effect, I would expect it not to be significant, if it had a positive impact, the message would be clear - more school means higher returns. However, the fact that an extra year reduces the amount of money an individual makes is surprising. We could interpret the results as meaning that the pupil loses some of his or her skills because of an extra year in school during which not much happens, but this does not seem likely.

The result is even more non-intuitive taking in account I have data on experience, which means I can discard the explanation that an extra year of education is equivalent to a year less of experience. I could also try to explain the effect by saying that my model underestimated the role of career growth. However, because both the reforms for those around 39 and around 70 meant that the ninth grade was canceled, it also means that those who did have it are older, and in the case of those around 39 they should have higher income. The fact that the coefficient is negative is again contrary to this interpretation.

As these results do not seem reasonable, they are quite hard to interpret. It is possible that the effect I tried to find is too small to be found by this model, or it is possibly outweighed by other factors for which the model does not account (or both). This in effect means that the assumption of the regression discontinuity design (cohorts before and after almost identical) does not hold. It seems that something is amiss. To find out what, I alter my model, and try examining the problem from a different perspective.

5.4 The Altered Mincer Function

The following method is loosely based on the extended Mincer equation (5.2). This time, I use dummy variables to estimate the coefficients independently for each level of highest attained education (***Educ***), and ignore the number of years that an individual has spent in school. Firstly, I use the same dummy variable *Ninth* as before, the significance of which determines the impact of another year of education

[10] To estimate the effect of dummy variables we must use the formula $\hat{g} = \exp \eta - 1$, where $\hat{g}$is the estimated growth of loged income, as explained for instance in Baltagi (2008), but for negative values this does not, understandably, change the estimated effect's sign. Nor is this relevant when comparing the values of the dummy variables.

on the individual. Then I will compare the coefficients for the certificate in cases when an individual had a ninth grade and did not have a ninth grade. Both of these approaches are analogous to the ones from the previous section, their difference is in the way education is represented. The regression is in the form (5.5)

$$\begin{aligned}\ln \text{Income} = \alpha + \gamma \text{WorkedYrs} + \delta \text{WorkedYrs2} \\ + \theta_1 \text{Sex} + \boldsymbol{\theta_2} \textbf{MStatus} + \boldsymbol{\theta_3} \textbf{Collected} \\ + \boldsymbol{\theta_4} \text{PRAGUE} + \boldsymbol{\zeta} \boldsymbol{Educ} + \lambda \text{Ninth} + \varepsilon. \quad (5.5)\end{aligned}$$

where the coefficient α represents the estimated returns for a single male, who does not live in Prague, who underwent eight year primary and secondary education, and who took part in the SILC 2009 data collection, and the various dummies represent the deviation from this setting. Since the conditions for an OLS regression are again not met, I used the robust regression[11], and got the following results for *Case 39* and *Case 70*, which are similar to the previous model. That the dummy *Ninth* is not zero cannot be rejected at the one percent level (but it can be rejected at the five percent level with a p-value of 0.044) in case of those around 39, but in the case of the cohorts around 70m *Ninth* is significant (p-value close to zero) and negative ($\lambda = -0.023$). I also tested whether the *Ninth* coefficient is significantly different if we include the interaction effects, and in both cases it is negative and significant.

Now we can compare the results of returns to education for each level of education attained. Here we use dummy variables with interaction effects, as noted for instance in Kennedy (2001). This means I multiplied all the other variables with the *Ninth* and *Eighth* dummy variables to get, for instance, ***EducationN*** for having both *Ninth* and a certain level of education. The regression had the form (5.6)

$$\begin{aligned}\ln \text{Income} = \alpha + \gamma_n \text{WorkedYrsN} + \gamma_e \text{WorkedYrsE} + \delta_n \text{WorkedYrs2N} \\ + \delta_e \text{WorkedYrs2E} + \theta_{1n} \text{SexN} + \theta_{1e} \text{SexE} + \boldsymbol{\theta_{2n}} \textbf{MStatusN} \\ + \boldsymbol{\theta_{2e}} \textbf{MStatusE} + \boldsymbol{\theta_{3n}} \textbf{Collected}\text{N} + \boldsymbol{\theta_{3e}} \textbf{CollectedE} + \boldsymbol{\theta_{4n}} \text{PRAGUEN} \\ + \boldsymbol{\theta_{4e}} \text{PRAGUEE} + \boldsymbol{\zeta_n} \boldsymbol{EducationN} + \boldsymbol{\zeta_e} \boldsymbol{EducationE} + \varepsilon. \quad (5.6)\end{aligned}$$

I then tested whether there are significant differences between the *EducationN and EducationE* coefficients. In general, the differences were not significant. For those around 39, we cannot reject the hypothesis of coefficient equality at the 5% level of significance in all cases, for those around 70 in all but two, and in the two we cannot reject one at the 1% level of significance (p-value = 0.031) in one case, in

[11] Because the OLS conditions are never met for this model, from now on I shall always use the robust regression.

the second one we can (p-value = 0.002), and the coefficient values of this case are 0.171 (0.014) with ninth grade, 0.230 (0.013) without. It seems that the returns to educational certificates do not change too much with a change in length by one year during primary or lower secondary education.

To sum up this approach, we have discovered two things. Firstly, the *Ninth* dummy variable truly tends to be negative, no matter the model we use. Secondly, the differences between the returns to the certificates themselves are largely negligible, and if there are some, they tend to correspond with the effect of the *Ninth* dummy: longer overall education means lower returns. As these are surprising results, it is necessary to check whether they are not caused by some underlying problem in the model rather than by the relationship we are trying to model itself.

5.5 Confounding variables

Even though my coefficient of determination is in a reasonable range for all the regressions I did, above 0.3, which is a reasonable social science threshold, it is obvious that there is still much scope for further explanatory variables. To test whether I have overlooked any significant explanatory variable which could have been correlated with both my explanatory and my dependent variable (a so-called confounding variable) I perform the regressions again, but this time on periods where "nothing happened", i.e. no changes in law took place. Then I determine whether I still get results different for the period before and after the cutoff point, and compare the magnitudes of the effects.

To be able to do this, I need to choose periods when nothing happened and where at the same time I can have three years of cohorts in both directions available for the regressions. That means I need a period of at least seven years with no educational reforms. There are only two such periods: from 1961 to 1983, and from 1985 to 1996. However, the second of these periods is interrupted in two ways, firstly by the change in the mandatory length of education in 1990, and secondly by the Velvet Revolution itself. For this reason I chose only the first period. These years represent the SILC 2009 cohorts between 40 and 61 (as can be seen in Table 7 on page 25), and for the cohorts between 43 and 58 we run the same regressions (I shall call these empty regressions) as we did in the last two sections (using the specific cohort as the cut-off point). In each case we drop the middle cohort and use the three previous and three following cohorts as the groups "before" and "after" the change. As both cases we examined dealt with canceling the ninth grade, I will model the same situation with the cohort "before" having a ninth grade, and the cohort "after" not having it.

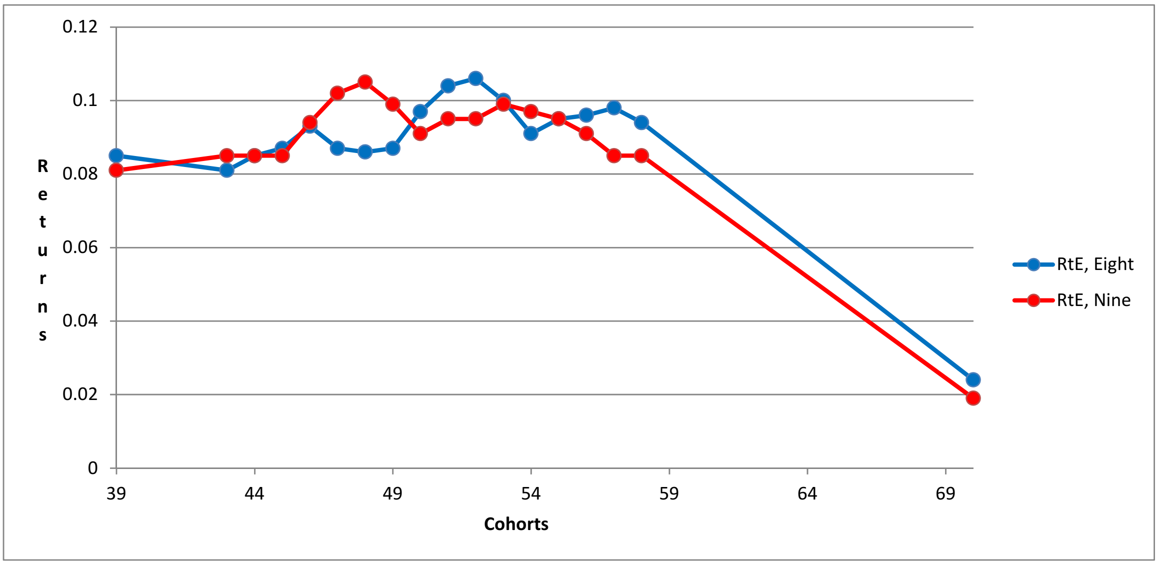
After running 16 (robust) regressions in the form (5.3), one for each year, I received the coefficients for education before and after the imaginary cutoff points. I assume that if the model I am using is good in predicting the dependent variable, the coefficients should be the same on both sides of the cutoff point, as the only difference in the model is a dummy with no "content" (it does not correspond to any changes in the structure of the microdata). However, outside the model, having the "no content" dummy is associated with being after the cutoff point, which means being 2 - 6 years older than those at the other side of the cutoff point. During this period, changes, which are not captured by the explanatory variables in my model, may have occurred.

I tested the equality of coefficients before and after the cutoff points, and in 9 cases out of 16 I was not able to reject the hypothesis of the equality of coefficients at the 10 percent level of significance, while in seven cases the results were significantly different, and out of that five times at 1 percent level of significance. The interesting fact about this result is the distribution of the positive and negative effects. For the cohorts 43-46 the differences are insignificant, for 47 to 49 they are significant and positive (being from an older cohort means getting more from your education) for the 50 cohort they are insignificant, for 51 and 52 significant and negative (being from an older cohort means getting less from your education), for 53 to 56 insignificant and for 57 and 58 significant and negative. This corresponds with the results which we got for the cohorts where education changes actually happened: insignificant changes for those around 39, significant and negative changes for those around 70. An interesting aspect of this distribution is also the fact that the average returns for before and after the cutoff points, which may be used as a rough approximation of the usual returns, are the same for both groups, 0.0930 and 0.0929 respectively. For numerical values see table Confounding Variables, returns, for a graphical representation figure Returns to Education, confounding

Before further interpretation, I will replicate also the other regressions. Firstly the significance of the dummy *Ninth* using imputed years. While in the previous cases with a real cutoff point we expected *Ninth* to be significant, because it was related to an extra year of education, this time we added a dummy *Ninth* for the three cohorts before the cutoff point (ages one, two, and three years higher than the cohort for which the cutoff happened). As in the previous case: if my model covers all the effects, we would expect *Ninth* to be insignificant. However, that is not the case: the coefficient *Ninth* is significant in all the regressions at 1 percent level, and has a negative coefficient (see table Confounding Variables, returns), which are the results I have already obtained before, and which were very non-intuitive.

As we can see, the negative coefficients are significantly larger than the ones we obtained from our regressions before. This is an important fact, as it implies that the net effect of a *Ninth* when an educational reform happened should be positive.

Figure 5.1: Returns to Education, confounding



It seems, that the coefficients for *Ninth* was actually composed of two parts - a coefficient for *Before (*the one we have just identified by running the regression with nothing happening), and a true coefficient *NinthTrue,* which represents the impact that the ninth grade had on the individual.

To finish this simulation, I also run the regression using dummy variables for individual certificate levels, and again test for the significance of *Ninth*, similarly as in the section The Altered Mincer Function. All the *Ninth* coefficients are significant at the 1% level and negative. Then we compare the returns to highest achieved education for individual cases. The differences are more often insignificant than not, the same as before. Out of the 102 tested relationships (16 regressions, 7 tests per regression) in 67 cases we cannot refuse the hypothesis that the coefficients are the same at the 5 percent level of significance, in 35 cases we are able to do so. The results are summed up in table 8.

The regressions for the cohorts, which have not undergone any educational reforms, have shown us that there probably are present confounding variables, which bias my results from the previous regressions. However, knowing the magnitude of these effects allows me to compensate for them. I shall discuss what sense can be made of the coefficient I have in Results. However, before I do so, I will replicate the just described regressions on a larger data set: the Structure of Earnings Survey.

Table 8: Confounding Variables, returns

Cutoff cohort, SILC 2009	Returns to Education Older	Returns to Education Younger	Difference Significant, Trend (↑increase, ↓decrease)	Coefficient of *Ninth*, using imputed years	Coefficient of *Ninth*, using dummy years
43	.085 (.003)	.081 (.004)	No. (p-value= 0.373)	-.066 (0.12)	-.072 (.011)
44	.085 (.003)	.085 (.003)	No. (p-value= 0.921)	-.044 (.011)	-.047 (.011)
45	.085 (.003)	.087 (.003)	No. (p-value= 0.583)	-.050. (.011)	-.046 (.011)
46	.094 (.003)	.093 (.003)	No. (p-value= 0.678)	-.059 (.011)	-.054 (.011)
47	.102 (.003)	.087 (.003)	Yes. ↑ (p-value=0.0)	-.075 (.011)	-.074 (.011)
48	.105 (.003)	.086 (.003)	Yes. ↑ (p-value=0.0)	-.081 (.011)	-.084 (.010)
49	.099 (.003)	.087 (.003)	Yes. ↑ (p-value= 0.001)	-.074 (.011)	-.082 (.010)
50	.091 (.003)	.097 (.003)	No. (p-value=0.138)	-.059 (.011)	-.067 (.010)
51	.095 (.003)	.104 (.003)	Yes. ↓ (p-value=0.014)	-.085 (.011)	-.090 (.010)
52	.095 (.003)	.106 (.003)	Yes. ↓ (p-value=0.003)	-.108 (.010)	-.111 (.010)
53	.099 (.003)	.10 (.003)	No. (p-value=0.765)	-.139 (.010)	-.135 (.009)
54	.097 (.003)	.091 (.003)	No. (p-value=0.13)	-.159 (.010)	-.151 (.009)
55	.095 (.003)	.095 (.003)	No. (p-value=0.89)	-.165 (.010)	-.158 (.009)
56	.091 (.003)	.096 (.003)	No. (p-value=0.179)	-.165 (.009)	-.159 (.009)
57	.085 (.003)	.098 (.003)	Yes. ↓ (p-value=0.0)	-.156 (.009)	-.159 (.009)
58	.085 (.003)	.094 (.003)	Yes. ↓ (p-value=0.009)	-.154 (.009)	-.160 (.009)

5.6 Regressing on Structure of Earning Survey data

The Structure of Earning Survey has already been discussed in the section Household Data. I shall just point out that as it is a data set consisting of microdata for employees of companies, all the respondents are below the retirement age, and thus all the run regressions are for the cohorts around those who were 39 in 2009. Furthermore, I ran all the regressions in two variants, one with hourly wage and one with quarterly wage, which takes into account the growth in the amount of time spent working, but, as the results are similar in trend, I shall list them only for *hwage* most of the time. Finally, as I do not have a variable "number of worked years", I must use age instead. The most important implication of this is that the returns I get from education are actually the returns from education net of the returns of an extra year of experience. As I did not have direct access to the data[12], which means I had to use the methodology just explained, I will describe these results comparatively in brief.

Firstly, I ran the regression in form (5.7) (analogous to (5.6)) to test the equality of coefficients for educational certificates ***Education***

$$\begin{aligned}\ln \text{hwage} = \alpha + \gamma_n \text{AgeN} + \gamma_e \text{AgeE} + \delta_n \text{Age2N} \\ + \delta_e \text{Age2E} + \boldsymbol{\zeta_n} \boldsymbol{EducationN} + \boldsymbol{\zeta_e} \boldsymbol{EducationE} + \varepsilon \end{aligned} \quad (5.7)$$

where the results were similar as in the previous case. Out of the eight compared coefficients, only in one case were they significantly (at the 5% level) different. If we compare this to the results of empty regressions, we find that it is very similar. Usually we are not able to reject the hypothesis of coefficient equality, with the exception of an occasional pair of coefficients.

Next, we shall impute the years of education, and compare the coefficients of the variable *EducYrs.* We use a regression in the form (5.8)

$$\begin{aligned}\ln \text{hwage} = \alpha + \gamma_n \text{AgeN} + \gamma_e \text{AgeE} + \delta_n \text{Age2N} \\ + \delta_e \text{Age2E} + \boldsymbol{\zeta_n} \text{EducYrsN} + \boldsymbol{\zeta_e} \text{EducYrsE} + \varepsilon \end{aligned} \quad (5.8)$$

and receive the returns $\zeta_n = 0.054$ and $\zeta_e = 0.1$, both with a standard error close to zero. We can reject the equality of these coefficients at the one percent level of significance, and we can see that the coefficient has declined with the presence of the ninth grade. This would seem to comply with the weaker hypothesis proposed earlier. However, it is still necessary to compare this result to the results of the empty

[12]Remote data access was generously granted by Stepan Jurajda.

regressions. After examining them we see that 14 out of the 16 pairs of coefficients had significant differences, all of them in the same direction as in our regression. However, the magnitude differs. The average values of the empty regression are $\overline{\zeta_{ne}} = 0.091$ and $\overline{\zeta_{ee}} = 0.087$, which is a significantly smaller difference (taking into account the miniscule standard errors) than the one we have received. Thus, we can conclude that we have really received results which agree with our weaker hypothesis: an extra year of education provides smaller returns than the previous ones.

Finally, we can deal with the question of the significance of the dummy *Ninth.* As previously, we shall do so by running regressions in the forms (5.9).

$$\begin{gathered}\ln \text{hwage} = \alpha + \gamma \text{Age} + \delta \text{Age2} + \boldsymbol{\zeta} \boldsymbol{Education} + \lambda_d \text{Ninth} + \varepsilon \\ \text{and} \\ \ln \text{hwage} = \alpha + \gamma \text{Age} + \delta \text{Age2} + \boldsymbol{\zeta} \text{EducYrs} + \lambda_i \text{Ninth} + \varepsilon\end{gathered} \tag{5.9}$$

In both cases we received similar results, that is $\lambda_d = 0.009$ and $\lambda_i = 0.01$ with standard errors 0.004 and 0.000 and p-values of 0.055 and 0.000 for the case with dummies for highest educational certificate and imputed years of education, respectively. Comparison with the empty regression show that these results seem fairly usual, because in 11 out of the 16 cases when we test for significance of *Ninth* in the imputed cases and in 10 out of the 16 in the dummy variable cases we cannot reject the null hypothesis that $\lambda_i = 0$ and $\lambda_d = 0$, respectively, at the five percent level of significance. That is similar to our current case, in which in one case the result is significant, and in the other is not. The general insignificance also manifests itself in the average values of $\overline{\lambda_i}$ and $\overline{\lambda_d}$ for the empty regressions, which are both 0.000. If this would be the result of educational legislature changes, rather than some unspecified fluctuation in the data, I would more expect the dummies to be significant in both cases, similarly as when we used SILC. Thus, I interpret these results for ninth as not indicating any change caused by the canceling of the ninth grade.

To sum up the results, using the Structure of Earning Survey data I was not able to find any impact of a ninth grade on the coefficients for highest attained education, which corresponds with the results using SILC. The comparison of returns to a year of education is more intriguing, leading to clearer results than in the SILC cases by finding a decline in the returns to a year of education related to the presence of ninth grade where the previous analysis provided uncertain results. As the main problem of the SILC regression was the lack of the discerning capabilities of the model used and the small data sample, I think it is likely that this result has improved on the previous estimates, finding a relationship before not clearly shown. Finally, we found

Ninth to not be significant when compared to the trend for the empty regressions.

Having said this, it is important to recapitulate that difference between using SILC and SES. The difference in results may be also caused by using the explanatory variable age instead of worked years. Definitely, any coefficients received from the SES study should be taken as somewhat higher when compared with the SILC results, as their coefficients are actually net of an extra year of experience. This helps us to conciliate the results for SILC and SES, as discussed further in the section Results.

6 Results

To make the orientation in the results I have obtained simpler, I shall tabulate them depending on the methods used for attaining them. I shall then interpret the differences that have come up throughout the thesis as a result of different regressions. The methods I have used can be split into the following groups:

- Regressions using *EducYrs* (recoded differently for cases with and without ninth grade), and then testing for equality of the returns to a year of education.
- Regressions using *EducYrs* (recoded in the same way for all cases), and then testing the extra dummy *Ninth* for significance.
- Regressions using dummies for each level of *Education*, and then testing the extra dummy *Ninth* for significance.
- Regressions using dummies for each level of *Education*, and then applying the test of equality to the returns to certificates for each level of certificates.

and furthermore,

- Empty regression for the cohorts 43 to 58, which did not experience any changes in law, and which I used to estimate the effect of variables left out from my model (most importantly those related to the passing of time). For illustration, I report the average values for returns to a year of education, the overall trends in their differences, and the average values of the coefficient for *Ninth*.

All of these regressions were run on both the SILC and SES datasets. Aside from the regressions in table Results I also ran the regressions for individual years of data collection (in case of SILC) and for the alternative financial explanatory variables. These did not have any important impact on my results, and so I do not list them. Otherwise the table contains estimate values, standard deviations (in brackets), significance levels, and ↓↑ symbols showing in which direction the coefficient changed with the presence of the ninth grade. In the case of returns to certificates, the reported coefficients are for the highest certificates attained by the individual (shortest vocational education to Ph.D), and the column Difference compares the previous two columns.

Firstly, I shall mention the regressions using dummies for each level of *Education*, and then compare their results. As these differences were largely insignificant among the cohorts where legislative changes occurred (12 to 2 in SILC, 7 to 1 in SES), and there was some variation among the empty regressions with more differences being insignificant (65 to 35 in SILC, 86 to 42 in SES), I interpret this as evidence of

Table 9: Results

	Returns to a Year of Education, 9 years of schooling	Returns to a Year of Education, 8 years of schooling	Difference significant, trend (↑increase, ↓decrease)	The significance of *Ninth*, using imputed years of education	The significance of *Ninth*, using dummies for attained certificate
Around 39, SILC	.081 (.003)	.085 (.003)	No. (p-value = 0.322)	-.021 (.011) (p-value = 0.061)	-.023 (.011) (p-value = 0.044)
Around 70, SILC	.019 (.001)	.024 (.001)	Yes, ↓ . (p-value = 0.009)	-.024 (.004) (p-value = 0.)	-.023 (.004) (p-value = 0.)
Average SILC with no reforms, cohorts 43-58,	.093	.093	9 No, 7 Yes.	-.102	-.103
Around 39, SES, using hourly wage	.054 (.000)	.1 (.000)	Yes↓ (p-value = 0.000)	.01 (.000) (p-value=0.)	.009 (.004) (p-value = 0.055)
Around 39, SES, using quarterly wage	.062 (.001)	.11 (.001)	Yes↓ (p-value = 0.000)	.012 (.000) (p-value=0.)	.007 (.006) (p-value = 0.264)
Averages SES with no reforms, 43-58, hourly / quarterly wage	.087 / .01	.091 / .105	2 No, 14 Yes. ↓	.000/.000	.000/.000

no relevant change in the returns to certificates after the occurrence of legislative changes. Because of the amount of numerical values involved, I do not report these results in the table. In the context of the stronger hypotheses stated in the beginning, this result complies - lengthening of education causes no changes in the returns to the certificate. The reliability of these results is strengthened by the fact that they hold for both the SILC and the SES datasets. As has been discussed in the beginning, the stronger hypothesis implies the weaker one, which again fits with our results, see bellow.

Now let us discuss the table. The first thing worth noticing is that the SILC returns to a year for those around 39 are reasonably close to the returns for the years where nothing happened (the closest cohort to 39 with an empty regression, 43, has returns equal to 0.085, and 0.81, see table Confounding Variables, returns), while the returns for those around 70 are much lower. This is unsurprising, because the pensions are only weakly income dependent, and thus the returns will understandably be significantly lower than the average for working individuals. Secondly, we see that in the SILC regression we were not able to find a significant difference in the returns to a year of education. However, there remains the fact of a significant negative change in the returns to a year of education for the cohort around 70 related to having gone through a ninth grade.

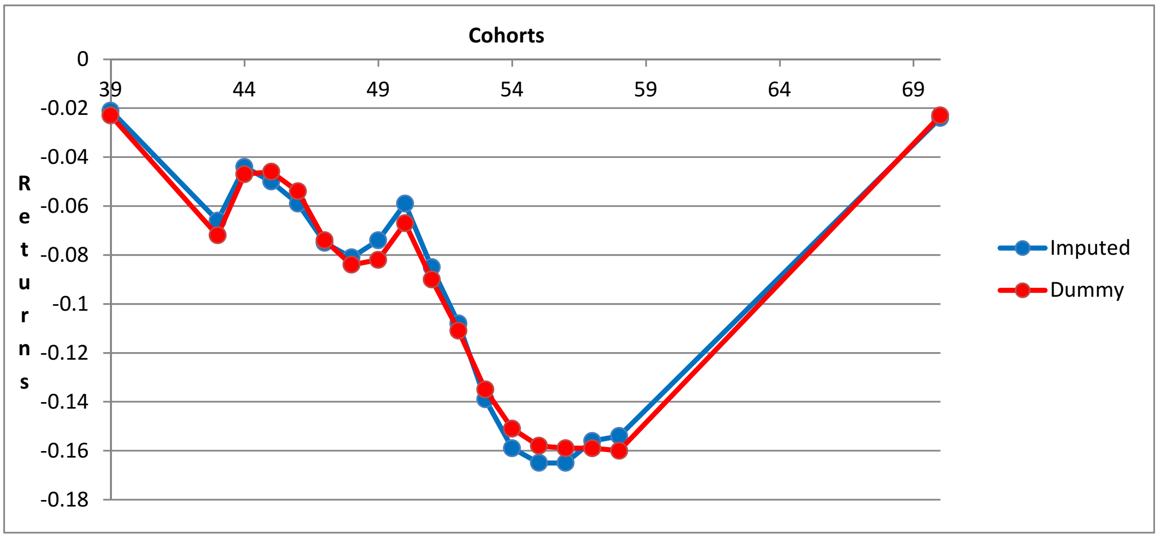
The question is the more interesting, if we take into account that the income from the pensions does not change with age (except for valorization with inflation), and so the returns should represent the incomes for the several last years of the productive life on both sides of the cutoff. One possible but false explanation is that the inequality of the cohorts on the different sides of the cutoff point is caused by a different number of working and retired individuals. However, as the average retirement age in the Czech Republic is under 61 years (Eurostat, 2011), and after examination of the data I found a negligibly larger number of individuals with at least a part-time job in the younger cohorts (a difference of 60 on a data set of more than 5,500 observations), we can be fairly certain that the cohorts are similar in this aspect. Thus, I can safely eliminate this explanation.

Another possible explanation is to argue that this negative change is only a fluctuation similar to the ones that occur among the empty regressions for non-educational reform reasons, and still cause significant differences in the estimates. However, regression on the SES data shows us that the reduction in the returns to a year of education also holds there. I consider these two regression on two different datasets to be sufficient evidence for concluding that we cannot reject our weaker hypothesis, the hypothesis of a reduction in returns to a year of education related to an extra experienced year of education. In other words, we have found diminishing returns to years of education. This makes sense, because since we were not able to reject the stronger hypothesis, it would be strange if we were able to reject the weaker one.

Finally, there is the question of the coefficients of the independent dummies *Ninth*. In SILC, which we shall discuss first, we have two sets of these results, which are very similar, and in both cases significant. However, because of their dummy variable nature they are also more likely to include effects not related to the educational reforms.

For SILC, the results show a significant difference in coefficients for *Ninth* between the empty regressions and the regressions with a change in law. It is worth mentioning that the average values of the empty regressions do not provide all the needed information here. The negative impact of *Ninth* had a trend in the empty regressions, beginning at roughly -0.06 for the cohorts after 43, and ending at -0.16 for the cohorts before 58. In both cases the difference between the empty regressions and the regressions with a cohort influenced by education reform is very large, for those around 58 and 70 even more so. The result is the same when comparing the dummies gained from the empty regressions using *Education* dummies (the coefficient starts at -0.07 and ends at -0.16). To see the trend and magnitude in detail, see table Table 8 on page 34, and figure Coefficients of *Ninth*.

Figure 6.1: Coefficients of *Ninth*



The interpretation I would propose here is the one I have already implied earlier. The fact that *Ninth* has a negative coefficient independent of the occurrence of any educational reform means that there is a negative trend in the model I use when being applied to the SILC data. We could expect the explanation of this trend to be connected to the different structure of cohorts caused by the different ages of the individuals of which they consist. Nevertheless, its nature is not that important. As the presence of *Ninth* when educational reforms occurred had a significantly smaller negative coefficient than in other periods, I would assume that ninth grade as such had a positive effect on the returns to education, but this effect is masked by the trend which makes the coefficient of *Ninth* negative. These results would then indicate that the change in the length of education did actually have a certain positive impact on the incomes of the individuals. We could make an extremely rough estimate of the value of the *NinthTrue* coefficient as the difference between the average return to the empty regressions and the regression coefficient we have gained, which gives us approximately 0.079.

Furthermore, the question still remains as to how these results comply with the results for the dummy *Ninth* used on the SES dataset. It must be noted that there is quite some uncertainty concerning these results, and I am not able to provide a clear-cut answer. As in one case the result is significant and in the same direction as when using SILC, and in the second it is insignificant, I think a clear interpretation of these results is not possible. An explanation for the observed differences could be for instance the usage of the variable age instead of the variable worked years in the SES regressions, which in effect reduces the coefficients that we have received from SES, and thus makes the comparison between the models more difficult.

Furthermore, there is uncertainty concerning the relationships with the previously received results - the results from a positive coefficient for *Ninth* imply that there should be at least some minor positive returns to a certificate of education, while the comparison of returns to certificates implied none. Further conciliation of these two sets of results requires inquiry above the scope of this thesis. The final conclusion we can make on this topic is that the returns to an educational certificate seem to be non-existent to very small and positive.

Finally, I would like to stress a point made in the beginning of my thesis, and that is that we cannot be sure to what extent the effect we have measured is caused by longer mandatory education and to which by the longer time spent in basic school because of its extension.

7 Conclusion

In my thesis I tried to discover what impact an extra year of education has on the incomes of individuals. To do this, I compared individuals who went through different lengths of primary and lower secondary education. I was able to do this by cohorts thanks to a natural experiment caused by legislative changes during (except one instance in 1996) the period of socialist rule in Czechoslovakia, which on several occasions changed the length of primary and lower secondary education from nine years to eight, or from eight years to nine.

I examined these occurrences on two datasets - the SILC (Statistics on Income and Living Conditions) and the SES (Structure of Earning Survey) - and along several lines. The first dividing line was the year in which the legislative change took place: I examined the cohorts which experienced the change in 1984 and those in 1953 (in both cases a reduction of length from nine years to eight years), i.e. those, who were 39 and 70 in 2009, respectively. Next, the regression I used depended on whether I chose imputed years of education, or dummy variables for the highest attained educational certificate. Finally, the methods differ in what I examined - whether the equality of educational coefficients for the cohorts between and after the educational change, or the significance of a dummy variable representing the presence of a ninth grade of education. To sum this up, I examined the following: equality of coefficients for years of education using imputed years of education, equality of coefficients for highest attained educational certificates using dummies for the highest level of education, and the significance of a dummy variable representing an extra year of education using first imputed years, then dummies for highest certificate.

To control for variables not included in my model I also ran comparison regressions on cohorts, which were not influenced by a legislative change, and then compared those results with the original results for the cohorts, which were influenced. I interpret the net received effect as a result of the change in the length of education.

My aim was to examine two aspects of the change in educational length, each related to a different form of the regression used. These were the changes in the returns to a certificate of education and the changes in the returns to an extra year of education. My results are that I was not able to find any changes in the returns to the certificate using the comparison of the certificates, and I was able to find a very small and positive change to the returns by using a regression with a dummy variable representing the presence of the ninth grade. The impact of an extra year of education on the returns to the average years of education was negative, which is intuitive, as it corresponds with the law of diminishing returns, and is implied by the presence of no (or very small) positive change in the returns to a certificate.

At the end of my thesis I would also like to mention possible ways in which the natural experiment with which I have worked with could be further utilized in economics research. I have chosen this topic for my thesis because I consider this historical period to be under-examined, and I believe that the information provided by these reforms has not yet been used to its full potential. There is still work to be done!

Firstly, I think the estimates I have made could be improved by using more data. The biggest source of such data would be the Slovak Statistics on Income and Living Conditions, which I have regretfully not been able to obtain. Secondly, I did not use the last cutoff year in 1996, because those who have undergone the reform have not yet been on the labor market long enough for their returns to education to differentiate sufficiently. However, in a few year's time the data will be ready for an analysis of this change, and as this change was very sharp and also the most recent, meaning the best data is available, I believe this will be a huge opportunity to better examine the effect that an extra year of education had.

Also, if one would have direct access to a larger dataset (such as the Structure of Earnings Survey) modeling of the returns to education for successive cohorts, and transferring the problem analyzed in my thesis into the domain of time series analysis, could provide further insights on the behavior of the returns to education. Regrettably, applying this method on a smaller dataset is unlikely to provide results because of the amount of noise in the data.

But why restrict the question to income? There are more aspects of life of the society that could be included, and which did not fit into the scope of this thesis. One obvious possibility would be to examine the impact of leaving school a year early on the number of children an individual has. This research would also have several upsides, one of them being that the number of children per female is not influenced by higher age after the beginning of menopause, which simplifies comparisons for different cohorts. And for older cohorts, there is even a dataset offering precisely this type of data: SHARE, the Survey of Health, Aging and Retirement in Europe.

The last direction of research based on these changes in law that I would like to propose is the impact on employment. Even though the relationship between education and employment is not that strong - over education being a serious problem - I think it would be worth examining, if there might be any link connecting the two, because reduced unemployment would be a great result of an extra year of education. Inspiration could be gained for instance from Nickell (1979). However, the data source is the problem - SILC does not provide enough observations for the unemployed, and understandably nor does the company Structure of Earnings Survey.

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8 Bibliography

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