BUDAPEST WORKING PAPERS
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BWP 2018/1

INSTITUTE OF ECONOMICS, CENTRE FOR ECONOMIC AND REGIONAL STUDIES
HUNGARIAN ACADEMY OF SCIENCES
BUDAPEST, 2018

Budapest Working Papers on the Labour Market

BWP – 2018/1

Institute of Economics, Centre for Economic and Regional Studies,
Hungarian Academy of Sciences

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January 2018

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Abstract

This paper examines the effects of increasing the compulsory school leaving (CSL) age from 16 to 18 in Hungary using a regression discontinuity design (RDD) identification strategy. The new CSL age was introduced for those entering their first year of elementary school in 1998. Identification is based on compliance with the age of elementary school start rule. Compliance with the age rule creates a discontinuity in the probability of starting school under the higher CSL age regime around a cutoff date of birth. The treated cohort had known about the change since age 6. This fact allows for testing on how the increase affected forward-looking decision making about secondary school track choice which occurs at age 14. The legislation change resulted in an increased probability that children would choose the academic high school track instead of vocational training schools. At the same time, those choosing vocational training schools are more likely to drop out under the higher CSL age scheme. Potential explanations of increased dropout rates include a decrease in the quality of teaching in vocational training schools due to supply constraints, and a shift in student composition to include more students from lower socioeconomic backgrounds.

Keywords: education, school choice, compulsory school leaving age, regression discontinuity design

JEL: J08, C21, I21, I26

Acknowledgement: I am grateful to Gabor Kezdi for his advice on how to proceed with this research. I thank Hessel Oosterbeek, Robert Lieli, Marta Lachowska, Adam Szeidl, Zoltan Hermann, Daniel Horn and Julia Varga for their comments to the earlier versions of this paper, and, to Agota Scharle and Balazs Varadi for their ongoing suggestions. I am thankful for all remarks to the participants of the weekly therapy sessions of Gabor Kezdi at CEU, the seminar at the Budapest Institute in Sept 2015, the 2015 MTA conference in Szirak, the MKE 2015 conference in Budapest, to Gyorgy Martonfi and the participants of the seminar at OFI in Feb 2016, and to the participants of the Workshop in Education Economics 2016 in Maastricht. I especially thank for all the help regarding data access to the Budapest Institute, to the Institute of Economics of the Hungarian Academy of Sciences, to Gabor Kezdi, Tibor Czegledi and the research room team of the Institute of Economics at the Hungarian Central Statistical Office. The usual disclaimer applies.

A kötelező iskolalátogatási korhatár emelésének hatása az iskolaválasztási döntésre és a lemorzsolódásra

Adamecz-Völgyi Anna

Összefoglaló

A tanulmány a kötelező iskolalátogatási korhatár 16-ról 18 évre emelésének hatásait vizsgálja Magyarországon. A megemelt korhatár azokra a gyerekekre volt először érvényes, akik 1998 szeptemberében kezdték az általános iskolát. A tanulmány szakadásos vizsgálati keretben becsli meg a korhatáremelés hatását, aminek alapja az iskolakezdési szabályhoz való alkalmazkodás. Azok közül, akik alkalmazkodtak az iskolakezdési szabályhoz, akik 1991 májusában születtek, még 1997-ben, míg azok, akik 1991 júniusában születtek, már 1998-ban, az új korhatár alatt kezdték az általános iskolát. Az iskolakezdési szabályhoz való alkalmazkodás tehát szakadási pontot hoz létre a gyerekek születési idejében 1991. június 1. körül, amely pontban megugrik az új korhatár alatti iskolakezdés valószínűsége. Az új korhatár ténye már a gyerekek 6 éves korában ismert volt, ezért a jogszabályváltozás lehetővé teszi a korhatáremelés a középiskolaválasztásra kifejtett hatásának vizsgálatát. A korhatáremelés hatására nőtt a gimnáziumban, és csökkent a szakiskolában való továbbtanulás valószínűsége a szakadási pont körül. Különösen emelkedett a gimnázumban való továbbtanulás és az érettségi megszerzésének valószínűsége az alacsony iskolai végzettségű szülők gyermekei között. Ugyanakkor azok, akik szakiskolába kerültek, kisebb valószínűséggel végezték el a választott iskolát, mint korábban. A megnövekedett szakiskolai lemorzsolódási arányok hátterében egyrészt kínálati oldali korlátok állhatnak, mert a megnövekedett létszámot nem kísérte a szakiskolák anyagi és módszertani erőforrásainak bővítése. Másrészt, a diákok összetétele is megváltozott azáltal, hogy több hátrányos helyzetű családból származó gyerek kerülhetett és maradhatott hosszabb ideig a szakiskolákban. A kutatásból levonható legfontosabb szakpolitikai tanulság, hogy a magasabb iskolalátogatási korhatár csökkentheti a társadalmi egyenlőtlenségeket, de a keresleti oldali oktatási expanziót kínálati oldali erőforrásbővítéssel együtt kell végrehajtani.

Köszönetnyilvánítás: Nagyon hálás vagyok Kézdi Gábor folyamatos tanácsaiért és bíztatásáért a kutatás során. Köszönöm Hessel Oosterbeek, Lieli Róbert, Marta Lachowska, Szeidl Ádám, Hermann Zoltán, Horn Dániel és Varga Júlia a tanulmány korábbi verzióihoz fűzött kritikáit és javaslatait, és Scharle Ágota és Váradi Balázs tanácsait. Köszönettel tartozom az adatokhoz való hozzáférésért a Budapest Intézetnek, az MTA KRTK Közgazdaságtudományi Intézetnek, Kézdi Gábornak, Czeglédi Tibornak, és a KSH-KRTK Kutatószoba stábjának. Minden fennmaradt hiba a sajátom.

JEL: J08, C21, I21, I26

Tárgyszavak: oktatás, iskolaválasztási döntés, kötelező iskolalátogatási korhatár, szakadásos vizsgálat

1 Introduction

Compulsory school leaving (CSL) age policies introduce a constraint in making a decision about how much time and effort to invest in attending school. A wide range of literature estimates the effects that increasing the CSL age has on social and economic outcomes such as wages (Meghir and Palme, 2005; Grenet, 2013, etc.), mortality (Lleras-Muney, 2005), fertility (Black et al., 2008), crime (Lochner and Moretti, 2004), or voting behavior (Milligan et al., 2004). The evidence of this literature is mixed. In some cases, increasing the CSL age has had positive wage returns (Oreopoulos, 2007; Devereux and Hart, 2010), while in other cases there have been no returns (Oosterbeek and Webbink, 2007; Pischke and Wachter, 2008). Some increases in the CSL age have had impact on fertility decisions (Cygan-Rehm and Maeder, 2013; Black et al., 2008), while in other cases there have been no impacts (McCrary and Royer, 2011). Grenet (2013) tries to open the black box of CSL age legislation changes by comparing increases of similar sizes in Britain and France, which resulted in positive wage returns in only one country. He finds that after the reform, the share of students dropping out decreased sharply in Britain but not in France. He comes to the conclusion that a higher CSL age brings labor market advantages only if it leads to increased rates of school completion as well.

In spite of the enormous literature on the social and economic impacts of an increased CSL age, we know very little about its effects within schools: on school quality, student composition, secondary school tracking choice, or school completion rates. Learning more about its in-school effects may deepen our understanding of why CSL age reform is successful in one context but not in another. The emerging evidence on the in-school effects of increasing the CSL age is quite controversial. Higher CSL age has been shown to reduce the effort levels that teachers put into teaching (Green and Navarro Paniagua, 2012), and to increase criminal behavior of students within schools (Anderson et al., 2013). Cabus and De Witte (2011) show that increasing the CSL age decreased dropout rates in the Netherlands, while Landis and Reschly (2011) find that a higher CSL age has an effect on the timing of dropping out, but not on high school completion rates.

This paper estimates the effects that an increase of the CSL age from age 16 to 18 in 1996 had on schooling outcomes in Hungary using a regression discontinuity design (RDD) strategy. The new CSL age came into force with students starting elementary school in September 1998. Identification is based on the age of elementary school start rule. Children compliant with the age rule started elementary school under the new CSL age scheme if they were born on June 1, 1991, or later. Those compliant with the age rule and born before this date had started elementary school in the previous year, under the old CSL age scheme. Thus, a natural cutoff point occurs at this date of birth which allows me to construct a fuzzy RDD strategy to estimate the intention to treat (ITT) effects of the increase using the 2011 Hungarian Census data. Typically, children start school either according to the age rule, or they will start a year later; early school start is rare. I estimate compliance with the age rule by birth month bins from an earlier 2001 Hungarian Census, which captures the relevant cohorts during their first 3-4 grades of elementary school. On average, about one-fifth of all first graders start primary school one year later than what is determined by the age rule, and this ratio is 54% right before the cutoff (see Figure 2 in Section 2). Compliance with the age rule on the treated side of the cutoff is 87%. Using actual compliance rates as a first stage, I estimate the Local Average Treatment Effects (LATE) of increasing the CSL age in a two-sample two-stage least squares (TS 2SLS) setup (Angrist, 1990). The probability of starting elementary school under the higher CSL age scheme jumps about 0.33 at the cutoff causing LATE coefficients to be roughly 3 times as big as the ITT impacts. The almost 90%-compliance on the treated side of the cutoff is close to the situation of one-sided non-compliance. This setup gives the rare possibility to identify the Average Treatment Effect on the Non-treated (ATNT), which is close to the LATE in this special case.

To put the analysis in context, it is important to note that the Hungarian reform is unique for three reasons. First, the increased CSL age was introduced to students starting elementary school in September 1998. Thus, the first treated cohort knew already at age 6 that they would have to stay in school for two years longer. This fact allows me to test whether this information affected forward-looking decision-making as measured by secondary school choice at age 14. Second, the increase of the CSL age was mostly an administrative change with no supply-side expansion at the time when the first treated cohort reached age 16. While most papers find substantial positive returns on higher CSL age, they usually examine the effect of comprehensive education reforms, including sometimes massive school expansion (Harmon and Walker, 1995; Oreopoulos, 2007; Devereux and Hart, 2010). On the contrary, the Hungarian case allows for testing the pure marginal effects of a change in CSL age legislation. Third, the increase happens in an education system with early tracking and strong selection mechanisms (OECD, 2015). Higher CSL age in this environment may strengthen the already existing phenomenon that students from lower socioeconomic backgrounds get selected to low quality schools.

I find that as a result of the CSL age increase, children at age 14 were more likely to choose the more demanding and more beneficial 4-year academic high school track instead of the 4-year vocational training school track. The legislation change did not influence more individuals to start secondary school, but those who did decide to start were more likely to opt for an academic high school rather than a vocational training school. At the same time, those who did choose the vocational training school track were more likely to drop out under the new scheme. The data suggest two potential explanations for this adverse effect. First, the financial and human resources allocated to vocational training schools were not adequate for the sudden increase in the number of students. Second, as the students from higher socioeconomic standings, and with stronger abilities chose academic high schools instead, and lower standing, lower performing students stayed in vocational training school longer, the distribution of students in vocational training schools shifted towards lower socioeconomic status students.

The last takeaway from this analysis is that increasing the CSL age may not always be a good instrumental variable (IV) for education. It harms the monotonicity assumption of the instrument if the quality of education for some students is negatively affected by the increase (Cygan-Rehm and Maeder, 2013). The monotonicity assumption requires students to be impacted by the instrument in the same way (Angrist and Pischke, 2009). In this context, it would assume that the legislation change induced some individuals to have *more* education and for no one to have *less* education, both in terms of length, tracks, quality, and earned degrees. In the case of the Hungarian reform, this concern is valid if one wants to use the increase of the CSL age as an IV to education, as the legislation change did increase dropout rates in vocational training schools.

The remainder of the paper is organized as follows. Section 2 introduces the main data sources. The Hungarian education system and the legislation change is presented in Section 3. Section 4 presents the identification strategy and the empirical methods, and Section 5 shows the main results. Section 6 looks at the heterogeneous effects of the legislation change by parental education. Section 7 provides several robustness checks, and Section 8 shows additional evidence on the findings and the potential mechanisms behind them. Section 9 summarizes and discusses the results.

2 Data

As I have to refer to the data when presenting the Hungarian education context and my identification strategy, I start by briefly describing the main data sources used in this paper. Some additional data sources which are used to estimate heterogeneous effects by parental education will be presented in Subsection 6.1.

Four main data sources are used in this analysis: the 2001 Hungarian Census, the National Assessment of Basic Competencies (NABC) database, the Public Education Statistics (PES) of the Public Education Information System, and the 2011 Hungarian Census.

As will be detailed in Section 4.1, I estimate the effects of increased CSL age in a fuzzy RDD framework. Compliance with the age of elementary school entry rule creates a discontinuity in the probability of being exposed to the new CSL age scheme around a cutoff in date of birth. This jump in the probability of starting elementary school under the new CSL age scheme around the cutoff is going to be the first stage of my RDD strategy. I estimate the first stage using the 2001 Hungarian Census. The 2001 Hungarian Census data were collected in the spring of 2001 when the cohort of interest was 9-10 years old. It contains information on the birth year and month of the individuals, and, for those in school, it registers which grade of school they were attending at the time. Knowing their grade level in 2001 allows me to estimate the jump in probability of starting school under the new CSL age regime in birth year and month bins.

The NABC [1] administrative database is used to provide a robustness check to school start compliance rates estimated from the 2001 Hungarian Census. The NABC database registers the results of centrally organized low stake math and reading tests taken each year in Grades 6, 8, and 10. The NABC database provides information on the students' birth year and month. Thus, it allows estimating the probability of starting elementary school under the higher CSL age scheme in birth year and month bins, just as the 2001 Hungarian Census. The main difference between these two data sources is that while the 2001 Hungarian Census covers the whole cohort of interest, the NABC data cover a subsample of 10$^{\text{th}}$-graders only[2].

[1] In English: http://edecon.mtakti.hu/?q=node/15, in Hungarian: http://www.oktatas.hu/kozneveles/meresek/kompetenciameres/alt_leiras

[2] The first NABC wave available to use is the one taken in the spring of 2006. Those starting elementary school in 1997, just before the CSL age increase, typically reached Grade 8 in the 2004/2005 academic year and Grade 10 in the 2006/2007 academic year. Those falling under the new CSL age legislation reach these

The PES of the Public Education Information System[3] is the official Hungarian school census database. It collects extensive information on schools, school programs, and students. For the cohorts and periods of interest for this paper, the PES provides aggregate level data across school cohorts and academic years. The PES data, along with the 2001 Hungarian Census and the NABC, are used to demonstrate that compliance with the age of elementary school entry rule did not change as a result of the CSL age increase (see Table 1).

The outcome measures used in this paper are constructed from the 2011 Hungarian Census data. While the 2001 Hungarian Census data were collected in the spring, the 2011 data were collected in October 2011, when the cohort of interest was 19-20 years old. Another important difference between the 2001 and the 2011 Censuses is that the latter contains information on the exact date of birth of individuals, including the day of birth, whereas the former does not. The 2011 Hungarian Census covers several educational outcome variables, i.e. the number of years completed successfully in the education system by school type, the highest earned degree, and whether one was in school at the time of the Census. Similarly to the 2001 Census, the 2011 data did not capture the important information on grade repetition. All educational outcome variables constructed from the 2011 Census data are defined in Table 29 in Appendix A.

3 The Hungarian Education System and the Legislation Change

3.1 The Context of Hungarian Education and the Public Education Act (1996)

The Hungarian education system has long faced challenges in providing high quality education for students of differing backgrounds (OECD, 2015). Student achievements continue to stand below the OECD average, and the effects of socioeconomic background on test scores were noted as being among the largest in the 2012 Program for International Student Assessment (PISA) study (OECD, 2014). Free elementary school choice and early tracking have hindered equity and have caused a high variance of student achievements across schools. In an attempt to improve this situation and specifically to reduce the number of early school leavers, and to close the education and employment gap between those of higher and lower socioeconomic backgrounds, the compulsory school leaving (CSL) age was increased from 16 to 18 in 1996.

Before the legislation change, students were obligated to attend school until the end of the academic year in which they turned 16. The Public Education Act (1996) increased compulsory school attendance from age 16 to age 18, requiring students to spend two more years in the education system. The new legislation was grandfathered in, as it first became binding with those students starting elementary school in September 1998. Thus, students

grade levels a year later. Thus, the Grade 10 waves cover both those born right before, and right after, the cutoff and can be used to estimate the jump in the probability of starting elementary school under the higher CSL age scheme (see Tables 28 in Appendix A for detailed information about the sample and estimation method).

[3] *KIR-STAT* in Hungarian.

knew already by age 6 that they had to stay in school two years longer. Although the Act introduced other measures as well, the increase in the CSL age was the only element causing sharp changes for those starting elementary school in the 1997/98 academic year versus those starting in the 1998/99 academic year. The Act also prescribed the gradual adaptation of the secondary school structure to meet the new CSL age by forcing all secondary school programs to have at least 4 grades (and thus not to end before age 18), a process that began during the 1998/1999 academic year. As a result, the first treated cohort was beginning secondary school at age 14 in 2006 at a time when the adjustments to secondary school program length had been adapted half a decade earlier.[4]

All actors within the education system supported the increase of the CSL age at the time of its enactment. However, it has been viewed controversially since the first treated cohort reached the age of 16. As it was grandfathered in, the Act pushed all implementation costs to the future government of 2008. This responsibility came about at the time that the first affected cohort reached age 16, the age at which students who would have dropped out in absence of the legislation change had to stay in school (National Institute of Public Education, 2010). Although the CSL age change and the number of potentially affected students had been predicted well in advance, the education policy at this time did not actively support the implementation of the increased CSL age. The schools and their leading bodies began to realize that they lacked the tools to handle the problems emerging in 2008 (National Institute of Public Education, 2010). The increased number of students put so much strain on unprepared schools that most school principals viewed the CSL age increase unfavorably, according to a 2009 survey (Mártonfy, 2011a). The most frequently expressed problems included that schools had no methods to engage unmotivated students in learning, that they were unable to offer a credible perspective on life to these mostly low socioeconomic standing and low-skilled students, and that they had no expertise in the development of students from troubled backgrounds (Mártonfy, 2011b). Due to the emerging problems and some other, mostly political considerations, the National Public Education Act (2011) reduced the CSL age from 18 back to 16, starting from September 2012. This paper exclusively evaluates the increase that occurred in 1996.

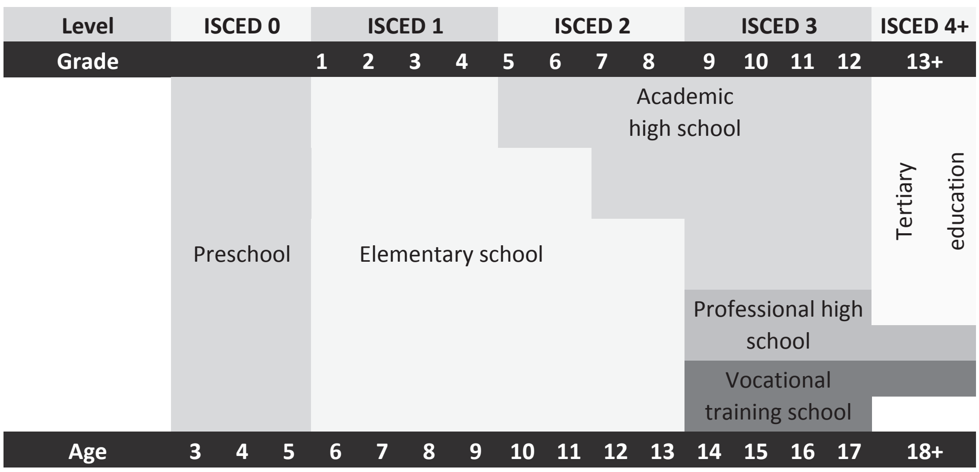
Despite the lapsed time since the CSL age increase was made and then retracted, there has not been any quantitative assessments on the casual effects of this legislation change to date. The only known attempt to evaluate the impact of the CSL age change was compiled by Dobos (2014) in an unpublished study. Dobos attempted to use a “discontinuity band” identification strategy but she was unable to capture any causal effects of the legislation change because her treated and control groups are too far apart in age. This paper aims to fill the gap by providing a quantitative evaluation to find a causal effect.

[4]Schooling outcomes may be affected by other schooling-related legislation changes. Such legislation may be the Child Benefit (“családi pótlék”) which is a family-type cash allowance given to parents after children, either until CSL age, or, until at most age 20 if children continue their studies in the formal education system until then. In 2010, its availability conditions were tightened and it was supposed to be stopped if the child accumulated at least 50 teaching hours unjustified absence from school in one academic year. The new legislation came into force in the 2010/2011 academic year (on Aug 31, 2010), when the cohorts relevant to this paper were already 18-19 years old. Thus, the change in Child Benefit legislation could not have affected the results of this research.

3.2 Compulsory Education in Hungary

Typically, Hungarian students reach CSL age while in secondary school. There are three types of secondary schools in Hungary: vocational training school, professional high school, and academic high school (see the structure of the Hungarian compulsory education system in Figure 1). The core programs in all types of secondary school last for 4 years during that time, both right before and right after the CSL age increase. There is a difference in the returns of vocational training school from those of the two types of high schools. Vocational education is considered as a dead end in the sense that, although theoretically it is possible, most students do not continue their studies after completing a vocational training school. The Mincer-type returns of vocational education are low but still positive in comparison to the returns of an elementary school degree (Kézdi, Köllő and Varga, 2009). A high school degree, on the other hand, brings substantial benefits.[5] Firstly, it is the prerequisite for starting tertiary education. Secondly, it gives high returns in the labor market, both in terms of employment probability, and wages. The average wage advantage of a high school degree is estimated to be about 25-30% higher when compared to a vocational degree, amounting to around 30,000 USD during a lifetime (Hajdú et al., 2015).

Figure 1: The Structure of the Hungarian Secondary Education System



Source: Horn, 2014. For the cohort of interest, preschool was compulsory from age 5. The CSL age was 16 in the case of those starting elementary school in September 1997 or earlier. The CSL age was 18 in the case of those starting elementary school in September 1998 but was then reduced to 16 again in September 2012 for students in any grades.

[5] Both high schools involve the completion of a "maturity exam" ("érettségi") at the end of Grade 12, which is similar to "Matura" or "Baccalaureat" examinations found in many European countries (Kézdi and Surányi, 2008).

Prior to the CSL age increase in 2001, 13% of 15-year-olds were attending primary school, and 83% were attending secondary school right before reaching the actual CSL age of 16 that was in place at that time.[6] Out of those in secondary school, 16% were in vocational training schools, 45% in professional high schools, and 39% in academic high schools. [7]

Students decide on their secondary school tracking choice at an early age in Hungary (OECD, 2015). Some highly selective elite academic high schools already recruit top-talent students during Grade 4 and Grade 6. However, most students choose their secondary school at age 14, before entering Grade 9. The cohorts of interest to this study chose between the 4+ year vocational training school, the 4+ year professional high school and the 4+ year academic high school tracks.

Primary school starts at age 6 and has 8 grades. According to the age of elementary school start rule, compulsory schooling starts on September 1 of the same year in which one reached age 6 by May 31. Those born on June 1, or later, during the same year start elementary school one year later. Thus, those compliant with the age rule and born before June 1, 1991, entered elementary school in 1997 under the old CSL age scheme. Those compliant with the age rule and born on June 1, 1991, or later, entered school in 1998 under the new CSL age scheme. This discontinuity in date of birth at June 1, 1991, is the base of my identification strategy.

In addition to the age rule, the school starting year is a joint decision of parents, preschool teachers, and in some cases, pedagogical and psychological counselors employed by public pedagogical service centers[8]. The decision itself is made during preschool. At the time of the legislation change, preschool attendance was compulsory from age 5. The decision process about elementary school entry starts with an official opinion of preschool teachers about whether the child is ready to start school. In the case of any doubts, preschool teachers can ask for a "school readiness examination" from the local pedagogical service center.

[6]Data Source: own estimation from the 2001 Hungarian Census. For 4% of the sample, the information on the type of school is either missing (3%), or they are not in school due to living with disabilities (1%). See more on the data sources used in this paper in Section 2.

[7]Compulsory schooling obligation can be fulfilled in homeschooling as well. However, even those in homeschooling belong to a school and are supposed to be covered by education statistics as those in school. Homeschooling is rare; in the 2013/14 academic year, the share of those in homeschooling was 0.68% (Education Office of the Ministry of Human Resources, 2014).

[8]*Pedagógiai szakszolgálat* in Hungarian.

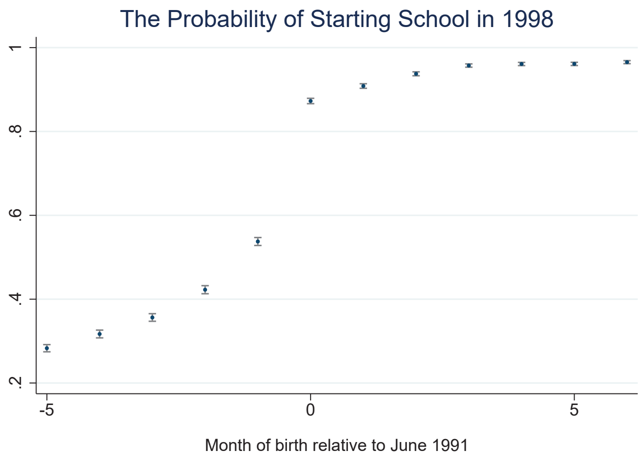
Table 1: Compliance with the Age of Elementary School Start Rule, PES Data

	Early starters (never-takers)	Compliers	Late starters (always-takers)	No. of students in Grade 1
Compliance by academic years, share of those starting elementary school at the given year				
1997/1998	0.02	0.80	0.18	127,214
1998/1999	0.02	0.78	0.20	125,875
1999/2000	0.01	0.78	0.21	121,424
Compliance by cohorts, share of cohort size				
Born between June 90-May 91	0.02	0.79	0.19	129,489
Born between June 91-May 92	0.02	0.78	0.20	126,294

Data Source: Public Education Statistics (PES) of the Public Education Information System (KIR-STAT). "Early starters" refers to those entering elementary school without reaching age 6 by May 31. "Compliers" refers to those entering elementary school according to the age of elementary school start rule at age 6. "Late starters" refers to those entering elementary school a year later than expected under the age rule. Individual level data are not available for this period in the PES.

On average, about 80% of a cohort starts elementary school according to the age rule, while the rest start a year later. Early school start is rare, at about 2%, according to the aggregate statistics of the PES (see Table 1). Around 54% of those born in May 1991, and around 87% of those born in June 1991 started school under the new CSL age scheme (see Figure 2). Thus, the probability of starting elementary school under the new CSL age scheme jumps 0.33 around the cutoff at June 1, 1991.

Figure 2: The Probability of Starting School under CSL Age 18



The average probability of starting school in 1998 among those born in 1991, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June 1991. No. of individual observations: 123,486.

4 Identification and Empirical Methodology

4.1 Identification Strategy

Children compliant with the age of elementary school start rule entered elementary school under the higher CSL age scheme if they were born on June 1, 1991, or later. As mentioned in Section 2, the 2001 Hungarian Census registers which year of elementary school those born in 1991 were attending in 2001. Knowing their grade level in 2001 allows me to estimate the jump in probability of starting school under the new CSL age regime, assuming that grade repetition patterns did not change between 1997 and 2001. Those who started elementary school under the old CSL age scheme were in Grade 4, and those who started elementary school under the new CSL age scheme were in Grade 3 in 2001, as long as they had not repeated a grade by that time. Although the 2001 Hungarian Census does not register grade repetitions, this information is available on an aggregate level from the PES database. Table 2 summarizes the share of students repeating a year during Grades 1-3, and shows that the prevalence of grade repetition in these grades is low in general, and did not change in this period (National Institute of Public Education, 2006). Those who started elementary school under the old CSL age scheme in 1997 should have been in a higher grade in 2001 than those who started school a year later, and thus, they are 1.5 percentage points more likely to repeat a grade by the time of observation (see Table 2, 3rd row). Consequently, this method will result in estimating a 1.5 percentage point lower jump in the probability of starting school under the new scheme because I cannot distinguish between a late school start and a grade repetition.

Table 2: Grade Repetition in Grades 1-3, % of Students in Grade

Academic year	Grade 1	Grade 2	Grade 3	CSL age
1995/1996	4.0	1.9	1.6	16
1996/1997	3.9	2.0	1.5	16
1997/1998	3.9	1.9	1.5	16
1998/1999	4.0	1.8	1.5	18
1999/2000	3.9	1.9	1.4	18
2000/2001*	4.2	1.9	1.4	18

*The 2000/2001 is the academic year in which the 2001 Census is taken. Data Source: National Institute of Public Education, 2006. Table 4.28 in the Appendix, page 478.

The 2001 Hungarian Census captures an individual's year and month of birth, but not the day. Thus, compliance with the age rule can be estimated in birth month bins. According to this data, 54% of those born in May, and 87% of those born in June, started elementary school under the new CSL age regime (see Figure 2 in Section 2). This allows me to identify the causal effects of the CSL age increase in a regression discontinuity design (RDD) by setting a cutoff at the June 1, 1991, date of birth. As the probability of falling into the treated cohort jumps from about 0.54 to 0.87 at the cutoff, this is a fuzzy RDD setup where compliance with the age of elementary school start rule is endogenous. Being born right after rather than before the cutoff is used as an instrument for starting school under the new CSL age legislation. As a first step, I estimate the intention to treat (ITT) effects of the legislation change around the cutoff using the 2011 Hungarian Census data that captures the cohort of interest at ages 19-20. In Section 3.3, the analysis is extended to a 2-stage procedure taking compliance with the age rule into account in a first stage, estimated from the 2001 Hungarian Census as mentioned before.

I identify the ITT effects of the rise in a potential outcome framework. Let's consider the population of those born in a small neighborhood within the cutoff date and define Z as:

$$Z = 1(\text{being born on or after June 1, 1991}).$$

Z is a binary instrumental variable for the treatment, i.e. entering elementary school under the new CSL age scheme, for this population. The potential treatment indicators are then defined as

$$D(0) = 1(\text{entering elementary school under CSL age 18 if one had Z=0})$$

$$D(1) = 1(\text{entering elementary school under CSL age 18 if one had Z=1}).$$

The actual treatment indicator is $D = ZD(1)+(1-Z)D(0)$. For those born before the cutoff, or $Z = 0$, the intended school starting date is 1997, which falls under the old CSL age of 16. However, some parents choose to withhold their child to start school one year later in 1998, which falls under CSL age 18. Therefore, before the cutoff,

$$D(0) = 0 \text{ for compliers and}$$

$$D(0) = 1 \text{ for always-takers (i.e., late starters).}$$

Both $D(0) = 0$ and $D(0) = 1$ are possible, and the choice between them is endogenous. Compliance with the age rule is 54% on this side of the cutoff (see Figure 2 in Section 2). For those born after the cutoff, or $Z = 1$, the intended school start date is 1998, under the new CSL age of 18. Therefore, after the cutoff:

$$D(1) = 1 \text{ for compliers, and}$$

$$D(1) = 0 \text{ for never-takers (i.e., early starters).}$$

As shown by the data, an early school start is rare and there is a 87% probability that D(1)=1 occurs (see Figure 2 in Section 2).

My identification strategy is based on three assumptions. First, I assume that the instrument is exogenous: whether the student was born right before or after the cutoff is random. As the Act was introduced in 1996, five years after the relevant cohort was born, manipulation of birth because of the legislation change is not an issue. However, there is a dispute in the literature about whether children born in different months of the year are inherently different from each other with regard to their outcomes later in life (see Buckles and Hungerman, 2013 vs. Fan et al., 2014). The primary concern within the literature is related to those born during the winter as opposed to those born in spring. This literature argues that babies born in the winter are more likely to be born to less educated women, and that the winter months may not provide as favorable of an environment to a newborn as the spring. In this paper, I compare children born before and after June 1. To the best of my knowledge, the question of whether children are inherently different when born in May or in June has not been raised in the literature. In spite of this, the exogeneity assumption of the date of birth is going to be relaxed for a robustness check in Section 7.4.

The second assumption is the exclusion restriction: the legislation change is the only channel through which being born before or after the cutoff affects the outcomes. This is not a trivial assumption in this case. Specifically among those staying in school until the CSL age, starting elementary school at an age closer to 6 rather than closer to 7 may result in the student spending a longer or shorter time in school, independently from the legislation change (Angrist and Kruger, 1991; Hámori and Köllő, 2011). Without the legislation change, both those born in May and June 1991 would have to stay in school until the end of the 2006/2007 academic year. Those born in May, however, if compliant with the age rule, were supposed to start elementary school one year earlier. Although the CSL age increase might have created two extra years to the schooling career of those born in June, the net impact of the legislation change around the cutoff on the compliers is closer to one year. I ran two robustness checks to learn more about this issue. In Sections 5, 6 and 7.2, impacts estimated around the real cutoff in 1991 are compared to the same cutoffs in other years. Excluding the probability of starting a vocational training school and earning a vocational school degree, I find no effects around cutoffs in other years, the effects of starting school at a later age and spending less time in school balance each other out.[9] In Section 7.4, I directly control for any potential impacts around cutoffs in other years.

The third identification assumption is that the instrument is continuous and no defiers exist. On the one hand, I assume that being born after the cutoff and thus being subject to an increased CSL age did not induce anyone to start school one year earlier to avoid the extra two compulsory years in school. This assumption would theoretically be violated if parents who "dislike" schooling want to manipulate the system and therefore send their children born after the cutoff to school one year earlier, to "save" them from the extra two compulsory school years. Similarly, I also assume that none of those born before the cutoff are sent to school one year later for the purpose of being subjected to the new CSL age. I assume that always-takers on the left side of the cutoff, and never-takers on the right side of

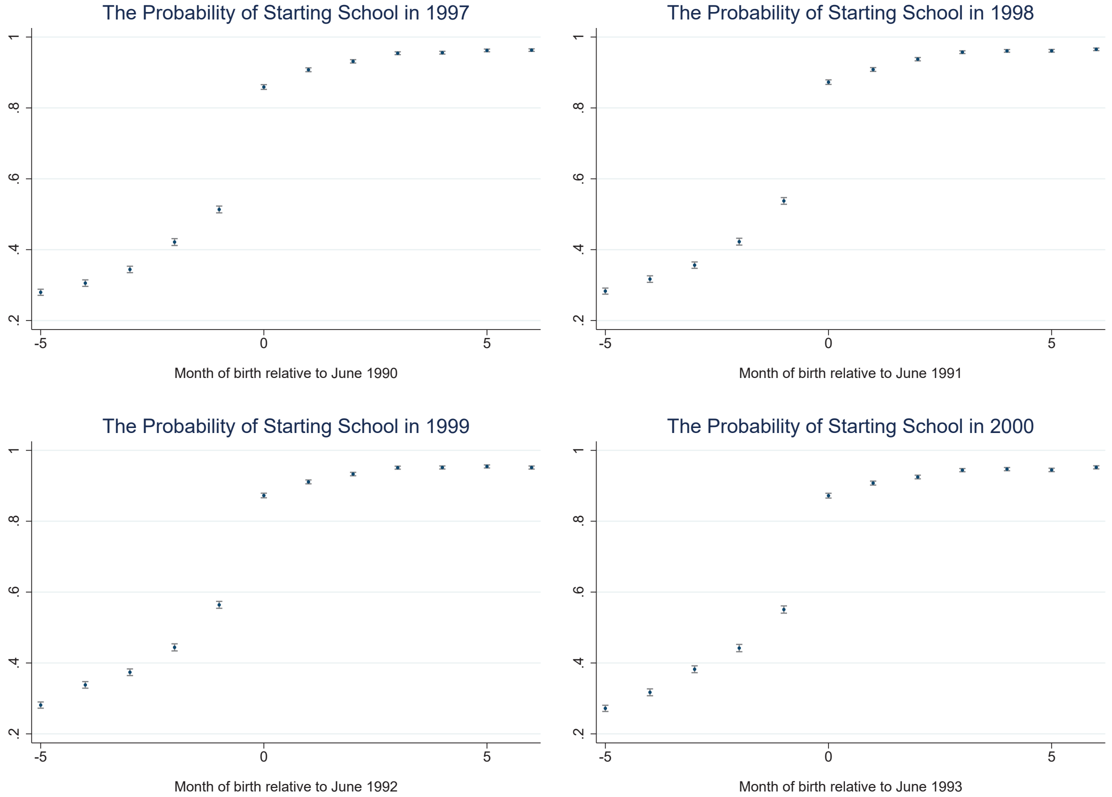
[9] I discuss this issue further in 7.2.

the cutoff, start school late or early because of their general preferences on what is the ideal time to start school, and not because they want to violate the age rule due to the legislation change.

Such violations are highly unlikely. First of all, the timing of the legislation change was not in favor of those who might want to manipulate school start. It was accepted in 1996, the first treated cohort started school in 1998, and they reached age 16 in 2008. To start school early in 1997, parents "disliking" school had to have been aware of the increase already in late 1996 or early 1997 to ask for an early school readiness examination in pre-school. Pre-school was compulsory from age 5 at that time, and some potential early-starters had just entered pre-school when the new legislation came out. Assuming that those parents who "dislike" school might be of a lower socioeconomic background, it is unlikely that they are informed of the details of the legislation change far enough in advance. The increase of the CSL age, as it became practically binding only 12 years later, did not receive much attention in the media in 1996. The Act made several prompt changes in the education system, and the media concentrated much more on those instead. It also seems quite unlikely that pre-school teachers suggested to parents in 1996-1997 to send their children to school early to avoid longer schooling. It sounds more reasonable that the information about longer schooling reached parents at the time that their children entered elementary school in 1998. By then it was too late to avoid longer compulsory school attendance. Furthermore, it was also unlikely that parents who "like" schooling manipulated the system by sending their children born before the cutoff to school a year later because CSL age is binding downwards. Parents who were fond of schooling could keep their children in school until whatever age they prefer, regardless of the actual CSL age legislation.

Secondly, I find no evidence in the data that such defiance occurred. As detailed in Section 2, I observe school starting patterns from three data sources. The first data source is the 2001 Hungarian Census, which allows me to estimate the school starting patterns of those born between 1990 and 1993, by birth month bins. Figure 3 shows that the share of never-takers and always-takers do not differ significantly across these four cohorts around the June 1 cutoff.

Figure 3: The Probability of Starting School in 1997-2000, Census Data



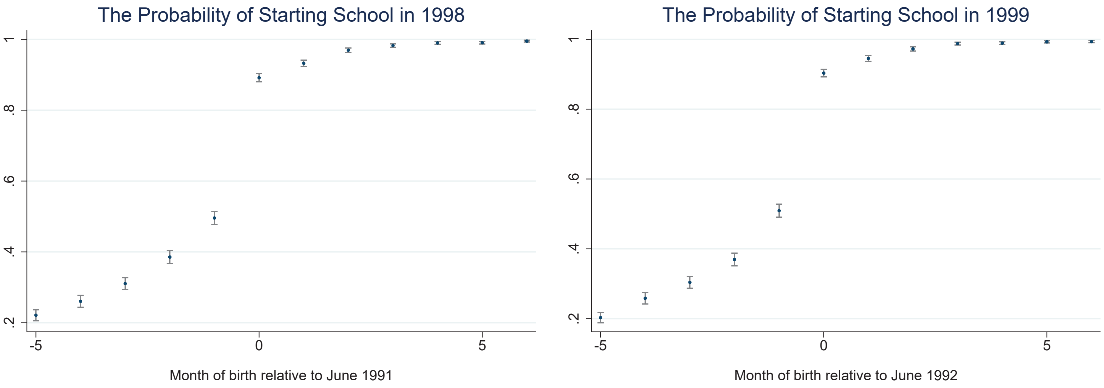
The average probability of starting school in 1997 for those born in 1990, in 1998 for those born in 1991, in 1999 for those born in 1992, and in 2000 for those born in 1993, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June. No. of individual observations: 123,719 in 1990, 123,486 in 1991, 118,500 in 1992, and 113,582 in 1993. The number of observations is decreasing in line with the number of live births.

The second data source is the individual level data of the Hungarian National Assessment of Basic Competencies (NABC) administrative database. This source allows for estimates of the probability of starting elementary school in 1998 among those born in 1991, and in 1999 among those born in 1992, for comparison.[10] Similarly to the 2001 Hungarian Census, the NABC data also suggest that the share of always-takers and never-takers are the same in both cohorts (see Figure 4).[11]

[10]The data do not cover the cohort born a year before, in 1990 (see in Section 2).

[11]Note that the size of the jump around the cutoff in the probability of starting school in 1998 is slightly larger in the NABC data than in the 2001 Hungarian Census. This difference comes from the fact that while the 2001 Hungarian Census captures the whole cohort, the NABC data cover only a subsample of those attending Grade 10 (see Section 2 for more details.). During this time, about 10% of students dropped out of elementary school before completing Grade 8, and the share of dropouts is even larger in Grades 9-10 in secondary school (National Institute of Public Education, 2006). Thus, those observed in the NABC data are positively selected, which results in a somewhat lower share of always-takers on the left-hand side of the cutoff.

Figure 4: The Probability of Starting School in 1998-1999, NABC Data



The average probability of starting school in 1998 for those born in 1991, and in 1999 for those born in 1992, plotted with the 95% confidence intervals of the means. Data source: own estimation from the 2006-2010 waves of the Hungarian Assessment of Basic Competencies Survey (NABC) taken in 10th grade. 0 on the x-axis refers to being born in June. No. of individual observations: 34,016 and 33,523, respectively.

The last data source on elementary school entry is the aggregate level administrative data of the Public Education Statistics (PES) of the Public Education Information System, which shows the share of early starters (never-takers) at 1-2%, compliers at 78-80%, and late starters (always-takers) at 18-20% in Grade 1 as stable between 1997 and 1999. This was true both across cohorts measured by date of birth, and measured by academic years (see Table 1 in Section 2). Consequently, all available data support the assumption that the legislation change did not alter school starting behavior.

4.2 Intention to Treat (ITT) Effects

The ITT effects of the CSL age legislation change are estimated from the 2011 Hungarian Census by using both a nonparametric and a parametric estimation strategy. **Nonparametric estimates** are generated by estimating weighted local linear regressions on both sides of the cutoff, within a certain bandwidth (Hahn et a., 2001; Imbens and Lemieux, 2008). For simplicity, weights are computed by applying a rectangular kernel function on the distance between each observation and the cutoff in terms of date of birth, measured in days. This is the standard method of RDD estimation as it has excellent properties in estimating the difference of two conditional expectations evaluated at the boundary points of the cut-off (Cheng, Fan, and Marron, 1997).

The following local linear models are estimated within a certain bandwidth:

$$y_i = \alpha_{ITT} + \beta_{ITT} * Z_i + \gamma_{ITT} * x_i + \delta_{ITT} * x_i * Z_i + \varepsilon_i,$$

where

y_i is the outcome variable;
Z_i is the instrument, which is 1 if individual i was born on June 1, 1991, or later, and 0 otherwise;

x_i is the running variable, number of days in date of birth before or after June 1, 1991, (and 0 if individual i was born on June 1, 1991); and
$x_i * Z_i$ is an interaction term of x_i and Z_i , allowing the local linear function to be different on the two sides of the cutoff.

Although recent literature agrees on using local linear regressions in this setup as the appropriate method (McCrary and Royer, 2011; Gulesci and Meyersson, 2013), there is an ongoing discussion about how to set the bandwidths. In other words, how close one has to go to the cutoff to believe that being born below or above is still random. In practice, such nonparametric estimates may be very sensitive to bandwidth choice. Traditional bandwidth selectors are usually balancing the trade-off between fit (minimizing squared errors of the local functions) and the variance of the RDD estimator, typically leading to too "large" bandwidths (Calonico, Cattaneo and Titiunik, 2014). Such optimization procedures include the family of rule-of-thumb bandwidths as in Lee and Lemieux (2010), the optimal bandwidth routine of Imbens and Kalyanaraman (2012), and the various cross-validation procedures. If the bandwidth is too large, estimates are going to be biased as the estimated means are going to be too far from the cutoff. This paper follows a conservative strategy and uses the strictest procedure: the optimal bandwidth routine from Calonico, Cattaneo and Titiunik (2014), abbreviated as CCT in the rest of the paper, along with the 50-150% versions of the optimal bandwidths as robustness checks. The details of the procedure can be found in Appendix D.

The optimal bandwidths set by the method are 100-150 days wide below and above the cutoff, depending on the outcome variable and the sample. As the optimal bandwidth is case specific, all main results are re-estimated using a 100-day bandwidth, making the estimated coefficients comparable across subsamples (see Appendix C).

A **parametric approach** is used for one of the robustness checks on a 5-year sample of individuals born in 1988-1992 using 4th-order global polynomial models. The estimated ITT models are the following:

$$y_i = \alpha_P + \beta_{ITT,P} * t_i + f(x_i, Z_i) + \varepsilon_i$$

where

$f(x_i, Z_i)$ is a 4th-order polynomial function of the running variable, which is different on the two sides of the cutoff.

There are two reasons to complement the nonparametric analysis with parametric models. First, they can accommodate additional control variables. In particular, birth month fixed effects will capture the impacts of any potential monthly seasonality of child quality, and birth year fixed effects will capture the potential effects of business cycles. An interesting feature of the Census data (and the Hungarian health system) is that the day of the week matters with respect to the probability of being born. One is more likely to be born Tuesday through Friday rather than Saturday through Monday, and this probability difference is weakly related to the educational status of the mother.[12] This paper does not want to document this phenomenon. However, because June 1 in 1991 fell on a Saturday, day of the week fixed effects are also included in the parametric models to control for this pattern.

[12]Own estimation from the 2011 Census.

Secondly, although recent RDD papers use the nonparametric approach, the early literature mainly used global polynomial models (diff-in-diffs) in a similar fashion (see Table 26 in Appendix A). Thus, I find it reassuring that both approaches lead to the same conclusion.

I estimate robust standard errors clustered by birth year and month throughout this paper. Furthermore, I test the impact of the legislation change on more outcomes and more subsamples at the same time. Testing several statistical hypotheses together increases the probability of finding significant effects by chance, known as the problem of multiple inference (Anderson, 2008). In considering a set of statistical inferences simultaneously, the probability of committing a type-I error increases. In other words, hypothesis tests that incorrectly reject the null are more likely to occur than initially intended by a single test at a time. Thus, I correct all hypotheses tests of ITT effects by estimating the number of tests done at once using the multiple testing procedure of Benjamini and Hochberg (1995). The procedure controls for the false discovery rate (FDR), which is defined as the expected share of type-I errors among all rejections. This is done by correcting upward the p-values of the tests according to two factors: the number of tests conducted together, and the relative magnitude of each p-value to the rest. A smaller original p-value of a hypothesis test results in a larger upward penalty imposed on the p-value by the procedure. I use the multiple testing p-value correction procedure in the parts of this analysis when I test whether an increase in the CSL age has an effect on a set of outcome variables. Original, uncorrected standard errors are reported along with corrected p-values for these cases. However, the procedure lowers the power of the tests by each additional hypothesis, and therefore increases the probability of type-II errors or false non-rejections. Thus, I do not use multiple testing correction when I estimate the magnitudes of the already established significant relationships (see LATE in the next subsection), nor for the robustness checks.

4.3 Local Average Treatment Effects (LATE)

I estimate the LATE of the legislation change around the cutoff using information about compliance with the age of elementary school entry rule. As the first stage is estimated from the 2001 Hungarian Census in birth month bins while the reduced from is estimated from the individual level data of the 2011 Hungarian Census, this is a two sample procedure in the fashion of Angrist (1990). A two-sample 2SLS strategy is valid only if both samples are taken from the same population (Angrist, 1990), which is indeed the case in this analysis.

Based on the data, children typically start school either according to the age rule at age 6 or one year later, at age 7. Only a small fraction of children go to school earlier than that (1-2% of a whole year cohort). Thus, compliance with the age of elementary school start rule is over 90% on the treated side of the cutoff, and non-compliance is relevant on the non-treated side only (see Figure 2 in Section 2). This is close to the situation of one-sided non-compliance, in which case the Average Treatment Effect on the Non-treated (ATNT) is close to the LATE (Angrist and Pischke, 2009). This fact makes estimating LATE even more interesting as it is quite rare to identify ATNT in practice. ATNT is the effect measured on the compliers on the non-treated side of the cutoff, i.e. it is interpreted as the effect on those who born before June 1, 1991, and started school in 1997, under the old CSL age scheme.

The LATE are estimated as the size of the ITT effects over the jump in the probability of being treated; thus, they are going to be roughly 3-times as large as the ITT effects. The 2-stage procedure simply re-scales the magnitude of the ITT effects.

Formally, the 2-stage procedure in the nonparametric approach involves estimating the same ITT equation on the individual-level data of the 2011 Hungarian Census just as before:

$$y_i = \alpha_{ITT} + \beta_{ITT} * Z_i + \gamma_{ITT} * x_i + \delta_{ITT} * x_i * Z_i + \varepsilon_i,$$

and, estimating the first stage equation using the same bandwidth as in the case of the ITT equation:

$$p\hat{9}8_m = \alpha_{FS} + \beta_{FS} * Z_i + \gamma_{FS} * x_i + \delta_{FS} * x_i * Z_i + u_i,$$

where:

$p\hat{9}8_m$ stands for the probability of starting elementary school under the new legislation, estimated in birth year and month bins from the 2001 Hungarian Census.

The LATE coefficient is estimated as:

$$\hat{\beta}_{LATE} = \frac{\hat{\beta}_{ITT}}{\hat{\beta}_{FS}}$$

Following Bjorklund and Jantti (1997), a bootstrap method is used to calculate the 95% confidence intervals of estimated LATE coefficients. First, I drew a birth year and month stratified bootstrap sample from the 2001 Hungarian Census data, and estimate the average probability of starting school under the new CSL age scheme in birth year and month bins ($p\hat{9}8_{m,1}$). Then, I drew a birth year and month stratified bootstrap sample from the 2011 Hungarian Census data, from which I estimate the ITT parameter ($\hat{\beta}_{ITT,1}$), and using ($p\hat{9}8_{m,1}$), the first stage parameter, $\hat{\beta}_{FS,1}$. Then, I estimate a LATE parameter as $\hat{\beta}_{LATE,1} = \hat{\beta}_{ITT,1}/\hat{\beta}_{FS,1}$. Repeating these four steps $B = 1000$ times yields an empirical distribution of $1,000$ estimated LATE coefficients ($\hat{\beta}_{LATE,1} - \hat{\beta}_{LATE,1000}$). The 95% confidence interval of $\hat{\beta}_{LATE}$ is then set as the middle 95% of this empirical distribution.

Table 3: Educational Outcomes of Those Born in Spring vs. Summer 1991

	Born March-May 1991	Born June-Aug 1991	Difference (t-test p-values)
A. Probability of being in school at the time of the Census			
Any school	0.619	0.679	0.060 (0.000)
B. Number of years completed successfully in the school system			
No. of school years	12.2	12.0	-0.264 (0.000)
C. Probability of starting a secondary school			
Any secondary	0.916	0.918	0.002 (0.371)
Vocational training school	0.220	0.206	-0.014 (0.000)
Professional high school	0.343	0.350	0.008 (0.045)
Academic high school	0.454	0.466	0.012 (0.003)
No. of observations	33,350	33,919	
D. Probability of dropping out of secondary school conditional on starting one			
Any secondary school	0.045	0.049	0.004 (0.019)
	[30,375]	[30,939]	
Vocational training school	0.122	0.140	0.018 (0.002)
	[7,347]	[6,972]	
Professional high school	0.022	0.029	0.006 (0.003)
	[11,446]	[11,941]	
Academic high school	0.016	0.016	0.001 (0.690)
	[15,095]	[15,783]	
E. Probability of earning a secondary degree			
Any secondary	0.814	0.790	-0.024 (0.000)
Vocational training school	0.144	0.123	-0.021 (0.000)
Professional high school	0.258	0.244	-0.014 (0.000)
Academic high school	0.414	0.425	0.011 (0.004)
No. of observations	33,350	33,919	

Data Source. own estimation from the 2011 Hungarian Census. The table compares the average educational outcomes of those born 3 months before and after the cutoff at June 1, 1991. Two-tailed t-test p-values are in parenthesis. In block D, no. of observations are in brackets.

5 Results

Results are presented based on the following logic: general ITT effects are presented on the number of completed school years in Section 5.1, on school choice in Section 5.2, on the probability of dropping out of secondary school in Section 5.3, and on the probability of earning a secondary degree in Section 5.4. I estimate LATE in Section 5.5, and heterogeneous ITT effects with respect to gender and ethnic minority (Roma) status in Section 5.6.

5.1 The Implementation of Increased CSL Age

This subsection investigates whether those born after the cutoff stayed in school longer than those born before the cutoff. The number of completed years in school cannot be compared directly across these two groups because about two-thirds of the estimation sample is still in school at age 20, when their schooling outcomes are observed. The share of those still in school is higher among those born right after the cutoff (62% among those born before vs. 68% among those born after the cutoff, see Table 3, Block A). Some of this difference may be caused by the CSL age change, and some may come from the fact that they started school later. Until the time of the Census, those born after the cutoff could have spent fewer academic years in the school system by design than those born before the cutoff (13 vs. 14 years). To take into account the share of those still in school, the effects of the CSL age change on the number of successfully completed years is estimated in an exponential survival model with right-side censoring at the time of the Census. Table 4 shows that increasing the CSL age decreased the probability of exiting school at any point in time with 8% on average, among those still in school at that time. In other words, the legislation change made students born after the cutoff more likely to stay in school longer.

Table 4: Effects on the Number of Successfully Completed School Years

	ITT effects	Robust clustered SE's	Multiple-testing corrected p-values	No. of obs.	Bandwidth (in days)
Probability of school exit	-0.080***	0.017	0.000	67,971	100

The probability of school exit is estimated in an exponential survival model, controlling for the same linear function of the running variable below and above the cutoff as in the case of local linear regressions, using a 100-day bandwidth. Negative coefficients indicate completing more years in school. Standard errors are robust and clustered by birth year and month. P-values are corrected by the number of hypothesis tests (13) done together in Tables 5 - 7 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after the multiple testing correction.

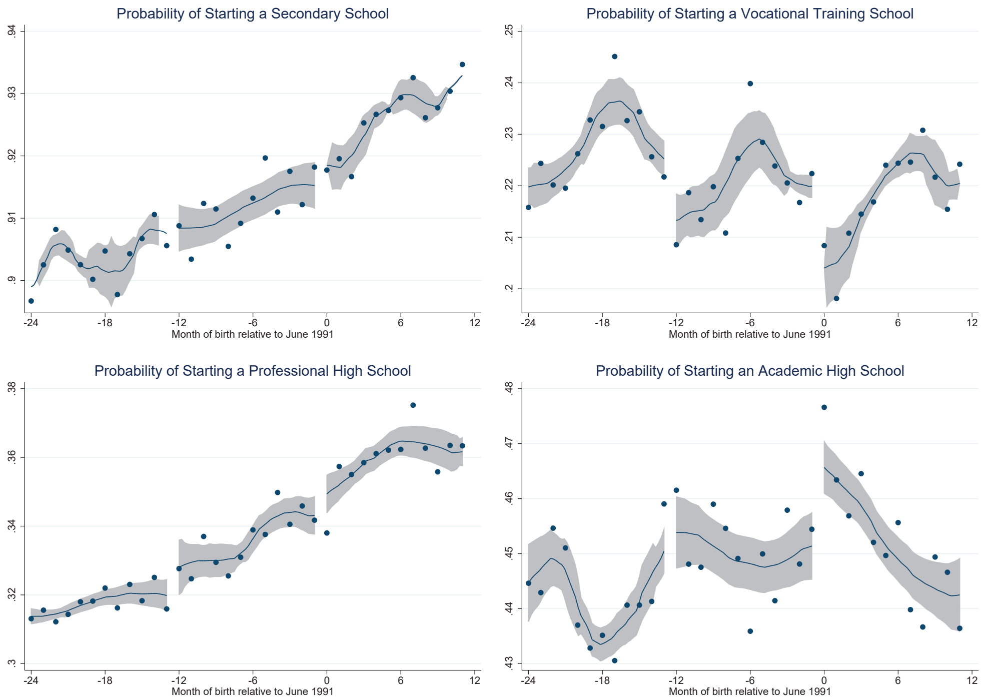
5.2 Effects on School Choice

The unique feature of the Hungarian reform is that it allows an estimation of the effects of the higher CSL age on secondary school tracking choice made at age 14. Figure 5 plots the average share of those starting each type of secondary school in birth year and month. On average, the majority of children (91%) started secondary school before the increase[13]. The share of those starting a secondary school is increasing in time, and the same is true for those starting a professional high school. The share of those starting a vocational training school or an academic high school is fluctuating between 20-24% and 43-47% during these years, respectively. To test whether the seasonality in the data is related to the cutoff, Figure 5 plots local linear regression functions below and above two cutoffs: the real cutoff in 1991, and the same cutoff in the previous year, in 1990. Indeed, the share of students starting a vocational training school decreased around the cutoff in 1990 as well as in 1991. For the rest three outcome variables I find so significant break around the cutoff in the previous year.

[13]Data Source: own estimation from the 2011 Hungarian Census.

The increase of CSL age did not affect the probability of starting secondary school around the real cutoff in 1991 (see Table 5 and Figure 5). However, it did affect the choice of school tracks, even though all tracks were at least 4 years in length both before and after the increase. Students on average were 1.5 percentage points more likely to choose the academic high school track under the higher CSL age scheme.

Figure 5: Effects on School Choice



Local linear regressions fit separately below and above the cutoff in 1990 and in 1991 on birth year and month averages, using the rule-of-thumb bandwidth of lpoly in Stata with 90% confidence intervals. Month of birth -12 represents June 1990, 0 represents June 1991. No. of individual observations: 349,925.

Table 5: Effects on School Choice

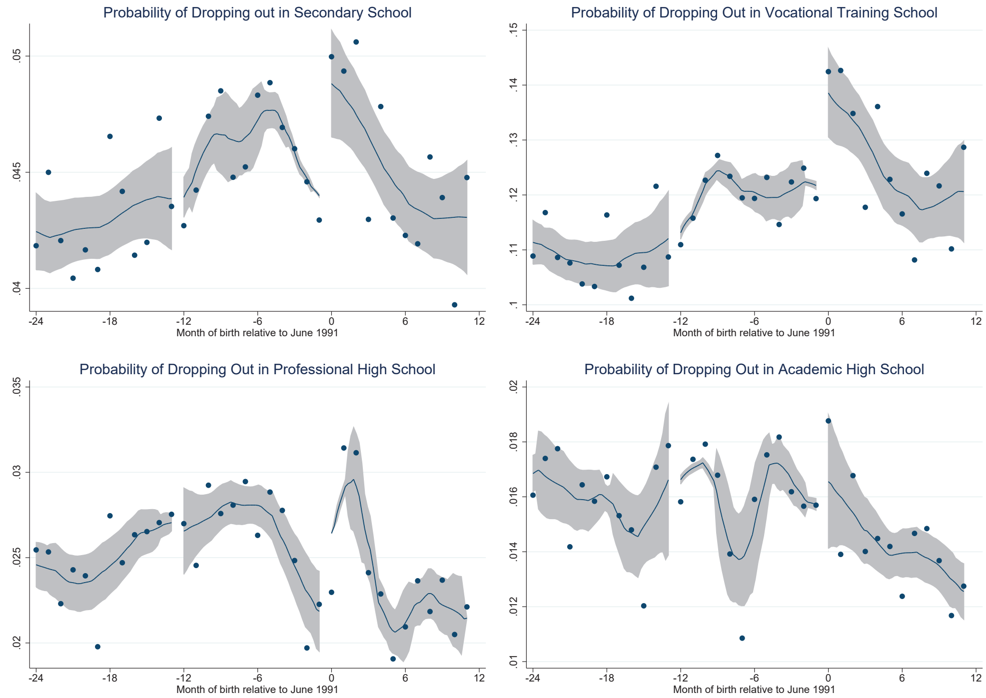
Effect on the probability of finishing at least one year in a secondary school					
	ITT effects	Robust clustered SE's	Multiple-testing corrected p-values	No. of obs.	CCT bandwidth (in days)
Any secondary school	-0.000	0.003	0.867	102,616	152.6
Vocational training school	-0.017*	0.006	0.066	80,505	118.2
Professional high school	-0.002	0.007	0.775	103,292	153.0
Academic high school	0.015*	0.006	0.074	86,292	127.6

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Standard errors are robust and clustered by birth year and month. P-values are corrected by the number of hypothesis tests (13) done together in Tables 5- 7 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by a 100-day bandwidth are in Table 31 in Appendix C.

5.3 Effects on Dropout Rates

The increase in CSL age causes an increase in the probability of dropping out of secondary school by 0.8 percentage points (see Table 6 and Figure 6). This effect is mostly driven by a 2.2 percentage point increase in dropout rates in vocational training schools.

Figure 6: Effects on Dropout Rates



Local linear regressions fit separately below and above the cutoff in 1990 and in 1991 on birth year and month averages, using the rule-of-thumb bandwidth of lpoly in Stata with 90% confidence intervals. Month of birth -12 represents June 1990, 0 represents June 1991. No. of individual observations: 314,933, 78,733, 114,076, and 156,416, respectively. Note that dropout rates are decreasing after the cutoff in all school types, parallel to the increasing probability of still being in school.

Table 6: Effects on Dropout Rates

	Effect on the probability of dropping out of ...				
	ITT effects	Robust clustered SE's	Multiple-testing corrected p-values	No. of obs.	CCT bandwidth (in days)
Any secondary school	0.008**	0.003	0.010	82,615	137.5
Vocational training school	0.022***	0.004	0.001	25,525	174.1
Professional high school	0.008*	0.004	0.085	32,570	138.7
Academic high school	0.003	0.002	0.177	48,077	156.9

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Standard errors are robust and clustered by birth year and month. P-values are corrected by the number of hypothesis tests (13) done together in Tables 5- 7 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by 100-day bandwidths are in Table 32 in Appendix C.

5.4 Effects on School Completion

Figure 7 plots the probability of earning a secondary degree in birth year and month bins. All of these functions are decreasing with date of birth: the younger is an individual in 2001 the less time s/he had to earn a secondary degree.

As a result of increasing the probability of choosing the academic high school track, higher CSL age increased the probability of earning an academic high school degree by 1.7 percentage points (see Table 7 and Figure 7).

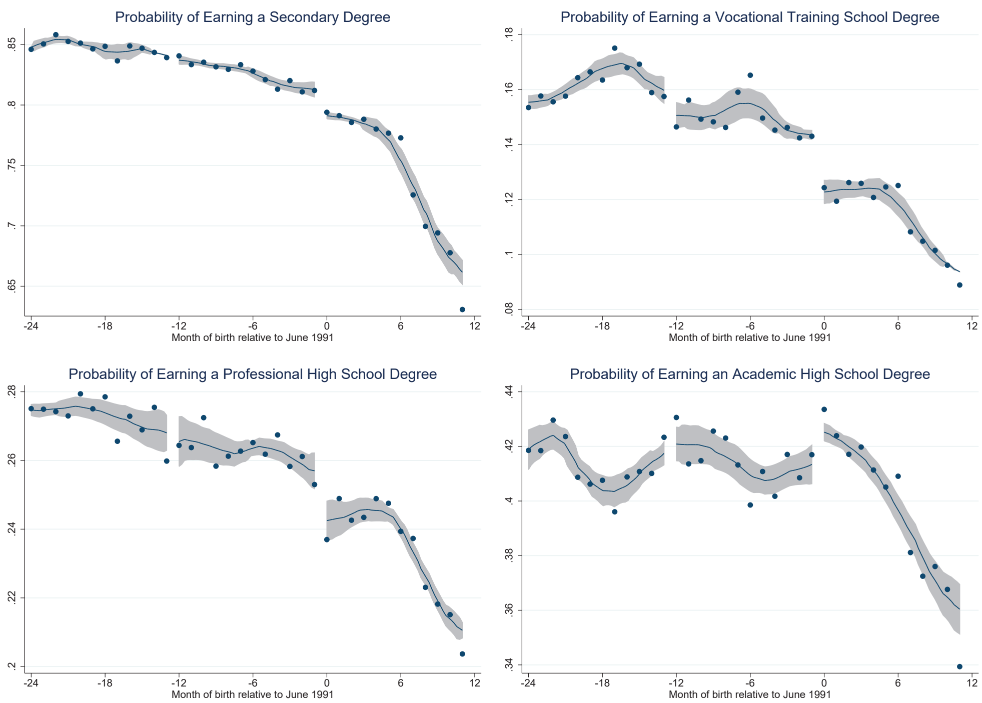
Table 7: Effects on School Completion

Effect on the probability of earning a secondary degree					
	ITT effects	Robust clustered SE's	Corrected p-values	No. of obs.	Bandwidth (in days)
Any secondary school degree	-0.015***	0.000	0.001	62,682	92.4
Vocational training school degree	-0.019***	0.002	0.005	70,804	104.4
Professional high school degree	-0.013*	0.006	0.085	83,827	123.5
Academic high school degree	0.017***	0.002	0.001	76,374	112.7

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Robust standard errors clustered by birth year and month are in parentheses. Observation numbers in the local regression are in brackets. Bandwidths in days are in braces. P-values are corrected by the number of hypothesis tests (13) done together in Tables 5- 7 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by 100-day bandwidths are in Table 33 in Appendix C.

At the same time, due to the fact that higher CSL age increases dropping out from this school type, the change decreased the probability of gaining a vocational degree as well. This effect is -1.9 percentage points large (see again Table 7 and Figure 7). Similarly, increased CSL age decreased the probability of gaining a professional high school degree as well.

Figure 7: Effects on Earning a Secondary Degree



Local linear regressions fit separately below and above the cutoff in 1990 and in 1991 on birth year and month averages, using the rule-of-thumb bandwidth of lpoly in Stata with 90% confidence intervals. Month of birth -12 represents June 1990, 0 represents June 1991. No. of individual observations: 349,925. Note that the decreasing trend comes from the fact that later a student was born the less likely s/he is to have earned a secondary degree by 2011.

The overall effect of the legislation change on the probability of gaining any secondary degree is significantly negative, -1.5 percentage points. My results suggest that the increased CSL age induced a selection mechanism. Some students chose and completed academic high school instead of vocational training school or professional high school. Meanwhile, those who were unable to upgrade their school choice and went to vocational training school ended up being less likely to earn a secondary school degree due to the increased dropout rates. I provide further evidence to support this hypothesis in Section 8.

5.5 Local Average Treatment Effects (LATE) of the Legislation Change

The three main results of this paper is that higher CSL age increased the probability of starting an academic high school, earning an academic high school degree, and dropping out of vocational training school. This subsection estimates the Local Average Treatment Effects (LATE) effect of the legislation change on these three outcomes to give an estimate on the magnitude of these effects around the cutoff. LATE estimates take into account that the jump in treatment probability at the cutoff is less than 1. Practically, they are estimated as the size of ITT effects over the first stage, which is the exact magnitude of the jump.[14] The LATE of the CSL age increase on the probability of starting an academic high school is 4.6 percentage points, while on the probability of earning an academic high school degree it is 5.5 percentage points (see Table 8). These local effects on the compliers are about three times as large as the ITT impacts. The average probability of starting an academic high school in the control group is 0.454 (see Table 3); thus, the LATE is 0.046/0.454=10%. Similarly, the average probability of earning an academic high school degree is 0.414; and, the higher CSL age increased the probability of earning an academic high school degree by 0.055/0.414=13% among the compliers.

Table 8: LATE on Starting an Academic High School, Earning an Academic High School Degree and Dropping Out of Vocational Training School

	Starting an academic high school	Earning an academic high school Degree	Dropping out of vocational training school
ITT effect	0.015**	0.017***	0.022***
	(0.006)	(0.002)	(0.004)
First stage (jump in the probability of starting school under the CSL age 18)	0.321***	0.311***	0.333***
	(0.025)	(0.022)	(0.025)
	[2,110.7]	[2,515.1]	[792.8]
Wald estimate of the LATE coefficient	0.046	0.055	0.065
95% bootstrapped confidence intervals of the LATE coefficient	0.004 - 0.086	0.008 - 0.100	0.015 - 0.114
No. of obs	86,292	76,374	25,525
Bandwidth (in days)	127.6	112.7	174.1

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Wald estimates are calculated as the ratios of ITT coefficients over the first stage coefficient (the size of the jump). Robust standard errors clustered by birth year and month are in parentheses, first stage F-statistics are in brackets. A */**/*** indicates significance on 10%/5%/1% level. The bootstrapped confidence intervals of the LATE coefficients are constructed following Bjorklund and Jantti (1997). First, a birth year and month stratified bootstrap sample is drawn from the 2001 Hungarian Census data, and birth year and month school entering probabilities ($p\hat{9}8_m$) are estimated. Then, a birth year and month stratified bootstrap sample is drawn from the 2011 Hungarian Census data, from which the LATE parameters ($\hat{\beta}_{LATE,1}$) are estimated using the school entering rates estimated in the first step. Repeating these two steps $B = 1,000$ times yields an empirical distribution of $1,000$ estimated LATE coefficients ($\hat{\beta}_{LATE,1} - \hat{\beta}_{LATE,1000}$). The 95% confidence interval of $\hat{\beta}_{LATE}$ is set as the middle 95% of this empirical distribution.

[14] First stage estimates are highly significant with high F-statistics. The reason for the high F-statistics, besides the strength of the first stage as demonstrated on Figures 2 and 3, is the following. F-statistics are estimated as $F = \frac{\frac{RSS_1 - RSS_2}{p_2 - p_1}}{\frac{RSS_2}{n - p_2}}$, where RSS_1 refers to the Residual Sum of Squares from a model of regressing the probability of starting school under the new CSL age scheme in month of birth bins, $p\hat{9}8_m$, on a constant (model 1); RSS_2 refers to the Residual Sum of Squares from the first stage model, $p\hat{9}8_m = \alpha_{FS} + \beta_{FS} * Z_i + \gamma_{FS} * x_i + \delta_{FS} * x_i * Z_i + u_i$, as detailed in Section 5.5 (model 2); $p_1$and p_2 refer to the number of parameters estimated in the two models; and, n is the number of observations. Model 2 is a fully saturated model; thus, it provides high Explained Sum of Squares and low Residual Sum of Squares. Consequently, the value of F-statistics are going to be high. Due to this fact, looking at the raw data on Figure 2 provides a more credible support for the strength of the first stage.

5.6 The Heterogeneity of the Effects of Gender and Roma Ethnicity

This subsection estimates the heterogeneous ITT effects of increased CSL age by gender and Roma ethnicity. The Roma are the largest ethnic minority in Hungary. The population of Roma in the 2011 Census is 315,583. However, based on earlier Roma studies, the actual number of the Roma population is estimated to be about two times larger than this (Hablicsek, 2007). Measurements on "being Roma" are not straightforward, which in part explains why the Census found half as many Roma people. Still, as being Roma is highly correlated with disadvantage, poverty, and poor access to public services, such as education, it makes sense to examine the heterogeneous effects of the CSL age increase with respect to Roma status.

Figure 8: The Probability of Starting School under CSL Age 18

The average probability of starting school in 1998 among those born in 1991, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June 1991. No. of individual observations: 60,302, 63,183, 4,626 and 113,102, respectively.

Figure 8 shows the first stage in these groups. The jump in the probability of starting school under the new CSL age scheme around the cutoff is higher for women (0.38) than for men (0.29), and it is highly significant for both. For the Roma, on the other hand, the data is much noisier. The size of the jump is 0.24, with large 95% confidence intervals of the means below and above the cutoff. The first stage of non-Roma individuals looks about the same as for the total sample.

The positive effect of the legislation change on starting an academic high school and earning an academic high school degree are of the similar magnitudes for women and men; however, they are significant in the case of women only (see Table 9). The negative effect of the increase, on the other hand, is present for men only. While men born around the cutoff are 4.2 percentage points more likely to drop out of vocational training school, the same impact on women is at 0.8 percentage points and not significantly different from zero.

Table 9: The Heterogeneity of the Effect by Gender and Roma Ethnicity (ITT effects)

	Starting an academic high school	Earning an academic high school degree	Dropping out of vocational training School
The heterogeneity of the ITT effect by gender			
Women	0.024***	0.021***	0.008
	(0.004)	(0.004)	(0.013)
	{0.003}	{0.004}	{0.714}
	[32,744]	[32,744]	[5,244]
Men	0.020	0.015	0.042***
	(0.010)	(0.008)	(0.010)
	{0.177}	{0.175}	{0.009}
	[34,515]	[34,515]	[9,075]
The heterogeneity of the ITT effect by Roma ethnicity			
Roma	-0.001	0.005	0.014
	(0.014)	(0.006)	(0.016)
	{0.949}	{0.612}	{0.630}
	[3,168]	[3,168]	[1,032]
Non-Roma	0.024***	0.020***	0.029***
	(0.004)	(0.003)	(0.006)
	{0.004}	{0.003}	{0.006}
	[61,574]	[61,574]	[12,701]
Information on ethnicity is missing	-0.004	-0.013	0.044
	(0.036)	(0.025)	(0.023)
	{0.918}	{0.702}	{0.179}
	[2,517]	[2,517]	[586]

Local linear kernel regressions using a 100-day bandwidth. Linear probability models. Robust standard errors clustered by birth year and month are in parentheses, p-values corrected by the fact that 15 hypotheses are tested together in this table using the FDR multiple testing procedure by Benjamini and Hochberg (1995) are in braces, number of observations are in brackets. A */**/*** indicates significance on 10%/5%/1% level after the multiple testing procedure.

Table 9 shows that the impact of the legislation change on the probability of starting an academic high school and earning an academic high school degree among the Roma is close to zero: -0.1 and 0.5 percentage points, respectively. The impact of higher CSL age on dropping out of vocational training school seems to be there for Roma individuals as well; however, the effect is not significant, probably due to the low sample size.

6 Heterogeneous Effects by Parental Education

6.1 Data Sources Used to Estimate Parental Education Data

This section will look at heterogeneous effects by parental education. For this, I use information on the education status of the parents from both the 2001 and the 2011 Censuses. Both Census data are collected at household and personal levels at the same time. They do not contain explicit information on parent-child relationships; however, they have information about the children of all individuals, including both mothers and fathers. The data register the year and month of birth of their first three children in 2001 and their first five children in 2011. This allows for linking the individuals of interest to their biological parents if they live in the same households. In the relevant cohort of those born in 1991, 99% of all individuals from the 2001 Census and 72% of all individuals from the 2011 Census live with at least one of their parents. For these subsamples, the education status of the parents is directly observed.

In order to gain more comprehensive coverage of parental education levels of the relevant cohort in the 2011 Census, I look at their Live Birth Records data. These records contain all live births in Hungary, and contain information about the exact date of birth, the residence of the parents, and several parental characteristics such as the exact birth date, and educational status, of the mother. I link the Live Birth Records data individually to the Census using two methods.

The first method (method 1) links the two databases by a two-level information comparison approach. In the case of those still living with their parents, the linking procedure uses all available information appearing in both datasets, including the date of birth of parents. In the case of those not living with their parents anymore, it uses maternal residence and exact date of birth only. With this method, 75% of those born in 1991 can be linked to the Census, out of which 80% live with their parents. The share of those living with their parents in the linked sample ratio is slightly higher than in the original Census sample of those born in 1991 (80% vs. 72%), because those living at home are easier to link.

For a potentially more comprehensive coverage of those living in smaller than 50,000-inhabitant settlements, the second linking method (method 2) links the two databases without using parental information and instead only uses residence at birth, date of birth, and gender. With this procedure, the data of an individual in the two databases can be linked together if on one given day, in one given settlement, no more than one boy and one girl were born. Under this method, 76% of those born in 1991 can be linked to the Census data.

Thus, three types of parental education data will be used in the paper. Main results are estimated using:

1. the educational status of the mother when the individual of interest was born, linked to the Census using method 1; while robustness checks are estimated using

2. the educational status of the mother when the individual of interest was born, linked to the Census using method 2; and
3. the educational status of the head of the household if the individual of interest lives with at least one of his/her biological parent.

6.2 Children of Less Educated Parents Are the Most Likely to Be Affected by the Increase

The CSL age increase has a direct effect on those for whom the new constraint is binding, i.e. who would otherwise exit school before reaching age 18. A potential subgroup of such students consists of the children whose mothers are less educated, i.e. finished primary school at most. The 2011 Hungarian Census data show that in the cohort born in 1990, right before the legislation change, children of less educated mothers completed on average 11.0 school years in formal primary and secondary education, while this average is close to, or above, 12.0 in all other parental groups (see Table 30 in Appendix A).

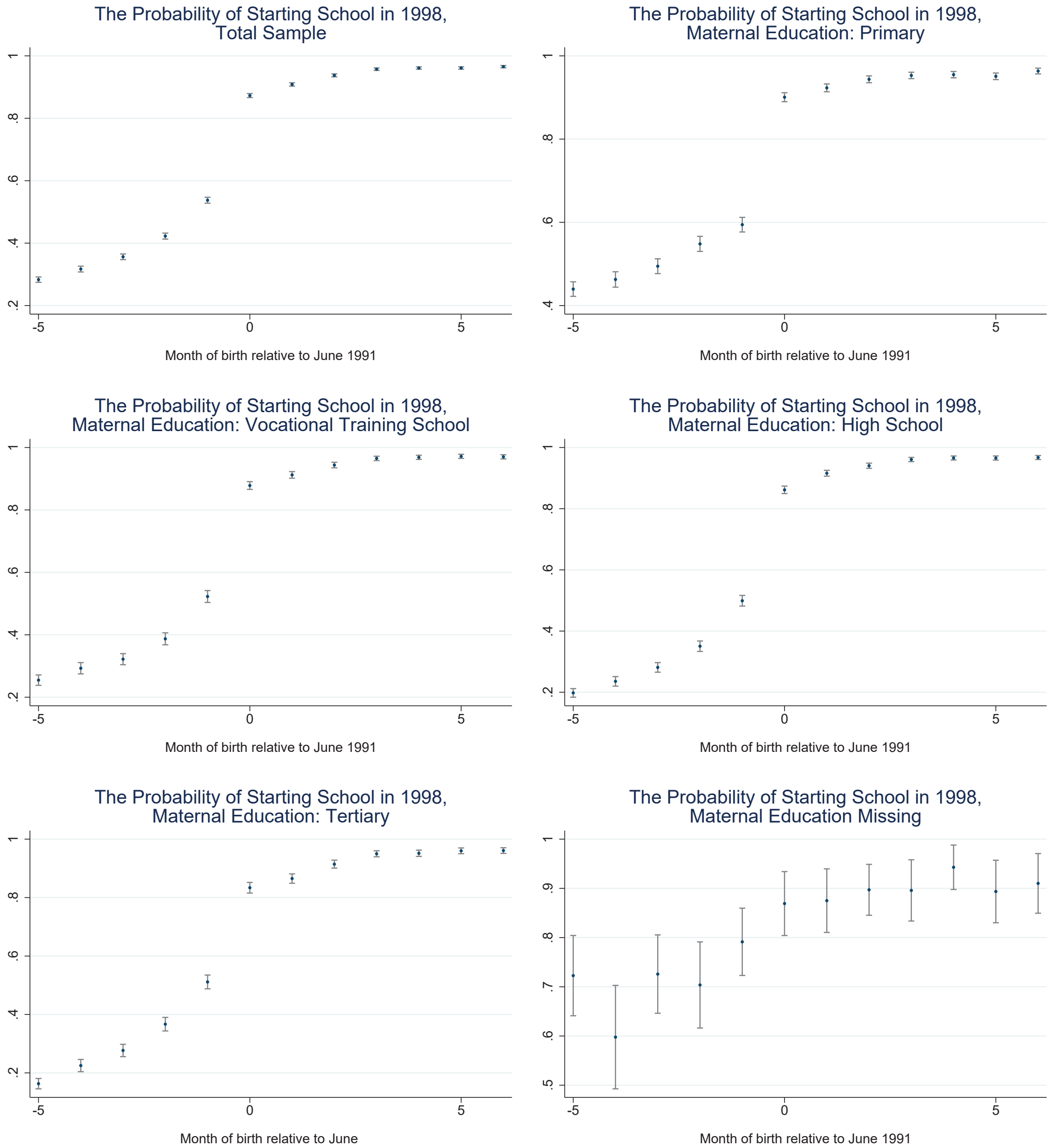
Children of less educated mothers lag behind in all educational outcomes: they are more than 20 percentage points less likely to earn a secondary degree in general, and around 40 percentage points less likely to gain a high school degree in particular, than children of educated mothers. In fact, closing this gap was the explicit intention of the legislation change (Mártonfi, 2011a). Therefore, in the forthcoming analysis, heterogeneous effects are estimated with respect to maternal education, and special attention is paid to results estimated for children of less educated mothers.

The potential heterogeneity of the effects of an educational legislation change with respect to family background is well established both in theory and in practice. Oosterbeek and Van Ophem (2000) show that children from low socioeconomic backgrounds decide on their schooling investment and consumption along different parameters than others. They discount future returns more heavily, and have lower abilities in, and lower preferences for, schooling. Meghir and Palme (2005) find that a major educational reform in Sweden had insignificant average, but significant heterogeneous, effects. They estimate positive wage returns in the case of children with unskilled fathers, and negative wage returns in the case of children with skilled fathers. They argue that the reform might have decreased the quality of education for this group - hence the negative effect.

6.3 The First Stage by Maternal Education

The jump in the probability of starting school under the new CSL age scheme is significant in all groups of maternal education, and its size varies between 0.31-0.36 (see Figure 9). In the group of 1,308 observations out of the 123,486 individuals born in 1991, maternal education data is not available in the 2001 Hungarian Census. In their case, school starting information is extremely noisy and a significant first stage relationship cannot be established (see Figure 9, second graph in the third row). The stability of the first stage across cohorts born between 1990 and 1993 are presented in Figures 13 - 17 in Appendix A.

Figure 9: The Probability of Starting School under CSL Age 18, by Highest Maternal Education



The average probability of starting school in 1998 among those born in 1991, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June 1991. No. of individual observations: 123,486; 35,979; 30,979; 36,197; 19,603; and 1,308; respectively.

6.4 The Implementation of Increased CSL Age

Heterogeneous effects by parental education are presented in the following four subsections in the same way as general effects are presented in Sections 5.1-5.5. The impact of increased CSL age on the number of completed school years is higher than average in the three lowest maternal education groups (see Table 10). Interestingly, the effect on schooling duration is the highest among children of mothers holding a high school degree.

Table 10: Effects on the Number of Successfully Completed School Years

		Mother's highest education at giving birth				
	Total sample	Primary	Vocational	High school	Tertiary	Missing
Probability of school exit	-0.080***	-0.095***	-0.130***	-0.177***	0.020	-0.001
	(0.017)	(0.023)	(0.019)	(0.047)	(0.065)	(0.019)
Corrected p-values	0.000	0.001	0.000	0.002	0.826	0.965
No. of obs.	67,971	15,588	12,097	16,697	5,950	17,127

The probability of school exit is estimated in exponential survival models, controlling for the same linear function of the running variable below and above the cutoff as in the case of local linear regressions, using a 100-day bandwidth. Negative coefficients indicate completing more years in school. Robust standard errors clustered by birth year and month are in parentheses. P-values are corrected by the number of hypothesis tests (78) done together in Tables 10 - 13 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction.

6.5 Effects on School Choice

The unique feature of the Hungarian reform is that it allows for an estimation of the effects of the higher CSL age on secondary school tracking choices made at age 14. On average, the majority of children (91%), did start a secondary school track before the CSL age increase. However, this proportion was at only 80% among children of less educated parents (see Table 30 in Appendix A). The increase of CSL age did not affect the probability of starting secondary school, neither in general, nor among children of less educated mothers. However, it did affect the choice of school tracks made at age 14, even though all tracks were at least 4 years in length both before and after the increase. Children of less educated mothers were 4.9 percentage points less likely to choose a vocational training school, and 4.1 percentage points more likely to choose an academic high school track under the higher CSL age scheme (Table 11). The effects on choosing academic high schools across maternal education groups are monotonic decreasing from 0.041 to 0.006 (see Table 11, last row.). The monotonicity of the coefficients adds credibility to the interpretation of the results.

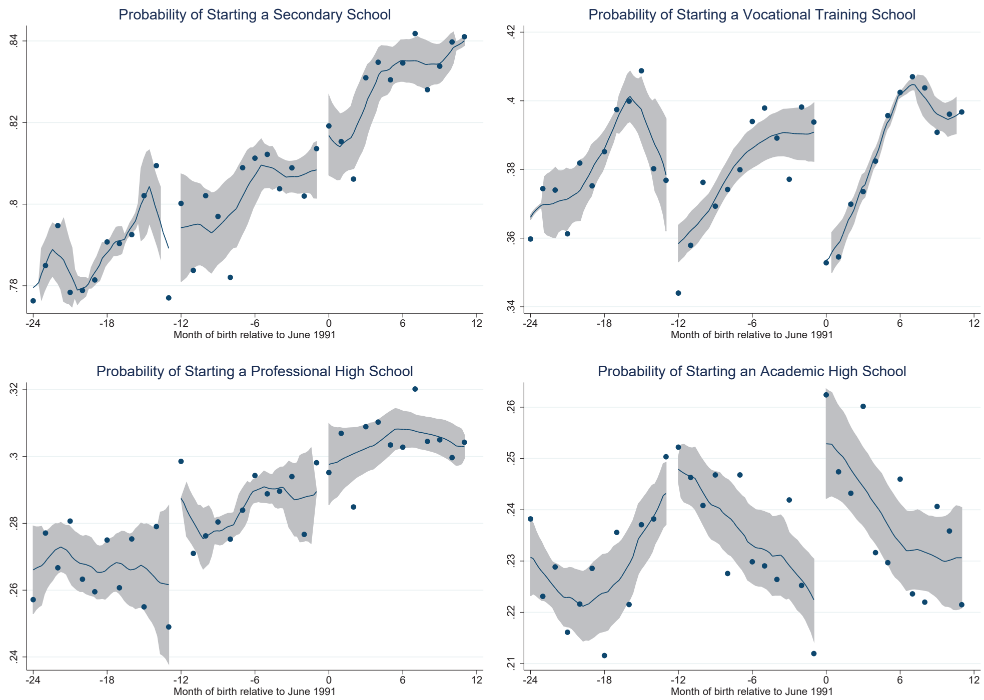
The positive effect on the probability of starting academic high schools instead of professional high schools or vocational training schools, although not robust across all specifications, is weakly present in the total sample (Figure 10). In fact, if this effect on the total sample was the only hypothesis to test, its coefficient (0.015) would be significant on 5% with a p-value of 0.046 (See Table 11, first item in the last row.). After the multiple testing procedure though, it is not considered as significant on 5% any more.

Table 11: Effects on School Choice

Tracks	Total sample	Primary	Vocational	High school	Tertiary	Missing
		Mother's highest education at giving birth				
Effect on the probability of finishing at least the first year in a secondary school						
Any secondary school	-0.000	0.003	-0.001	-0.004	-0.002	0.000
	(0.003)	(0.006)	(0.003)	(0.002)	(0.001)	(0.007)
	[102,616]	[26,242]	[19,474]	[25,567]	[9,061]	[16,928]
	{152.6}	{170.0}	{160.1}	{153.8}	{152.8}	{100}
Corrected p-values	0.913	0.743	0.901	0.238	0.232	0.970
Vocational training school	-0.017*	-0.049***	-0.045	-0.003	0.005	0.005
	(0.006)	(0.005)	(0.021)	(0.003)	(0.003)	(0.009)
	[80,505]	[19,323]	[14,234]	[29,280]	[7,829]	[16,928]
	{118.2}	{123.9}	{116.1}	{177.2}	{130.2}	{100}
Corrected p-values	0.084	0.000	0.144	0.376	0.241	0.742
Professional high school	-0.002	0.004	-0.010	-0.016	-0.033*	0.023**
	(0.007)	(0.010)	(0.014)	(0.010)	(0.012)	(0.006)
	[103.292]	[21,735]	[16,804]	[23,159]	[8,057]	[16,928]
	{153.0}	{139.6}	{138.1}	{138.2}	{134.9}	{100}
Corrected p-values	0.809	0.805	0.636	0.242	0.068	0.027
Academic high school	0.015	0.041***	0.032**	0.028**	0.006	-0.023
	(0.006)	(0.008)	(0.008)	(0.008)	(0.014)	(0.010)
	[86,292]	[24,082]	[21,467]	[25,220]	[9,247]	[16,928]
	{127.6}	{140.9}	{177.7}	{151.8}	{155.8}	{100}
Corrected p-values	0.099	0.003	0.010	0.021	0.814	0.132

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Robust standard errors clustered by birth year and month are in parentheses, observation numbers are in brackets, bandwidths in days are in braces. P-values are corrected by the number of hypothesis tests (78) done together in Tables 10 - 13 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by a 100-day bandwidth are in Table 31 in Appendix C.

Figure 10: Effects on School Choice, Children of Mothers with a Primary Degree



Local linear regressions fit separately below and above the cutoff in 1990 and in 1991 on birth year and month averages, using the rule-of-thumb bandwidth of lpoly in Stata with 90% confidence intervals. Month of birth -12 represents June 1990, 0 represents June 1991. No. of individual observations: 76,984.

6.6 Effects on Dropout Rates

The increase in CSL age causes an increase in the probability of dropping out of vocational training schools (Table 12). The average effect is an increase by 2.2 percentage points. Comparing this to the share of dropouts from vocational training schools in the previous cohort, which is 11.5% (see Table 30), this is a 2.2/11.5=19% effect.

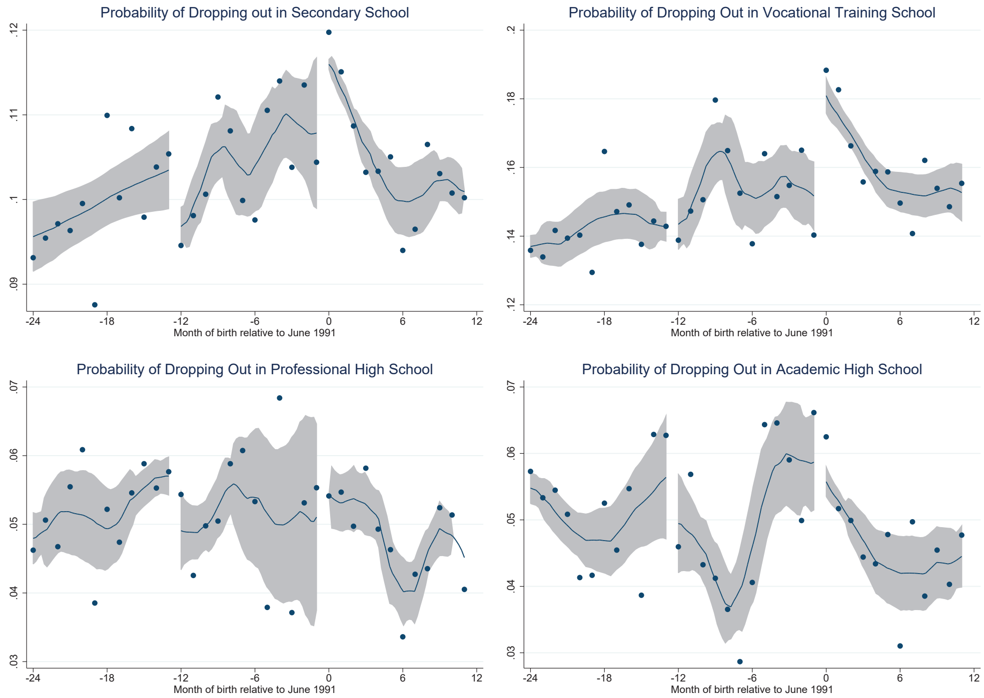
The ITT effect on dropping out of vocational training schools is not significant on 5% in the case of mothers with the lowest educational outcomes due to the multiple testing procedure (p-value=0.010).

Table 12: Effects on Dropout Rates

		Mother's highest education at giving birth				
Tracks	Total sample	Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of dropping out of ...						
Any secondary school	0.008**	0.011	0.010*	0.005	-0.002	0.006
	(0.003)	(0.009)	(0.005)	(0.003)	(0.003)	(0.006)
	[82,615]	[19,234]	[19,580]	[17,678]	[9,682]	[20,111]
	{137.5}	{160.5}	{178.5}	{117.3}	{164.1}	{156.2}
Corrected p-values	0.017	0.319	0.067	0.246	0.562	0.473
Vocational training school	0.022**	0.037***	0.017*	0.026	-0.028	0.016
	(0.004)	(0.012)	(0.007)	(0.010)	(0.044)	(0.013)
	[25,525]	[9,733]	[5,314]	[2,106]	[337]	[3,238]
	{174.1}	{165.2}	{161.4}	{112.0}	{167.3}	{100.0}
Corrected p-values	0.001	0.003	0.082	0.091	0.671	0.373
Professional high school	0.008	0.004	0.010	0.006	0.004	0.003
	(0.004)	(0.005)	(0.005)	(0.004)	(0.003)	(0.010)
	[32,570]	[8,122]	[9,913]	[9,600]	[1,534]	[4,985]
	{138.7}	{178.0}	{182.9}	{136.2}	{112.3}	{100.0}
Corrected p-values	0.134	0.617	0.173	0.245	0.335	0.797
Academic high school	0.003	-0.001	0.010***	0.000	-0.002	0.006
	(0.002)	(0.008)	(0.003)	(0.003)	(0.002)	(0.004)
	[48,077]	[6,049]	[6,423]	[13,067]	[9,472]	[8,289]
	{156.9}	{164.6}	{139.2}	{138.8}	{203.9}	{100.0}
Corrected p-values	0.249	0.970	0.001	0.965	0.425	0.351

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Robust standard errors clustered by birth year and month are in parentheses. Observation numbers are in brackets. Bandwidths in days are in braces. P-values are corrected by the number of hypothesis tests (78) done together in Tables 10 - 13 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by 100-day bandwidths are in Table 32 in Appendix C.

Figure 11: Effects on Dropout Rates, Children of Mothers with a Primary Degree



Local linear regressions fit separately below and above the cutoff in 1990 and in 1991 on birth year and month averages, using the rule-of-thumb bandwidth of lpoly in Stata with 90% confidence intervals. Month of birth -12 represents June 1990, 0 represents June 1991. No. of individual observations: 61,279, 29,254, 21,170 and 18,476, respectively.

6.7 Effects on School Completion

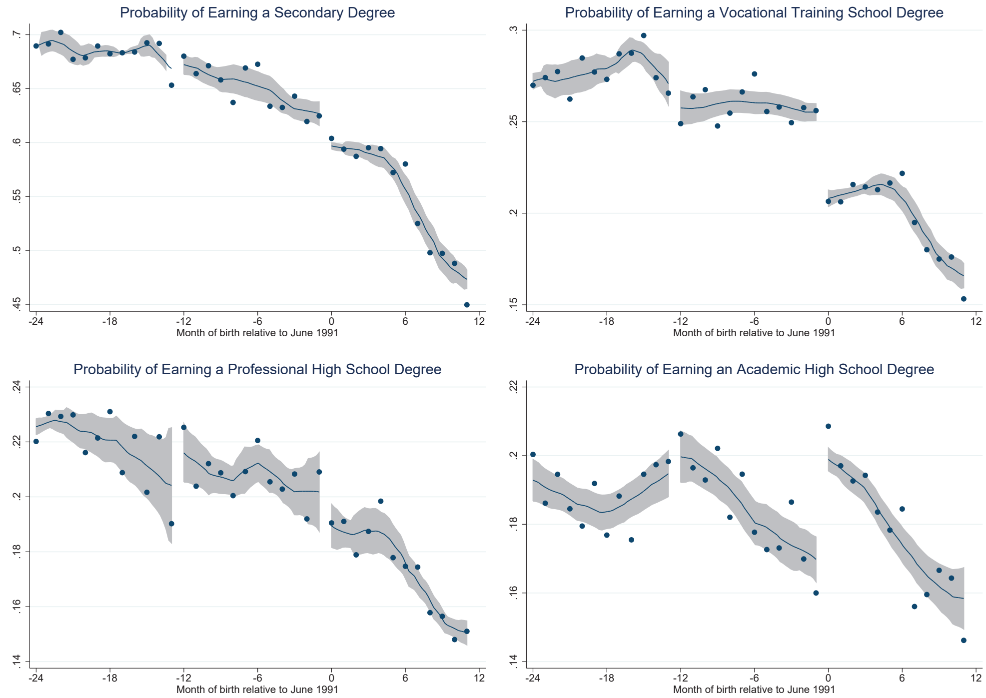
The negative effect of the CSL age increase on the higher probability of not earning a secondary degree, and in particular, a vocational training school degree, is the highest in the two lowest maternal education groups (see Table 13), mostly because they are the most likely to attend vocational schools (see Table 30 in Appendix A.). In the same two groups, the higher CSL age has a quite large positive effect on the probability of gaining an academic high school degree, at 4.4 and 3.5 percentage points, respectively (see Table 13). However, it is worrying that this same effect is at a significant -5 percentage points for the group of children with missing maternal education data (see Table 13, last column). The group of children with missing maternal education data are the observations that could not be linked to the Vital Statistics records which contain information on maternal education. The overall positive effect of the legislation change on the probability of gaining an academic high school degree is 1.7 percentage points and highly significant. Thus, the findings of this paper holds without estimating any heterogeneous effects by parental education as well.

Table 13: Effects on School Completion

		Mother's highest education at giving birth				
	Total sample	Primary	Vocational	High school	Tertiary	Missing
Any secondary degree	-0.015***	-0.022**	-0.020**	-0.011***	-0.004	-0.017**
	(0.002)	(0.006)	(0.006)	(0.002)	(0.009)	(0.005)
	[62,682]	[20,359]	[12,615]	[14,319]	[4,819]	[16,928]
	{92.4}	{130.2}	{103.9}	{84.1}	{80.1}	{100}
Corrected p-values	0.002	0.020	0.040	0.005	0.762	0.029
Vocational training school degree	-0.019**	-0.054**	-0.045**	-0.011***	0.003	0.002
	(0.004)	(0.002)	(0.012)	(0.002)	(0.002)	(0.005)
	[70,804]	[20,359]	[16,343]	[21,901]	[8,109]	[16,928]
	{104.4}	{114.4}	{134.8}	{130.1}	{135.6}	{100}
Corrected p-values	0.009	0.000	0.014	0.003	0.348	0.800
Professional high school degree	-0.013	-0.010	-0.020*	-0.024*	-0.020	0.026*
	(0.006)	(0.008)	(0.007)	(0.008)	(0.012)	(0.004)
	[83,827]	[28,655]	[18,040]	[19,796]	[9,184]	[7,852]
	{123.5}	{185.7}	{148.8}	{117.3}	{154.5}	{46.9}
Corrected p-values	0.125	0.332	0.056	0.051	0.228	0.024
Academic high school degree	0.017***	0.044***	0.035***	0.028***	0.019	-0.050**
	(0.002)	(0.005)	(0.006)	(0.005)	(0.015)	(0.007)
	[76,374]	[23,200]	[16,695]	[19,111]	[7,526]	[9,693]
	{112.7}	{149.9}	{137.3}	{113.5}	{125.5}	{57.6}
Corrected p-values	0.001	0.000	0.002	0.005	0.350	0.020

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Robust standard errors clustered by birth year and month are in parentheses. Observation numbers in the local regression are in brackets. Bandwidths in days are in braces. P-values are corrected by the number of hypothesis tests (78) done together in Tables 10 - 13 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by 100-day bandwidths are in Table 33 in Appendix C.

Figure 12: Effects on School Completion, Children of Mothers with a Primary Degree



Local linear regressions fit separately below and above the cutoff in 1990 and in 1991 on birth year and month averages, using the rule-of-thumb bandwidth of lpoly in Stata with 90% confidence intervals. Month of birth -12 represents June 1990, 0 represents June 1991. No. of individual observations: 76,984.

6.8 Local Average Treatment Effects (LATE) of the Legislation Change

Similarly to the general results as presented in Section 5.5, LATE are about three times as large as the ITT impacts (see Table 14). The LATE of increased CSL age on the probability of starting an academic high school is the highest in the lowest maternal education group, at 14 percentage points. Comparing this to the share of children starting at an academic high school before the legislation change, 24% (see Table 30 in Appendix B), this is a 58% LATE on the compliers. The LATE on the probability of earning a secondary degree is of the same magnitude in the group of children of the least educated mothers, 14.9 percentage points, or 0.149/0.192=78%.

Table 14: LATE on Starting an Academic High School and Earning an Academic High School Degree

	Total sample	Mother's highest education at giving birth				
		Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of starting an academic high school						
ITT effect	0.015**	0.041***	0.032***	0.028***	0.006	-0.056***
	(0.006)	(0.008)	(0.008)	(0.008)	(0.014)	(0.006)
First stage (jump in the probability of starting school under CSL age 18)	0.321***	0.029**	0.361***	0.380***	0.310***	N/A
	(0.025)	(0.011)	(0.030)	(0.041)	(0.031)	
	[2,110.7]	[3,019.8]	[711.1]	[922.1]	[1263.7]	
Wald estimate of the LATE coefficient	0.046	0.140	0.089	0.074	0.018	-
95% bootstrapped confidence intervals of the LATE coefficient	0.004 - 0.086	0.069 - 0.215	0.017 - 0.159	0.010 - 0.141	-0.082 - 0.121	-
No. of obs.	86,292	24,082	21,467	25,220	9,247	6,990
Bandwidth (in days)	127.6	155.1	177.1	151.8	155.8	41.5
Effect on the probability of earning an academic high school degree						
ITT effect	0.017***	0.044***	0.035***	0.028***	0.019**	-0.050***
	(0.002)	(0.005)	(0.006)	(0.005)	(0.007)	(0.007)
First stage (jump in the probability of starting school under CSL age 18)	0.311***	0.297***	0.354***	0.346***	0.290***	N/A
	(0.022)	(0.010)	(0.034)	(0.034)	(0.028)	
	[2,515.1]	[2,2828.4]	[1,210.4]	[1,489.4]	[1,602.5]	
Wald estimate of the LATE coefficient	0.055	0.149	0.098	0.082	0.066	-
95% bootstrapped confidence intervals of the LATE coefficient	0.008 - 0.100	0.080 - 0.215	0.015 - 0.182	0.001 - 0.167	-0.065 - 0.206	-
No. of obs.	76,374	23,200	16,695	19,111	7,526	9,693
Bandwidth (in days)	112.7	149.9	137.3	113.5	125.5	57.6

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Wald estimates are calculated as the ratios of ITT coefficients over the first stage coefficient (the size of the jump). Robust standard errors clustered by birth year and month are in parentheses, first stage F-statistics are in brackets. A */**/*** indicates significance on 10%/5%/1% level. The bootstrapped confidence intervals of the LATE coefficients are constructed following Bjorklund and Jantti (1997). First, a birth year and month stratified bootstrap sample is drawn from the 2001 Hungarian Census data, and birth year and month school entering probabilities ($p\hat{9}8_m$) are estimated. Then, a birth year and month stratified bootstrap sample is drawn from the 2011 Hungarian Census data, from which the LATE parameters ($\hat{\beta}_{LATE,1}$) are estimated using the school entering rates estimated in the first step. Repeating these two steps $B = 1000$ times yields an empirical distribution of $1,000$ estimated LATE coefficients ($\hat{\beta}_{LATE,1} - \hat{\beta}_{LATE,1000}$). The 95% confidence interval of $\hat{\beta}_{LATE}$ is set as the middle 95% of this empirical distribution.

Table 15: LATE on Dropping Out of Vocational Training School

	Total sample	Mother's highest education at giving birth				
		Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of dropping out of vocational training school						
ITT effect	0.022***	0.037**	0.017**	0.026**	-0.028	-0.014
	(0.004)	(0.012)	(0.007)	(0.010)	(0.044)	(0.011)
First stage (jump in the probability of starting school under CSL age 18)	0.333***	0.284***	0.362***	0.344***	0.313***	N/A
	(0.025)	(0.016)	(0.033)	(0.033)	(0.030)	
	[792.8]	[3,257.6]	[672.6]	[1,707.0]	[1,524.3]	
Wald estimate of the LATE coefficient	0.065	0.130	0.047	0.076	-0.089	-
95% bootstrapped confidence intervals of the LATE coefficient	0.015 - 0.114	0.023 - 0.240	-0.029 - 0.121	-0.031 - 0.189	-0.365 - 0.179	-
No. of obs.	25,525	9,733	5,317	2,106	337	2,183
Bandwidth (in days)	174.1	165.2	161.4	112.0	167.3	67.1

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Wald estimates are calculated as the ratios of ITT coefficients over the first stage coefficient (the size of the jump). Robust standard errors clustered by birth year and month are in parentheses, first stage F-statistics are in brackets. A */**/*** indicates significance on 10%/5%/1% level. The bootstrapped confidence intervals of the LATE coefficients are constructed following Bjorklund and Jantti (1997). First, a birth year and month stratified bootstrap sample is drawn from the 2001 Hungarian Census data, and birth year and month school entering probabilities ($p\hat{9}8_m$) are estimated. Then, a birth year and month stratified bootstrap sample is drawn from the 2011 Hungarian Census data, from which the LATE parameters ($\hat{\beta}_{LATE,1}$) are estimated using the school entering rates estimated in the first step. Repeating these two steps $B = 1000$ times yields an empirical distribution of $1,000$ estimated LATE coefficients ($\hat{\beta}_{LATE,1} - \hat{\beta}_{LATE,1000}$). The 95% confidence interval of $\hat{\beta}_{LATE}$ is set as the middle 95% of this empirical distribution.

Increasing the CSL age raised the probability of dropping out of vocational training school by 13 percentage points among children of mothers with primary education around the cutoff (see Table 15). In per cent terms, this is an 0.130/0.149=87% impact on the compliers.

7 Robustness Checks

This section supports the main findings of Section 5 and 6 using several robustness checks.

7.1 Parental Education Data from Other Sources Give Similar Results

Until this point, all results based on parental education in Section 7 were estimated using a subsample of individuals whose birth records data, containing information on maternal education, could be linked to the 2011 Hungarian Census using linking method 1. As detailed in Section 6.1, method 1 is based on a two-level information comparison approach. In the case of those still living with their parents, the linking procedure uses all available information appearing in both datasets, including the date of birth of parents. In the case of those not living with their parents anymore, it uses maternal residence and exact date of birth only. Thus, those living at home in 2011 are easier to link using this method. As it is also detailed in Section 6.1, there are two additional methods for gaining information on parental education: linking birth records without using any information on parents (linking method 2), and using the subsample of those still living at home at the time of the 2011 Hungarian Census. It would be worrying if the three methods would lead to different results. However, this is not the case. Table 16 shows that all three methods lead to similar results.

Table 16: Robustness Check 1 - Parental Education from Different Sources (ITT effects)

	Total sample	Highest parental education: Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of starting an academic high school						
Maternal education at giving birth - linking with method 1 (the same as in Table 31 in Appendix C)	0.021***	0.050***	0.038***	0.036***	0.022	-0.023**
	(0.004)	(0.009)	(0.008)	(0.006)	(0.013)	(0.010)
	[67,259]	[15,588]	[12,097]	[16,696]	[5,950]	[16,928]
Maternal education at giving birth - linking with method 2		0.058***	0.010	0.046**	0.035	0.011**
		(0.008)	(0.010)	(0.015)	(0.022)	(0.005)
		[9,432]	[6,396]	[6,743]	[1,529]	[43,159]
Educational status of the household head		0.062***	0.041***	-0.001	0.013	0.011
		(0.005)	(0.007)	(0.009)	(0.008)	(0.007)
		[8,121]	[20,011]	[12,446]	[7,820]	[18,861]
Effect on the probability of earning an academic high school degree						
Maternal education at giving birth - linking with method 1 (the same as in Table 33 in Appendix C)	0.018***	0.055***	0.028**	0.030***	0.025	-0.030
	(0.003)	(0.005)	(0.010)	(0.007)	(0.018)	(0.010)
	[67,259]	[15,588]	[12,097]	[16,696]	[5,950]	[16,928]
Maternal education at giving birth - linking with method 2		0.065***	0.005	0.044**	0.048	0.004
		(0.009)	(0.012)	(0.016)	(0.027)	(0.004)
		[9,432]	[6,396]	[6,743]	[1,529]	[43,159]
Educational status of the household head		0.053***	0.032***	0.001	0.007	0.011
		(0.008)	(0.005)	(0.011)	(0.006)	(0.006)
		[8,121]	[20,011	[12,446]	[7,820]	[18,861]
Effect on the probability of dropping out of vocational training school						
Maternal education at giving birth - linking with method 1 (the same as in Table 32 in Appendix C)	0.029**	0.052**	-0.002	0.028*	0.061***	0.016
	(0.005)	(0.012)	(0.007)	(0.012)	(0.012)	(0.013)
	[14,319]	[5,841]	[3,210]	[1,841]	[189]	[3,238]
Maternal education at giving birth - linking with method 2		0.035*	-0.023	0.012	-0.043	0.041***
		(0.012)	(0.013)	(0.021)	(0.070)	(0.007)
		[3,785]	[1,894]	[921]	[61]	[7,658]
Educational status of the household head		0.036*	0.033**	0.009	0.064**	0.014
		(0.015)	(0.006)	(0.009)	(0.007)	(0.012)
		[3,313]	[5,897]	[1,640]	[344]	[3,125]

Local linear kernel regressions using a 100-day bandwidth. Robust standard errors clustered by birth year and month are in parentheses, number of observations are in brackets. A */**/*** indicates significance on 10%/5%/1% level.

7.2 No Significant Effects at Cutoffs in the Before and After Years

One might be right to worry about whether the effects captured by this analysis are due to either starting elementary school at different ages (just over the age of 6 vs. almost age 7), or are just due to time (or academic-year) trends. If either of these were true, we should see the same effects for students born around the June 1 cutoff in other years. Two things happen around cutoffs in other years: (1) those born in June start school at an older age, and (2) those born in June spend one year less in school before reaching the CSL age. Theoretically, starting school at an older age can affect education outcomes positively or negatively. Black, Devereaux and Salvanes (2011) argue that starting school at an older age may be beneficial for learning because older children are at a more advanced stage of their developmental life. In addition, social development may depend on a child's age relative to the class. If being older than one's peers is beneficial, starting school at an older age would be beneficial as well. However, it is not clear whether this is really the case.

On the other hand, starting school at an older age may be harmful if children are able to learn more in school than in pre-school (or at home). Furthermore, parental investment in helping children with their school work may depend on school starting age as well – parents may provide less help to children if they start school when they are older. Black, Devereaux and Salvanes (2011) examine the effect of school starting age on education outcomes and they find very small positive effects of starting school when younger. In the Hungarian case, Hámori and Köllő (2011) examine the effect of school starting age on test results taken in Grades 4 and 8. They find a positive effect of starting school at an older age when in Grade 6 but the effect becomes much smaller by Grade 8. However, they cannot separate the effect of school starting age from the effect of age at time of the test, as these two are perfectly collinear.

Figures 5 - 7 in Section 5 plot all outcomes variables around the same cutoff in the previous year along with the cutoff of the legislation change in 1991. In the case of the probability of starting a vocational training school and earning a vocational school degree there is a break in the data around the cutoff in the previous year as well. Those born right after June 1 in both 1990 and 1991 are less likely to start a vocational training school and earn a vocational training school degree than those born right before that (see Figures 5 and 7). However, I do not find significant effects for the rest of the educational outcomes around the cutoff in 1990.

Table 17 shows the ITT effects of cutoffs in the year before and after 1991, alongside the real cutoff in 1991, for the three main outcome variables. On the probability of starting and completing academic high school, and, dropping out of vocational training school, the 1991 cutoff produces the largest, and the solely significant, coefficients. Thus, the effects of starting school when older and spending fewer years in school balance each other out in the comparison cohorts of those born in 1990 and 1992.

Table 17: Robustness Check 2 - Effects of Cutoffs in Other Years (ITT effects)

		Mother's highest education at giving birth				
Cutoff: June 1	Total sample	Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of choosing an academic high school						
1990	-0.001	-0.004	-0.003	0.024	-0.015	-0.015
	(0.006)	(0.008)	(0.004)	(0.014)	(0.012)	(0.010)
	[65,408]	[14,969]	[11,490]	[15,872]	[5,824]	[17,253]
1991	0.021***	0.050***	0.038***	0.036***	0.022	-0.023**
	(0.004)	(0.009)	(0.008)	(0.006)	(0.013)	(0.010)
	[67,259]	[15,588]	[12,097]	[16,696]	[5,950]	[16,928]
1992	0.008	0.001	0.024*	-0.013	0.005	0.026
	(0.007)	(0.004)	(0.012)	(0.010)	(0.013)	(0.020)
	[64,903]	[15,250]	[12,091]	[16,223]	[5,971]	[15,3768]
Effect on the probability of earning an academic high school degree						
1990	0.003	-0.003	0.013	0.027	-0.015	-0.003
	(0.006)	(0.009)	(0.007)	(0.017)	(0.013)	(0.009)
	[65,408]	[14,969]	[11,490]	[15,872]	[5,824]	[17,253]
1991	0.018***	0.055***	0.028**	0.030***	0.025	-0.030
	(0.003)	(0.005)	(0.010)	(0.007)	(0.018)	(0.010)
	[67,259]	[15,588]	[12,097]	[16,696]	[5,950]	[16,928]
1992	-0.019	-0.013	-0.019	-0.039**	-0.029**	-0.010
	(0.010)	(0.008)	(0.010)	(0.014)	(0.011)	(0.013)
	[64,903]	[15,250]	[12,091]	[16,223]	[5,971]	[15,368]
Effect on the probability of dropping out of vocational training school						
1990	-0.005	-0.015	0.015	0.014	0.060	-0.022
	(0.006)	(0.010)	(0.017)	(0.016)	(0.035)	(0.019)
	[14,452]	[5,604]	[3,223]	[1,918]	[203]	[3,504]
1991	0.029**	0.052**	-0.002	0.028*	0.061***	0.016
	(0.005)	(0.012)	(0.007)	(0.012)	(0.012)	(0.013)
	[14,319]	[5,841]	[3,210]	[1,841]	[189]	[3,238]
1992	-0.002	-0.001	0.002	0.012	-0.008	-0.016
	(0.007)	(0.005)	(0.016)	(0.014)	(0.049)	(0.011)
	[13,882]	[5,801]	[3,053]	[1,762]	[190]	[3,076]

Local linear kernel regressions using a 100-day bandwidth. Robust standard errors clustered by birth year and month are in parentheses, number of observations are in brackets. A */**/*** indicates significance on 10%/5%/1% level.

7.3 Alternative Optimal Bandwidth Choices Give Similar Results

Another concern may be whether these results are sensitive to bandwidth choice. Tables 18 and 19 suggest that using the 50-150% versions of the CCT bandwidth gives very similar results, both in the terms of the magnitude of the coefficients and their p-values.

Table 18: Robustness Check 3/A - Effects Using 50-150% Versions of the CCT bandwidth (ITT effects)

		Mother's highest education at giving birth				
Version of CCT (2014) optimal bandwidth	Total sample	Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of starting an academic high school						
50%	0.018***	0.055***	0.034**	0.034***	0.030**	-0.02
	(0.002)	(0.006)	(0.009)	(0.004)	(0.010)	(0.011)
75%	0.023***	0.045***	0.031***	0.034***	0.021*	-0.019
	(0.004)	(0.009)	(0.008)	(0.005)	(0.010)	(0.022)
100%	0.015**	0.041***	0.032***	0.028***	0.006	-0.023
	(0.006)	(0.008)	(0.008)	(0.008)	(0.014)	(0.010)
	[86,292]	[24,082]	[21,467]	[25,220]	[9,247]	[16,928]
	{127.6}	{140.9}	{177.7}	{151.8}	{155.8}	{100}
125%	0.018***	0.043***	0.022**	0.017	0.004	-0.016
	(0.005)	(0.008)	(0.009)	(0.010)	(0.013)	(0.015)
150%	0.016***	0.036***	0.030***	0.023***	0.015	-0.010
	(0.005)	(0.008)	(0.007)	(0.008)	(0.012)	(0.007)
Effect on the probability of earning an academic high school degree						
50%	0.016**	0.051***	0.042***	0.035***	0.042**	-0.048**
	(0.003)	(0.003)	(0.010)	(0.004)	(0.010)	(0.013)
75%	0.021***	0.052***	0.029**	0.030***	0.031	-0.046***
	(0.004)	(0.005)	(0.009)	(0.004)	(0.016)	(0.008)
100%	0.017***	0.044***	0.035***	0.028***	0.019	-0.050**
	(0.002)	(0.005)	(0.006)	(0.005)	(0.015)	(0.007)
	[76,374]	[23,200]	[16,695]	[19,111]	[7,526]	[9,693]
	{112.7}	{149.9}	{137.3}	{113.5}	{125.5}	{57.6}
125%	0.016**	0.045***	0.032***	0.019**	0.010	-0.033**
	(0.004)	(0.006)	(0.006)	(0.009)	(0.017)	(0.010)
150%	0.015***	0.043***	0.025***	0.017	0.004	-0.015
	(0.003)	(0.006)	(0.006)	(0.010)	(0.018)	(0.016)

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Robust standard errors clustered by birth year and month are in parentheses. Number of observations are in brackets, 100% CCT bandwidths in days in braces. A */**/*** indicates significance on 10%/5%/1% level.

Table 19: Robustness Check 3/B - Effects Using 50-150% Versions of the CCT bandwidth (ITT effects)

		Mother's highest education at giving birth				
Version of CCT (2014) optimal bandwidth	Total sample	Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of dropping out of vocational training schools						
50%	0.029***	0.059***	0.002	0.012	0.051**	0.008
	(0.004)	(0.010)	(0.006)	(0.010)	(0.013)	(0.004)
75%	0.027***	0.050***	0.010	0.016	-0.020	0.010
	(0.005)	(0.011)	(0.007)	(0.007)	(0.043)	(0.004)
100%	0.022***	0.037**	0.017**	0.026**	-0.028	0.016
	(0.004)	(0.012)	(0.007)	(0.010)	(0.044)	(0.013)
	[25,525]	[9,733]	[5,314]	[2,106]	[337]	[3,238]
	{174.1}	{165.2}	{161.4}	{112.0}	{167.3}	{100.0}
125%	0.022***	0.033***	0.020**	0.019	-0.035	0.006
	(0.004)	(0.010)	(0.007)	(0.012)	(0.046)	(0.003)
150%	0.023***	0.036***	0.015**	0.027*	-0.039	0.006
	(0.004)	(0.009)	(0.006)	(0.013)	(0.037)	(0.003)

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014). Robust standard errors clustered by birth year and month are in parentheses. Number of observations are in brackets, 100% CCT bandwidths in days in braces. A */**/*** indicates significance on 10%/5%/1% level.

7.4 Global Polynomial Estimation Gives Similar Results

Even though cutoffs in others years do not yield statistically significant impacts, their coefficients are not significant zeros. As a result, one may still be worried about seasonality in the data or may question the assumption that date of birth is exogenous in general (see Section 4.1). To control for such seasonality effects, global polynomial models are estimated so that in addition to the 4th-order polynomial functions of the assignment variable below and above the cutoff, birth year and month fixed effects (FE) are explicitly controlled. Day of the week of birth, county, and settlement type FE's are also explicitly controlled. Table 20 shows that the estimated coefficients are very similar in their signs, magnitude, and significance to the earlier results.

Table 20: Robustness Check 4 - Global Polynomial Estimation (ITT effects)

	Total sample	Mother's highest education at giving birth: Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of starting an academic high school						
Local linear effect	0.015**	0.041***	0.032***	0.028***	0.006	-0.023
	(0.006)	(0.008)	(0.008)	(0.008)	(0.014)	(0.010)
	[86,292]	[24,082]	[21,467]	[25,220]	[9,247]	[16,928]
	{127.6}	{140.9}	{177.7}	{151.8}	{155.8}	{100}
Global polynomial effect	0.011***	0.034***	0.022	0.018**	0.008	-0.023
	(0.004)	(0.009)	(0.010)	(0.008)	(0.013)	(0.012)
	586,283	136,513	103,329	143,259	51,010	[152,172]
Effect on the probability of earning an academic high school degree						
Local linear effect	0.017***	0.044***	0.035***	0.028***	0.019	-0.050**
	(0.002)	(0.005)	(0.006)	(0.005)	(0.015)	(0.007)
	[76,374]	[23,200]	[16,695]	[19,111]	[7,526]	[9,693]
	{112.7}	{149.9}	{137.3}	{113.5}	{125.5}	{57.6}
Global polynomial effect	0.013**	0.043***	0.024**	0.015	0.014	-0.025*
	(0.006)	(0.006)	(0.011)	(0.011)	(0.018)	(0.014)
	[586,283]	[136,513]	[103,329]	[143,259]	[51,010]	[152,172]
Effect on the probability of dropping out of vocational training schools						
Local linear effect	0.022***	0.037**	0.017**	0.026**	-0.028	0.016
	(0.004)	(0.012)	(0.007)	(0.010)	(0.044)	(0.013)
	[25,525]	[9,733]	[5,314]	[2,106]	[337]	[3,238]
	{174.1}	{165.2}	{161.4}	{112.0}	{167.3}	{100.0}
Global polynomial effect	0.028***	0.063***	0.004	0.004	-0.089	0.012
	(0.007)	(0.009)	(0.010)	(0.014)	(0.064)	(0.008)
	[132,209]	[52,303]	[28,937]	[17,194]	[1,797]	[31,978]

Local linear kernel regressions using the bandwidth optimization routine of CCT (2014) and global polynomial estimations using data from 1988-1992, controlling for a 4th order polynomial function of the running variable separately below and above the cutoff, and the following FE's: year of birth, month of birth, day of the week of birth, county, settlement type. Robust standard errors clustered by birth year and month are in parentheses. Number of observations are in brackets, CCT 2014 bandwidths in days are in braces. A */**/*** indicates significance on 10%/5%/1% level.

The fact that the results of my nonparametric and parametric empirical strategy coincide is important. Results in the CSL age literature tend to be sensitive to using an econometric method. For example, several papers use geographical and timing variations of CSL age legislation changes to assume a common time trend in these geographical units. They control separately for state and cohort fixed effects (Acemoglu and Angrist, 2001; Lochner and Moretti, 2004; Lleras-Muney, 2005; Oreopoulos, 2006). However, Stephens and Yang (2014) show that once state-specific time trends are introduced into the original models using the same US Census data that these papers used before, the estimated positive effects of longer education on wages, occupation, unemployment, and divorce will lose significance, and that many would even turn sign. Devereux and Hart (2010) reevaluate a CSL age increase in 1947 in England, evaluated by Harmon and Walker (1995) and Oreopoulos (2007). They use an RDD setup and get 3 to 5 times lower wage returns than earlier papers using other methods.

This global polynomial method is similar to the diff-in-diffs strategy applied by a wide strand of the CSL age literature (e.g. Meghir and Palme, 2005; Oreopoulos, 2007; Oosterbeek and Webbink, 2007; and Pischke and Wachter, 2008). It is relieving that in this particular case, the results of the diff-in-diffs and the RDD techniques coincide.

8 Supportive Evidence from Other Data Sources and Potential Channels

8.1 The Share of those Attending Academic High Schools in the NABC Data

One of the main findings of this paper is that the probability of starting at an academic high school is positively affected by the CSL age increase. Table 21 shows the share of students attending the three types of secondary school in the Grade 10 waves of the National Assessment of Basic Competencies (NABC) data. This may be a biased sample in the sense that the probability of making it to Grade 10 and being observed by the survey may be related to the CSL age increase by design. Nonetheless, not finding the same result in this data would be worrying. As Table 21 shows, the ratio of those attending academic high school in Grade 10 is significantly higher among those born within three months after the cutoff. The difference is that of about 4 percentage points, a little higher but quite similar in magnitude to the effects estimated in the Census data.

Table 21: The Probability of Attending Secondary Schools by Type, NABC data

	Born in March-May 1991	Born in June-Aug 1991	Difference	t-test p-value
Vocational training sch.	0.484	0.415	-0.069***	0.000
Professional high school	0.387	0.414	0.027	0.048
Academic high school	0.129	0.171	0.042***	0.000

Own estimation from the Grade 10 waves of the 2006-2010 NABC data. No. of observations: 5,669. A */**/*** indicates significance on 10%/5%/1% level.

8.2 Potential Explanation to the Increase in Dropouts: Change in Student Composition

One potential channel through which a higher CSL age may increase dropout rates could be its effect on student composition. This works in two ways. Firstly, if the change induces more students to start secondary school at the lower end of the distribution, lower ability students would mainly attend vocational training schools, which may mechanically lead to higher dropout rates. This hypothesis is not likely in our case. As is seen in Table 31 and Table 11, higher CSL age did not increase the probability of starting a secondary school in general, neither on average nor among children of less educated mothers. However, the probability of starting at a vocational training school decreased by 4.9 percentage points on the lower end of the distribution, while there is an increase of similar size in the probability of starting at an academic high school. Thus, it is quite likely that the students who switched from the vocational to the high school track were higher ability students, which could have an effect on student composition in vocational training schools. Secondly, keeping students in school for longer should have a composition effect with regard to those who would have otherwise dropped out.

Table 22: Heterogeneity of the Effect with Respect to the Share of the Less Educated (ITT effects)

Effect on the probability of dropping out of vocational training school			
	ITT effect	Robust SE	No. of obs.
1st quintile	0.028	0.025	1,736
2nd quintile	0.000	0.001	2,144
3rd quintile	0.005	0.005	2,675
4th quintile	0.048**	0.017	3,525
5th quintile	0.043**	0.020	4,239

Outcome variable: probability of dropping out of vocational training schools. Local linear kernel regressions using a 100-day bandwidth. The first column indicates which quintile the settlement where the individual was born belongs to with respect to its share of less educated adults. All rows represent a separate regression. Robust standard errors are clustered by birth year and month. A */**/*** indicates significance on 10%/5%/1% level.

There are signs that the higher CSL age changed student composition in vocational training schools. Assuming that there is a composition effect, dropout rates will be higher in settlements where the share of students from lower socioeconomic background is larger. Table 22 shows the effect of the legislation change by quintiles with respect to the share of less educated adults (hence, signaling the share of children of less educated mothers) in the settlement where the individual was born. The effect of dropout rates is indeed the highest and most significant in the top two quintiles where the share of less educated mothers is also the highest.

Table 23: Student Composition in Vocational Training Schools

	Avg. share of Roma students	Avg. share of students receiving child protection subsidy	Avg. share of students living in financially deprived family	Avg. share of students with at least one unemployed parent
		In vocational training schools		
2006	0.232	0.336	0.484	0.318
2007	0.178	0.267	0.410	0.285
2008	0.265	0.351	0.499	0.371
2009	0.354	0.407	0.462	0.408
2010	0.373	0.413	0.506	0.444
2011	0.408	0.436	0.522	0.435

Data source: own calculation from the school (*telephely*) level data of the Hungarian Assessment of Basic Competencies database. Schools offering primary education or academic high school track are excluded.

Also, the school (*telephely*) level data of the NABC data suggest that the share of disadvantaged students increased quite dramatically in vocational training schools after the reform (Table 23). In fact, according to school principals, the share of Roma students almost doubled between 2006 and 2011, and the share of students with at least one unemployed parent grew by about a quarter.

8.3 Potential Explanation to an Increase in Dropouts: Supply Constraints in Vocational Training Schools

Another reason behind the increase of vocational training school dropout rates is that no sufficient resources were allocated to schools to compensate for their extra workload in terms of the number and the composition of students. In line with some earlier evidence from a survey research (Mártonfi 2011a, 2011b), the available administrative data support this hypothesis as well. Table 24 shows the number of students and the average expenditures of schools during this period.

Table 24: Number of Students and Financing of Vocational Training Schools

		Vocational training programs only in school			Both vocational training and professional high school programs in school		
			Expenditures			Expenditures	
	No. of students in vocational education+	Avg. no. of students in school	In the previous fiscal year	Per student in current year	Avg. no. of students in school	In the previous fiscal year	Per student in current year
			index, 2006=100			index, 2006=100	
2006	126,211	246	100.0	100.0	704	100.0	100.0
2007	124,466	272	101.6	73.4	721	111.9	99.5
2008	129,066	284	82.5	99.4	726	114.1	103.4
2009	128,848	244	116.6	75.6	775	119.3	90.9
2010	135,268	248	76.2	60.5	783	112.1	93.8
2011	138,489	257	69.4	-	674	116.8	-

A + indicates data from the Statistical Yearbook of Education (Ministry of Human Resources, 2013). All other data are based on own calculation from the school (*telephely*) level data of the NABC database. Schools offering primary education or academic high school tracks are excluded. Expenditures are calculated from current prices, 2006=100. Students in all vocational school programs.

The structure of the available data and the school system is such that pinpointing exact numbers is not straightforward. Data on the number of students are available from two sources: the administrative data of the yearly Yearbooks of Education, and the school level data of the NABC database. However, as one school may offer programs in more than one track (i.e. offer both vocational training and professional high school tracks within the same institution), it is impossible to follow the expenditures of vocational schools only. Table 24 presents the expenditures of schools by fiscal (not academic) years, as well as their average expenditures per student. The data shows that schools did receive extra funding in 2008, the year when the first treated cohorts reached age 16 and were forced to continue their studies. In spite of this, on average, per capita expenditures of schools decreased between 2006 and 2011, even by current terms. However, there are differences between vocational schools as well: the decrease of expenditures in small schools offering vocational training programs only is much larger than in big schools offering both vocational and professional high school tracks.

Table 25: Teachers in Vocational Training Schools

	Number of students in vocational education$_{+}$	Number of teachers in vocational education^{+}	Student per teacher ratio in vocational education	Share of schools where at least one person teaches with no qualifications^{ł}	Avg. number of teachers with no qualification^{ł} in school
2006	126,211	8,938	14.1	0.565	2.32
2007	124,466	8,947	13.9	0.532	2.40
2008	129,066	8,942	14.4	0.520	2.35
2009	128,848	8,706	14.8	0.426	1.76
2010	135,268	8,824	15.3	0.605	4.15
2011	138,489	9,314	14.8	0.592	4.96

A + indicates data from the Statistical Yearbook of Education (Ministry of Human Resources, 2013); A ł indicates data from my own calculation using the school (*telephely*) level data of the NABC database. Schools offering exclusively vocational tracks are included. Students in all vocational programs.

The same is true with respect to student-per-teacher ratios. Table 25 shows the number and distribution of teachers working in vocational training schools. The number of students per teachers increased between 2006 and 2011, as well as the share and number of teachers teaching without proper qualifications.

9 Discussion

Increasing the CSL age from 16 to 18 in Hungary has a robust effect on the probability of choosing an academic high school at age 14, and earning an academic high school degree. This effect is realized at an intensive margin as it induced students to choose academic high schools over vocational schools but it did not affect their choice on whether to enter any secondary school or not. Most of the positive impacts on the probability of choosing and completing an academic high school was realized among children of less educated parents, who completed primary school at most. Considering the benefits that a high school degree has in the labor market, and also the fact that in Hungary 19% of children (468,000 children) at or below age 16 live in a family where the head of the household is less educated[15], this is a favorable outcome. Educational attainment is shown to be highly correlated across generations (Hertz et al., 2007) and it may be one of the strongest channels through which disadvantaged status and poverty is inherited (Haveman and Wolfe, 1995, and d'Addio, 2007). Closing the gap between the educational outcomes of the rich and the poor may bring substantial short- and long-term social returns.

This paper, however, documents an unexpected negative effect of the higher CSL age that caused an increase in the probability of students dropping out from vocational training schools. The fact that the new legislation put a strain on vocational training schools became obvious when the treated cohort reached age 16, which resulted in a growth in the number

[15] Own calculation from the 2011 Hungarian Census.

of students in these schools (Mártonfi, 2011a). The reason why vocational (and not high) schools were negatively affected derives from the highly selective and segregative nature of the Hungarian education system, which through free school choice and early tracking pushes children of low socioeconomic backgrounds into vocational schools.

There are two potential channels of this adverse effect on dropout rates. Firstly, although the demand for education services increased in vocational schools, they could not handle the increased workload due to supply side constraints. This finding is in line with the experience of development programs using demand-side interventions only. The literature on conditional cash transfers, such as cash benefits given to the poor on the condition of school attendance or participation in medical check-ups, concludes that one of the main elements of success is finding the right balance between demand and supply side components (Adato and Hoddinott, 2010). Secondly, composition of students shifted quite heavily towards children of very low socioeconomic background, causing a mechanical increase in dropout rates.

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A Appendix

Table 26: Literature on the Wage Returns of Higher CSL Age

Paper	Country and year of the reform	CSL age increase from ... to ...	Other elements of the reform?	Identification	Main result	Main conclusion
			Wage returns			
Harmon and Walker (1995)	England & Wales in 1947 and 1972	Age 14 to 15 and	supply side expansion	comparing pre- and post-reform cohorts*	10-15% wage return	wage returns are large
Meghir and Palme (2005)	Sweden, 1940's	Grade 7-8 to 9 (age 14-15 to 16)	no streaming, means-tested subsidies	Diff-in-diffs	positive (-) wage return on children of unskilled (skilled) fathers	abolishment of streaming might have reduced school quality
Oreopoulos (2007)	England,1947	Age 14 to 15	supply side expansion	Diff-in-diffs	15% lifetime wealth returns	teenagers must be myopic
Oosterbeek and Webbink (2007)	The Netherlands, 1975	Age 15 to 16	vocational education extended from 3 to 4 years	Diff-in-diffs	no effects on wages	general education in the extra year, one year less work experience
Pischke and Wachter (2008)	Germany, 1940's-1950's	Grade 8 to 9 (age 14 to 15)		Diff-in-diffs	no effect on wages	important skills are learn in earlier Grades
Devereux and Hart (2010)	England,1947	Age 14 to 15	supply side expansion	RDD	3% wage return	no effect on qualifications, heterogeneous LATE
Grenet (2013)	France, 1967 and Britain, 1972	Age 15 to 16		RDD	6-7% wage return in GB, no return in FR	effect on qualifications is key
Leonard and Sweetman (2013)	Newfoundland, 1983	Grade 11 to 12	(no change in the curriculum)	RDD	little to no general equilibrium impact on wages	too early measurement, economic crisis

Source: own collection. Papers investigating the wage effects of CSL age raise directly or using it as instrument only. Papers using school entry policies (i.e. Angrist and Kruger, 1991), several types of CSL legislation changes at the same time (i.e. Acemoglu and Angrist, 2001), or school time shortening measures (i.e. Büttner and Thomsen, 2010) are excluded. *Callan and Harmon (1999), Brunello and Miniaci (1999), Brandolini and Cipollone (2002), Levine and Plug (1999), Vieira (1999) and Pons and Gonzalo (2002) are using the same pre- vs. post-cohort methodology, criticized later by Card (1999) and Oreopoulos (2006) for not controlling for cohort fixed effects (Grenet, 2013).

Table 27: Literature on the Effects of Higher CSL Age Within Schools

Paper	Country and year of the reform	Raise from...	Other elements of the reform?	Identification	Main result	Main conclusion
Schooling Outcomes						
Wenger (2002)	USA, 1977-92	Age 13+ to at most 18		Diff-in-diffs	positive effect on high-school completion	negative effects may be expected on student composition
Landis and Reschly (2010)	USA, 2001-2005	Age 16-17 to 17-18		Diff-in-diffs	small effect on the timing of dropping out, no effect on high-school completion	individualized interventions addressing student engagement could be more effective
Cabus and De Witte (2011)	The Netherlands, 2007	Age 17 to 18		Diff-in-diffs	decreases dropping out by 2.5 percentage points	"effect" in the control group only

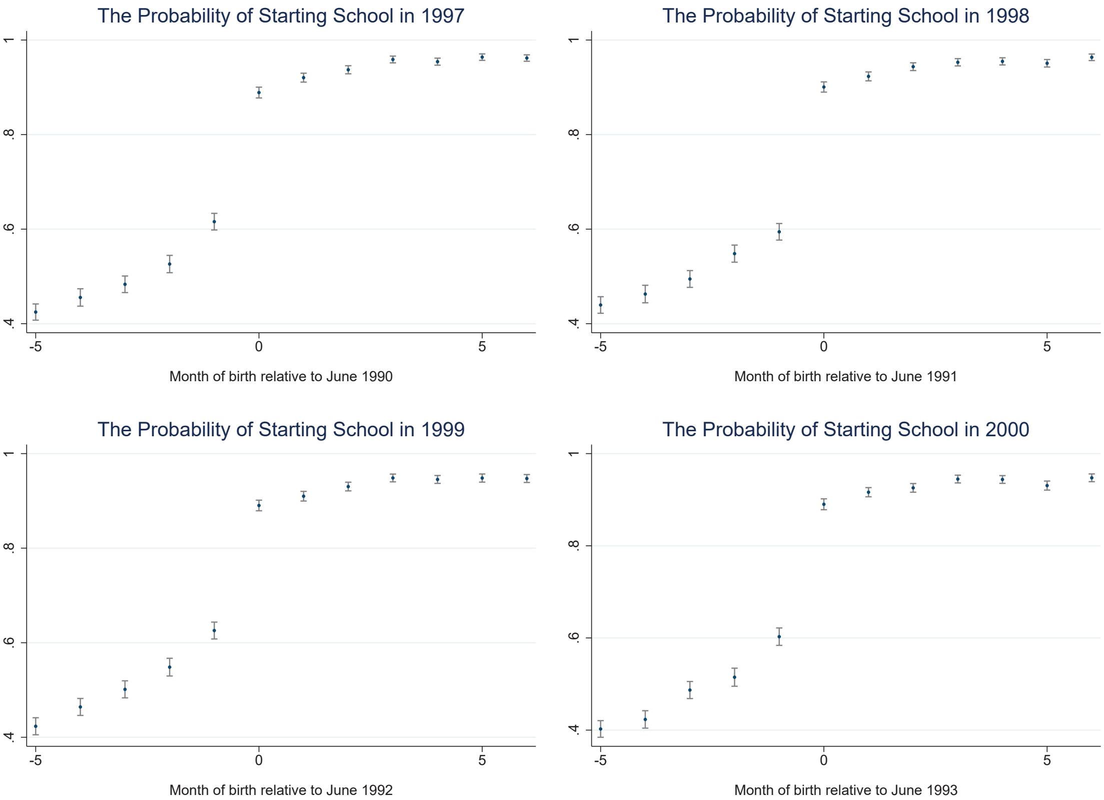
Source: own collection. Papers investigating the wage effects of CSL age raise directly or using it as instrument only.

Table 28: The 10-Grade Waves of the National Assessment of Basic Competencies (NABC) Survey Data Used in this Paper

Wave	Birth dates of the main cohort covered by each wave	Coverage	No. of students	No. of students with complete birth data	No. of students born June 90 - May 91	No. of students born June 91 - May 92	No. of students born in May 1991	No. of students born in June 1991	Sample taken
2006	June 89-May 90	30 students per school programs	43,602	35,115	629	4	1	0	no
2007	June 90 - May 91	30 students per school programs	43,775	36,605	26,085	664	1,421	284	no
2008	June 91 - May 92	all students	112,409	111,343	17,502	60,153	3,350	5,516	37,654*
2009	June 92 - May 93	all students	108,960	108,907	5,312	26,402	854	1,176	37,289*
2010	June 93 - May 94	all students	107,274	107,142	1,270	6,194	198	221	36,935*
Total			401,620	399,112	50,798	93,417	5,824	7,197	

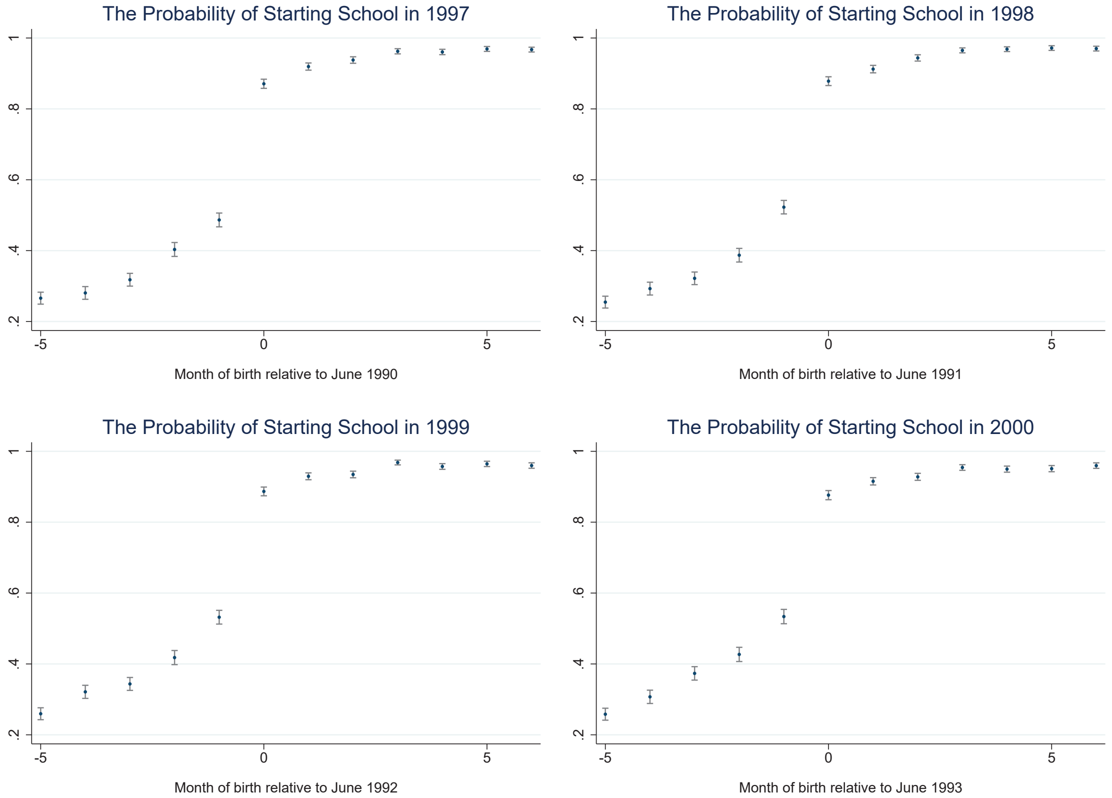
Data source: own calculations from the 10-grade waves of the 2006-2010 NABC data. *The 2006-2007 waves cover a subsample of 10-graders while the 2008-10 samples cover the full student population. Those born right before the cutoff are covered by the earlier waves, while those born after covered by the later ones. Thus, random samples of 22 students are taken from each program in each school to match the sampling strategy of waves 2006-2007, taken into consideration the fact that in the waves 2006-2007 the share of observations with complete information about year and month of birth is much lower. The sampling number 22 was chosen to match the number of observations in the samples of waves 2008-10 to the number of observations with date of birth information in waves 2006-2007.

Figure 13: The Probability of Starting School in 1997-2000, Children of Mothers with a Primary Degree



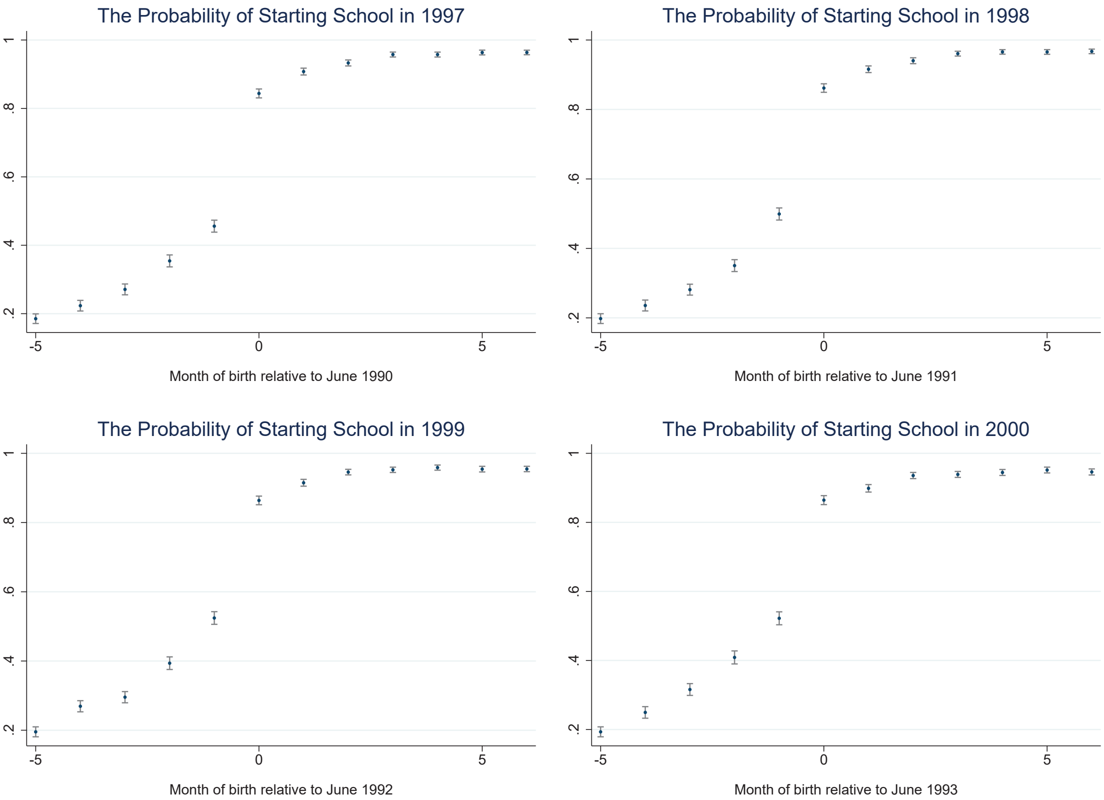
The average probability of starting school in 1997 for those born in 1990, in 1998 for those born in 1991, in 1999 for those born in 1992 and in 2000 for those born in 1993, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June. No. of individual observations: 36,053, 35,399, 34,266 and 33,045, respectively.

Figure 14: The Probability of Starting School in 1997-2000, Children of Mothers with a Vocational Training School Degree



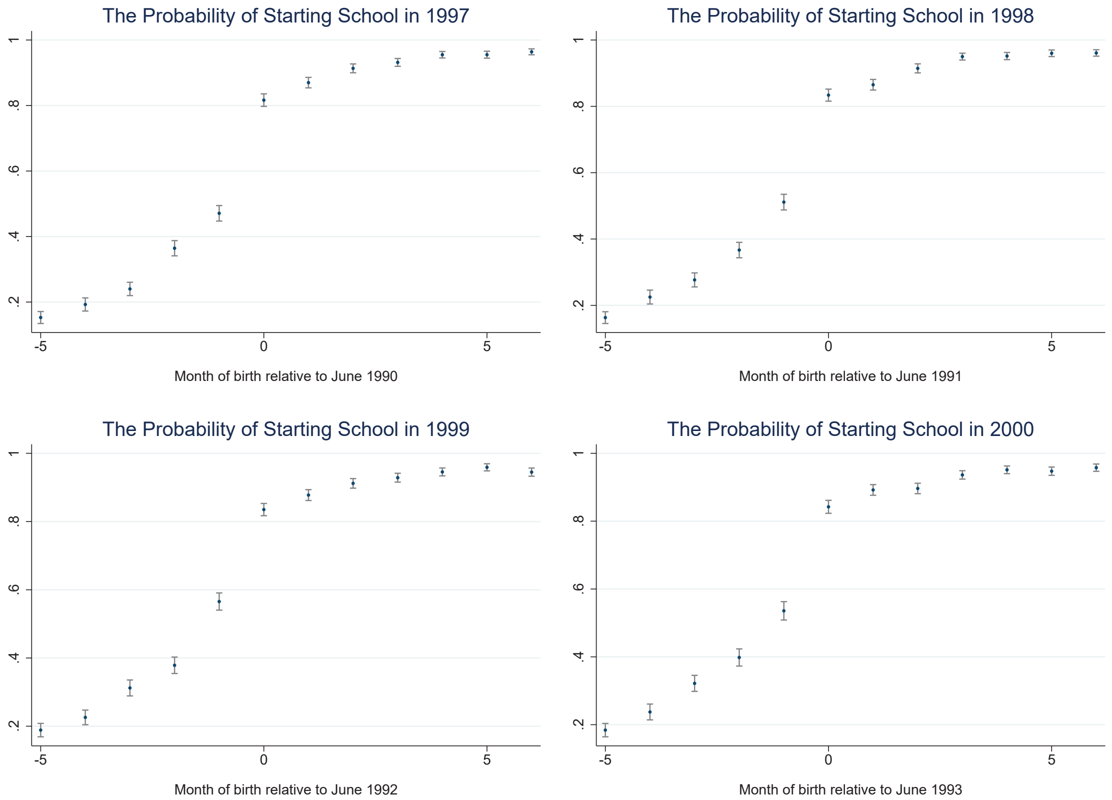
The average probability of starting school in 1997 for those born in 1990, in 1998 for those born in 1991, in 1999 for those born in 1992 and in 2000 for those born in 1993, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June. No. of individual observations: 30,784, 30,979, 30,410 and 29,894, respectively.

Figure 15: The Probability of Starting School in 1997-2000, Children of Mothers with a High School Degree



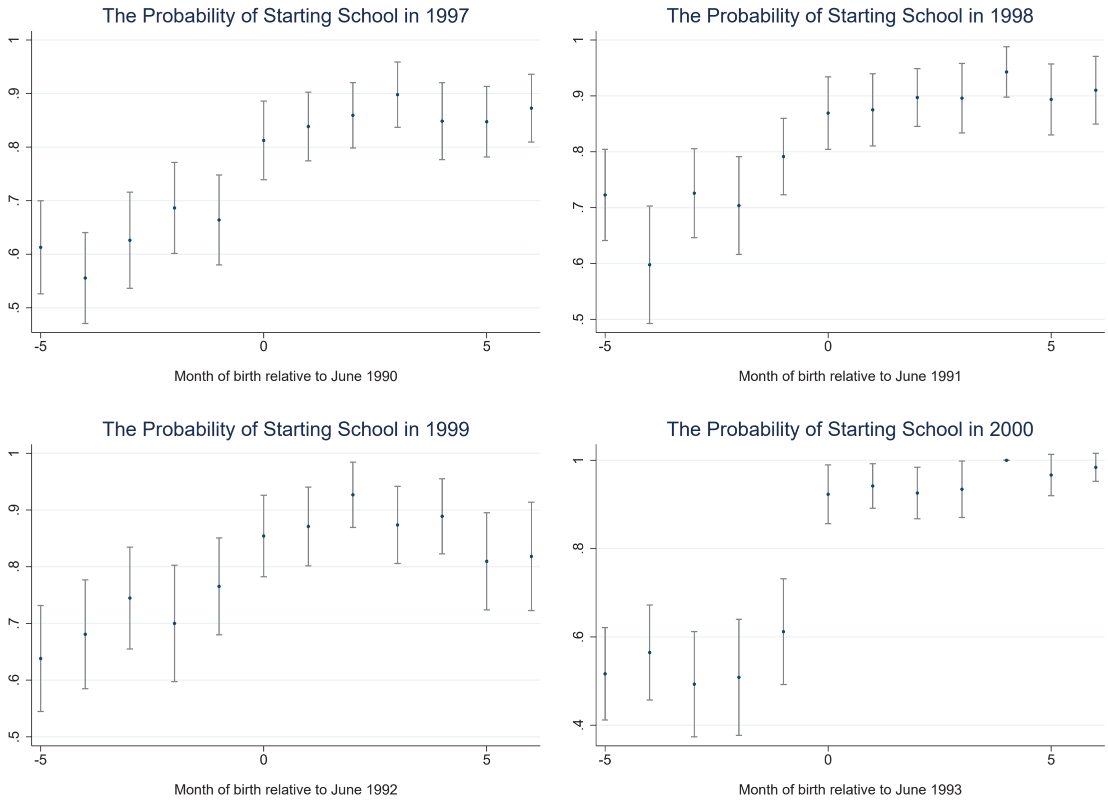
The average probability of starting school in 1997 for those born in 1990, in 1998 for those born in 1991, in 1999 for those born in 1992 and in 2000 for those born in 1993, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June. No. of individual observations: 35,993, 36,197, 34,557 and 32,843, respectively.

Figure 16: The Probability of Starting School in 1997-2000, Children of Mothers with a Tertiary Degree



The average probability of starting school in 1997 for those born in 1990, in 1998 for those born in 1991, in 1999 for those born in 1992 and in 2000 for those born in 1993, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June. No. of individual observations: 19,477, 19,603, 18,190 and 16,942, respectively.

Figure 17: The Probability of Starting School in 1997-2000, Information on Maternal Education is missing



The average probability of starting school in 1997 for those born in 1990, in 1998 for those born in 1991, in 1999 for those born in 1992 and in 2000 for those born in 1993, plotted with the 95% confidence intervals of the means. Data Source: own estimation from the 2001 Hungarian Census. 0 on the x-axis refers to being born in June. No. of individual observations: 1,412, 1,308, 1,077 and 858, respectively.

B Appendix

Table 29: The Definition of Outcome Variables Constructed from the 2011 Hungarian Census

Outcome variable	Definition	Unit of measurement
	Length of schooling	
Number of successfully completed years in the school system	The number of academic years one completed in each school types together. It does not include grade retentions, and unfinished or not incomplete (failed) school years. It is smaller or equal to the no. of years one spent in school, which is not measured by the data.	Academic years
	Highest degree obtained	
Any secondary school	Earned at least a secondary degree from any track.	binary variable
Vocational training school	Earned a vocational training school degree (but not a high school degree).	binary variable
Professional high school	Earned a high school degree (érettségi) in a professional high school.	binary variable
Academic high school	Earned a high school degree (*érettségi*) in an academic high school.	binary variable
	Secondary school track choice	
Any secondary school	Finished at least one academic year successfully in any secondary school above Grade 8.	binary variable
Vocational training school	Finished at least one academic year in a vocational training school.	binary variable
Professional high school	Finished at least one academic year in a professional high school.	binary variable
Academic high school	Finished at least one academic year in an academic high school above Grade 8.	binary variable
	Dropping out of secondary school	
Any secondary school	Finished at least one academic year in a secondary school but did not earn any secondary degree and was not in school at the time of the Census.	binary variable
Vocational training school	Finished at least one academic year in a vocational training school but have not earned any secondary degree and was not in school at the time of the 2011 Hungarian Census.	binary variable
Professional high school	Finished at least one academic year in a professional high school but did not earn any secondary degree and was not in school at the time of the 2011 Hungarian Census.	binary variable
Academic high school	Finished at least one academic year in an academic high school but did not earn any secondary degree and was not in school at the time of the 2011 Hungarian Census.	binary variable

Table 30: Education Outcomes of Students Born in 1990

	Total sample	Mother's highest education at giving birth				
		Primary	Vocational	High school	Tertiary	Missing
	Number of years completed successfully					
Primary, secondary and tertiary education	12.5	11.4	12.7	13.2	13.5	12.3
	Probability of starting secondary school					
Any secondary	0.907	0.796	0.959	0.986	0.993	0.869
Vocational training school	0.225	0.380	0.286	0.123	0.035	0.203
Professional high school	0.326	0.275	0.425	0.405	0.215	0.268
Academic high school	0.447	0.240	0.371	0.550	0.799	0.471
	Probability of earning a secondary degree					
Any secondary degree	0.837	0.671	0.898	0.953	0.979	0.789
Vocational training school	0.158	0.270	0.214	0.084	0.020	0.137
Professional high school degree	0.266	0.210	0.353	0.345	0.172	0.214
Academic high school degree	0.414	0.192	0.331	0.525	0.787	0.440
	Probability of dropping out of secondary school					
Any secondary	0.045	0.102	0.031	0.026	0.005	0.055
Vocational training school	0.115	0.149	0.06	0.055	0.471	0.150
Professional high school	0.027	0.054	0.019	0.013	0.007	0.037
Academic high school	0.016	0.046	0.015	0.008	0.002	0.017
No. of obs.	118,987	27,474	21,055	28,827	10,302	31,329

Data source: 2011 Hungarian Census.

C Appendix

Table 31: Effects on School Choice (ITT effects, 100-day bandwidth)

		Mother's highest education at giving birth				
Tracks	Total sample	Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of finishing at least the first year in a secondary school						
Any sec school	-0.002	0.003	-0.003	-0.005*	-0.000	0.000
	(0.003)	(0.007)	(0.005)	(0.002)	(0.001)	(0.007)
Corrected p-values	0.544	0.806	0.727	0.059	0.960	0.958
Vocational training school	-0.015*	-0.050***	-0.028	-0.009*	0.011***	0.005
	(0.005)	(0.006)	(0.018)	(0.003)	(0.002)	(0.009)
Corrected p-values	0.055	0.002	0.250	0.053	0.005	0.742
Professional high school	-0.008	0.003	-0.008	-0.031***	-0.051***	0.023
	(0.004)	(0.011)	(0.012)	(0.006)	(0.009)	(0.006)
Corrected p-values	0.156	0.859	0.634	0.006	0.006	0.024
Academic high school	0.021***	0.050***	0.038***	0.036***	0.022**	-0.023**
	(0.004)	(0.009)	(0.008)	(0.006)	(0.013)	(0.010)
Corrected p-values	0.005	0.006	0.007	0.005	0.218	0.119
No. of obs.	67,259	15,588	12,097	16,697	5,950	16,928

Local linear kernel regressions using 100-day bandwidth. Robust standard errors clustered by year-and-month-of-birth are in parentheses. P-values are corrected by the number of hypothesis tests (72) done together in Tables 31 - 33 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by CCT bandwidths are in Table 11 in Section 6.

Table 32: Effects on Dropout Rates (ITT effects, 100-day bandwidth)

Tracks	Total sample	Mother's highest education at giving birth				
		Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of dropping out of ...						
Any secondary school	0.009***	0.017*	0.005	0.004*	-0.002*	0.013
	(0.002)	(0.006)	(0.003)	(0.002)	(0.001)	(0.006)
	[61,314]	[12,532]	[11,574]	[16,405]	[5,904]	[14,899]
Corrected p-values	0.005	0.054	0.262	0.077	0.069	0.126
Vocational training school	0.029***	0.052***	-0.002	0.028*	0.061***	0.016
	(0.005)	(0.012)	(0.007)	(0.012)	(0.012)	(0.013)
	[14,319]	[5,841]	[3,210]	[1,841]	[189]	[3,238]
Corrected p-values	0.005	0.009	0.843	0.082	0.005	0.364
Professional high school	0.004	0.002	0.006	0.001	0.001	0.003
	(0.003)	(0.005)	(0.004)	(0.002)	(0.004)	(0.010)
	[23,387]	[4,578]	[5,480]	[7,011]	[1,333]	[4,985]
Corrected p-values	0.268	0.808	0.274	0.734	0.856	0.792
Academic high school	0.003	-0.005	0.012***	-0.001	-0.005	0.006
	(0.002)	(0.009)	(0.002)	(0.003)	(0.001)	(0.004)
	[30,878]	[3,732]	[4,585]	[9,460]	[11,081]	[8,289]
Corrected p-values	0.424	0.745	0.007	0.836	0.006	0.330

Local linear kernel regressions using 100-day bandwidth. Robust standard errors clustered by year-and-month-of-birth are in parentheses, number of observations are in brackets. P-values are corrected by the number of hypothesis tests (72) done together in Tables 31 - 33 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by CCT bandwidths are in Table 12 in Section 6.

Table 33: Effects on School Completion (ITT effects, 100-day bandwidth)

		Mother's highest education at giving birth				
	Total sample	Primary	Vocational	High school	Tertiary	Missing
Effect on the probability of gaining a secondary degree						
Any sec degree	-0.017***	-0.014	-0.022**	-0.014***	-0.007	-0.017**
	(0.0013)	(0.009)	(0.005)	(0.002)	(0.012)	(0.005)
Corrected p-values	0.008	0.252	0.013	0.003	0.705	0.026
Vocational training school degree	-0.019***	-0.050***	-0.029	-0.015***	0.004	0.002
	(0.003)	(0.003)	(0.014)	(0.003)	(0.002)	(0.005)
Corrected p-values	0.005	0.000	0.141	0.006	0.161	0.795
Professional high school degree	-0.015**	-0.017*	-0.020*	-0.028**	- 0.037***	0.010
	(0.004)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
Corrected p-values	0.028	0.087	0.059	0.012	0.009	0.407
Academic high school degree	0.018***	0.055***	0.028*	0.030**	0.025	-0.030*
	(0.003)	(0.005)	(0.010)	(0.007)	(0.018)	(0.010)
Corrected p-values	0.005	0.001	0.053	0.012	0.310	0.053
No. of obs.	67,259	15,588	12,097	16,697	5,950	16,928

Local linear kernel regressions using 100-day bandwidth. Robust standard errors clustered by year-and-month-of-birth are in parentheses. P-values are corrected by the number of hypothesis tests (72) done together in Tables 31 - 33 using the FDR multiple testing procedure by Benjamini and Hochberg (1995). A */**/*** indicates significance on 10%/5%/1% level after multiple testing correction. The same results estimated by CCT bandwidths are in Table 10 in Section 6.

D Appendix

The bandwidth optimization routine of Calonico, Cattaneo and Titiunik (2014)

The CCT bandwidth optimization procedure works as follows. Let's denote the p^{th}-order local polynomial reduced form estimator along a series of bandwidths h_n, as $\hat{\beta}_p(h_n)$. Then,

$$\hat{\beta}_p(h_n) = \hat{\beta}_{+,p}(h_n) - \hat{\beta}_{-,p}(h_n)$$

where $\hat{\beta}_{+,p}(h_n)$ and $\hat{\beta}_{-,p}(h_n)$ and denote the intercept at the cutoff of a weighted p^{th}-order local polynomial regression for treated (above-the-cutoff) and control (below-the-cutoff) observations only. More precisely, $\hat{\beta}_{+,p}(h_n)$ and $\hat{\beta}_{-,p}(h_n)$ are the solutions of the minimization problems of minimizing the following sum of squared errors at bandwidths h_n:

$$\hat{\beta}_{+,p}(h_n) = argmin \sum_{i=1}^{n} 1(x_i \geqslant 0) \{y_i - r_p(x_i)'\beta\}^2 K_{h_n}(x_i)$$

$$\hat{\beta}_{-,p}(h_n) = argmin \sum_{i=1}^{n} 1(x_i < 0) \{y_i - r_p(x_i)'\beta\}^2 K_{h_n}(x_i)$$

where

1() is an indicator function;
$r_p(x_i)$ is a p^{th}- degree polynomial function of the running variable;
$K_h(u) = K(u/h)/h$ with $K()$ being a Kernel function; and
$h_n$is a positive bandwidth sequence.

The $\hat{\beta}_p(h_n)$ local polynomial regression estimator offers a series of consistent estimators of the treatment effect at the cutoff. The estimator is consistent if in its MSE expansion as

$$MSE_p(h_n) = E\left[\left\{\hat{\beta}_p(h_n) - \beta\right\}^2 |R_n\right] \approx h_n^{2(p+1)} B_{n,p}^2 + \frac{1}{n * h_n} V_{n,p}$$

where $B_{n,p}$and $V_{n,p}$ represent the leading asymptotic bias and variance of $\hat{\beta}_p(h_n)$, $h_n \to 0$ and $n * h_n \to \infty$.