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Zero returns to compulsory schooling Is it certification or skills that matters?

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Zero returns to compulsory schooling:
Is it certification or skills that matters?

Sander Gerritsen*

Abstract

This paper evaluates the effects of the raising of the minimum school leaving age (ROSLA) from 14 to 15 in the Netherlands in 1971. The policy goal was to increase the number of high school graduates. The analysis shows that the change led to a decrease in the high school dropout rate of approximately 20%. However, there were no benefits in terms of employment or higher wages. I investigate several explanations for this finding and present suggestive evidence in support of the skill-based explanation that no more labor-market relevant skills were learned during this extra year of school compared to those skills previously learned out of school.

JEL Codes: I2, J24

Keywords: compulsory schooling, educational policy, regression discontinuity

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1 Introduction

Changes in compulsory schooling laws have often been used by policymakers to increase the educational level of the population, in particular for those in the left tail of the ability distribution. Typically, these changes come in the form of rises in the minimum school leaving age, such as the rise from 14 to 15 in the UK in 1947 or from 14 to 16 in France in 1967. The most prominent argument for implementing these changes is that this would improve the labor market prospects for those considered to underinvest in schooling. However, the literature that evaluates the effects of changes in compulsory schooling laws shows that these changes do not always lead to better labor-market outcomes. Whereas studies that use Anglo-Saxon data find positive returns to increases in compulsory schooling, studies that use data from continental European countries find no returns.[1] This puzzling result has led researchers to speculate about what explains the differences between these countries. One explanation is that it matters whether labor-market relevant skills are learned in the additional year, as these skills are important for later labour market outcomes [Pischke and von Wachter, (2008); Oosterbeek and Webbink (2007)]. Another explanation is that it matters whether the additional year is accompanied by academic credentials [Grenet (2013), Dickson and Smith (2011)].

The aim of this paper is to explore this puzzling result further by evaluating a change in a compulsory schooling law that was specifically targeted at increasing the number of high school graduates. If certification is the leading explanation, an increase in the number of high school graduates should result in higher earnings and a better labor market position. If it matters what type of skills are learned in school, this has not necessarily to occur: when no relevant labor-market skills are learned but only a certificate is obtained (for example for the acquisition of other type of skills), an additional year in school may not result in higher earnings or a better labor market position.

To this end, I exploit the raising of minimum school leaving age from 14 to 15 in the Netherlands. The policy goal of this reform was to enable low ability students to obtain a high school diploma. The change in the compulsory schooling law was implemented in 1971 to make the duration of compulsory education equivalent to the duration necessary to complete the lowest track of secondary school. It extended the duration of compulsory schooling from 8 to 9 years without changing the curriculum. Because at that time the Netherlands applied a school entry rule with a cutoff date set at the first of October, I am able

[1] See section 2 for a short review of this literature.

to estimate the impact of this change in a regression discontinuity design. Individuals born on or after 1 October 1956 were the first to be affected by the change in the law. By using data on the educational level, and labor market position I can compare the outcomes of those born around this date. I exploit a unique dataset that includes information on earnings and employment over the period 2000-2009 and on social welfare benefits since the fifteenth birth day of the respondents. This enables me to estimate effects on these outcomes over a period that spans the respondents working life.

I find that the change led to a significant decrease in the high school dropout rate of approximately 20%. However, I do not find benefits in terms of employment or higher wages when the population is aged 43-53. I explore several explanations for this finding, and give some suggestive evidence in support of the skill-based explanation similar to that of Pischke and von Wachter (2008) that no more labor-market relevant skills were learned during this extra year of school compared to those skills previously learned out of school. I show that the change also did not lead to gains in literacy skills or labor market outcomes other than earnings.

I contribute to the current literature in two ways. First, I give a new piece of evidence that increases in compulsory education do not lead to better labor market outcomes. This is important as they are often considered as an effective policy to improve the labor market position of students of low ability. Second, I show that the differences in returns between countries are not necessarily explained by certification. In both the UK and the Netherlands the raising of the minimum school leaving led to more certificates, but only for the UK the change also led to higher earnings. Moreover, the UK- reform did also lead to better (literacy) skills whereas the Dutch reform did not. This suggests that differences between countries are more likely to be explained by differences in the type of skills learned in an additional year in school than by differences in certification.

The remainder of this paper proceeds as follows. In the next section I discuss the previous studies. Section 3 provides additional information on the reform. In section 4 I give a description of the data. Section 5 illustrates the empirical strategy and Section 6 shows results. In Section 7 I explore several explanations for the main findings, and Section 8 concludes.

2 Related literature

This paragraph discusses the literature that estimates the (financial) returns to changes in compulsory schooling laws.[2] In particular I focus on the returns to educations literature that uses increases in the minimum school leaving age as instrument for schooling in a Mincerian wage equation. However, I will not focus on the returns to schooling, but rather on the direct impact of the change of the law on earnings. This impact is implicitly estimated in reduced form regressions where an outcome for earnings is regressed on the instrument (for example a dummy for a birth cohort that has been first affected by the change in the law). I do so because the aim of this paper is to investigate what the effects are of changes in compulsory schooling laws, not what the effects are of extra schooling. Moreover, I focus on studies that use credible exogenous variation in the timing of implementation of these laws to estimate this impact. As pointed out by Grenet (2013) these studies use 'this type of variation across states or regions within a difference-in-difference framework or studies in which smoothly is controlled for the evolution of education and earnings across cohorts in a regression-discontinuity design'. This means, for example, that they adequately control for a smooth function in birth cohort when using the change of the compulsory schooling law as instrument for schooling. Hence, the reported effects below are the reduced form estimates from the returns to education literature with credible research designs. I will distinguish between Anglo-Saxon countries and continental European countries because the findings differ between these countries.

Anglo-Saxon countries

For the UK, USA and Canada positive returns have been found to changes in compulsory schooling. One of the reforms that have been studied most is the raising of the minimum school leaving age from 14 to 15 in the United Kingdom in 1947. This reform affected a large part of the British population and increased the number of years of schooling on average by about 0.4 years. This reform has been exploited by Oreopoulos (2006) and Devereux and Hart (2010) to estimate the returns to schooling. Depending on the specification and outcome measure used (log hourly wages or annual/weekly earnings), reduced form estimates of the effects of this change range from 1 to 7 %. In addition, Grenet (2013) evaluates the effects of

[2] There is also a large and fast growing body of research that uses changes of compulsory schooling laws to estimate its impact on outcomes of cognitive and non-cognitive skills, subjective well-being, health, crime, teenage childbearing and political involvement, see for example Grenet (2013) for a recent overview of this literature.

the raising of the minimum school leaving age from 15 to 16 in 1972 for the same country. He finds that this reform increased the average number of schooling years by about 0.3 years and that estimated returns are in the order of 2% using the log hourly wage as outcome measure. An important difference between the 1947-reform and the 1972-reform is that the latter was accompanied by certification,[3] whereas the former was not.

For the United States and Canada changes in the minimum school leaving age from 14 to 15 have been exploited by Oreopoulos (2006). He finds that the impact of these changes affected a smaller part of the population than in the UK as they increased the number of years of schooling on average by about 0.1 years. Estimated returns are also somewhat smaller and range between 1-2%.

Continental European countries

For Sweden, Germany, France and the Netherlands no returns are found to similar changes in compulsory schooling. Meghir and Palme (2005) evaluate a social experiment in Sweden where compulsory schooling was increased from seven or eight to nine years. Overall the change increased the years of schooling by about 0.3 years, but did not have an impact on earnings. Pischke and von Wachter (2008) use the extension of the duration of the basic vocational track by one additional year in Germany just after the Second World War. They find that the change increased the years of schooling by about 0.2 years but document zero returns. Grenet (2013) finds no returns for France, exploiting the Bedoin reform that was adopted in 1959, which raised the minimum school leaving age from 14 to 16 in 1967 and increased the years of schooling by about 0.3 years. Oosterbeek and Webbink (2007) find no returns to an extra year of basic vocational education in the Netherlands in which the extra year was accompanied by an increase of the minimum school leaving age from 15 to 16 in 1975.

These European studies show that it is not obvious that increases in duration of compulsory schooling would improve labour market outcomes. Moreover, the studies show a remarkable difference between the Anglo-Saxon countries and the countries from continental Europe. In the literature two different explanations are given for these differences. A first explanation, given by Grenet and also by Dickson and Smith (2011), is that it matters whether the additional year in school is accompanied by certification. Another explanation, given by Piscke and von Wachter, is that it matters whether relevant labor-market skills are

[3] Especially the number of junior secondary schooling certificates increased due to the 1972-reform, see Grenet (2013).

learned. The aim of this paper is to explore this issue further by exploiting a reform in the Netherlands that was specifically targeted at increasing the number of high school certificates. If certification is the leading explanation, an increase in the number of high school certificates should result in higher earnings and a better labor market position. If it matters what type of skills have been learned in school, this has not necessarily to occur: when no relevant (labor-market) skills are learned but only a certificate is obtained, an additional year in school may not result in higher earnings or a better labor market position.

It is important to note that the reform studied in this paper is different from the one studied by Oosterbeek and Webbink. Whereas they exploit the extension of the duration of the basic vocational track by one year in 1975 which was accompanied by the rise of the minimum school leaving age from 15 to 16, I exploit the 1971-rise of the minimum school leaving age from 14 to 15 which was not accompanied by a change in the length or content of the curriculum. Hence, the curriculum remained the same in the 1971-reform.

3 Reform & institutional background

The compulsory schooling law was changed in 1971, with the argument that the years of compulsory schooling needed to be adapted to the 'current requirements that society imposes upon those who will participate in the labor market' (*Memorie van Toelichting, 1968*). The duration of compulsory education was extended from 8 to 9 years. This was equivalent to raising the minimum school leaving age from 14 to 15 as the starting age of compulsory schooling remained unchanged at six.[4] The reason for the change was that the old law, with 8 years of compulsory schooling, did not cover the complete duration of any track that would lead to a secondary school certificate. Primary school took 6 years, and finishing the lowest track of secondary school took at least another 3 years. Hence, a minimum of 9 years was required to obtain a high school diploma. The policy document that addressed the preparation of the law stated this in the following way: "One of the basic ideas [...] is that after six years of primary school every pupil should have the possibility to receive both general and vocational education that suits his abilities and capacities the best as possible. To realize this idea, it is therefore necessary to extent the duration of compulsory schooling, as receiving a certificate of the lowest vocational track in secondary school (LBO) takes at least 3 years'' (*Memorie van Toelichting 1968*).

[4] More formally, the rule stated that a student had to go to school in the school year in which he became 7 years old.

At that time, the Dutch secondary education system was a highly differentiated system. After six years of primary school, students (aged 12) were tracked into one of the four tracks of the secondary school system. These tracks can be considered as different levels of education. The two lowest tracks, LBO and MAVO, offered students a basic vocational program. Finishing these tracks took at least 3 years.[5] The third track (HAVO) prepared students for high vocational training and took 5 years. The highest track (VWO) gave access to university and took 6 years.

The main purpose of the policy change was to enable students, who otherwise would have dropped out of school, to obtain a high school diploma. These students were in particular (low ability) students who after eight years in school were either in one of the two lowest tracks of the secondary school system (LBO or MAVO) or still in primary school because of (multiple) retention (*Memorie van Toelichting 1968*). The individuals who were first affected by the new law were born in October 1956: they were 14 years old at the end of the school year 1970/1971 and had to stay one year longer in school than individuals born before that date. This October cutoff was based on the rights and obligations for students to enroll in primary school. The Appendix will provide more information on this rule.

The result of the increase in the minimum school leaving age was an increase in the high school completion rate. Figure 1 shows the relationship between high school completion and birth month. Each dot represents the fraction that completed high school (y-axis) for a cohort of individuals born in a given month (x-axis). The cohort of students that was first affected by the law, the cohort born in October 1956 has been rescaled to 0. The discontinuity in the high school completion rate is visible at that point; the rate suddenly rises by 2 percentage points, from 90% to 92%. This suggests that the change in compulsory schooling law resulted in a decrease in the high school dropout rate from 10% to 8%, which means a reduction of school dropout by 20%. In the remainder of this paper, I will use this discontinuity to estimate the returns to the change in the compulsory schooling law in a regressions discontinuity framework.

[5] Until 1975 around half of all graduates from Dutch basic vocational schools finished a 3-years program, the other half finished a 4 years program. In 1975 all 3 years programs were extended to four years. This change is studied by Oosterbeek & Webbink (2007).

4 Empirical strategy

The aim of the empirical strategy is to estimate the causal effect of the change in the compulsory schooling law on education and labor market outcomes. I exploit the cutoff date of 1 October 1956 to estimate this impact. By using data on date of birth, education and labor-market position I can compare the outcomes of those born around this date. That is, I am able to evaluate the effect of the change in the compulsory schooling law in a regression discontinuity (RD)-framework [see Oreoupoulos (2006) or Grenet (2013), for examples].

The basic assumptions in RD-models are that individuals on both sides of the cutoff are very similar and that the relationship between date of birth and the outcome is smooth around the discontinuity. I will follow the standard approach as proposed by Lee and Lemieux (2010) by estimating the following equation for each outcome Y_i,

(1) $Y_i = \alpha_0 + \alpha_1 OCT1956_i + f_g(Z_i) + f_g(Z_i) * OCT1956_i + \alpha_2 \boldsymbol{X}_i + \varepsilon_i$

Herein is $OCT1956_i$ a dummy for being born on or after 1 October 1956 and $f_g(Z_i)$ a smoothing polynomial of order g in birth month Z_i, with Z_i=0 referring to October 1956. I include a linear (g=1), square (g=2), cubic (g=3), quartic (g=4), quintic (g=5) and sextic (g=6) function of Z_i in my models and interactions of $OCT1956_i$ with this function ($f_g(Z_i) * OCT1956_i$). By including the interactions I allow the polynomial to be different at either side of the cutoff. By increasing the order, the polynomial should become flexible enough to pick up nonlinear age effects.[6] In the analysis I use the Akaike Information Criterion to choose the optimal order of the polynomial [see Lee and Lemieux (2010)]. $\boldsymbol{X}_i$ is a vector of dummies for gender and ethnicity. Equation (1) is estimated by OLS and standard errors are adjusted for heteroskedasticity and clustering at the birth month level. The main parameter of interest is $\alpha_1$which represents the effect of the change in the compulsory schooling law, i.e. the effect of being born after the cutoff date implying 9 years of compulsory schooling in stead of 8 years. I can only interpret this coefficient as such if I may assume that the cohorts born on or after 1 October 1956 are not confronted with other changes in laws or regulations compared to those born before this date. To my knowledge there were no other laws implemented that affected this birth cohort.

[6] Assuming that E[$\varepsilon_i|Z_i$], the conditional expectation of the unobserved determinants of $Y_i$given the birth month, is continuous, we can approximate it by a polynomial of order g, and the approximation will become arbitrarily accurate as g goes to infinity ($g \rightarrow \infty$) [Cellini et al., 2010].

5 Data

The data for the analysis come from the Dutch Labour Market Panel [*Arbeidsmarktpanel (Centraal Bureau voor de Statistiek)*], which is a large panel dataset constructed by Statistics Netherlands. The sample is representative for the Dutch population. It contains information about the labor-market position of about 1.2 million Dutch inhabitants over the years 1999-2009. The data contain information on completed education, date of birth, income, employment, and social security dependency. I restrict the sample to those individuals who are born between October 1946 and September 1966. This means that I have 10 year cohorts that were and 10 year cohorts that weren't affected by the change of the compulsory schooling law. This yields a sample of approximately 390,000 observations. In the analysis I will exploit all years except 1999 because no information on gross hourly wage is available for this year. Also, I focus the analysis on the period 2006-2009 because for the earlier years there are more missing values on this variable.[7] Other outcomes (see below) have been consistently measured over the period 2000-2009.

In the main analysis, I use as dependent variables three measures: (1) high school completion (2) a dummy for whether the respondent is employed in a given year and (3) log gross hourly wage in a given year. High school completion is a dummy that equals 1 if the respondent completed at least the lowest track of secondary school, LBO (equivalent to 9.5 years of formal schooling)[8] and 0 if the respondent only finished primary school (equivalent to 6 years of formal schooling). I use this variable rather than completed years of schooling because the aim of the law was to enable students to obtain a high school certificate in the lowest tracks of secondary school. The second measure, a dummy for working, is used because for low ability students one of the benefits of having a high school degree is that they are able to acquire a job rather than to earn more conditional on having a job.

Besides these main outcomes I will use dummies for being self-employed, and dummies for participation in social welfare, unemployment and disability benefits. In addition, I will use three labor-market related measures that cover a larger period of the respondents working life. A special feature of the Dutch Labor Market Panel is that it provides information about 1) the number of years that people had a paid job since age 15, 2) the number of years they were unemployed since age 15 and 3) the number of years they

[7] I loose about half the number of observations for hourly wage over the period 2000-2005 which may raise concerns about the representativeness of this sample.
[8] Based on the fact that about half of students finished a 3-year track and the other half a 4-year track in secondary education at that time. Hence, 6+(3+4)/2=6+3.5=9.5.

received disability benefits since age 15. These outcomes were obtained by surveys in which respondents were asked to retrospectively evaluate their employment status since their fifteenth birthday. These variables will be exploited in section 7 when I look at the impact of the change on these outcomes.

As independent variable I use a dummy that equals 1 if the respondent is born on or after 1 October 1956 (hence aged 53 years or less in 2009) and 0 if he/she is born before that date.

Also, covariates are added to the models. In a regression discontinuity framework the most important covariate is the 'forcing' variable. In my case, this is birth month. I rescale the birth month of the cohort that was first affected by the law to zero. That is, I assign a zero to the individuals born in October 1956. This means that those born in September 1956, August 1956 are assigned -1, -2, etcetera, and those born in November 1956, December 1956 are assigned 1, 2 and so on (as in figure 1). Other covariates in the models are ethnicity and gender.

In table I, I present descriptive statistics (i.e. averages) of the main outcomes and covariates for each birth cohort born between October of a given year and September next year. The discontinuity in high school completion is clearly visible: the rate rises from 0.90 to 0.92 if one switches from the year-cohort born before the cutoff date to the cohort born thereafter. However, there seems not to be a discontinuity in log gross hourly wage and employment; the values of these variables after the cutoff date are not much different from those before. This suggests that the change in the law did not have an impact on these outcomes.

6 The impact of the reform on education, employment and earnings

This section presents the estimates of the impact of the raising of the minimum school leaving age from 14 to 15 on high school completion, employment and earnings based on equation (1). It consists of two parts. In the first part the main estimates are shown for the outcomes measured in the last year of my dataset, 2009 (when the respondents born in October 1956 are about 53 years old). With the outcomes from this year various robustness analyses are performed with two estimation samples, the total sample and the low track sample, see below. In the second part estimates are given for each year over the period 2000-2009 for both estimation samples using the preferred specification. Hence, in this part I estimate the impact of the change in the law separately on the 2000-outcome, the 2001-

outcome, 2002-outcome and so forth. These estimates will give the complete earnings and employment profiles for the population aged 43-53.

Part I: The effect of the reform on 2009-outcomes

Total sample

Table II presents estimates of the change of the compulsory schooling law on high school completion (panel A), employment (panel B), and earnings (panel C)[9] for the total sample. Columns (1)-(6) include a linear, square, cubic, quartic, quintic and sextic function of birth month respectively. At the bottom of each panel the Akaike Information Criterion (AIC) is reported which is a goodness-of-fit test on which we base the order of the polynomial that should be included. The lower the AIC, the better the fit is. In almost all analyses this statistic is smallest for third degree polynomials or higher. This indicates that at least a cubic should be included to let the polynomial to be flexible enough for our RD-design.[10] Hence, results based on linear and squares in birth month should not be considered reliable.

Panel A shows that the rise of the minimum school leaving age from 14 to 15 significantly increased the level of schooling. The estimates are robust to the inclusion of different orders of the birth month polynomial. Based on the most flexible specification in column (6), I find that the fraction of individuals that obtained a high school certificate has increased with about 1.8 percentage points. This is equivalent to an increase of about 0.1 years of schooling, which is about the same size as the impact of the changes in compulsory schooling laws on education in Canada and the USA (see section 2). However, in contrast to these Anglo-Saxon countries the change in the compulsory schooling law did not lead to an increase in earnings or a higher probability of being employed in 2009. The estimated coefficients in panels B and C are precise, close to zero and insignificant. For earnings the estimated coefficients are even negative in all specifications; the point estimate in column (5) indicates that the change in the law decreased income by 1%. Hence, the change in the compulsory schooling law caused an increase in the level of schooling but did not lead to higher wages.

[9] The estimation sample for earnings is smaller than for being employed, because information on earnings is only available for employed individuals.

[10] The estimated coefficients of the polynomial are highly significant when using a linear or square function of birth month. The estimated coefficients turn insignificant when including a third order or higher.

Low track sample

The purpose of the change in the law was to enable low ability students to obtain a high school certificate. These students were in particular students that after eight years of schooling were in one of the two lowest tracks of the secondary school system, LBO or MAVO. Hence, one may expect that this group would benefit most from the new law. One way of investigating this is to look at the instrumental variables estimates of the returns to high school completion. They would give the returns to high school completion for those who comply with the change, such that one could obtain an idea of whether those affected would have (financially) benefited from the change of the law. These estimates are easily obtained by dividing the reduced form coefficients in panels B or C by the first stage coefficients from panel A.[11] However, as the reduced form estimates are close to zero and insignificant (and for earnings even negative), these instrumental variables estimates will be little informative.

To investigate whether low ability students may have benefited from the law, I focus on a subsample of the total sample, consisting of individuals who reported that their highest level of completed education is at most a certificate of one of the two lowest tracks of secondary school (LBO or MAVO). The individuals in this sample either obtained a LBO/MAVO high school diploma or only finished primary school. Hence, individuals with a diploma higher than MAVO are excluded. I define this sample as the low track sample (see table A.1 and figure A.1 in the Appendix for descriptive statistics and a discontinuity graph).

A possible concern with this approach is that this sample is selected on the outcome. Such a selective sample may give biased results because I exclude individuals that could have obtained certificates higher than MAVO through the change in the law. However, this seems unlikely to be a problem as at that time low ability students in the lowest tracks of the secondary school system (LBO, MAVO) often did not end up in higher tracks.

Table III presents estimates of the change in the compulsory schooling law on the main outcomes for the low track sample. As expected, the estimated coefficient for high school completion is larger in this sample than in the total sample (panel A). Based on the most flexible specification in column (6), high school completion among low ability students increased 5 percentage points through the change in the compulsory schooling law (equivalent to an increase of about 0.2 years of schooling). Again, however, I do not find evidence that the change resulted in higher earnings or higher probability to be employed; the

[11] Estimates and standard errors are obtained by an instrumental variables approach to a Mincerian wage equation in which high school completion is instrumented by the dummy for being born on or after 1 October 1956. Estimates in panel A in tables II and III should then be considered first stage estimates. The F-statistic of the first stages are above 10 (they range between 12 and 16), indicating that there is no weak instrument problem.

estimates in panels B and C are not significantly different from zero. Hence, the change in the compulsory schooling law increased the level of schooling for students of low ability, but the change has not been financially beneficial for them.

Robustness

Estimates from a regression discontinuity design could be sensitive to the window of the estimation sample chosen and the degree of the smoothing polynomial included [see Lee and Lemieux (2010)]. In this section I investigate to what extent my estimates change when I restrict the sample to 5 and 3 years distance to the cutoff and use a square and linear function of birth month as smoothing polynomial respectively.

The results of my robustness analysis are shown in table IV. In panel A I report the results of regressions where I use the sample of individuals that are born between October 1951 and September 1960, which is a 5 year distance to the cutoff. In this estimation sample I include a square function of birth month. In panel B I restrict the sample further to individuals that are born between October 1953 and September 1959, which is a 3 year distance to the cutoff. Because of the smaller sample size, I only include a linear term in birth month. By using this estimation window, I exclude individuals that also were affected by the 1975-reform studied by Oosterbeek and Webbink.

The estimates in panel A and B are given for the total sample and the low track sample. Table IV shows that the main results do not change: also when using different estimation windows and smoothing polynomials the estimates are about the same size for high school completion and insignificant for outcomes employed and earnings. In the next part I will give the estimates for each year over the period 2000-2009, thereby showing employment and income profiles.

Part II: Employment and Income profiles over 2000-2009

The outcomes used in tables II and III come from the year 2009 when the respondents who were first affected by the law (born in October 1956) are about 53 years old. Bhuller, Mogstad and Salvanes (2012) point out that estimates may differ at different ages because of life cycle bias. In addition, the effect on lifetime income may be different from the effect on income measured at a particular age. Perhaps respondents gained from the change in the law earlier in life, and these gains are not reflected in what is measured in 2009.[12] Therefore, I will investigate returns at other ages by estimating the impact of the change of the law on employment and earnings for each year (2000, 2001, 2002, etc) in the dataset. Hence, I estimate the effect of the change in the law on earnings and employment when those born in October 1956 are 43 years old in 2000, 44 years old in 2001 and so on. The results of these analyses are summarized in figures 2 and 3. I call these figures the employment and income profiles. The y-axis represents the estimated effect (solid line) and the 95% confidence interval (dotted lines). The x-axis represents the year used in the estimation, which ranges from year 2000 to 2009. Estimates are based on the sample with a 5 year distance to the cutoff and a square in birth month that differs at either side of the cutoff. The order of the polynomial is based on the Akaike Information Criterion.[13] The estimates are robust to the various robustness analyses presented in the first part.

The figures show that the estimates for employment and earnings (solid line) are not significantly different from zero in all years 2000-2009: over this period the zero is covered by the 95% confidence interval (dotted lines). This holds for both the total and the low ability sample.[14] For the total sample, the estimates for income are even negative in most years. The 95% confidence interval shows that positive effects of 1% can be excluded. In comparison, for Anglo-Saxon countries returns have been found between 1 and 7% (see section 2). Hence, the results for the Netherlands are different from those for the Anglo-Saxon countries and do not depend on the year the outcomes have been measured, nor on the estimation window used or the order of the birth month polynomial included.

7 Why zero returns?

Why did the raising of the minimum school leaving age from 14 to 15 in the Netherlands not lead to higher wages while researchers do find returns in the Anglo-Saxon countries when exploiting similar changes? In this section, I investigate three possible explanations.
First I investigate whether the zero returns are a result of possible wage rigidity differences between the Netherlands and the Anglo-Saxon countries. Second, I investigate two other explanations that have previously been given in the literature: whereas Grenet postulates that it matters whether the additional year in school is accompanied by certification, Pischke and

[12] Because 2009 is a recession year, one could argue that in particular in recessions persons would benefit from extra schooling such that we should have seen returns to extra schooling especially in this year.
[13] In many estimations with this sample (i.e. the 5-year distance to the cutoff sample), the AIC was smallest when using a square function of birth month.
[14] Except for employed in the low track sample in 2002

von Wachter postulate that it matters whether labor-market relevant skills are learned in the extra year.

1) Wage rigidity?

An explanation for not finding returns could lie in the wage setting institutions in the Netherlands, which, in contrast to those in the UK or USA, may prevent the adjustments necessary to reflect any returns. For example, the fraction of workers that is covered by collective bargaining agreements is much higher in the Netherlands than in the UK or USA: 82% versus 35 or 12% respectively (OECD, 2009).[15] This could mean that productivity differences between individuals are not reflected in earnings because negotiated wages are kept higher than the market-clearing level wages. In that case the change in the compulsory schooling law may have contributed to the productivity of the individuals who were affected, but this higher productivity is not reflected in their wages. I give some suggestive evidence that may rule out this possibility.

First, because employers should be more willing to employ the more productive individuals with the extra schooling than the equally expensive individuals without the extra schooling, it might be expected that employers hire them more often. But as shown in the previous section, the estimated effect of the change in the compulsory schooling law on being employed is not significantly different from zero.

Second, if individuals notice that their extra skills are not recognized or rewarded by employers, they may decide to start their own business. In that case, it might be expected that they would be self-employed more often. However, estimates of the impact of the reform on a dummy for self-employed are also not significantly different from zero, see figure 4. This suggests that, although the change in the compulsory schooling led to a higher educated population, it did not lead to a more productive one. Pischke and von Wachter (2007) and Grenet (2013) perform similar analyses for their countries and also find no effects on being (self-)employed, suggesting that wage rigidity is also not important for explaining the zero returns for Germany and France.

[15] http://www.oecd.org/els/emp/43116624.pdf.

2) What matters most: Certification or Skills?

Another possible explanation for not finding returns is that the additional year in school was not accompanied by academic credentials (i.e. certifications). This explanation is given by Grenet. In his paper, he finds no returns for France when exploiting the Bedoin Reform which raised the minimum school leaving age by 2 years from 14 to 16 in 1967, whereas he does find returns for the UK in the 1972-reform that increased the minimum school leaving age by only one year (from 15 to 16). Because the latter was accompanied by a certificate, and the former not, he argues that the certification differences between the countries could explain the differences in returns.

However, the results in the previous sections suggest that it is not certification that explains the zero returns. In both the UK and the Netherlands the raising of the minimum school leaving age led to more certificates, but only for the UK the change also led to higher earnings. A reason for this difference might be that the group of people affected by the 1971-ROSLA from 14 to 15 in the Netherlands was smaller than the group of people affected by the 1972-ROSLA from 15 to 16 in the UK (+0.1 years of schooling versus +0.3 years of schooling). However, the impact of the reform on education in the Netherlands was about the same size as in the USA and Canada where positive effects of increases in compulsory schooling on earnings have been found. Hence, one might expect that estimates of the effect of the Dutch reform on earnings would lie in the same ballpark of the estimates found for these countries. But as shown in figure 3, effects larger than 1% can be excluded.

Further suggestive evidence that certification alone does not explain the zero results may come from a comparison between the two reforms in the UK: the 1947-ROSLA from 14 to 15 (without certificate) and the 1972-ROSLA from 15 to 16 (with certificate). Irrespective of whether the additional year was accompanied by academic credentials, the reforms led to both more cognitive skills and positive financial returns, suggesting that skills are important.[16] Hence, a comparison of these reforms might be more in favor of a skill-based explanation than of an explanation that solely or heavily relies on certification.

Such a skill based explanation is given by previous studies that find zero returns to increases in compulsory education. Pischke and von Wachter give some suggestive evidence for this explanation in their study for Germany. They show that German individuals perform much better on quantitative and mathematics test than people in a similar position in the UK,

[16] See Banks and Mazonna (2012) for the effect of the 1947-reform on cognitive skills and Grenet (2013) for the effect of the 1972-reform on literacy skills.

Canada or USA, thereby suggesting that most labor-market relevant skills were learned earlier in the education system in Germany than in the Anglo-Saxon countries. This would mean that children did not learn more relevant skills in the additional year in school than in the year out of school. Oosterbeek and Webbink follow the same line of reasoning for the Netherlands, which is a country very similar to Germany, and also come up with this skill based explanation for the zero returns to the Dutch 1975-reform.

In this paper, which is also set in the Netherlands, I will add a few pieces of evidence that may support this skill based view. First, I investigate whether the raising of the minimum school leaving age from 14 to 15 led to higher cognitive skills as measured by the International Adult Literacy Survey (IALS) in 1994 (when those born in October 1956 are about 38 years old). Table V presents reduced form regressions of the impact of the reform on three measures of literacy skills (see figures A.2-A.4 for graphs). As can be seen, the estimates are not significantly different from zero. It should be noted, though, that the sample size is limited, which may make it difficult to detect any small effects of the reform on these skills. However, the estimates do rule out modest or large impacts.[17] This stands in contrast with the UK-reforms in which positive effects on cognitive skills have been found.

Second, I investigate whether the reform led to gains in other labor-market outcomes than earnings. If there were any (small) effects of the reform on relevant labor-market skills, one might expect that the change would affect other labor-market outcomes such as the probability to live on disability benefits.[18] In figures 4-8 I present estimates of the impact of the reform on dummies for participation in social welfare, disability and unemployment benefits. The estimates of the effects of the change in the compulsory schooling law on these three measures are insignificant in all years.

Finally, I investigate whether individuals may have gained from the change before age 43, as estimated effects on outcomes only go back as far as the year 2000 (when those born in October 1956 are about 43 years old). One might argue that the gains from the change in the law are internalized in their adolescent years, meaning that the outcomes measured in 2000-2009 do not capture the total number of years being (un)employed or having a paid job since the respondents left school. As pointed out in the data section, a nice feature of the Dutch Labour Market Panel is that it provides information on the number of years that people had a paid job, were unemployed and living on disability benefits since their fifteenth

[17] The raw scores have been used as outcome measure. If I translate these effect sizes in percentage of standard deviations I am able to rule out effects larger than 0.2σ using the 95% confidence interval.
[18] The acquisition of those skills may lead to other type of jobs that may be less stressful. This in turn may reduce the risk to become disabled.

birthday. Regressions results with these three outcomes are presented in columns (1)-(3) of table VI. They show that the change in the compulsory schooling law did not result in more years of having a paid job, less years of being unemployed or less years of living on disability benefits since age 15. Hence, the evidence presented in this section suggests that the change in the compulsory schooling law did not lead to higher literacy skills or to better labor-market outcomes in general. This can be interpreted as evidence supporting a skill-based explanation for the zero returns found in this paper.

8 Conclusions

In this paper I evaluated the raising of the minimum school leaving age from 14 to 15 in the Netherlands in 1971. The policy goal of this change was to increase the number of high school certificates. The analysis showed that the change reduced the high school dropout rate by approximately 20%. However, there were no benefits in terms of employment or higher wages when the population was aged 43-53. These results are consistent with previous work from continental Europe that documents zero returns to compulsory schooling, but stands in contrast with that from Anglo-Saxon countries in which positive returns are found. As a reason for not finding returns I give some suggestive evidence in support of the skill-based explanation that no more labor-market relevant skills were learned during this extra year of school compared to those skills previously learned out of school. I show that the change also did not lead to gains in literacy skills, nor in labor market outcomes other than earnings.

The contributions of these results to the current literature will be twofold. First, I give an additional piece of evidence that changes in compulsory schooling might not be effective in improving the productivity of the population. Second, I show that differences between countries are more likely to be explained by differences in the type of skills learned in an additional year in school than by differences in certification. In both the UK and the Netherlands the raising of the minimum school leaving led to more certificates, but only for the UK the change also led to higher (literacy) skills and earnings.

Taken together, this suggests that (further) increasing compulsory schooling may be ineffective if no labor-market relevant skills are learned in the additional schooling year(s) and the goal is to improve the productivity of the population.

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Tables

Table I: Descriptives for 2009-outcomes and covariates, total sample

Birth cohort	high school completion	log hourly wage	log annual earnings	employed	self-employed*	disability benefits	unemployment benefits	welfare benefits	years having paid job since age 15	years unemployed since age 15	years living on disability benefits since age 15	Dutch**	female	N
Not affected by the compulsory schooling law														
October 1946 - September 1947	0,861	2,864	9,611	0,260	0,181	0,113	0,016	0,031	17,433	0,551	1,590	0,875	0,510	18856
October 1947 - September 1948	0,866	2,895	9,873	0,392	0,143	0,109	0,018	0,031	17,500	0,586	1,489	0,873	0,500	18358
October 1948 - September 1949	0,867	2,904	10,031	0,496	0,118	0,102	0,020	0,030	17,556	0,580	1,429	0,869	0,493	17680
October 1949 - September 1950	0,870	2,915	10,134	0,600	0,102	0,097	0,024	0,030	17,616	0,641	1,423	0,867	0,490	17184
October 1950 - September 1951	0,880	2,921	10,175	0,646	0,101	0,095	0,022	0,028	17,696	0,637	1,303	0,864	0,499	17439
October 1951 - September 1952	0,886	2,922	10,181	0,680	0,099	0,084	0,021	0,028	17,754	0,674	1,166	0,865	0,499	17913
October 1952 - September 1953	0,889	2,914	10,195	0,702	0,098	0,080	0,019	0,025	17,589	0,686	1,116	0,866	0,505	18198
October 1953 - September 1954	0,895	2,918	10,218	0,724	0,098	0,074	0,020	0,027	17,632	0,716	1,081	0,866	0,503	18424
October 1954 - September 1955	0,900	2,917	10,233	0,752	0,099	0,062	0,017	0,025	17,628	0,700	0,948	0,868	0,505	18816
October 1955 - September 1956	0,897	2,916	10,234	0,759	0,095	0,061	0,020	0,025	17,542	0,757	0,910	0,861	0,500	19180
Affected by the compulsory schooling law														
October 1956 - September 1957	0,918	2,917	10,241	0,779	0,097	0,056	0,018	0,024	17,486	0,748	0,846	0,860	0,506	19547
October 1957 - September 1958	0,922	2,907	10,240	0,793	0,102	0,051	0,016	0,023	17,372	0,768	0,825	0,861	0,506	19444
October 1958 - September 1959	0,922	2,913	10,245	0,794	0,101	0,047	0,019	0,025	17,218	0,758	0,737	0,855	0,510	20600
October 1959 - September 1960	0,920	2,897	10,238	0,801	0,101	0,043	0,018	0,024	16,966	0,764	0,657	0,844	0,510	20440
October 1960 - September 1961	0,929	2,895	10,239	0,809	0,100	0,041	0,019	0,023	16,704	0,776	0,679	0,856	0,513	20692
October 1961 - September 1962	0,928	2,890	10,219	0,811	0,105	0,037	0,018	0,025	16,467	0,744	0,640	0,848	0,512	20924
October 1962 - September 1963	0,929	2,885	10,213	0,821	0,107	0,036	0,017	0,022	16,164	0,739	0,572	0,842	0,508	21089
October 1963 - September 1964	0,936	2,888	10,218	0,824	0,108	0,034	0,018	0,022	15,791	0,736	0,536	0,837	0,506	20980
October 1964 - September 1965	0,939	2,880	10,203	0,820	0,109	0,034	0,018	0,023	15,307	0,707	0,501	0,832	0,507	21107
October 1965 - September 1966	0,943	2,880	10,192	0,821	0,111	0,030	0,018	0,021	14,879	0,663	0,480	0,832	0,508	20661

* Conditional on being employed. **The Dutch definition *autochtoon* is used: both parents were born in the Netherlands. In our regressions we also distinguish between Moroccan, Turkish, Surinam and Antilles

Table II: Estimated effects on 2009-outcomes, total sample

Panel A	**Dependent variable: high school completion**					
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for individual born in or after October 1956	0.0105***	0.0142***	0.0178***	0.0156***	0.0154***	0.0183***
	(0.00180)	(0.00271)	(0.00346)	(0.00412)	(0.00497)	(0.00578)
Birth month controls	linear	square	cubic	quartic	quintic	sextic
Observations	387532	387532	387532	387532	387532	387532
R-squared	0.055	0.055	0.055	0.055	0.055	0.055
Akaike Information Criterion (AIC)	122084.5	122084.1	122081.7	122084.3	122088.3	122089.6
Panel B	**Dependent variable: employed 2009**					
Dummy for individual born in or after October 1956	-0.0742***	0.0427***	0.00395	-0.00467	0.00524	0.00599
	(0.0112)	(0.00712)	(0.00666)	(0.00874)	(0.0115)	(0.0138)
Birth month controls	linear	square	cubic	quartic	quintic	sextic
Observations	387532	387532	387532	387532	387532	387532
R-squared	0.152	0.159	0.159	0.159	0.160	0.160
Akaike Information Criterion (AIC)	423237.9	419869	419640.7	419640.1	419610.3	419613.3
Panel C	**Dependent variable: earnings 2009**					
Dummy for individual born in or after October 1956	-0.00919**	0.0121**	-0.00264	-0.00349	-0.0114	-0.00239
	(0.00379)	(0.00547)	(0.00671)	(0.00832)	(0.00967)	(0.0117)
Birth month controls	linear	square	cubic	quartic	quintic	sextic
Observations	248081	248081	248081	248081	248081	248081
R-squared	0.103	0.104	0.104	0.104	0.104	0.104
Akaike Information Criterion (AIC)	287383.3	287339.3	287332.3	287335.9	287337.2	287339.2

Notes: Each cell is an OLS-regression. All regressions include dummies for gender and ethnicity. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustering at the birth month level .*** p<0.01, ** p<0.05, * p<0.1

Table III: Impact of 1971-ROSLA on 2009-outcomes, low track sample

Panel A	**Dependent variable: high school completion**					
Independent variable:	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for individual born in or after October 1956	0.0279***	0.0356***	0.0441***	0.0490***	0.0495***	0.0507***
	(0.00489)	(0.00752)	(0.00953)	(0.0110)	(0.0122)	(0.0129)
Birth month controls	linear	square	cubic	quartic	quintic	sextic
Observations	125767	125767	125767	125767	125767	125767
R-squared	0.057	0.057	0.057	0.057	0.057	0.057
Akaike Information Criterion (AIC)	150421.3	150423.2	150423.4	150425.7	150429.1	150429.7
Panel B	**Dependent variable: working 2009**					
Dummy for individual born in or after October 1956	-0.0603***	0.0399***	0.00917	-0.00209	0.0161	0.0119
	(0.0106)	(0.00905)	(0.0105)	(0.0135)	(0.0164)	(0.0199)
Birth month controls	linear	square	cubic	quartic	quintic	sextic
Observations	125767	125767	125767	125767	125767	125767
R-squared	0.159	0.164	0.164	0.164	0.164	0.164
Akaike Information Criterion (AIC)	157332.3	156618	156587.2	156585.7	156573.9	156577.6
Panel C	**Dependent variable: earnings 2009**					
Dummy for individual born in or after October 1956	-0.0136**	0.0162**	0.00156	0.0140	0.0223	0.0165
	(0.00585)	(0.00755)	(0.0101)	(0.0124)	(0.0152)	(0.0182)
Birth month controls	linear	square	cubic	quartic	quintic	sextic
Observations	66443	66443	66443	66443	66443	66443
R-squared	0.117	0.118	0.118	0.118	0.118	0.118
Akaike Information Criterion (AIC)	45810.34	45763.7	45763.1	45764.54	45765.8	45769.5

Notes: Each cell is an OLS-regression. All regressions include dummies for gender and ethnicity. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustering at the birth month level .*** p<0.01, ** p<0.05, * p<0.1

Table IV: Impact of 1971-ROSLA on 2009-outcomes for various estimation windows

	Dependent variable:		
	high school completion	employed 2009	earnings 2009
Independent variable:	(1)	(2)	(3)
Panel A: 5 year distance to cutoff			
Total sample			
Dummy for individual born in or after October 1956	0.0161***	0.00665	-0.000
	(0.00378)	(0.00701)	(0.00699)
Observations	193254	193254	132705
Low track sample			
Dummy for individual born in or after October 1956	0.0472***	0.0102	0.00794
	(0.0104)	(0.0112)	(0.0106)
Observations	61245	61245	35767
Panel B: 3 year distance to cutoff			
Total sample			
Dummy for individual born in or after October 1956	0.0154***	0.00518	-0.00232
	(0.00316)	(0.00594)	(0.00604)
Observations	116011	116011	80285
Low track sample			
Dummy for individual born in or after October 1956	0.0441***	0.0106	0.00893
	(0.00876)	(0.00988)	(0.00893)
Observations	36442	36442	21437

Notes: Each cell is an OLS-regression. The regressions in panel A include a square in (rescaled) birth month and interactions of the dummy for individual born in or after October 1956 with this square. In panel B it is a linear term. All regressions include dummies for gender and ethnicity. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustering at the birth month level .*** p<0.01, ** p<0.05, * p<0.1

Table V: Impact of 1971-ROSLA on literacy skills from Adult Literacy Survey 1994

	Dependent variable:		
Independent variable:	prose literacy score	document literacy score	quantitative literacy score
Dummy for being born in or after 1957	-0.762	-0.943	-0.315
	(4.066)	(4.893)	(5.055)
Observations	1517	1517	1517
R-squared	0.018	0.018	0.010

Notes: Each cell is an OLS-regression. The regressions include a linear term of rescaled birth year (with 0 referring to birth year 1957) and the interaction of the dummy for being born in or after 1957 with this term. Birth year is constructed by subtracting age from the survey year, hence 1994-age. Sample is restricted to those born between 1946 and 1966. All regressions include a dummy for gender. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustering at the birth year level .*** p<0.01, ** p<0.05, * p<0.1

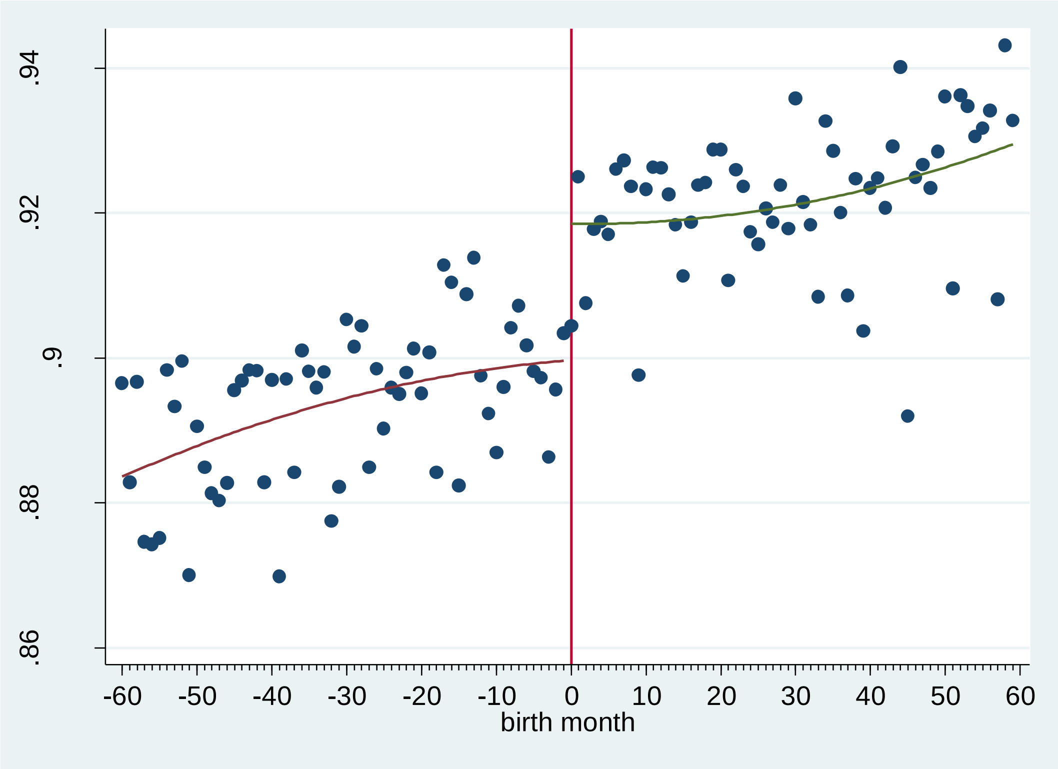
Table VI: Impact of 1971-ROSLA on employment and living on disability benefits since age 15 of the respondent

	Years having job since age 15	Years unemployed since age 15	Years disabled since age 15
Independent variable:	(4)	(5)	(6)
<u>Panel A: 5 year distance to cutoff</u>			
Total sample			
Dummy for individual born in or after October 1956	-0.0448	0.00498	0.0665
	(0.0553)	(0.0323)	(0.0406)
Observations	186105	190841	190958
Low track sample			
Dummy for individual born in or after October 1956	0.0542	-0.0910	0.106
	(0.0975)	(0.0641)	(0.0755)
Observations	56858	59861	59901
<u>Panel B: 3 year distance to cutoff</u>			
Total sample			
Dummy for individual born in or after October 1956	-0.00388	0.0106	0.0327
	(0.0471)	(0.0275)	(0.0342)
Observations	111845	114581	114622
Low track sample			
Dummy for individual born in or after October 1956	0.0637	-0.0682	0.0902
	(0.0811)	(0.0565)	(0.0644)
Observations	33910	35627	35626

Notes: Each cell is an OLS-regression. The regressions in panel A include a square in (rescaled) birth month and interactions of the dummy for individual born in or after October 1956 with this square. In panel B it is a linear term. All regressions include dummies for gender and ethnicity. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustering at the birth month level .*** p<0.01, ** p<0.05, * p<0.1

Figures

Figure 1: High school completion by birth month



Each dot represents the fraction that completed high school for a cohort of individuals born in a given month. The x-axis ranges from October 1951 (-60) to September 1960 (59). The cohort born in October 1956 is represented by 0 on the x-axis. Quadratic polynomial fitted at either side of the cutoff.

FIGURES 2-8: Each estimate in figures 2-8 is based on an OLS-regression of the outcome on a dummy for being born in or after October 1956 and a square function in birth month. Covariates in the model are ethnicity and gender. Standard errors have been adjusted for heteroskedasticity and clustering at the birth month level.

Figure 2: The impact of 1971-ROSLA on being employed in years 2000-2009

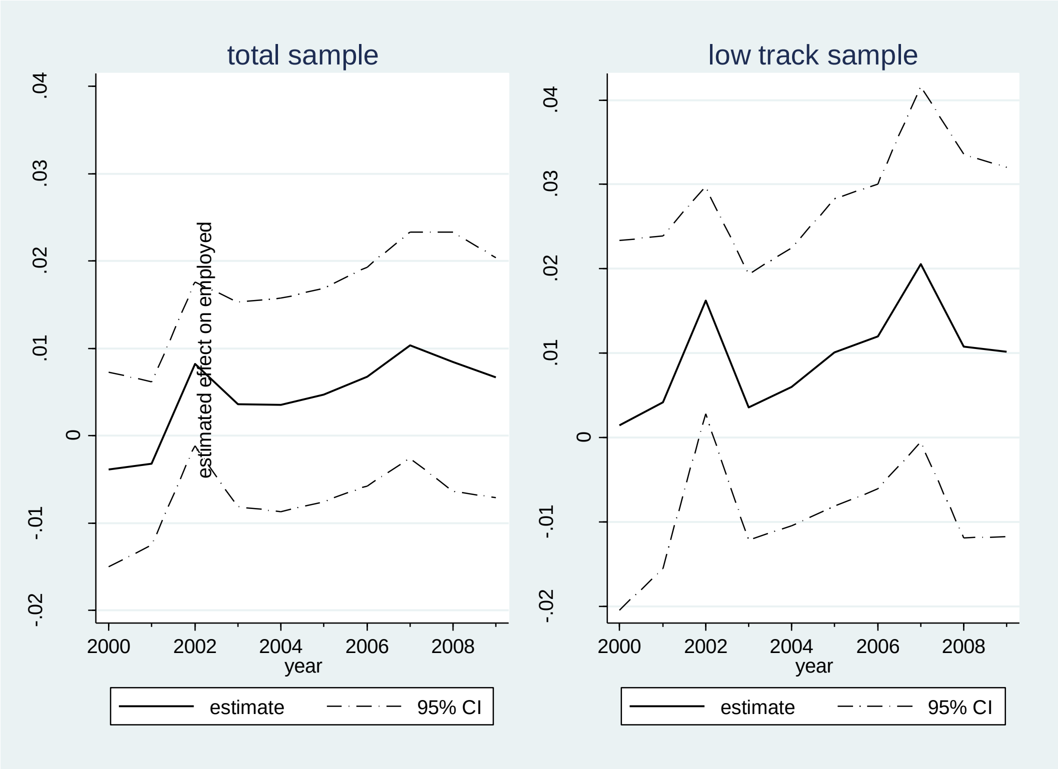


Figure 3: The impact of 1971-ROSLA on log hourly wage in years 2000-2009

Figure 4: The impact of 1971-ROSLA on being self-employed in years 2000-2009 (conditional on being employed).

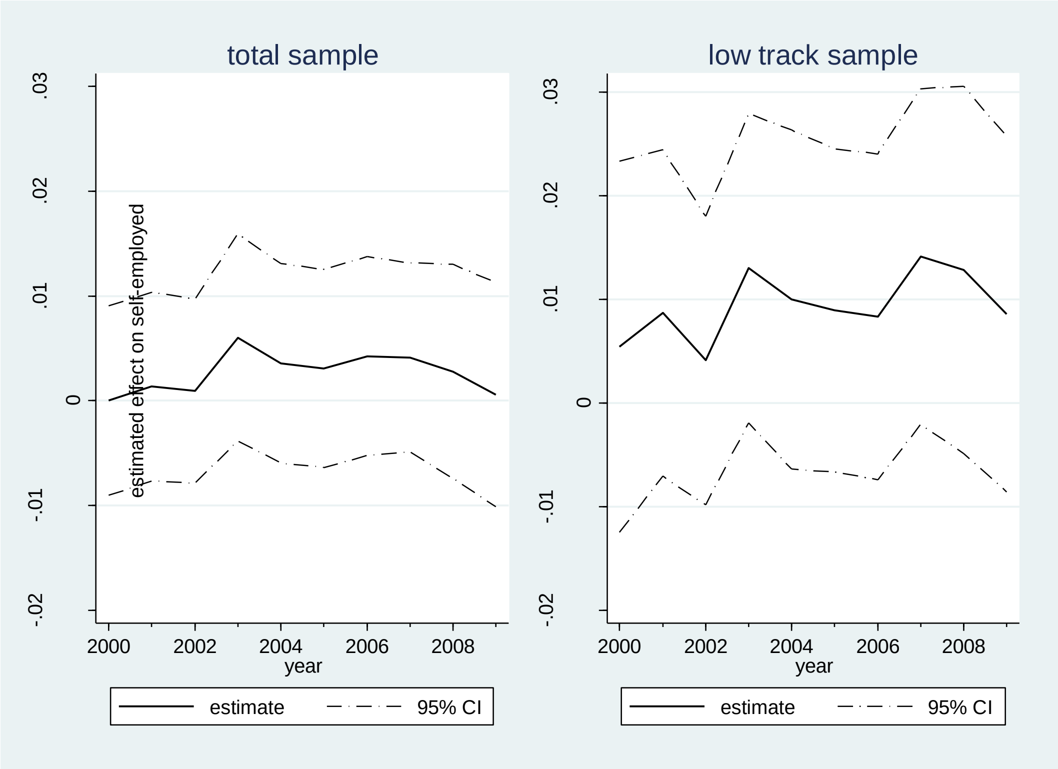


Figure 5: The impact of 1971-ROSLA on having disability benefits in years 2000-2009

Figure 6: The impact of 1971-ROSLA on having unemployment benefits in years 2000-2009

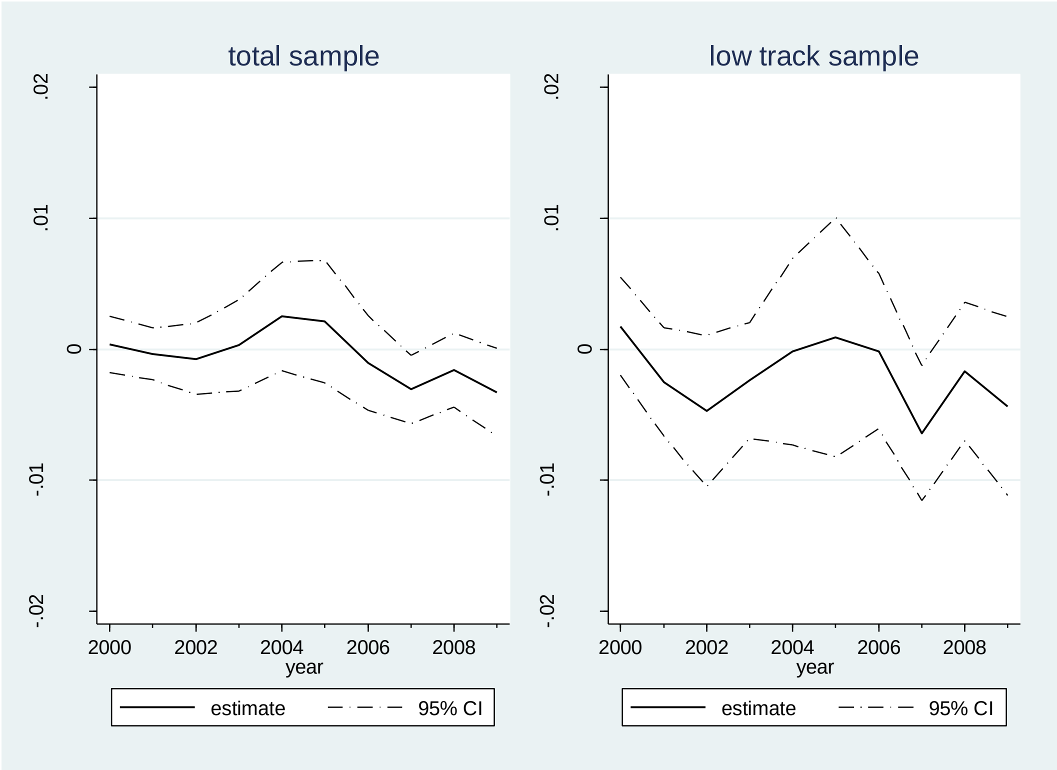


Figure 7: The impact of 1971-ROSLA on having welfare benefits in years 2000-2009

Figure 8: The estimated effect of the change in the law of having benefits* in years 2000-2009

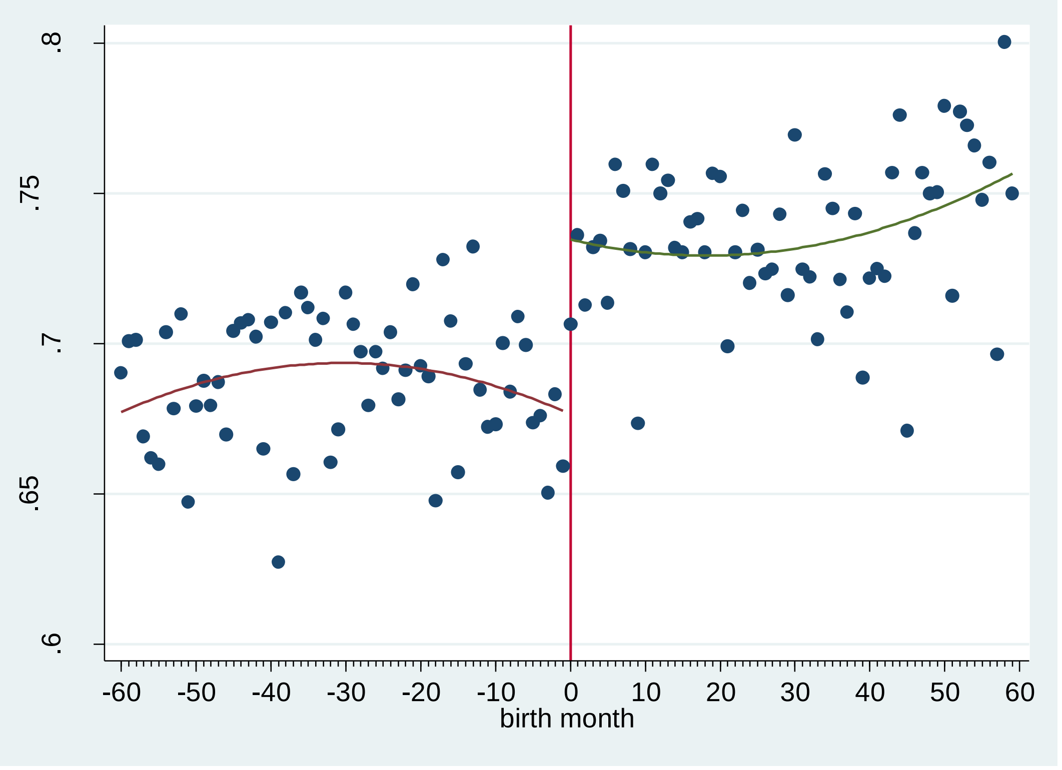
*Unemployment, welfare or disability benefits

Appendix

A.1 Derivation of the October cutoff date

A Dutch school year runs from August 1 of a given year to July 31 next year. In the period under investigation, students had to go to school in the school year in which they became 7 years old. There was an exception, however, for students born in August or September: they had the right to enroll in the current school year (starting in August) or in the school year prior to this year (starting in August one year earlier). Hence, students born from October onwards had to start primary school in the current school year. This means that the students born in October 1956 enrolled primary school in the school year 1963/1964 (i.e. started school on the first of August 1963). They completed 8 years of compulsory schooling on 31 July 1971 (6 years of primary school, and 2 years of secondary school if they did not retain). They had to complete a ninth compulsory year in the school year 1971/1972.

Figure A.1: Discontinuity in low track sample



Each dot represents the fraction that completed high school for a cohort of individuals born in a given month. The x-axis ranges from October 1951 (-60) to September 1960 (59). The cohort born in October 1956 is represented by 0 on the x-as. Quadratic polynomial fitted at either side of the cutoff.

Table A.1: Descriptives for 2009-outcomes and covariates for the low ability sample

Birth cohort	high school completion	log hourly wage	log annual earnings	employed	self-employed*	disability benefits	unemployment benefits	welfare benefits	years having paid job since age 15	years unemployed since age 15	years living on disability benefits since age 15	Dutch**	female	N
Not affected by the compulsory schooling law														
October 1946 - September 1947	0,668	2,594	9,230	0,195	0,174	0,140	0,016	0,047	15,978	0,597	2,081	0,883	0,626	7870
October 1947 - September 1948	0,673	2,609	9,477	0,302	0,133	0,136	0,019	0,048	16,106	0,647	1,938	0,873	0,612	7510
October 1948 - September 1949	0,666	2,643	9,651	0,394	0,113	0,133	0,019	0,048	16,169	0,615	1,941	0,875	0,595	7041
October 1949 - September 1950	0,670	2,667	9,781	0,482	0,094	0,126	0,022	0,047	16,272	0,728	1,960	0,865	0,592	6782
October 1950 - September 1951	0,679	2,645	9,793	0,528	0,086	0,122	0,024	0,047	16,375	0,715	1,794	0,861	0,594	6499
October 1951 - September 1952	0,681	2,651	9,795	0,565	0,078	0,117	0,022	0,048	16,411	0,786	1,693	0,855	0,592	6398
October 1952 - September 1953	0,686	2,641	9,795	0,576	0,080	0,109	0,021	0,046	16,222	0,792	1,516	0,863	0,603	6434
October 1953 - September 1954	0,696	2,648	9,846	0,602	0,089	0,108	0,021	0,049	16,396	0,794	1,577	0,854	0,587	6378
October 1954 - September 1955	0,695	2,648	9,862	0,634	0,090	0,089	0,019	0,048	16,449	0,828	1,362	0,851	0,583	6180
October 1955 - September 1956	0,681	2,626	9,835	0,637	0,082	0,095	0,026	0,049	16,378	0,939	1,378	0,843	0,570	6173
Affected by the compulsory schooling law														
October 1956 - September 1957	0,728	2,630	9,838	0,662	0,088	0,086	0,024	0,048	16,392	0,903	1,319	0,839	0,569	5900
October 1957 - September 1958	0,739	2,634	9,877	0,685	0,096	0,081	0,019	0,047	16,482	0,952	1,358	0,836	0,554	5810
October 1958 - September 1959	0,731	2,629	9,868	0,680	0,088	0,082	0,020	0,055	16,266	1,061	1,245	0,820	0,555	6001
October 1959 - September 1960	0,726	2,627	9,882	0,688	0,087	0,074	0,022	0,055	15,995	1,070	1,069	0,803	0,556	5983
October 1960 - September 1961	0,755	2,614	9,894	0,712	0,094	0,065	0,020	0,051	16,034	1,058	1,153	0,820	0,540	5988
October 1961 - September 1962	0,755	2,634	9,896	0,709	0,091	0,065	0,023	0,054	15,885	1,040	1,084	0,807	0,535	6162
October 1962 - September 1963	0,749	2,613	9,848	0,716	0,094	0,064	0,022	0,053	15,747	1,112	1,018	0,797	0,530	5983
October 1963 - September 1964	0,763	2,612	9,870	0,719	0,104	0,067	0,024	0,049	15,446	1,115	0,951	0,786	0,505	5698
October 1964 - September 1965	0,772	2,615	9,877	0,715	0,100	0,062	0,025	0,054	15,093	1,147	0,869	0,782	0,501	5644
October 1965 - September 1966	0,778	2,613	9,863	0,727	0,106	0,058	0,026	0,048	14,661	1,096	0,894	0,771	0,509	5333

* Conditional on being employed. **The Dutch definition *autochtoon* is used: both parents were born in the Netherlands. In our regressions we also distinguish between Moroccan, Turkish, Surinam and Antilles

Figure A.2-A.4: Birth year 1957 is represented by zero on the x-axis; at each dot the number of observations is given that was used for the calculation of the averages.

Figure A.2: No impact of 1971-ROSLA on prose literacy

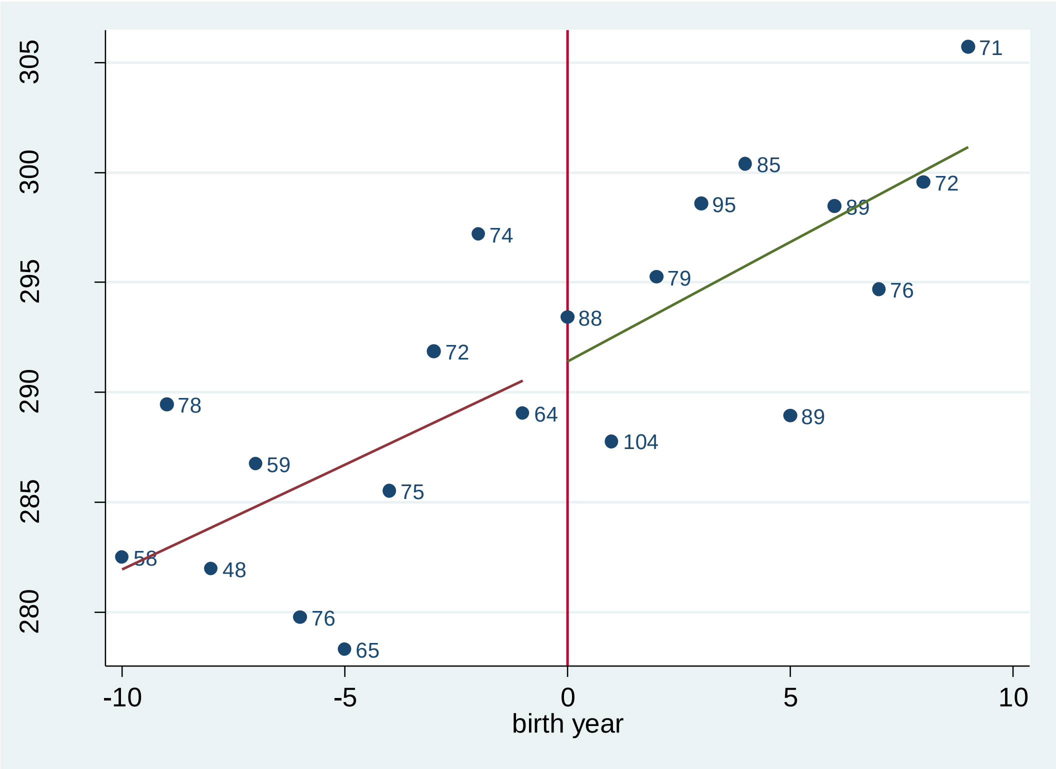


Figure A.3: No impact of 1971-ROSLA on document literacy

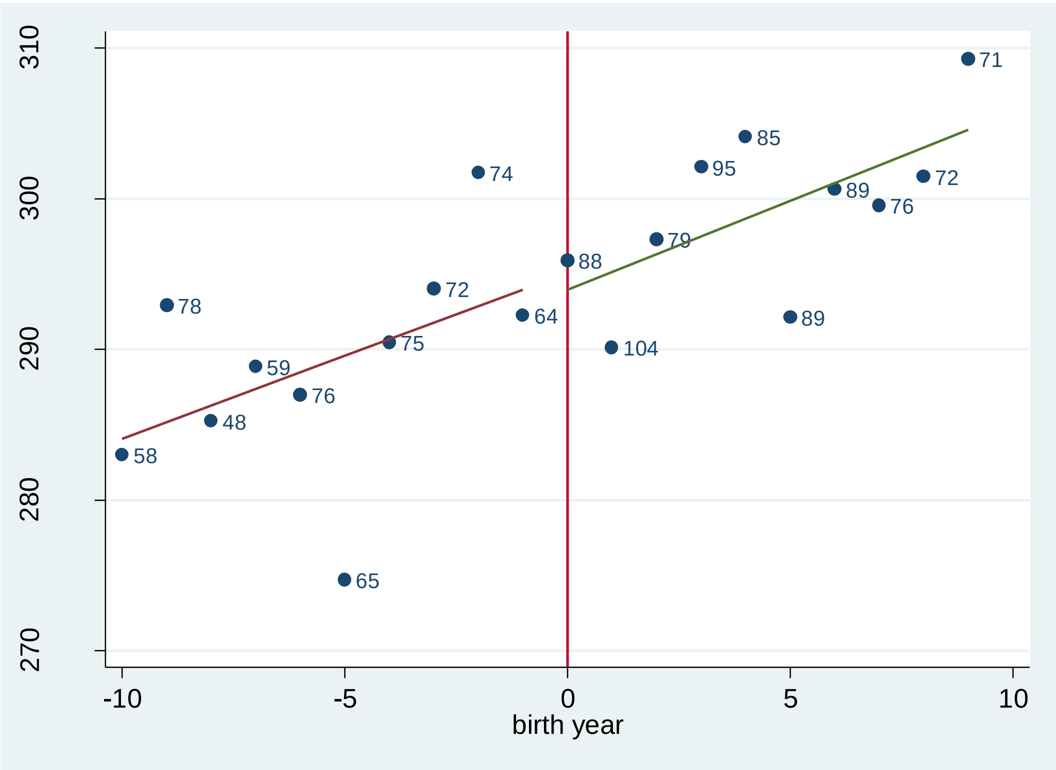
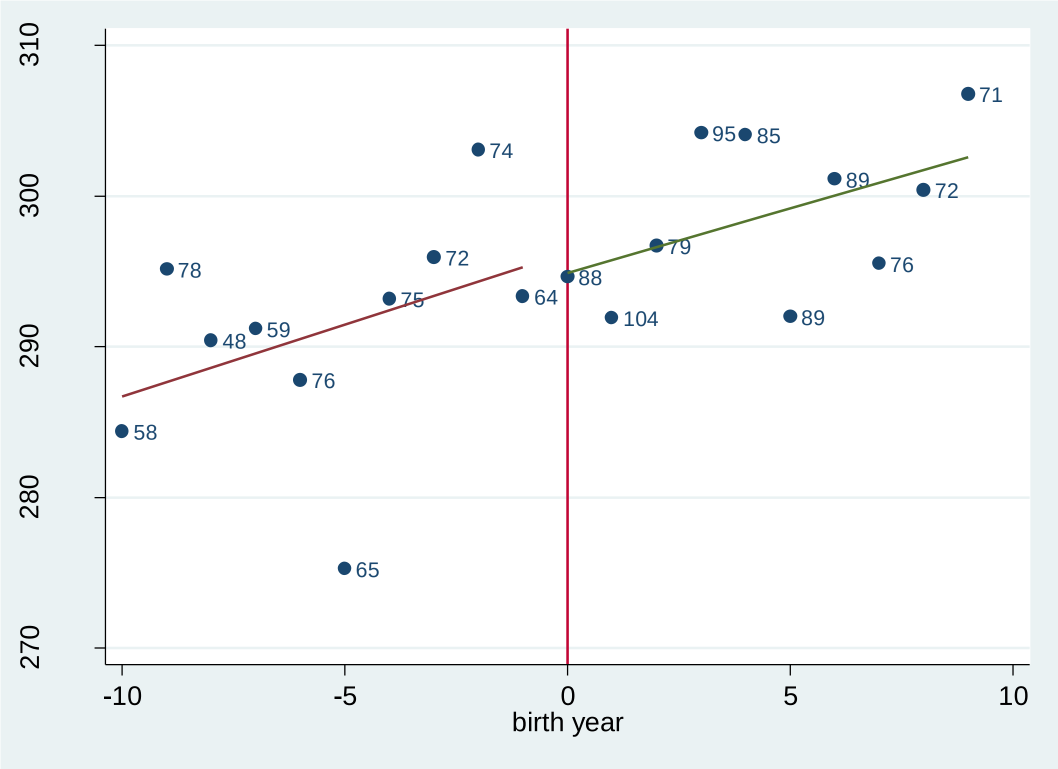


Figure A.4: No impact of 1971-ROSLA on quantative literacy



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