ISSN 1011-8888

INSTITUTE OF ECONOMIC STUDIES

WORKING PAPER SERIES

W13:09

December 2013

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

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by

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Abstract

This study investigates the effects of schooling on the incidence of preterm birth and low birth weight using a secondary-school reform that took place in 1985 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. Regression discontinuity analysis was performed on data from the Icelandic Birth Registry to compare births of female cohorts born before and after the school reform. The size of the Icelandic society and the resulting ease at which the reform could be implemented simultaneously for the whole country, along with high quality birth data makes for a good research opportunity. Our main results show that the reform had a very small, statistically significant positive causal effect on birth weight conditional on gestational age, indicating a greater growth of the fetus for the females who were subject to longer mandatory schooling. Former research show ambiguous results, but ours results are in accordance with recent studies on the subject where the effects of education on various health outcomes have been studied. While the effects are small, the results indicate that the human-capital investments made through the school system can be far reaching and extend between generations.

JEL Classification: I12, I20, I28, I10

1 Introduction

Policy changes, such as the school reform in Iceland that is 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 used in this study to assess the effects of the school reform in Iceland on birth outcomes, has been utilized 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 previously employed methods, 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 LBW babies is used as a sign of comprehensive public health status, and on an individual level a predictor of mortality and morbidity. 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. An infant's low birth weight can be caused by either PTB or by intrauterine growth restriction (IUGR) causing the infant to be small for gestational age (SGA), or a combination of both (Kramer, 2003). A fetal growth curve based on Swedish data (Marsal, Persson, Larsen, Lilja, Selbing & Sultan, 1996) is used to determine whether an infant is considered SGA. This curve has been shown to be applicable for Icelandic fetuses (Geirsson, 1997). The incidence of PTB has been on the rise worldwide over the last few decades and some of the possible explanations 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. Ardent 2005, 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) as well as Jürges, Kruk and Reinholds' (2013) 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 neither Clark and Royer nor Jürges, Kruk and Reinholds did. The latest studies on the subject show ambiguous results on the causal effects of education on health (see for example Clark & Royer 2010, Meghir, Palme & Simeonova 2012 and Jürges, Kruk & Reinhold 2013), contradicting to some extent conventional wisdom and previous literature. It is thus clear that robustness of results across data, context and outcome measures is still needed. Here, such a study is added to the literature. The causal effects of school reforms on health have neither been examined in Iceland before with an RD design, and only one study in the literature examines the effects of school reforms on birth outcomes (Chevalier and O'Sullivan 2007). However, the school reforms examined there took place in 1947, whereas the results presented here are arguably as close in time as one can get with this research question. Moreover, the sharpness of the reform with regards to implementation is most likely greater in our case due earlier reforms of the school system which accommodated the one studied here. 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 1974 major changes were made to schooling laws in Iceland, implementing a restructure of the secondary-school system right away. To accommodate a planned lengthening of compulsory schooling from 7-15 to 7-16 in advance, 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 simultaneously throughout the country, so students turning 16 years old in 1986 (born in 1970) were affected, as separation into classes goes by calendar year in Iceland. A school year in Iceland starts in the fall and ends in the spring, and children started their schooling in the fall of the calendar year in which they turned 7 years old (Guttormsson, 2008). For this study the incidence of PTB and LBW given gestational age are studied for women exposed to higher compulsory schooling as compared to prior age cohorts not exposed. Additionally, the results of LBW estimations are examined in comparison to the alternative specification of SGA as an outcome measure.

Changes to school systems in large countries can take time to execute, resulting in a blurry implementation. While observations will be fewer, an advantage of data collection in such a small country as Iceland is the high certainty of the completion of the reform at the same time for every school in the country. This is especially true for this particular reform, since the country's school system underwent changes a decade earlier to accommodate it.

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 after the cutoff, which was January 1st 1970, and that date separates the exposed and unexposed women. Using the RD design allows us to study the causal relationship between the schooling reform and birth outcomes by examining whether there is a difference between the cohorts before and after the exogenous change. 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 since 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. The results show that this was indeed the case. We found a very small positive effect on birth weight in continuous form and a small negative effect on the risk of LBW. Statistically significant effects were not found in the case of PTB.

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 and existing literature

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

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. For 16 year olds it has been at or above 90% in recent years, but attendance for 18 year olds has risen from around 68% in 1999 to 82% in 2010, which is the available time span for these data published by Statistics Iceland (2011). The oldest data point, that from 1999, shows that 79.6% of females were in school at the age of 17, when schooling is no longer mandatory for them. However, the school reforms under examination took place 14 years prior to that. Ideally we would be interested in knowing the fraction of 16 year old females who were in school just before it became mandatory. The most reliable data on that is not as detailed as we would have hoped, but shows that in 1980, 54.3% of the total 16-19 year old populace was in school (Hagskinna, 1997). The educational level of the nation, and especially of women, is potentially an important factor for the results, since a large portion of students continue their studies beyond what is compulsory. However, if changes in compulsory schooling are not binding, then assumptions on their causality are not warranted. The above overview shows that the increase in mandatory schooling did have an effect, although we do not know the exact number of females who truly would have quit if not forced to take the extra year of schooling. Unfortunately, data on individuals' educational attainment in Iceland is not available and hence reduced form regressions will be used, as discussed below.

The Icelandic school system is and has always been considerably different from ones in other countries, especially in the United States. Currently, the structure is as follows: 1^{st} -7^{th} grade is primary school and 8^{th}-10^{th} grade is secondary school. Separation into classes goes by calendar year with children starting their mandatory schooling on the year they turn 6 years old and finishing on the year they turn 16 years old. The school year starts in the fall and ends in the spring. Junior college or high school is from 16-20 years of age and is completed with a matriculation examination, a typical requirement for university acceptance. The system has of course been constantly evolving, but the most drastic changes occurred in the 20^{th} century, with four significant school reform laws implemented as the government's role in education was increasing. The first one was in 1907 when schooling was first made compulsory and the compulsory schooling age was 10-14 years old. The second major reform was in 1946 when the so-called Educational law was passed. Among other things 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. Primary school level started in the fall of the year children turned 7 and ended in the spring they turned 13, secondary school started the following fall, i.e. in the year they turned 13. The three programs that were available were 2, 3 or 4 years in length. Those children who decided to take 4 years in secondary school were thus longer in school than mandated by law (Jóhannesson, 1984). The third major school reform was in 1974, implemented in two stages, as described above. The latter one is the subject of this study, while the former implementation of change served as an important preparation for the extension of mandatory schooling in 1985. The fourth major school reform was in 1990 when compulsory schooling age was set to 6-16 years (Primary School Act no. 52/1990). The reason that the 1985 reform was chosen for study here but not others 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 be included in this study due to a lack of outcome measures. Most important though is the distinct and sharp change that took place in 1985, which when coupled with quality data and the statistical methods chosen should result in a high degree of internal validity. It can be argued that the rich data and high accuracy in terms of implementation of the reform gets us as close to a laboratory setting as one can get on this challenging research question.

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. In the time since this report was published economists have increasingly focused on the correlation and causal effects of socioeconomic factors on health.

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. Fuchs points out that there are difficulties in deriving statistical conclusions and drawing reliable policy applications from these correlations.

Currie and Moretti (2003) 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 seventeen, and therefore endogenous mobility is potentially problematic for the IV strategy chosen. The authors find that higher maternal education improves infant health, as measured by birth weight and gestational age, with OLS estimates suggesting 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, which largely hold up when using the IV specification. The authors also identify different pathways of the effects, such as through increased likelihood of marriage, reduced smoking and increased use of prenatal care.

Lleras-Muney (2005) used a quasi-natural experiment to study whether education had a causal effect on mortality. At least 30 states in the U.S. changed their compulsory schooling laws and child labor laws between 1915 and 1939 and the data used are the 1960-1970 and 1980 censuses of the U.S. (a 1% random sample of the population), with those who were 14 years old between 1915 and 1939 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 indicate a causal effect although statistical power is limited. 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%.

Arendt (2005) uses Danish panel data to assess whether the relationship between education and health can be interpreted causally. Self-reported health is used as a main outcome and body mass index and smoking as supplemental outcomes. An IV method is used to assess the effects of school reforms that took place in Denmark in 1958 and 1975, yielding results that show that longer education positively affects self-reported health. Arendt (2008) similarly examines the effect of those results on hospitalizations, finding effects for females and partially for males.

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 an RD design: The research most comparable to ours, and the only one using school reforms to examine the effect of education on birth outcomes, is by Chevalier and O'Sullivan (2007). They find positive causal effects of maternal education on birth weight, using the 1947 natural experiment where the minimum school leaving age was increased from 14 to 15. They find children from lower social classes to gain the most. The authors use OLS and IV estimates in a reduced form model. For each model in their study, the instruments are found to be valid and over-identification is always rejected. The authors conclude that the effect of maternal education on birth weight is mostly direct. Oreopoulos (2006) uses an RD and IV analysis to estimate returns to education and finds significant gains to health, employment and other labor-market outcomes. He estimates that an additional year of schooling lowers the likelihood of self-reported poor health by 3.2 percentage points. Furthermore, Silles (2009) performs a similar study of the effect of schooling on self-reported health. Linear probability models are used, as well as logit models which produce similar results. OLS estimates reflect a 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. RD design is used to examine the robustness of these results and yields similar findings.

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. The results of this study contradict 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. The results of Jürges, Kruk & Reinhold (2013) are in accordance with those of Clark and Royer (2010), where causal effects of the same British school reforms on health are ambiguous and statistically insignificant. Similar results are found by Meghir et al. (2012) where changes in compulsory schooling in Sweden implemented between 1949 and 1962 are studied and only small effects are found.

3 Data

The data used is from the Icelandic Birth Registry, which includes all births in Iceland and is held by The Directorate of Health. Records are available from the year 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, used here to determine exposure, 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 in kilograms and gestational age 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 methods are used to measure gestational age; firstly with an ultrasound and secondly with the mother's menstrual cycle. The ultrasound, considered a more accurate way of measurement, 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. A binary variable for SGA was constructed by placing an infant in the SGA category if its weight was more than two standard deviations below the mean of the Swedish fetal growth curve (Karsal et. al. 1996).

Table 1 contains summary statistics for singleton live births by maternal birth cohort, representing the periods before and after both school reforms. Statistics for both five and eight cohorts around the cutoff are presented. The mean birth weight for all births in the dataset is 3.63 kg and for all live births 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 stillborns 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 3.8% 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. 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.

Table 1: Summary statistics for singleton live births

	Mean	Std.Dev	Obs.	Mean	Std.Dev	Obs.
5 cohorts around cutoff	Birth cohorts: 1965-1969			Birth cohorts: 1970-1974		
Birth weight (kg)	3.670	0.563	22,135	3.704	0.556	20,685
Gestational age (weeks)	39.895	1.851	22,135	39.929	1.840	20,684
Number of previous births	0.945	0.985	22,135	0.904	0.947	20,685
Mother's birth year	1966.920	1.411	22,135	1971.995	1.386	20,685
Mother's age	27.700	5.927	22,135	27.900	5.609	20,685
Dummy for LBW	0.027	0.161	22,135	0.225	0.148	20,685
Dummy for PTB	0.034	0.182	22,135	0.037	0.189	20,685
8 cohorts around cutoff	Birth cohorts: 1962-1969			Birth cohorts: 1970-1977		
Birth weight (kg)	3.667	0.562	36,397	3.703	0.556	31,389
Gestational age (weeks)	39.915	1.807	35,397	39.930	1.839	31,389
Number of previous births	0.988	1.000	35,397	0.871	0.926	31,389
Mother's birth year	1965.397	2.252	35,397	1973.342	2.235	31,389
Mother's age	27.702	5.832	35,397	27.678	5.356	31,389
Dummy for LBW	0.025	0.157	35,397	0.022	0.147	31,387
Dummy for PTB	0.033	0.178	36,397	0.038	0.191	31,389

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 around 1970 and those women are still at child bearing age. 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. A variable for 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.

The statistical software STATA 11.0 was used for all analysis.

4 Statistical framework

An 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, 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 make a good comparison. Following the strategy of previous literature (Clark & Royer, 2010) and choice of controls in birth-outcome estimations (Chevalier and O’Sullivan, 2007) the following sharp linear regression function was used:

$$H_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + \beta_3 (D_i * X_i) + \beta_4 P_i + \beta_5 A_i + \beta_6 G_i + \beta_7 Dgest_i + u_i$$

With H_i as the health outcome for observation i (either birth weight or gestational age), β_0 a constant, D_i a dummy for the assignment variable (whether a mother was a part of the affected birth cohorts or not), X_i a mother’s birth year, P_i parity, A_i mother’s age, G_i is gestational age, which was added when the dependent variable is birth weight, and $Dgest_i$ a binary variable indicating which measurement method was used for gestational age. Making the birth weight conditional on gestational age thus provided a meaningful measure of weight for gestational age, indicating if the treatment affected intra-uterus growth, rather than possibly estimating an effect that could be a byproduct of gestational age. This use of the variables is considered more appropriate than omitting gestational age as a control, since it is a more important health concern than birth weight alone, especially when preterm birth is also considered. The use of gestational age as a control is in line with the strategy used by Chevalier and O’Sullivan (2007). However due to concerns about gestational age as a “bad control” (Angrist & Pischke, 2008), we compared those results with estimations using a different specification or that of an SGA as a dependent variable. The gestational age variable is not used as control in regressions on SGA. Using this outcome measure thus eliminates the problem of gestational age as a control variable. However, it has its own shortcomings as it is based on a fetal curve calculation involving a limited amount of infants in a different country that is likely to cause misclassification and the associated attenuation bias and loss of precision in estimations. Furthermore the birth weight specifications generate more meaningful and easily interpretable coefficients. We thus consider those to be our main birth weight results. 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 the 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. Together, these two parameters signify the total effect of the school reform on the dependent variables. The cutoff point in mothers' birth years for the school reform is the year 1970. The use of a sharp RD design rather than a fuzzy RD design was deemed appropriate due to the sharpness of the change.

Time trends are always a concern in RD analyses, especially as the bin size and/or number increases. In this case one might be concerned about trends in birth outcomes over time. However, the child's birth year is not the variable separating the treated and untreated cohorts, but the birth year of the mother. For this reason children's births overlap for treated and untreated cohorts and time trends in birth outcomes is therefore controlled for. We thus note that while the child's year of birth is not directly included in the estimation equation above, it is indirectly controlled for with the inclusion of the mother's birth year and age. Thus including the child's birth year directly would have caused multicollinearity problems. However, the direct inclusion of the child´s birth year was tried and then with the exclusion of either of the other two variables separately to prevent multicollinearity. As expected, results were robust to such changes.

In assessing the data, it was split into a few bins, both above and below the cutoff point. 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 how to best decide on the bins size. Too narrow bins will lead to highly imprecise estimates, but biased estimates can result from too wide bins if the slope of the regression line is not adequately accounted for. A comparison of both sides of the cutoff point loses credibility with too wide bins 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 since the mothers' birth date and month were unknown. Both five and eight bins on each side of the cutoff point were studied in the main analysis since they were determined 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 observations for statistical power. However, sensitivity analyses were performed where the number of bins used was varied. Ideally, one would want to be able to study a shorter period with more data points, but since information on mothers' exact date of birth is lacking, the period studied had to be broadened. Figures 1 and 2 show the graphs for the mean birth weights of each birth cohort of mother, with a trend line for five and eight years respectively. Similarly Figures 3 and 4 show the graphs for mean gestational age using the raw unadjusted data with trend lines for five and eight years. As can be seen in the figures, one can expect to see rather different results depending on the number of bins chosen in the analysis, both in terms of the visual jump at the threshold and the slope of line.

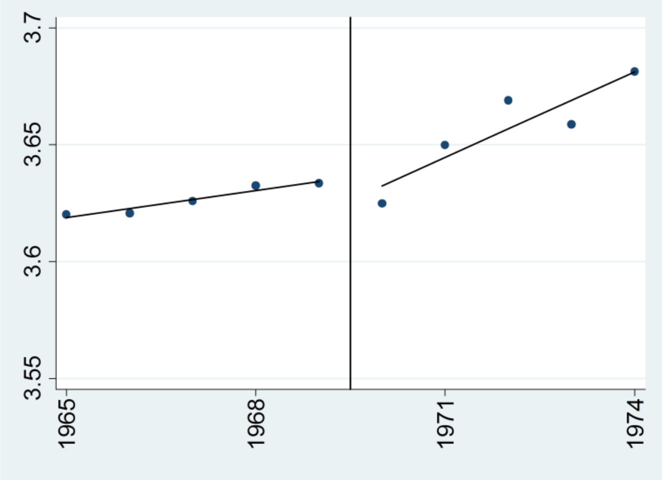

Figure 1 – Mean birth weight in kilograms (vertical axis) per birth cohort of mothers (horizontal axis). Trend line five years. Cutoff point 1970

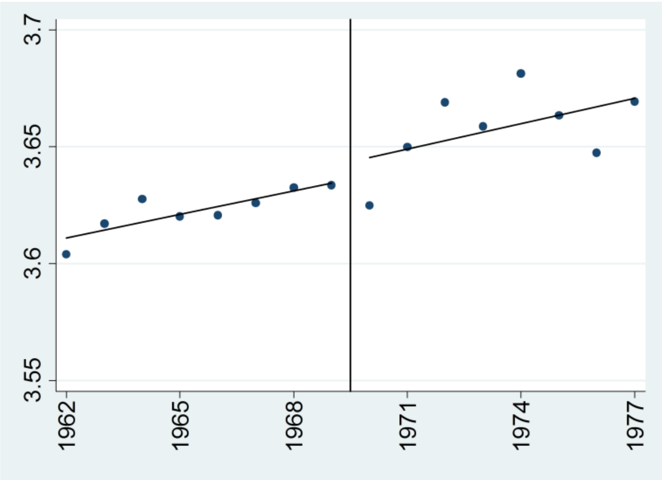

Figure 2 – Mean birth weight in kilograms (vertical axis) per birth cohort of mothers (horizontal axis). Trend line eight years. Cutoff point 1970

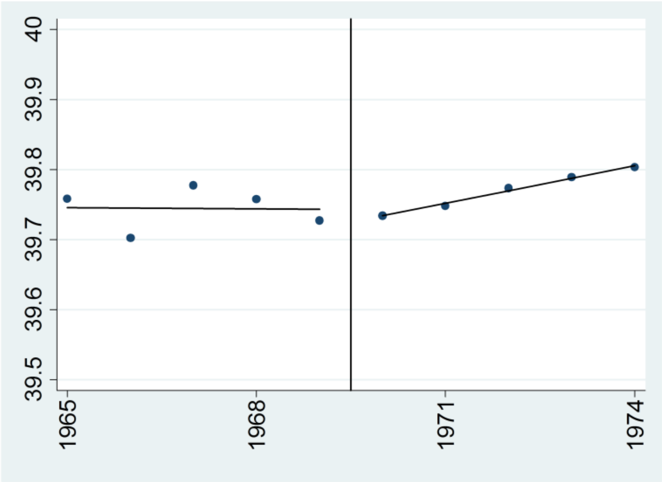

Figure 3 – Mean gestational age in weeks (vertical axis) per birth cohort of mothers (horizontal axis). Trend line five years. Cutoff point 1970

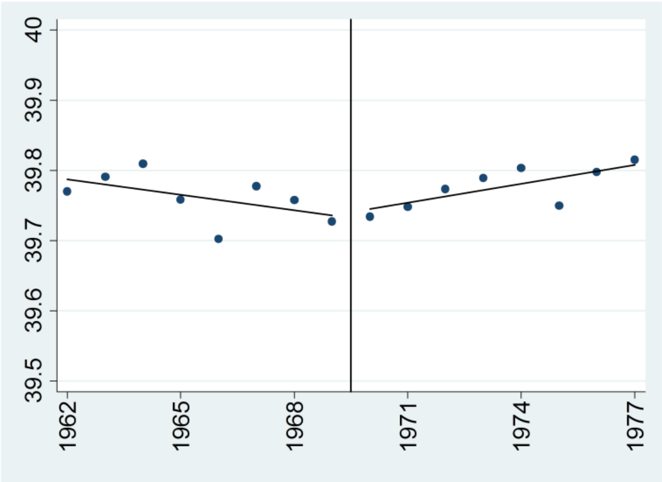

Figure 4 – Mean gestational age in weeks (vertical axis) per birth cohort of mothers (horizontal axis). Trend line eight years. Cutoff point 1970

The analyses were restricted to include only singleton 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). 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 is to be expected in an analysis of this kind.

Additionally a logit model was used with the dependent binary variables indicating whether a newborn's birth weight placed it in the LBW category or not on one hand, or whether the gestational length was categorized as PTB or not on the other hand. Otherwise, the regression equation was the same as before and the list of independent variables the same as summarized above, with gestational age added to the list when the dependent variable was LBW. To compare the two groups of women who were born before and after the cutoff date, marginal effects were calculated, revealing the difference in the probability of a particular health outcome (here low births weight or preterm birth) with all other independent variables kept at their means. 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. For comparison to the LBW estimations, an analysis was done using the outcome variable SGA. Since regressions of LBW are run with gestational age as a control variable, the results from the regressions should be comparable.

A crucial assumption of the RD design is 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 evidence of such a jump was found at the threshold here.

To check the validity of the study design, we carried out a falsification test by running our models using an arbitrary maternal birth-year cut-off within a period where no school reform was introduced (the period between 1959 and 1970). A zero effect in such estimations would suggest that non-zero effects of the school reforms were not a product of the RD model specification and would strengthen us in the belief that the estimated effects were caused by other societal changes using the same cut-off dates and assignment variables (Van Der Klaauw, 2008).

5 Results

Results for the linear estimations can be found in Table 2. The sample was restricted to women born firstly in 1965 to 1974 (n=42,819) and secondly in 1962 to 1977 (n=67,784) for the analyses on five and eight years respectively around the cutoff point. The results show that none of the analyses including only five years around the cutoff point showed statistically significant results and therefore the majority of results discussed are the other main analyses, which included eight years around the cutoff point.

The total effect of the reform includes both the treatment effect parameter and the interaction parameter. The parameter for the treatment effect was positive and statistically significant for birth weight ($p<0.05$) and the interaction parameter was negative and statistically significant ($p<0.05$) when eight years on each side of the cutoff were studied. This indicates a causal effect of the change of compulsory schooling on birth weight conditional on gestational age where women who were affected by the reform had heavier babies than women who weren't regardless of the duration of their pregnancies. 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 means), the total effect of the reform amounted to a difference in birth weight of approximately 5 grams and a total effect at the cutoff point (the year 1970) of 1.815 grams. The results show that the reform had a positive total effect on gestational age, with the slope changing directions after the cutoff, although not statistically significant ($p>0.1$). Interestingly the sign of the treatment effect changes when comparing the five and eight year analyses. Looking at Figures 1 and 2 this comes as no surprise, since the first data point after the reform lies somewhat lower on the chart than for the previous years. For the following cohorts of mothers, we see however an upwards shift in birth weight, which is captured in our results.

Furthermore, binary variables for malformations at birth and mother's chronic diseases were not found to be mediators for the school reform effects. By looking at the marginal effects of the logit analyses, shown in Table 3, we found a small negative statistically significant effect ($p<0.05$) for LBW in the eight year analysis, but only insignificant effects in the five year analysis. 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 on PTB were very small and statistically insignificant.

Table 2 – Results and sensitivity analysis using linear RD design

	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	42,819	67,784	82,469	122,823
Interaction term coefficient	0.003	-0.003	-0.005	-0.008
P-value	(0.284)	(0.035)	(0.000)	(0.000)
Observations	42,819	67,784	82,469	122,823
	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	42,819	67,784	82,469	122,823
Interaction term coefficient	0.000	0.009	0.011	-0.005
P-value	(0.984)	(0.142)	(0.020)	(0.012)
Observations	42,819	67,784	82,469	122,823

Table 3 – Results and sensitivity analysis using logit

	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	42,819	67,784	82,469	122,823
	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	42,820	67,786	82,473	122,829

Sensitivity analysis was performed on the data for both the linear RD design and the logit analysis and is shown alongside the results in Tables 2 and 3. The number of bins used was varied to include 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 parameters were statistically insignificant, both for the linear regression and the logit regression. Most parameters were statistically significant when the sample was broadened to include ten years around the cutoff, except for preterm birth in the logit analysis and the same results were found when no restrictions were put on mothers' birth year and the whole dataset used.

The logit sensitivity analysis yielded results with lower estimation precision than the linear regressions, but with the signs of the parameters consistent across all regressions. As can be seen in Table 3, the most robust variable was LBW. Robustness of results to changing the outcome measure to SGA showed consistency with results from LBW estimations, with a treatment effect coefficient of -0.0015 and -0.002 when using five and eight bins respectively on each side of the treatment, although not statistically significant in either case ($p>0.1$). Sensitivity analysis showed the sign of the parameters to be consistent over all regressions with the coefficient ranging between -0.0015 to -0.003 and statistical significance increasing with larger samples (full results available upon request).

Falsification tests were carried out to check the validity of the study design. This was done by running the same regressions as before but using a time period where no school reform was being introduced, specifically the period between 1959 and 1970. A zero effect would suggest that non-zero effects of the school reform were not a product of the specific RD model specification. It would also decrease 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). Since no statistically significant results were found in the main five year analysis, we do not expect to see statistically significant effects here either. However, point estimates closer to zero than from the main regressions are expected. The results of these falsification tests can be found in Table 4. The results of the test are better comparable to the analysis that includes five bins on each side of the cutoff, rather than eight bins, since it includes the same number of years. For our dataset it is unfortunately not possible to run a falsification test that includes more than five bins on each side, since a period that long would always include some other known school reforms. Both the treatment effect and interaction coefficient represent the total effects for the linear regressions. The results from the linear RD analyses for both birth weight and gestational age show insignificant total effects, i.e. treatment effects and interaction coefficients ($p>0.4$ 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. The point estimates for both parameters in the logit analysis are considerably closer to zero than in the main analyses. When total effects of the linear RD analysis at the cutoff point are considered, we also see point estimates much closer to zero than before. These results strengthen the credibility of significant effects of the actual school reforms on birth outcomes.

Table 4 – Falsification test

Falsification test	Data from 1960-1969	
	Dependent variable: Birth weight	Dependent variable: Gestational age
Treatment effect coefficient	5.402	-39.625
P-value	(0.442)	(0.134)
Interaction coefficient	-0.003	0.020
P-value	(0.442)	(0.134)
Observations	44,542	44,542
	Dependent variable: LBW	Dependent variable: PTB
Treatment effect coefficient	0.001	0.000
P-value	(0.587)	(0.900)
Observations	44,542	44,542

6 Discussion

The conclusion of this study is first and foremost that the 1985 extension of compulsory schooling in Iceland had some, although very small effect on birth weight conditional on gestational age. By using the RD design to isolate those effects the causal nature of the relationship was established. The linear regressions showed a statistically significant positive relationship on birth weight, only when eight years on each side of the cutoff were included, although this effect is neither substantial in magnitude nor robust across estimations. Evaluation of the marginal effects of the logit analyses showed a negative relationship between the school reform and the probability of both low birth weight and preterm births, while only statistically significant for LBW. 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 point was increased, which is to be expected when more data points are included in the analyses.

The likely mechanisms through which the effects can be attributed are various. As discussed above, determinants of birth outcomes are numerous and a mother’s education is only one of them. One could also see how education had both direct and indirect effects on birth outcomes. The direct effects could include a person’s increased knowledge about various relevant school subjects, for example biology and health science. The indirect effects could include, for example, more education leading to higher income and better socioeconomic status, as found by Chevalier and O'Sullivan (2007). This analysis includes the total effects of the reform, both direct and indirect.

The internal validity for the 1985 reform is arguably as good as one can hope for and in this natural experiment setup with quality data it is as close to a well-stocked research lab as one can get. The direction of the relationship for both dependent variables is in accordance with a priori hypotheses, although only statistical significant for birth weight.

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, showing ambiguous result depending on context. 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 demands made by society. The results of this study show that the extension of compulsory schooling in 1985 had a small, but positive effect on health for those affected by it. 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.

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. While controlled for in the study, it should thus be noted that the affected mothers of the school reform were born in 1970 and those women are still at child bearing age.

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 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 effects of the school reform on the health indicators used here. If those factors are believed to play a substantial role, then the policy guidance from the reform 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. The choice of using an RD design is especially appropriate since a clean break in the compulsory-schooling age is certain for the whole of Iceland. It is also clear that individuals' control over whether they are assigned to treatment or control groups is none. 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.

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 reform was to some extent binding in the sense that it had an actual effect on educational attainment.

The reduced form coefficients rest on that assumption. Additionally, more precise information on mothers' time of birth would have enabled us to split the data into more bins and thereby given us more data points around the cutoff date. This can lead to some bias in our results.

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 compulsory schooling extension 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 1985 reform the results point to small positive effect on the birth outcomes chosen. The change made to the system was naturally made without certainty of its effect on various factors and such decisions often spark discussion in society about the validity of them. In the 1980'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.

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