Meistararitgerð

í heilsuhagfræði

Education and Health: Effects of School Reforms on Birth Outcomes in Iceland

Kristín Helga Birgisdóttir

Tinna Laufey Ásgeirsdóttir

Hagfræðideild

Febrúar 2013

HÁSKÓLI ÍSLANDS

Education and Health: Effects of School Reforms on Birth Outcomes in Iceland

Kristín Helga Birgisdóttir

Lokaverkefni til MS -gráðu í hagfræði

Leiðbeinandi: Tinna Laufey Ásgeirsdóttir

Hagfræðideild

Félagsvísindasvið Háskóla Íslands

Febrúar 2013

Education and Health: The Effects of School Reforms on Birth Outcomes

Ritgerð þessi er 30 eininga lokaverkefni til MS prófs við Hagfræðideild, Félagsvísindasvið Háskóla Íslands.

Prentun: Háskólaprent

Reykjavík, 2013

Formáli

Verkefni þetta er meistararitgerð í Heilsuhagfræði og er til 30 ECTS eininga. Leiðbeinandi minn er Tinna Laufey Ásgeirsdóttir, doktor í hagfræði og lektor við Hagfræðideild Háskóladeild Háskóla Íslands. Vil ég þakka henni fyrir góða leiðsögn og gagnlegar leiðbeiningar.

Einnig vil ég þakka Örnu Varðardóttur, doktorsnema við Stockholm School of Economics, fyrir miklar og góðar leiðbeiningar og góð ráð við tölfræðivinnslu á verkefninu. Eiginmaður minn Sæmundur Karl Finnbogason fær sérstakar þakkir fyrir hvatningu, stuðning og hjálp við verkefnið, sem og foreldrar mínir, systir og Vala Jónsdóttir sem öll veittu mér aðstoð við gerð þess.

Útdráttur

Markmið: Að nota breytingar á gagnfræðaskólakerfi Íslands til að rannsaka afleiðingar menntunar á tíðni fyrirburafæðinga og lágrar fæðingarþyngdar. Breyting á lögum um grunnskóla árið 1974 hafði áhrif á uppbyggingu gagnfræðaskólakerfis og var að hluta til innleitt árið 1985 þegar lengd skólaskyldu var aukin um eitt ár. Báðar breytingarnar voru rannsakaðar.

Gögn og tölfræðivinnsla: Gögnin sem notuð voru eru úr Fæðingaskrá Íslands og innihalda allar fæðingar frá árinu 1982 til miðs árs 2012. Úrtakinu var skipt eftir því hvort mæður tilheyrðu þeim árgöngum sem skólabreytingarnar höfðu áhrif á eða ekki. Lagabreytingin sem gekk í gegn árið 1974 hafði first áhrif á nemendur sem fæddust árið 1959. Breytingin árið 1985 hafði á sama hátt áhrif á nemendur sem fæddust árið 1970.

Ósamfelld línuleg aðhvarfsgreining var notuð til að meta áhrif skólabreytinganna á háðu breyturnar fæðingarþyngd og meðgöngulengd. Einnig var logit aðferð notuð til að rannsaka áhrif á tvíkostabreyturnar léttburafæðing og fyrirburafæðing og mæður sem fæddar voru sitthvoru megin við ákveðinn tímapunkt voru bornar saman.

Útkomur: Útkomur úr ósamfelldu línulegu aðhvarfsgreiningunni sýndi að breytingarnar á skólakerfinu höfðu tölfræðilega marktæk áhrif á fæðingarútkomur. Breytingin 1974 hafði jákvæð áhrif á meðgöngulengd og breytingin 1985 hafði jákvæð áhrif á fæðingarþyngd og neikvæð áhrif á meðgöngulengd. Útkomurnar úr logit greiningunum voru samskonar, þannig að þær konur sem skólabreytingarnar höfðu áhrif á voru ólíklegri til að eignast léttbura og fyrirbura.

Niðurstöður: Breytingar á íslenska grunnskólakerfinu höfðu jákvæð áhrif á meðgöngulengd og fæðingarþyngd, með einni undantekningu. Þessar niðurstöður eru í samræmi við fyrri rannsóknir þar sem áhrif menntunnar á heilsu hafa verið rannsökuð með því að nota ýmsa heilsumælikvarða sem háðar breytur.

Abstract

Objectives: To study the effects of schooling on the incidence of preterm births and low birth weight using secondary school reforms in Iceland. A law change in 1974 immediately affected the structure of Iceland's secondary school system and was in part implemented in 1985 when one year was added to compulsory schooling. Both changes of the secondary-school system were studied.

Data and Methods: The data used is from the Icelandic Birth Registry with records of all births from the year 1982 to mid-year 2012. To compare the effects of the school reforms, the sample was split into birth cohorts of affected and unaffected women. For the 1974 implementation the change affected females born in 1959 and later. Similarly the 1985 implementation affected women born in and after 1970.

Linear regression discontinuity method was used with birth weight and gestational age as dependent variables. Furthermore, logit analyses were performed with binary variables for low birth weight and preterm births, and women born before and after the cutoff points were compared.

Results: The results from the linear regression discontinuity method showed that the school reforms had statistically significant causal effects on birth outcomes. The former school reform had a positive effect on gestational age and the latter school reform had a positive effect on birth weight. The logit analyses further strengthened the findings that the school reforms had an effect on health, where women affected by the school reforms were less likely to have low birth weight babies and give premature births.

Conclusion: School reforms in Iceland had positive effects on gestational age and birth outcomes with one exception. These results harmonize with former research on the subject where the effects of education on health have been studied using various health measurements as dependent variables.

Table of Contents

List of Tables and Figures

1 Introduction

Reykjavík's Mayor Jón Gnarr recently expressed his view that compulsory schooling should be abolished ("Jón Gnarr", 2011), sparking a discussion started on the blog sphere where Margrét Pála Ólafsdóttir, a prime mover in the educational and pedagogic field in Iceland, also expressed her opinion in support of the Mayor's („Margrét Pála Ólafsdóttir", 2011). Similar views have been expressed in other countries, e.g. on the U.K.'s Libertarian Alliance's website (O'Keeffe, 2004), where the author offers philosophical and historical arguments for his case. Policy changes, such as the school reforms in Iceland that are the subject of this study, provide natural experiments that are well suited for economic analysis, especially where causal relationships are to be studied. To abolish or change the compulsory schooling law is a major policy decision that may be guided by empirical research on possible consequences. The regression discontinuity (RD) design, which is used in this study to assess the effects of school reforms in Iceland on birth outcomes, has been used in previous publications with similar structures and research questions as ours. This method is argued to be better suited to estimate the causal nature of the relationship than previous methods used, such as ordinary least squares (OLS) or instrumental variables (IV) (Lee & Lemieux, 2009).

Low birth weight (LBW) is a health indicator used both for populations and on an individual basis. On an aggregate level, the proportion of low birth weight babies is used as a sign of comprehensive public health problems, and on an individual level a predictor of newborn mortality, morbidity and future health. LBW is defined as less than 2500 grams irrespective of gestational age (World Health Organization, 2006). Preterm birth (PTB) is defined as childbirth occurring at less than 37 weeks of gestation, and like LBW it can affect both neonatal and future health of the child. The incidence of PTB has been on the rise worldwide in the last few decades and some of the possible factors that could explain that trend are increasing rates of multiple births, more use of various reproduction treatments and the rising age of parents (Beck, et al., 2010). Determinants of LBW and PTB are various and most of them are linked to parents' overall health and health behaviors, both before and during pregnancy. These include: racial origin, parental height and weight, history of prior LBW infants, gestational weight gain and caloric intake, general illness, cigarette smoking and alcohol consumption. In developed countries, the most important single factor is cigarette smoking, followed by poor gestational nutrition and low pre-pregnancy weight (Kramer, 1987). Education may affect birth weight and prematurity through the non-genetic factors listed above, for instance by improving a mother's probability and productivity of health investment, improving the financial resources available through the labor market and marital outcomes and other family planning decisions (Chevalier & O'Sullivan, 2007).

The causal relationship between education and health has been studied using various statistical methods, such as with instrumental variables (e.g. Chevalier & O'Sullivan, 2007 and Currie & Moretti, 2003) and more recently with regression discontinuity design (e.g. Lleras-Muney 2005, Oreopoulos 2006, Silles 2009 and Clark & Royer 2010). Clark and Royer's results (2010) conflicted with Oreopoulos's (2006) when studying the effects of the same British school reform on various health criteria, where Oreopoulos found a statistically significant positive effect of the school reform on health but Clark and Royer did not. However, the studies are few and robustness of results across data, context and outcome measures is still needed. Here, such is provided and added to the literature. The causal effects of school reforms on health have neither been examined in Iceland before with a regression discontinuity design, nor with birth outcomes as a health measurement. Various elements are important in birth outcomes' determination, mothers' education being one of them, which makes them interesting as a health measurement for this study.

In Iceland, data on birth outcomes is accessible electronically from 1982 through the Icelandic Birth Registry. This includes information on standard variables such as date of mother and child's birth, birth weight and length, gestational length, information on diseases and some although limited background variables on the mother. In the time span available the incidence of LBW in Iceland has been rather steady each year around 3,50-4,00%, the lowest incidence of 3,03% in 1989 and highest in 1997 of 4,86% with the average incidence over the whole time span 3,78% per year.

In 1974 major changes were made to schooling laws in Iceland, including a change in compulsory schooling age which changed from 7-15 years to 7-16 years, as well as a restructuring of the secondary-school system. To accommodate the change, the secondary-school level was immediately set to three years (previously two to four years depending on student preference) with the completion of a standardized test. In 1985 the reform concerning compulsory schooling age was implemented, so students turning 16 years old in

1986 (born in 1970) were affected, as separation into classes goes by calendar year in Iceland. (Guttormsson, 2008). For this study the incidence of preterm births and low birth weight are studied for women exposed to the new school system in the 1970's and 80's, as compared to those not exposed.

The difference in PTB and LWB is studied for two cut-off points in the data; firstly for the schooling reforms in 1974, and secondly for the raise of compulsory schooling age that took place in 1985. The data was collected for all births in the years 1982 through spring 2012. The mothers studied were born in the years just before and just after the changes of the school system. Using the RD design allows us to study the causal relationship between the schooling reforms and birth outcomes by studying whether there is a difference between the cohorts before and after the changes. We hypothesize that there are positive effects of the school reforms on birth outcomes. We expect, however, that the data will show only small effects, perhaps not statistically significant. The main reason being that many factors affect LBW and PTB and the most important ones, parental health and health behaviors, are determined by various elements, education being only one influencing factor. We expect to see larger effects for the 1974 change than the one in 1985, as it was a more drastic change to the whole secondary school system and probably affected the education of a larger group of students. However, more data is available for mothers born around the later reform and therefore those results may be statistically stronger. Contrary to our hypothesis regarding the size of the effects of the school reforms, the results showed larger effects on birth weight from the latter reform than the former. The direction of the relationship was as hypothesized, i.e. positive effect of the reforms on the health variables. For the latter change, the sign of the unadjusted relationship was negative for gestational age as dependent variable, but when meaningful control variables were included the relationship became positive.

This research has been granted permission from The Icelandic National Bioethics Committee (VSNb2012010011/03.7), The Data Protection Authority (2012010010ÞS/--) and the Directorate of Health (2010050296/5.6.1/HBS/hbs).

2 Background

Below is a section on the main changes of the Icelandic educational system relevant to this study as well as a section containing background on related research on the subject.

2.1 Education in Iceland

School attendance in Iceland for students at junior college level age (traditionally 16-20 years old) has in the last few years been steadily increasing. Up until 1998, when it was changed to 18 years, the majority age in Iceland was 16 years (Law on Legal Majority no. 71/1997). School attendance for 16 year olds has been at or above 90% for a number of years, but attendance for 18 year olds has risen in the last few years, from around 68% in 1999 to 82% in 2010 (Statistics Iceland, 2011). The educational level of the nation, and especially women's, is potentially an important factor for the results, since there are relatively few people that do not continue their studies beyond what is compulsory. However, if changes in compulsory schooling are not binding, then assumptions on their causality are not warranted. Unfortunately, data on individuals' educational attainment in Iceland is not available and hence the reduced form regression will be used, as discussed in Chapter 4 here below.

The Icelandic school system has continuously evolved since around 1900, when Iceland was still under Danish rule. Four significant school reform laws have been set since then, with the government's role in education increasing incident to the government's overall role in Icelandic society. The first one was in 1907 when schooling was first made compulsory and the compulsory schooling age was 10-14 years old. Students were expected to be home schooled before that age and were supposed to be able to read and write when formal school attendance started. The second major reform was in 1946 when the so-called Educational law was passed. The objective of this law was to organize the clutter that dominated the Icelandic schooling system and make it more efficient. Primary, secondary and regional schools all over the country set their own rules and did not consider each other. Thus, children were very unequally prepared for college. To level the playing field, standardized tests were administered by secondary schools to graduating students, which served in effect as entrance exams for junior college (formal entrance exams in colleges were discontinued). In addition, these reforms changed the compulsory schooling age to 7-

15 years old. The structure of secondary schools was fairly complex in those years, where there were three programs available for students, and each one had a different completion time and exam, which gave various rights for future education and profession (Jóhannesson, 1984). The third major school reform, which is the subject of this study, was in 1974 and implemented in two stages, as described in Chapter 1 and in more detail here below. The fourth major school reform was in 1990 when compulsory schooling age was set to 6-16 years (Law on Primary School no. 52/1990). The reason that the 1974 and 1985 changes were chosen but not other ones that were of equal importance to Iceland's society, is that no database is available on electronic format for births prior to 1982. Furthermore, the last school reform in 1990 is too close in time to include in this study.

Children born in 1959 were in the first class of Icelanders who experienced the different secondary-level school system implemented in the school year starting in autumn 1974. After the change, compulsory schooling was completed three years into secondary school, but was previously completed two years into it. It was a natural change to make to the system, since the completion of the secondary school level and the compulsory school age coincided. Those students who would have taken the fourth year in the school year 1974-1975 (16 years of age in 1975) were affected by these changes, since that class was discontinued. If those students wanted to stay in school they had to apply to junior college. When students had already started secondary school they were inclined to finish their studies, even though that exceeded their compulsory schooling age. Therefore, it is possible that the change in the structure of the secondary school had an effect on those who did not intend to go to junior college, but had intended to finish the last year of secondary school (Guttormsson, 2008).

The school reforms in Iceland which are used in this study provide a natural experiment setting for evaluation on their causal effects on health, which has been the preferred research setting in health economics and other social sciences fields for a long time. The use of natural experiments to estimate causal effects has however been criticized by Cutler and Lleras-Muney (2006). A chapter on their main arguments can be found in Appendix 1.

2.2 Economic research

Kramer´s (1987) meta-analysis of medical literature for The World Health Organization (WHO) on the determinants of LBW reviewed a total of 895 publications and 43 potential determinants were identified. The report lists maternal education as one of the factors that have an indirect causal influence on intrauterine growth and the organization recommends policies aimed at improving maternal education since that may improve nutrition and reduce cigarette smoking and other harmful practices during pregnancy. This is considered particularly important in developing countries, but also in developed countries. In the time since this report was published economists have increasingly focused on the correlation and causal effects of socioeconomic factors on health.

In Grossman´s (1972) influential model of health as human capital, the production of health relies on inputs and also on the personal efficiency of each individual. Efficiency in health production is defined in this context as the amount of health an individual can obtain from a given amount of health inputs. An efficient individual would gain greater health from the same amount of health inputs than an inefficient individual. Length of formal schooling could play a large role in this respect, i.e. education can make people more efficient health producers through their cognitive skills.

In his survey of prior literature on the subject Grossman (2006) focuses on causal effects of education and on the mechanisms via which they operate. As mentioned above, education might have a direct effect on health and health behaviors via its influence on productive and allocative efficiency. Indirect effects might include the effect of education on labor-market opportunities (higher employment and earnings) which in turn could influence health by increasing the affordability of health-improving goods and access to health care. The education-health relationship might also be affected by education through social and peer effects. It is unlikely that income and health care completely explain the connection between health and education, as Cutler and Lleras-Muney (2006) deduct from the fact that there are education gradients in various health behaviors for which neither income nor access to health care are important factors. Fuchs (2004) points out how many socio-economic variables are correlated with health in one context or another. There is undoubtedly correlation between income and education and some argue that education is serving as a proxy for long-term income while others counter that education's favorable effect on health works, in part, through higher income. An alternative explanation is that time preference influences whether people stay in school and participate in behavior that contribute to better health. He points out that there are difficulties in deriving statistical conclusions and drawing reliable policy applications from these correlations.

The research that is closest in subject matter (although not in methodology) to the one carried out here is by Currie and Moretti (2003). They used Vital Statistics Natality data from 1970 to 1999 to examine the effect of maternal education on birth outcomes. The data covers virtually all births in the United States. The authors also assess the importance of four channels through which maternal education may improve birth outcomes; the effect of education on the use of prenatal care, education of spouse, health behaviors (in particular smoking) and lastly fertility. The availability of colleges in the woman's county in her seventeenth year is used as an instrument for maternal education. The data offers information on where women lived when they had their babies, but not when they were aged seventeen, and therefore two types of endogenous mobility are potentially problematic for the IV strategy chosen. First, women may move to counties where there are college openings to attend college. Second, women may move to counties with college openings after they themselves graduate from college because of better job opportunities that are associated with the college opening. The authors cannot rule out the possibility of the first type of mobility, but they offer evidence that indicates that the second type does not appear to be very significant. The authors find that higher maternal education improves infant health, as measured by birth weight and gestational age (OLS estimates suggest that an additional year of maternal education reduces the probability of low birth weight by 0,5 percentage points and lowers the risk of prematurity by 0,44 percentage points relative to the means). The authors also conclude that education improves infant health through a number of different pathways, for example it increases the probability that a new mother is married, reduces smoking and increases use of prenatal care. Much of the positive impact of higher education on birth outcomes may be caused by reduction in smoking, since an additional year of education is found to reduce the probability of smoking by more than 30%. The study shows that, on average, maternal education levels were higher in the ten years following the opening than they were in the preceding decade. Furthermore there was no particular trend in the county prior to the opening of a new college. Both OLS and IV results show the same results and are statistically significant.

Other research has identified a positive relationship between the education level of a close relative and one's own health and health behavior. Cutler and Lleras-Muney (2006) list a few publications where, for example, a mother's education affects her infant's and baby's health, a child's education level can affect parents' health behavior, and a spouse's education can affect the other's health. It is thus clear that the relationship between education and health is complex.

Lleras-Muney (2005) was the first to use a natural experiment to study whether education had a causal effect on health, measured as mortality. She used a quasi-natural experiment, where at least 30 states in the U.S. changed their compulsory schooling laws and child labor laws between 1915 and 1939. The data used are the 1960-1970 and 1980 censuses of the U.S. (a 1% random sample of the population), and those who were 14 years old between 1915 and 1939 were selected. The cohorts are matched to the compulsory attendance and child labor laws that were in place in their states. Several IV estimations of the effect of education on mortality are used and the results provide evidence of a causal effect. Those states that changed their laws saw lowered mortality rates, but because of limitations of the data the estimates are not statistically significant. RD approach is used to assess the direct effects of changes in compulsory schooling on mortality, where 7 cohorts based on year of birth are used per legislative change (3 years before, 3 after and the cohort of the change). Individual deaths are not observed but rather group deaths rates. Using a reduced form equation with the RD design, the author finds that the direct effects of the laws on mortality is 0,3%.

The school reforms in 1947 and 1974 in the UK have been used in many studies with similar objectives as ours, which is to study the relationship between education and health using a RD design. Oreopoulos (2006) compares educational attainment and adult earnings for students just prior and just after the policy change combining U.K. General Household Surveys from 1983 to 1998 with Northern Ireland Continuous Household Surveys from 1985 to 1998. The effects from compulsory schooling on earnings are between 10 to 14%, depending on the region of the UK. The author also identifies significant gains to health, employment and other labor-market outcomes.

Complementing the analysis of Currie and Moretti (2003), Chevalier and O'Sullivan (2007) also use British data to study the effects of the 1974 school reform. Their analysis is based on data from the 1958 National Child Development Study, which is a longitudinal study of babies born in Great Britain between March 3rd and 9th 1958. The causal effects of maternal education on birth weight is tested, using the 1947 natural experiment where the minimum school leaving age was increased from 14 to 15. The effects on birth weight are found to be positive, with children from lower social class gaining the most. The authors use OLS and also IV estimates in a reduced form model, including only dummies for the mother's birth cohort, mother's social class, residence, maternal education, child's gender and parity. For each model in their study, the instruments are found to be valid and the test of over-identification is always rejected. Whereas Currie and Moretti's identifying strategy is based on an educational reform that affected individuals at the higher end of the schooling distribution, this study focuses on effects at a low level of education. The authors point out that the differences between the studies could indicate diminishing returns to education since their results point to larger effects than Currie and Moretti's. The authors conclude that the effect of maternal education on birth weight is mostly direct. Robustness checks support the validity of the identification strategy.

Silles (2009) performs a similar study of the effect of schooling on self-reported health, again using the compulsory schooling law reforms in Britain as cut-off dates. The data used is from the General Household Survey (GHS) for England, Scotland and Wales. The key explanatory variable is "years of schooling" and the sample includes 200.552 individuals between 25 and 60 years of age obtained from pooling repeated cross sections between 1980 and 2003/2004. Four different questions in the GHS offer general information on responders' assessment of their own health. Linear probability models are used, as well as LOGIT models which produce similar results. OLS estimates reflect statistically significant positive correlation between health status and years of schooling, with additional year of education improving the chances of being in good health by 2,6 percentage points. The two-stage-least-squares estimates are larger than the OLS and according to the results increasing education by 1 year increases the probability of being in good health by 4,5 percentage points. Exogeneity of schooling with respect to health outcomes is strongly rejected. RD design is used to examine the robustness of these results and yield similar results. In interpreting the results the author points out that it is important to take into account income, as it is likely to somewhat overplay the direct effect of education on health.

A few years later, Clark and Royer (2010) add to the literature on the effects of the British school reform on health outcomes, in particular on mortality, self-reported health, weight and blood pressure, as well as on health behaviors such as smoking, drinking and exercise. Mortality data from the Office for National Statistics is used to estimate the mortality effects and Health Survey of England data is used as a main source for the health data. Standard RD framework is used to estimate the effects of compulsory law changes on educational attainment. The outcomes of this study are in contrast with both the findings of Silles (2009) and Oreopoulos (2006). The reform in 1947 had no significant impact on mortality and the one in 1974 only had small effects on mortality. Both reforms had at best, small impacts on a wide range of health outcomes and health behaviors. The authors suggest that these types of education policies may not be effective to achieve health goals.

3 Data

The data used is from the Icelandic Birth Registry, which records all births in Iceland and is held by The Directorate of Health. Records have been held from the year of 1972 but the earliest data in electronic format is from 1982. Documented data on 127.254 individual births from 1982 to 2012 are used for this study. A few observations in the registry have coding mistakes that can be obviously corrected. It was thus considered acceptable to recode them instead of marking them as missing values or not using them in the regression. A list of those variables can be accessed upon request. Otherwise this registry data includes few missing values.

The data includes background and health related variables about the mothers and pregnancies, babies' length and weight and infant diseases. One of the main background variables is a mother's birth year and in the dataset births are recorded for mothers born in the years 1937-1999. The variables which will be used as dependent variables in the regression are birth weight and gestational age. Birth weight is recorded in kilograms and length of gestational in weeks. In the dataset gestational length is given in the format weeks + days, but for ease of statistical computation it was converted to non-integer format (weeks + days/7). Two ways are used to measure gestational age; firstly with an ultrasound and secondly with the mother's menstrual cycle. The ultrasound is considered a more accurate way of measurement, but it wasn't implemented until 1988 and hence the dataset contains both measurements for some births, but only one for others. Gestational age using ultrasound will be the primary variable used in the analyses, but where it is not available the mother's menstrual cycle will be used. A binary variable was created to indicate which measurement form was used (0 for ultrasound and 1 for menstrual cycle) and was controlled for in regressions. In unreported results a positive statistically significant relationship was found between the binary variable and gestational age, meaning that the use of menstrual cycle as a measure of gestational age most likely overstated the reported gestational age.

Table 1 contains the summary statistics for single live births, which is divided into four groups of birth cohorts, representing the eight cohorts on each side of the cutoff points in the regressions. The mean birth weight for all births in the dataset is 3,63 kg and for all live births (all births excluding fetal deaths and stillborn) the mean births weight is 3,64 kg. As can be seen in Table 1 the mean birth weight for the birth cohorts used in this study is quite comparable to those figures. The number of fetal deaths and stillborn in the dataset is 495 and 183 respectively. The frequency of LBW and PTB in the dataset is 3,77% and 4,90% respectively. For the time span of the dataset the incidence of LBW in Iceland has been rather steady each year around 3,5-4%, the lowest incidence of 3,03% in 1989 and highest in 1997 of 4,86% with the average incidence over the whole time span 3,78% per year. The incidence of PTB has been on the rise however in this time period, with the lowest incidence of 2,71% in 1984 and the highest of 6,84% in 2006. Mothers' mean age has been rising in Iceland, both for first time mothers and for all births (sorted by birth year of child). For the time span of the dataset the mean age of mothers has risen steadily from 25,6 years to 29,5 and for first time mothers from 21,8 years to 26,0. As can be seen in the summary statistics the average age of mothers for the first eight birth cohorts studied is considerably higher than the later cohorts. This is due to the time span of available data and its implications are discussed further in Chapters 4 and 7.

Table 1 – Summary statistics for single live births

	Mean	Std.Dev	Obs.		Mean	Std.Dev	Obs.
1974 reform	Birth cohorts: 1951-1958				Birth cohorts: 1959-1966		
Birth weight (kg)	3,664	0,551	16990		3,663	0,558	35575
Gestational age (weeks)	39,980	1,720	16990		39,943	1,771	35575
Number of previous births	1,665	1,096	16990		1,100	1,035	35575
Mother's birth year	1955,341	2,162	16990		1962,693	2,248	35575
Mother's age	31,546	4,339	16990		28,016	5,591	35575
Dummy for LBW	0,023	0,149	16990		0,024	0,152	35575
Dummy for PTB	0,027	0,163	16990		0,030	0,170	35575
1985 reform	Birth cohorts: 1962-1969				Birth cohorts: 1970-1977		
Birth weight (kg)	3,667	0,562	36397		3,703	0,556	31389
Gestational age (weeks)	39,915	1,807	35397		39,930	1,839	31389
Number of previous births	0,988	1,000	35397		0,871	0,926	31389
Mother's birth year	1965,397	2,252	35397		1973,342	2,235	31389
Mother's age	27,702	5,832	35397		27,678	5,356	31389
Dummy for LBW	0,025	0,157	35397		0,022	0,147	31387
Dummy for PTB	0,033	0,178	36397		0,038	0,191	31389

The biggest limitation of the data is its short time span. For the implementation of the school reform that took place in 1985 the mothers were born in 1970 and those women are still at child bearing age. Similarly, the first part of the reform affected mothers born in 1959 and surely part of those women already started having children before 1982, which is our first available data. Another limitation of the data is that some important background variables have been rendered useless for statistical purposes because of how they were registered in the database. The variable “marital status” was not listed in the database until 2006 and cannot be used for statistical purposes for births occurring prior to that time. Another variable that will not be used is “mother’s employment” which contains no classification of employment status or industry but rather details of the women’s place of work and/or profession without any specific categorization.

Variables relating to illnesses and other diagnoses refer to the relevant ICD-10 codes. A binary variable indicating whether or not the newborn was diagnosed with any malformations was created; 0 for none and 1 for some. Similarly a binary variable indicating a mother’s diagnoses for chronic diseases was created. In unreported results the addition of these variables to the regression functions showed a negative relationship between them and the dependent variables as expected.

For this study STATA software was used for statistical analysis of the data.

4 Statistical framework

A RD design is used to estimate the treatment effect when the treatment is determined by whether the observed assignment variable exceeds a known cutoff point. In this study the treatment was the school reform (either in 1974 or 1985), the assignment variable was mother‘s year of birth and the dependent variable was birth outcome. The main idea behind the RD design is that individuals just above and just below the cutoff point are good comparisons. A more detailed description of the RD design and its use can be found in Appendix 2.

Following the strategy of previous literature (Clark & Royer, 2010) the following basic linear regression function was used for each of the school reforms:

$$H_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + \beta_3 (D_i * X_i) + \beta_4 f(R_i) + u_i$$

Equation 1

With H_i as the health outcome for observation i (either birth weight or gestational age), β_0 a constant, D_i a dummy variable for the assignment variable (whether a mother was a part of the affected birth cohorts or not), X_i a mother's birth year and $f(R_i)$ a function of health related variables. The term u_i is the error term representing the unobservable factors affecting the health outcome. The parameter of ultimate interest here was β_1, which is the effect of the school reform on birth outcome. The other important parameter that was analyzed was β_3, which represents whether the slope of the regression line is different before and after the cutoff. These two parameters represent the total effect of the school reforms on the dependent variables. The cutoff points for the two school reforms are the years 1959 and 1970.

In assessing the data, it was split into a few bins, both above and below the cutoff point. By using an interaction term between D and X in the regression function the incline was not constrained to be the same on either side of the cutoff, which is in the spirit of the RD design. The size of the bins can be varied to see what fits best for the data available. Lee and Lemieux (2009, p. 33) offer some advice as to how best to decide on the size of bins. Too narrow bins will lead to highly imprecise estimates, but too wide bins may lead to biased estimates if the slope of the regression line is not accounted for. A comparison of both sides of the cutoff point will be less credible if the bins are too wide since they are no longer just to the left and right of the cutoff point. Practically, when deciding the size of bins, one wants to make the graph of the relationship look informative. For the Icelandic data the most apparent way was to have one birth year (mothers') in each bin and eight bins on each side of the cutoff point was decided to be the appropriate number for the graph to look informative and clear. Another reason for choosing that number was that it seemed a short enough period to follow the spirit of the RD design but long enough for a regression to have enough number of observations for statistical strength. Figures 1 and 2 show the graphs for the mean birth weights of each birth cohort of mothers and similarly Figures 3 and 4 show the graphs for mean gestational age. As can be seen by reviewing the figures the treatment effect can be expected to be larger for the former reform than the latter one, and the effect of the 1985 reform on gestational age can be expected to be very small. Cutoff points are drawn vertically at the cutoff dates.

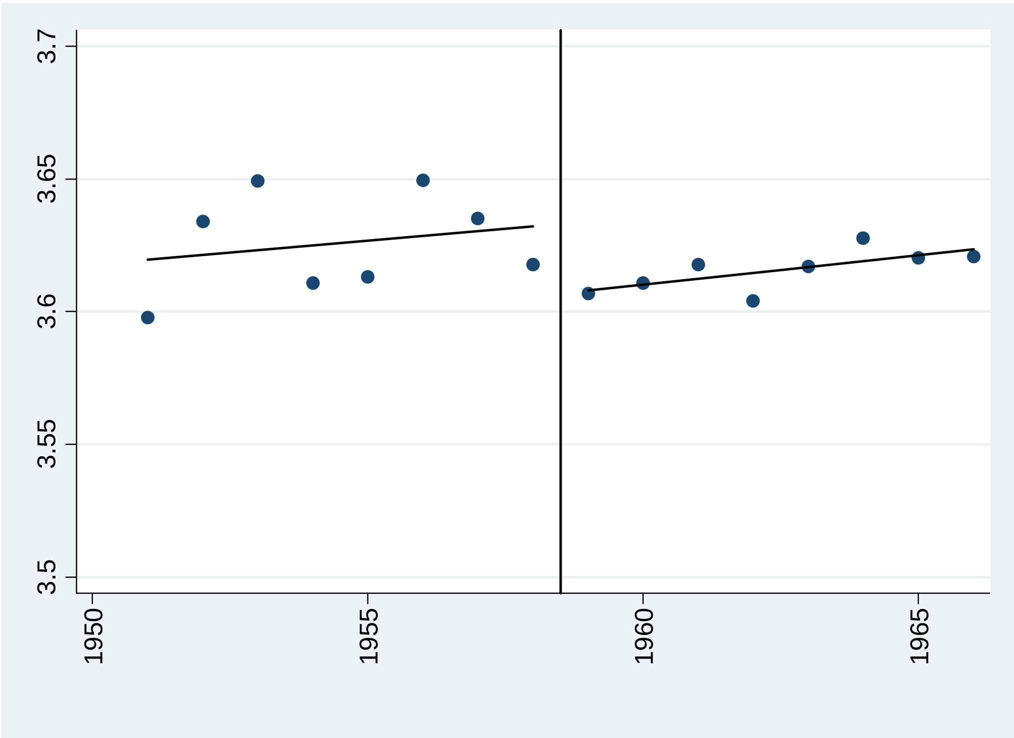

Figure 1 - Mean birth weight in kilograms (vertical axis) per birth cohort of mothers (horizontal axis). Cutoff point 1959.

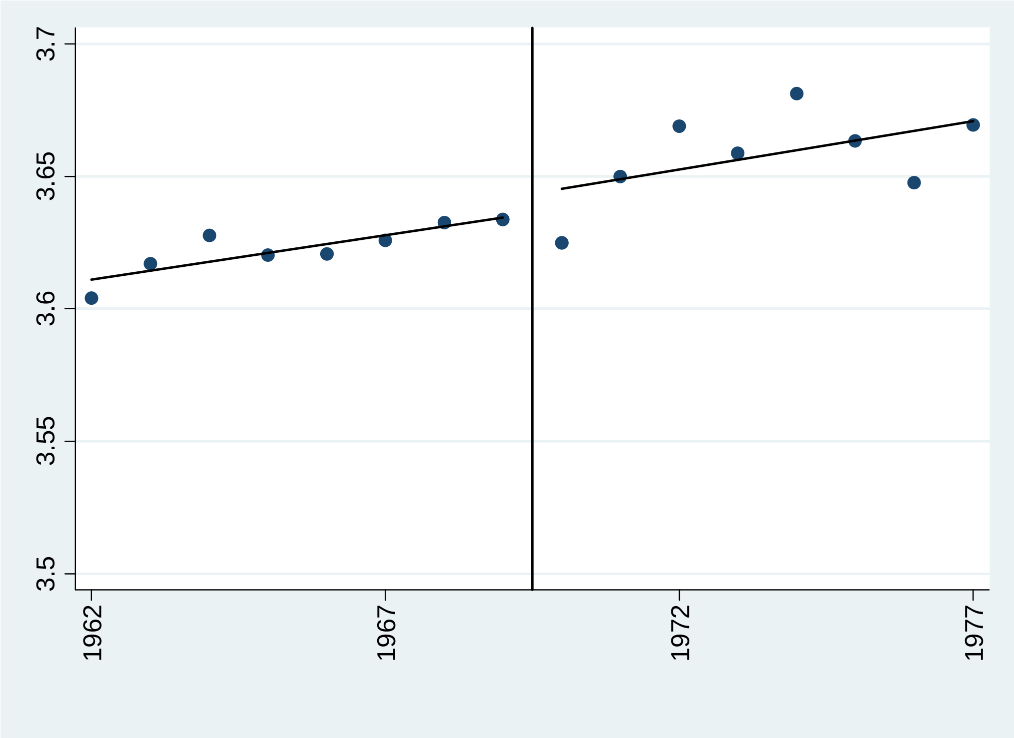

Figure 2 – Mean birth weight in kilograms (vertical axis) per birth cohort of mothers (horizontal axis). Cutoff point 1970.

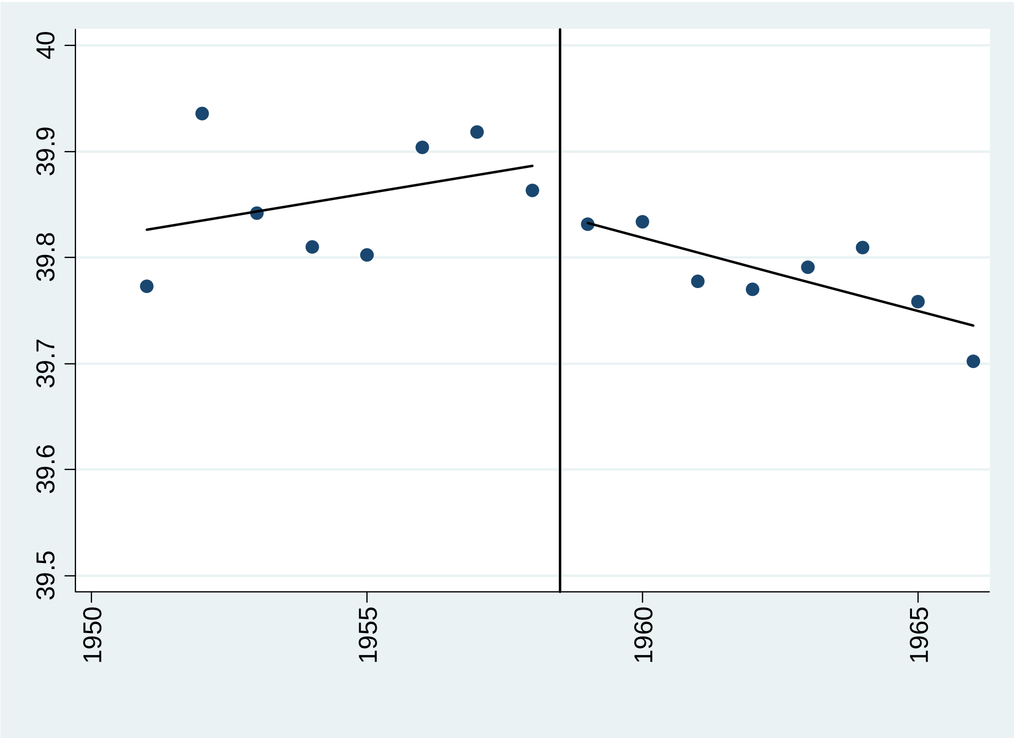

Figure 3 – Mean gestational age in weeks (vertical axis) per birth cohort of mothers (horizontal axis). Cutoff point 1959.

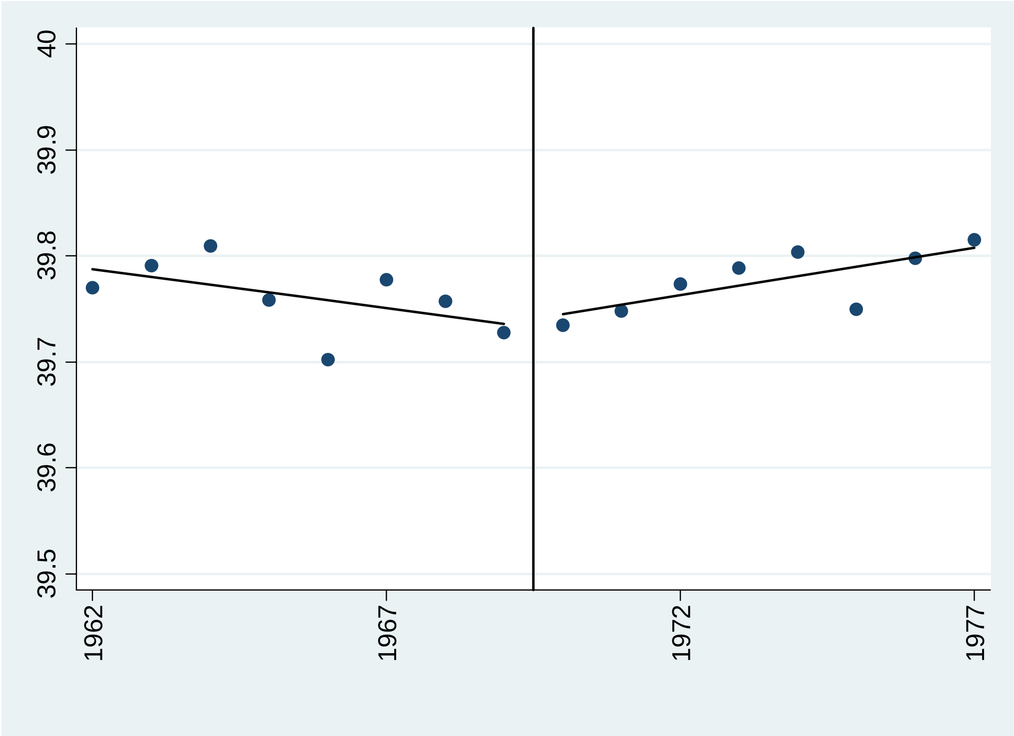

Figure 4 – Mean gestational age in weeks (vertical axis) per birth cohort of mothers (horizontal axis). Cutoff point 1970.

For each dependent variable separate regressions were run for both school reforms. The analyses were restricted to include only single live births. Here, the World Health Organization's definition of live births is used, i.e. it excludes fetuses that died in the womb or during birth (World Health Organization, 2012). The dataset contained a total of 15 and 495 fetal deaths and stillbirths respectively. Standard F-tests and t-tests of the parameters were performed. Additionally, variance inflation factors were computed to detect collinearity of the regressors. High collinearity was only found between the interaction term and the two variables it consisted of, which was expected in an analysis of this kind.

The base model's function $f(R_i)$ includes the independent variables: number of a mother's previous births, mother's age, gestational age (added when dependent variable is birth weight), and a binary variable indicating which measurement method was used for it. Additionally, in unreported results binary variables for malformations at birth and mother's chronic diseases were added to the analyses as possible mediators and both showed negative and statistically significant relationship (p<0,1) to both dependent variables.

As a second step a logit model was used with the dependent binary variables indicating whether: a) a newborn's birth weight placed it in the LBW category or not, or b) whether the gestational length was categorized as PTB or not. Otherwise, the regression equation was the same as Equation 1 and the list of independent variables the same as summarized above (with gestational age added to the list when dependent variable is LBW). To compare the two groups of women who were born before and after the cutoff dates, marginal effects were calculated, revealing the difference in the probability of a particular health outcome (here low births weight or preterm birth) by comparing otherwise average women, with one born before the cutoff and the other after the cutoff. Here, the term average refers to that all other independent variables are kept at their mean values. Marginal effects of the interaction term are not reported, since the value of the interaction term can't change independently of the values of the component terms. In unreported results, qualitative robustness was checked by running the logit analyses with and without the interaction term, and those yielded the same results for the treatment effects. Standard t-tests of the parameters were performed for the marginal effects.

A crucial assumption of the RD design is, as reported in more detail in Appendix 2, that individuals cannot influence the assignment variable. Although sorting is not theoretically possible here we still test this assumption and to demonstrate that treatment and control groups (below and above the threshold) are similar in their observed baseline covariates a simple examination of the density of the assignment variable is performed. A jump in the density at the threshold is probably the most direct evidence of some degree of sorting around the threshold (Lee & Lemieux, 2009). No visible jump was found at the thresholds for either cutoff point, although admittedly data on mothers born around the former cutoff point bears marks of missing data. Those women are among the first ones in the dataset and it is certain that a large group already had given birth before 1982 which is when data on births were first recorded electronically. This is controlled for in the study by including mothers' age in the list of independent variables and is not considered to be a problem in this study, as explained further in Chapter 7 where internal validity concerns are addressed.

5 Results

The results are reported in two subsections. These correspond to the two analysis methods used to assess the relationship between the school reforms and the health indicators, i.e. the linear RD method and the logit analysis. For both methods, results are given for each school reform and each dependent variable. Tables 2 and 3 show the results as well as the sensitivity analysis.

5.1 Linear RD design results

For the first school reform, a restricted sample of women born in the years 1951 to 1966 (n=52565) showed a statistically insignificant causal effect (p>0,7) of the 1974 school reform on birth weight. For the 1985 reform the sample was restricted to women born in 1962 to 1977 (n=67784). The treatment effect was positive and statistically significant (p<0,05) and the interaction parameter was negative and statistically significant (p<0,05). This indicates a causal effect of the latter school reform on birth weight where women who were affected by the reform had heavier babies than women who weren't. The positive parameter for mothers' birth year means that controlling for other independent variables, babies' birth weight increased for each birth cohort of mothers. The negative sign of the interaction parameter indicates however that the babies' birth weight increased less for women who were born after the school reform. For the average individual in the restricted sample (independent variables at their mean values), the total effect of the latter school reform amounts to a difference in birth weight of circa 5 grams.

In the linear causal analysis of the 1974 reform with gestational age as dependent variable, the parameters of interest were statistically significant. The treatment effect of the reform shows that it had a statistically significant positive effect (p<0,01) on gestational age, i.e. a jump in gestational age at the cutoff. The statistically significant negative sign (p<0,01) for the interaction term indicates that gestational age increased less for women who were born after the school reform, each birth year decreasing gestational age of total 0,0026 weeks. The point estimates of the latter school reform on gestational age have opposite signs of those of the former reform. These effects are however not statistically significant (p>0,1). The results show that the 1985 reform had a negative effect on gestational age but the positive interaction term indicates that gestational age decreased less for women who were born after the school reform. As with many regressions, it is not particularly informative to get results that are based on all other parameters set at zero and therefore by inserting into the regression equation the mean values of the parameters a better understanding was obtained. It showed that for the average individual in the sample, the school reform had a positive total effect on gestational age of circa 0,03 weeks.

5.2 Logit results

By looking at the marginal effects of the logit analyses, we found only a small and insignificant effect for the first school reform ($p>0,7$) on the variable "LBW". For the 1985 school reform however, there was a small negative statistically significant effect ($p<0,05$). The marginal effects of the logit analysis show that for two otherwise average mothers, one born before the reform and the other one after the reform, the odds of having a low birth weight baby were 0,43 percentage points lower for a mother born after the reform.

The marginal effects of the former school reform on the odds of preterm births were small and not very statistically significant ($p<0,15$). For two otherwise average mothers born on either side of the cutoff, the probability of a preterm birth was 0,047 percentage points lower for the woman born after the school reform than the woman born before it. The marginal effects of the latter school reform were very small and statistically insignificant.

Table 2 – Sensitivity analysis using linear RD design

1974 reform	5 bins on each side of cutoff	8 bins on each side of cutoff	10 bins on each side of cutoff	Whole dataset
	Dependent variable: Birth weight			
Treatment effect coefficient	-4,468	1,242	3,793	10,112
P-value	(0,543)	(0,752)	(0,215)	(0,000)
Observations	34293	52565	62568	122823
Interaction term coefficient	0,002	-0,001	-0,002	-0,005
P-value	(0,544)	(0,750)	(0,214)	(0,000)
Observations	34293	52565	62568	122823
	Dependent variable: Gestational age			
Treatment effect coefficient	44,899	42,880	27,833	31,201
P-value	(0,097)	(0,003)	(0,015)	(0,000)
Observations	34293	52565	62568	122823
Interaction term coefficient	-0,023	-0,022	-0,014	-0,016
P-value	(0,097)	(0,004)	(0,015)	(0,000)
Observations	34293	52565	62568	122823

1985 reform	5 bins on each side of cutoff	8 bins on each side of cutoff	10 bins on each side of cutoff	Whole dataset
	Dependent variable: Birth weight			
Treatment effect coefficient	-6,858	6,668	10,519	16,196
P-value	(0,283)	(0,035)	(0,000)	(0,000)
Observations	42819	67784	82469	122823
Interaction term coefficient	0,003	-0,003	-0,005	-0,008
P-value	(0,284)	(0,035)	(0,000)	(0,000)
Observations	42819	67784	82469	122823
	Dependent variable: Gestational age			
Treatment effect coefficient	0,527	-18,386	-21,005	10,574
P-value	(0,983)	(0,143)	(0,020)	(0,012)
Observations	42819	67784	82469	122823
Interaction term coefficient	0,000	0,009	0,011	-0,005
P-value	(0,984)	(0,142)	(0,020)	(0,012)
Observations	42819	67784	82469	122823

Table 3 – Sensitivity analysis using logit

1974 reform	5 bins on each side of cutoff	8 bins on each side of cutoff	10 bins on each side of cutoff	Whole dataset
	Dependent variable: LBW			
Treatment effect coefficient	0,000	-0,001	-0,003	-0,001
P-value	(0,926)	(0,780)	(0,179)	(0,272)
Observations	34293	52565	62568	122823
	Dependent variable: PTB			
Treatment effect coefficient	-0,005	-0,005	-0,004	-0,003
P-value	(0,157)	(0,127)	(0,173)	(0,220)
Observations	34293	52565	62568	122829

1985 reform	5 bins on each side of cutoff	8 bins on each side of cutoff	10 bins on each side of cutoff	Whole dataset
	Dependent variable: LBW			
Treatment effect coefficient	-0,002	-0,004	-0,005	-0,003
P-value	(0,359)	(0,031)	(0,007)	(0,011)
Observations	42819	67784	82469	122823
	Dependent variable: PTB			
Treatment effect coefficient	-0,003	-0,002	-0,003	-0,001
P-value	(0,452)	(0,521)	(0,225)	(0,631)
Observations	42820	67786	82473	122829

6 Sensitivity analysis

Sensitivity analysis was performed on the data for both the linear RD design and the logit analysis. The number of bins used was varied to include five and ten years around the cutoff point as well as using no restrictions to include the whole dataset. On the whole, the results were as expected. When the sample was limited to a five year frame measurement precision was reduced to the point where all but one parameter was statistically insignificant. The parameter for gestational age for mothers affected by the former school reform was positive and statistically significant at the 10% level. Most parameters were statistically significant when the sample was broadened to include ten years around the cutoff, except for birth weight for the former reform. When no restrictions were put on mothers' birth year and the whole dataset used, it showed positive statistically significant treatment effect of both school reforms on birth weight and on gestational age ($p \leq 0{,}01$). For the former school reform, robustness of the variable "gestational age" is greater than for "birth weight", but for the latter reform the variable "birth weight" is more robust.

The logit sensitivity analysis yielded similar results as the linear RD sensitivity analysis, although generally with less statistical significance but with the signs of the parameters consistent over all regressions. No parameter for marginal effects for either dependent variable was statistically significant ($p>0{,}1$) when the sample was restricted to five bins on either side of the cutoff. As can be seen in Table 3 the most robust variable was "LBW" for the latter reform. The parameter for "PTB" in the former reform was close to being statistically significant in the original regression ($p<0{,}13$), but significance decreased in the sensitivity analysis.

Falsification tests of the data were run to check for robustness of the analysis. This was done by running the same regressions as before but using a time period where no school reform was being introduced (the period between 1959 and 1970). A zero effect would suggest that non-zero effects of the school reforms were not a product of the specific RD model specification. It would also rule out the possibility that the estimated effects were caused by other changes in Icelandic society using the same cut-off dates and assignment variables (Van Der Klaauw, 2008). The results of these can be found in Table 4. The results from the linear RD analyses for both birth weight and gestational age show insignificant treatment effect ($p>0{,}4$ and $p>0{,}1$ respectively). Similarly, the logit analyses showed very small and insignificant effects (p>0,5) in this period for both dependent variables. These results strengthen the findings presented in Chapter 5 of significant effects of the school reforms on birth outcomes.

Table 4 – Falsification test

Falsification test	Data from 1960-1969	
	Dependent variable: Birth weight	Dependent variable: LBW
Treatment effect coefficient	5,402	0,001
P-value	(0,442)	(0,587)
Observations	44542	44542
	Dependent variable: Gestational age	Dependent variable: PTB
Treatment effect coefficient	-39,625	0,000
P-value	(0,134)	(0,900)
Observations	44542	44542

7 Conclusion and Discussion

The conclusion of this study is first and foremost that school reforms in Iceland had an effect on the birth outcomes studied. By using the RD design to isolate those effects the causal nature of the relationship was established. The linear regressions showed that both school reforms had positive effects on birth weight (statistically significant for the latter reform) which is what was hypothesized beforehand. The former reform had a positive effect on gestational age (statistically significant) and the latter had a negative effect on gestational age (not statistically significant). However, when meaningful values were inserted into the regression function the relationship became positive. The changing sign of the parameter is not in accordance with the theory proposed beforehand, but when Figure 4 is considered it is however clear that one would not expect to see large treatment effect on gestational age of the latter reform and that the sign of the parameter would change.

Evaluation of the marginal effects of the logit analyses showed a negative relationship between the school reforms and the probability of both low birth weight and preterm births, although the only statistically significant parameter was "LBW" for the latter school reform. The sign of the parameters are in accordance with the original hypotheses put forward. On the whole statistical significance increased when the time span around the cutoff points were increased which is to be expected when more data points are included in the analyses.

The results are in accordance with former research on the subject. Various studies have shown a positive relationship between education and health, and more recent research has used an RD design to study the causal relationship. Our linear RD results point in the same direction as those of Lleras-Muney (2005) and Oreopoulos (2006), where education affects health, although with different health variables than used in our study. The marginal effects of our logit results, where the probability of low birth weight and preterm birth were lower for an average individual born after the school reforms than before them, are similar to the results found by Currie and Moretti (2003).

The educational system is constantly evolving and the changes made reflect the demand made by society. The results of this study show that the school reforms in 1974 and 1985 had a positive effect on health for those affected by them. Effects of the school reform made in 1990 on health have not been studied due to its proximity in time, but it is an interesting subject matter for future research. A study on diminishing marginal effects of education on health would be warranted in that regard. The changes in majority age in Iceland in 1998 is another factor that will likely have effects on educational attainment and perhaps birth outcomes and other health variables.

An internal validity concern is that the data available spans from 1982 to mid-year 2012. Although this spans only 30 years, the number of observations is rather large, 127.354 in total. For the law reform that was implemented in 1985 the affected mothers were born in 1970 and those women are still at child bearing age, but this is controlled for in this study. Similarly, the first part of the reform affected mothers born in 1959 and surely part of those women already started having children before 1982, which is our first available data. Furthermore, no data on fathers is available in the birth registry, but fathers' overall health and health-related behavior is potentially an important variable when assessing LBW and PTB. Another relevant concern is whether other medicinal, societal or political changes paralleled the school reforms which were not taken into consideration and could have affected the educational attainment of the mothers studied or the health outcomes of their children. To the best of the authors' knowledge this was not the case. These shortcomings of the data are relevant and can lead to biased parameters. For future research it would be optimal to get further information on the births occurring before 1982, which is at present only available in non-electronic format.

While the internal validity of the study is arguably good, external validity concern is more questionable, both regarding transferability across time and across social and institutional settings. The trend for more and better education has continued across the globe. Variability across time and space in other factors is also a concern, for example women's education and job market participation as well as parental leave after child birth vary considerably. Such societal changes in Iceland may already be reflected in the differential effects of the two school reforms on the health indicators used here. If those factors are believed to play a substantial role, then the policy guidance from the reforms studied here may be of more relevance to other societies than Iceland in future decision making.

Despite some limitations and validity concerns this research sheds light on the causal relationship between education and health. One of the main strengths of this study is the natural experiment setup of the data which is the preferred experimental setup in most social science research due to its unbiased nature. It is also clear that individuals' control over whether they are assigned to treatment or control groups is none, making the choice of using RD method appropriate. The data used for the study is an exhaustive list of all child births in Iceland for the time specified which is important for the statistical accuracy of the results and sample selection concerns between treatment and control groups that might otherwise exist.

Although the study provided evidence of a causal relationship between changes in the educational system in Iceland and birth outcomes this is only the first research in Iceland utilizing the RD design and the Birth Registry data. Future research is needed to shed more light on this relationship and consideration needs to be taken to the limitations that applied to this one.

Individual level data on education would have been ideal for this research. This is currently not available in Iceland and therefore a reduced form regression is performed. When this data becomes available a repetition of the study would be warranted, with the inclusion of individual education as the pathway of the school-reform effect. The strength of the study suffers because of this, since causality assumptions rest on the belief that the school reforms were to some extent binding in the sense that they had an actual effect on educational attainment.

This research adds to the literature on the causal relationship between education and health and its results are supported by other studies. In Iceland this relationship has not been studied much and many questions are still unanswered. Determinants of LBW and PTB include various health behaviors of parents which were not controlled for in this study and should be included in future research if possible, for example cigarette smoking, physical activity, stress and anxiety. A study on the relationship between the school reforms and other health variables such as mortality would also complement the research done by Lleras-Muney (2005) on U.S. data and Clark and Royer (2010) on data from the U.K.

Improvement of population health, logically, often entails a reforming of health-care systems, but as introduced in this study social policies could be used as well, such as educational policy. In the case of the 1974 and 1985 reforms the results point to small positive effect on the birth outcomes chosen. The changes made to the system were naturally made without certainty of their effect on various factors and such decisions often spark discussion in society about the validity of them. In the 1970's and 80's it may have seemed an obvious policy change since demand from the public for more and better education provided by the government was high. Schooling affects multiple outcomes of which only two, and arguably relatively minor ones, are studied here. Policy thus needs to take multiple other results into account, such as current state of education (both quantity and quality) and possible diminishing marginal returns of education. Due to the uncertainty of results from recent changes of the system it is not possible to make policy decisions based on the study's results alone, but certainly an abolition of compulsory schooling would not be recommended based solely on them. Such a decision would reflect other objectives of society than health promotion.

References

Beck, S., Wojdyla, D., Say, L., Betran, A. P., Merialdi, M., Requejo, J. H., et al. (2010). The worldwide incidence of preterm birth: A systematic review of maternal mortality and morbidity. *Bulletin of the World Health Organization , 88* (1), 31-38.

Chevalier, A., & O'Sullivan, V. (2007). *Mother's Education and Birth Weight.* Bonn: IZA.

Clark, D., & Royer, H. (2010). *The effect of education on adult health and motality: Evidence from Britain.* Working Paper 16013. Cambridge: National Bureau of Economic Research.

Currie, J., & Moretti, E. (2003). Mother's education and the intergenerational transmission of human capital: Evidence from college openings. *Quarterly Journal of Economics , 118* (4), 1495-1532.

Cutler, D. M., & Lleras-Muney, A. (2006). *Education and Health: Evaluating Theories and Evidence.* Working paper 12352. Cambridge: National Bureau of Economic Research.

Fuchs, V. R. (2004). Reflections on the socio-economic correlates of health. *Journal of Health Economics , 23*, 653-661.

Grossman, M. (2006). Education and nonmarket outcomes. In E. A. Hanushek, & F. Welch (Eds.), *Handbook of the economics of education, Volume 1.* Amsterdam: Elsevier B.V.

Grossman, M. (1972). On the Concept of Health Capital and the Demand for Health. *Journal of Political Economy , 80*, 223-255.

Guttormsson, L. (Ed.). (2008). *Almenningsfræðsla á Íslandi 1880-2007: 2. bindi.* Reykjavík: Háskólaútgáfan.

Jóhannesson, I. Á. (1984). *Menntakerfi í mótun; Barna- og unglingafræðslan á Íslandi 1908-1958.* Reykjavík.

Jón Gnarr vill afnema skólaskyldu. (2011, August 23). *DV.* Retrieved January 9, 2012, from http://www.dv.is/frettir/2011/8/23/jon-gnarr-vill-afnema-skolaskyldu/

Kramer, M. S. (1987). Determinants of low birth weight: Methodological assessment and meta-analysis. *Bulletin of the World Health Organization , 65* (5), 663-737.

Law on Legal Majority no. 71/1997.

Law on Primary School no. 52/1990.

Lee, D. S., & Lemieux, T. (2009). *Regression discontinuity designs in economics.* Working Paper 14723. Cambridge: National Bureau of Economic Research.

Lleras-Muney, A. (2005). The relationship between education and adult mortality in the United States. *Review of Economic Studies , 72*, 189-221.

Margrét Pála sammála borgarstjóra. Afnema ber skólaskyldu. (2011, September 13). *Eyjan.* Retrieved January 9, 2012, from http://eyjan.is/2011/09/13/margret-pala-sammala-borgarstjora-afnema-ber-skolaskyldu/

O'Keeffe, D. (2004). *Compulsory education: An oxymoron of modernity* . London: Libertarian Alliance & Dennis O'Keeffe. Retrieved April 9, 2012, from http://www.libertarian.co.uk/lapubs/educn/educn036.pdf

Oreopoulos, P. (2006). Estimating average and local average treatment effects of education when compulsory school laws really matter. *American Economic Review , 96* (1), 152-175.

Silles, M. A. (2009). The causal effect of education on health: Evidence from the United Kingdom. *Economics of Education Review , 28*, 122-128.

Statistics Iceland. (2011). *Skólasókn eftir kyni, aldri og landsvæðum 1999-2010*. Retrieved November 28, 2011, from http://hagstofa.is/?PageID=2604&src=/temp/Dialog/varval.asp?ma=SKO00002%26ti=Sk%F3las%F3kn+eftir+kyni%2C+aldri+og+landsv%E6%F0um+1999%2D2010%26path=../Database/skolamal/yfirlit/%26lang=3%26units=Fj%F6ldi

Van Der Klaauw, W. (2008). Regression-discontinuity analysis: A survey of recent developments in economics. *LABOUR , 22* (2), 219-245.

World Health Organization. (2012). *Health statistics and health information systems*. Retrieved October 10, 2012, from http://www.who.int/healthinfo/statistics/indneonatalmortality/en/

World Health Organization. (2006). *Promoting optimal fetal development: Report of a technical consultation.* Retrieved December 12, 2011, from http://www.who.int/nutrition/topics/fetal_dev_report_EN.pdf

Appendix 1 – Natural experiment

Experiments involving human behavior can be both costly and unethical, and that can be a major constraint for economic research. Man-made experiments, such as job-training programs and class organization in schools, can lack generalizability and suffer from endogeneity problems that make economic research problematic. Natural experiments have been seen as a cure for this problem and most economists relish the chance to work with data which arrive from that kind of setting. Of course it is not a perfect solution and has received some criticism, although most researchers agree that natural experiments often offer the best available data. As Cutler and Lleras-Muney (2006) point out, a major drawback of the research where natural experiments, such as school reforms, are used to estimate a causal relationship is that compulsory schooling laws were intended to affect those on the lower end of the education distribution and most likely did not affect those who were already planning on pursuing further education. The effects of policies and programs aimed at the population as a whole or different population groups are therefore difficult to estimate in comparison with the one studied.

Although the statistical methodology used here (the RD design) works well with natural experiments, since the individuals do not have control of the assignment variable (here: individuals do not have control over when they were born and therefore do not have control over the law environment in which they live in), the generalization of the outcome needs consideration. The general atmosphere in society needs to be considered; are Icelanders the same now as they were in the 1970's and 1980's? What have educational researchers have to say on the matter of compulsory schooling and other school reform issues? The answers to those questions are critical when considering how the results can be used is policy decisions.

Appendix 2 – The regression discontinuity design

A RD design is used to estimate the treatment effect when the treatment is determined by whether the assignment variable exceeds a known cutoff point. The main idea behind the RD design is that individuals just above and just below the cutoff point are good comparisons.

A simple linear RD design equation could look like this:

$$Y = \alpha + D\tau + X\beta + \varepsilon$$

D is a dummy variable which represents the receipt of treatment or not. X is the assignment variable with a cutoff point c, so $D = 1$ if $X \geq c$, and $D = 0$ if $X < c$. ε is an error term and τ can be viewed as an estimate of the causal effect of treatment, which is effectively the parameter of interest.

In their article, Lee and Lemieux (2009) give good advice and instructions on how to implement an RD design in a research. First of all, there are some identification issues that need to be considered when using it.

1. When is an RD design the appropriate statistical method to use?
2. What are the identification assumptions of RD design?
3. Can the RD results be generalized?

First, the most important question when deciding whether to use RD design or not, is whether and how individuals are able to influence the assignment variable. It is appropriate to use RD design if individuals do not have precise influence on their assignment to treatment or control groups (and therefore fulfill one of the most important identification assumptions of the RD design, as listed below). Individuals' control over the assignment variable can be debatable and the imprecision of it can be tested with an examination of the density of X itself. In the case of our research with the Icelandic data, it is clear that individuals do not have precise influence over which birth cohort they fall into and consequently which school law environment was prevalent.

Second, there are several identification assumptions regarding the RD design. First of all, it is assumed that individuals cannot precisely manipulate the assignment variable. The imprecise control of the assignment variable means that variation in the treatment near the cutoff point is "as good as randomized". Secondly, it is essential that all other factors that influence Y in the regression equation are continuous with respect to X. This condition enables the researcher to use the average outcome of those individuals who fall right below the cutoff as a valid counterfactual for those just above it. If the other variables also jump at the cutoff point, then the gap τ will be susceptible to bias towards the treatment effect of interest.

Thirdly, the generalization of the results can be surprisingly strong, but careful consideration needs to be put into the interpretation of the treatment effect. According to Lee and Lemieux, the discontinuity gap in an RD design can be interpreted as a weighted average treatment effect across all individuals if the treatment effect is heterogeneous (Lee & Lemieux, 2009), although it may not make much practical sense in many contexts to study the treatment effect away from the discontinuity threshold.

One of the appeals of the RD design is the graphical presentation it offers. A standard way of presenting the data is to divide the assignment variable into a number of bins, both above and below the cutoff point and marking the cutoff point clearly vertically. Then the average value of the outcome variable can be computed for each bin and graphed against the mid-points of the bins. The advantage of graphing the data in this way is that it provides a simple way of visualizing what the functional form of the regression looks like on either side of the cutoff point. The magnitude of the jump in the regression function of the treatment, i.e. at the cutoff point, is indicated by viewing the graph. Also, if there are other jumps visible on the graph, the interpretation of the jump at the cutoff point as a causal effect of the treatment becomes questionable. If there is no visible jump on the graph it is unlikely that the regression will show a significant treatment effect. Furthermore, if the data does not seem to show a linear relationship but rather a curvature, one can expect the choice of bandwidth to be more important to the results. Figure A 1 shows a simple setup of the RD design where a jump at the cutoff point is marked clearly.

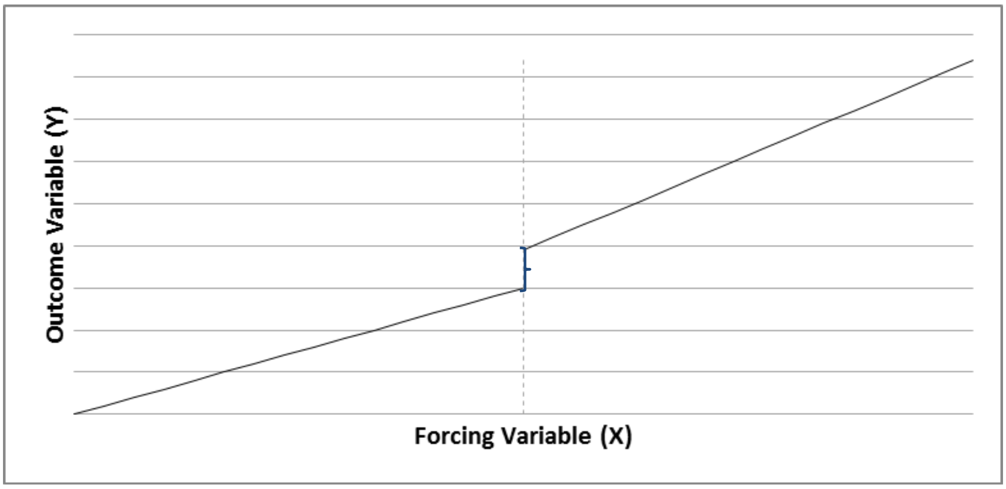

Figure A 1: Simple Linear Regression Discontinuity Setup