Chapter 2

Education and Fertility: Evidence from a Natural Experiment*

by

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Abstract

Fertility continues to be an issue of public concern, even in developed countries that have experienced demographic transition and reached a state where both mortality and birth rates are low. Often when low birth rates and fertility patterns are discussed, women's trade-off between childcare and education and employment opportunities are brought forward as one explanation. However, many factors influence decisions on fertility, education and employment, very likely including unobservable factors that cannot be controlled for. Thus, causation is difficult to establish. In this paper, we make use of an educational reform to trace the causal effect of education on fertility outcomes. Our data enables us to estimate the effect of education on the timing of births as well as completed fertility, including the probability of being childless, after allowing for cohort effects. As the cohorts we study were born between 1947 and 1958, our data includes the most recent generation of women with completed fertility histories. The results indicate that increasing education at the lower tail of the education distribution leads younger women to postpone first births, most remarkably away from teenage motherhood towards having the first birth in their twenties and, for a smaller group, even until an age of 35 to 40 years. This result cannot be explained as a mere "incarceration" effect, and we interpret it mainly as the result of increased human capital accumulation from the reform. However, while the length of education and various fertility outcomes are found to be highly *correlated*, the data do not support any strong *causal* relationship other than the postponement of first birth. In particular, evidence is not found that more education results in more women being childless or leads to women having fewer children.

2.1 Introduction

Fertility continues to be an issue of public concern, even in developed countries that have experienced the demographic transition and reached a state where both mortality and birth rates are low. Low population growth and higher dependency ratios are argued to strangle economic growth. Recent OECD projections suggest that, because of demographic changes, the growth rate of per capita income will decline from 1.7% to 1.1% by 2050 in European countries and from 1.7% to 1.2% in the United States (Turner *et al.*, 1998). Often when low birth rates and fertility patterns are discussed, women's trade-off between childcare and education and employment opportunities are brought forward as one explanation. The observed relationship between fertility and female education varies between different countries and time periods, but there is much empirical support for strong correlations (Schultz, 1997; Cochrane, 1979). However, many factors influence decisions on fertility, education and employment, very likely including unobservable factors that cannot be controlled for. Thus, causation is difficult to establish. [34] In this paper, we make use of an educational reform to trace the causal effect of education on fertility outcomes. [35]

Nordic countries have a relatively high fertility rate (Sleebos, 2003), but this is an imperfect measure of long-run fertility as it aggregates behaviour over cohorts and ignores the timing of births. With respect to population development that is sustainable, the major concern in Nordic countries is the increasing number of childless women and the fact that the younger cohorts of women are having fewer children (Skrede and Rønsen, 2006). Our data enables us to estimate the effect of education on the timing of births as well as completed fertility, including the probability of being childless, after allowing for cohort effects. As the cohorts studied were born between 1947 and 1958, our data includes the most recent generation of women with completed fertility histories.

We study the relationship between the education of women and three fertility outcomes: the timing of children; childlessness; and the number of children. Our data confirms the expected correlation between fertility outcomes and education: women with more education are more often childless; they have fewer children and postpone births. Despite these statistically significant correlations, we do not find evidence of a causal relationship between the length of education on one hand, and completed fertility or

[34] Educational policy is rarely implemented to change fertility, but may still have fertility consequences. The interesting question is whether there is an intrinsic conflict between having a more educated population and obtaining sustainable total fertility levels in developed countries.

[35] Black, Devereux and Salvanes (2006) use this reform to assess the impact on teenage motherhood, while this paper analyses the effect on total fertility and the timing of first births in general.

childlessness on the other, when using the reform as an instrument for education. Our main finding is that increased mandatory education lead to the postponement of births; there are fewer cases of teenage motherhood and more first births among women aged 35 to 40 years.

The paper unfolds as follows. In section 2, we outline the major elements of the support system for parents in Norway and provide a short overview of the literature on fertility decisions, particularly the relationship between fertility and education. Section 3 describes the change in compulsory schooling that is used as an instrument in this study. The identification strategy and the data are described in sections 4 and 5. The results are presented and discussed in section 6. Section 7 concludes.

2.2 Background information

2.2.1 Previous literature

Economists model fertility in terms of the costs and benefits of children. There is a well-established literature describing how different types of costs affect fertility. Education may affect fertility decisions through several channels; in some contexts through better knowledge of contraceptives or through educational activity being incompatible with pregnancy or taking care of small children. Of special interest is the effect that goes through the labour market. In theory, the predicted effect of a rise in female wages on completed fertility is unclear because it depends on the magnitude of the different substitution and income effects. However, the conventional prediction consistent with Becker (1960) and Willis (1973) is that as child-rearing is a time-intensive activity, higher wages that raise a woman's opportunity cost of time will lead her to want fewer children, but probably put more resources into each child's upbringing. Hotz *et al.* (1997) provide a review of static models on completed fertility.

Theoretical models on the timing of births present a trade-off between the greater pleasure of early births and the lower costs of later births with a focus on the latter.[36] Much attention has been given to models that consider lifetime earnings, consumption smoothing and career planning rather than current incomes and wages. The literature points to woman's career costs as the most important explanation in favour of postponing births. In addition to the direct wage loss during labour force withdrawals, there is a loss in the returns to human

[36] Happel *et al*. (1984) assume that there is no pure time preference associated with the household's "effective" number of children.

capital in later periods due to depreciation.[37] For a review of dynamic fertility models, see Gustafsson (2001).

Sleebos (2003) provides a broad picture of the development in fertility in OECD countries from 1970 to 2000. Among the stylized facts is a general increase in the mean maternal age at first childbirth, with the exception of the USA and Korea. Countries differ remarkably in the degree to which women who have postponed childbirth then give birth at higher ages. This recuperation effect is stronger in Nordic countries, France and the United Kingdom than in Southern European and a number of other Continental European countries.

With respect to Norway, Lappegård and Rønsen (2005) have studied trends in the timing of first births for women born in the period between 1955 and 1969. They use longitudinal data up to 2001 and estimate a hazard model where education is treated as a time-varying covariate. Education is studied in several dimensions, including activity, length and field. They conclude that being a student clearly delays motherhood, but that the effect of length of education primarily works through the prolonged participation in the educational system. Field of study is found to have a separate impact, and is interpreted as mirroring different educational and career aspirations. However, this study does not correct for selection, i.e., the possibility that omitted, probably unobservable, factors influence both educational and fertility decisions.

To identify the causal effects of education, several studies have employed rules and regulations concerning school entry or dropout. An early and important contribution to the "natural experiment" literature is given by Angrist and Krueger in their 1991 paper where they used the quarter of birth as an instrument for educational attainment in earnings equations. The quarter of birth is correlated with the length of education because pupils were allowed to leave school by 16 years of age. To study the effects of education on fertility outcomes, several recent papers have used the same source of variation. McCrary and Royer (2006) use the school entry date as an instrument, and data from Texas and California. Their sample is selected because their source of data is birth certificates. They find no effect of mother's education on the timing of first births for women 23 years of age or younger.

Fort (2006) has utilized an Italian mandatory school reform enforced in 1963 that prescribed junior high school attendance, so that compulsory schooling increased from five to

[37] Gustafsson (2001) presents a list of parameters that will have a positive partial effect on the tendency to postpone births: the amount of pre-maternity human capital; the rate of depreciation of human capital due to non-use of human capital; the rate of return to human capital investment; and the length of time spent out of the labour force. In addition, the profile of human capital investments may play a role. Ignoring depreciation, theory has been ambiguous about whether a steep earnings function leads to earlier or postponed births (Cigno and Ermisch, 1989). Gustafsson argues that commonly used earnings profiles favour birth postponement.

eight years. The implementation period turned out to be unintentionally long and compliance was poor, especially in Southern Italy. This analysis does not, however, control for region; this raises/reintroduces the issue of selection.[38] As the author points out, economic conditions, traditions regarding fertility and labour market aspirations differed profoundly between regions. The estimated effects are restricted to those who had at most eight years of schooling. Her findings suggest that the reform lead these women to postpone their first childbirth, but they caught up with this delay in fertility before turning 26 years of age.[39]

Closest to our paper is work by Black, Devereux and Salvanes (2006) on the probability of teenage motherhood.[40] The authors use changes in compulsory schooling laws in the USA and Norway as an instrument for education in the two countries to identify the effect under two very different institutional environments. The Norwegian school reform will be described in detail later. Black *et al.* find evidence that increasing mandatory education reduces the incidence of teenage motherhood. Moreover, the size of the effect is estimated to be quite similar in the USA and Norway. Their results indicate that the effect of compulsory schooling laws goes beyond a pure "incarceration" effect. The current paper is an extension of Black *et al*. in that it studies fertility over the woman's fertile period, rather than just her teenage years, enabling us to examine the human capital effect of education on the full fertility history of women.

2.2.2 Institutional setting

Decisions on fertility are intertwined with decisions on marriage (or union) formation, education and employment. There have been changes over time in both norms towards single mothers, support systems for children such as day care and direct support, as well as the availability and acceptance of contraception. Skrede and Rønsen (2006) argue that what is regarded as Nordic "family policy" has not been aimed at fertility outcomes, e.g., sustainable total fertility levels, but rather at facilitating the combination of workforce participation and involvement in domestic tasks by both parents.

The support system for parents differs somewhat according to family type. For cohabitating or married mothers, the programs with the greatest implications for the cost of a child are the statutory universal rights parents have in connection with birth and the supply of

[38] Fort notes that sample sizes are too small to include region as a covariate. In addition, she lacks information on where the women lived at the time when they were around junior high school age.
[39] Effects of education on the timing of first childbirth are estimated for age levels 18 to 26 years, and are found to be statistically negative for ages 19 to 21 years.
[40] To our knowledge, this is the only existing paper that aims to estimate a causal relationship between fertility and education using Norwegian data.

subsidized childcare (discussed below) (Rønsen, 2004). A universal right to 12 weeks paid maternity leave was introduced in 1956, but the income compensation was relatively low. The major extension came in 1978 when maternity benefits were raised to cover 100% of the pre-birth income for 18 weeks.[41] This entitlement was extended to 20 weeks in 1987 and 22 weeks in 1988. Since 1993, mothers may choose between 42 weeks maternity leave with full pay and 52 weeks with 80% compensation. Since 1977, parents have been entitled to unpaid leave with job security until the child is one year old.[42] The cohorts in this study were in their peak fertility ages, i.e., 20 to 31 years old, when the first major extension came in 1978. However, the much more generous maternity leave reform in 1993 came too late to have any widespread impact as the women studied were then aged between 35 to 46 years.

The support system for single mothers is even more extensive, and from the early 1960s onwards, became very generous (Rønsen and Strøm, 1991). The system for single parents consists of several elements. The main part is a right-based support via the social security system ensuring single parents an income and temporary assistance to enable them to support themselves until the child is ten years old. This system was introduced in 1964 and became a part of the social security system in 1971. Together with other benefits, this enabled non-working single parents to take care of their children without working. The support is, by definition, temporary and is meant to support single parents (i.e., not living with the child's other parent, but may live with other partners). It is also meant to support single parents to become independent and provides support for education. All documented expenses for education are provided. Support was, and still is, income-dependent, i.e., reduced if the mother is working. The system was made less generous in 1998 (Skrede and Rønsen, 2006),[43] but by then "our cohorts" had virtually completed their fertility.

Another important element of the support system is income-dependent support for housing. Single mothers also receive financial support from the father if the father's name is registered with the authorities, and the authorities assist in enforcing child support payments. Single parents pay a reduced rate for day care (Rønsen, 2004). All parents receive a tax-free child allowance in Norway and single parents get about 1000 NOK extra per month in 2007. The attitude towards teenage mothers became more accepting in Norway during the 1970s

[41] To be eligible for maternity leave, the mother has to have worked for a certain period during pregnancy. From 1977, the requirement is six of the last ten months prior to birth. Alternatively, she gets a tax-free cash benefit at delivery, NOK 4730 in 1988 (Rønsen, 2004) and NOK 33,584 in 2007.

[42] Women working in the public sector can have longer unpaid leave, up to three years in total, but not less than one year per child. For instance, parents with three children are entitled to 3+1+1 years of unpaid leave.

[43] The duration of the support period was reduced from ten to three years in 1998. However, more incentives were given to work because the resulting reduction in the monthly support was not so drastic.

than before, which is also reflected in the fact that knowledge about sexual behaviour was made part of the compulsory school curriculum, and contraceptives (such as the pill) became more widely available. The pill was introduced in the late 1960s and was widely used; we know that among teenage girls aged 18 to 19 years in 1977, only 10% of those who had sexual intercourse did not use the pill or another type of contraceptive. In 1988, an even higher proportion used contraceptives (Noack and Østby, 1981 and Blom, Noack and Østbye, 1993). Abortion was legalized in Norway in 1979.[44] Although there is not one date that can be pinpointed when this started, the early to mid-1970s appears to be usually agreed upon (see Furre, 1992 for a general text on modern Norwegian history). These changes started within our period of analyses and again apply to the latter born, but not the earlier cohorts, within our data.

Public day care, which is subsidized in Norway, is subject to excess demand. Enrolment rates have risen sharply, from 5% in 1973 to 21% by 1980, 36% in 1990, 40% by 1992 and 54% in 2001 (Rønsen, 2004). The excess demand has been met by different forms of private childcare. A large proportion of Norwegian women work part time.

2.3 Compulsory schooling laws

In 1959, the Norwegian Parliament legislated mandatory school reform that increased the minimum level of education by extending the number of compulsory years of education from seven to nine years (thereby increasing the minimum dropout age from 14 to 16, as students start school at age seven). There were no exemptions to these laws. In addition, the reform standardized the curriculum and increased access to schools, as nine years of mandatory schooling was eventually made available in all municipalities.

The parliament mandated that all municipalities (the lowest level of local administration) implemented the reform by 1973. As a result, although it was started in 1960, implementation was not completed until 1972.[45] This suggests that for more than a decade Norwegian schools were divided into two separate systems; the system you were in depended on your year of birth and your municipality of residence. The first cohort that could have potentially been subject to the reform was that born in 1947. These individuals started school

[44] One would expect that access to legalized abortion may explain the drastic reduction in the number of teenage births in Norway from the late 1970s onwards, but in fact, the incidence of abortion has decreased, especially among teenagers from the early 1980s onwards (Lappegård, 2000).

[45] The reform had already started on a small and explorative basis in the late 1950s, but applied to a negligible number of students because only a few small municipalities, each with a small number of schools, were involved. See Lie (1974), Telhaug (1969) and Lindbekk (1992), for descriptions of the reform.

in 1954, and either finished pre-reform compulsory school in 1961, or went to primary school from 1954 to 1960, followed by post-reform middle school from 1960 to 1963. The last cohort who could have gone through the old system was born in 1958. This cohort started school in 1965 and finished compulsory schooling in 1972.

To receive funds from the government to implement the reform, municipalities needed to present a plan to a committee under the Ministry of Education. Once approved, the costs of teachers and buildings were provided by the national government. While the criteria determining selection by the committee are somewhat unclear, the committee wanted to ensure that implementation was representative across the country, conditional on an acceptable plan. (Telhaug, 1969, Mediås, 2000).[46] Figure 1 in the Appendix depicts the spread of the reform, focusing on the number of municipalities implementing the reform each year.

While it is not necessary for our estimation strategy, it would be useful if the timing of the implementation of the reform across municipalities were uncorrelated with teenage pregnancy rates, one of our outcomes of interest. To test this, we examine the relationship between the timing of the reform (by municipality) and teenage pregnancy rates prior to the reform (1960). We also look at other characteristics that may be associated with teenage pregnancy rates. For example, one could believe that poorer municipalities would be among the first to implement the reform given the substantial state subsidies, while wealthier municipalities would move at a much slower pace. However, work examining the determinants of the timing of implementation finds no relationship between municipality characteristics such as average earnings, taxable income and educational level, and the timing of implementation (Lie, 1973, 1974). Municipalities that are located geographically near municipalities that had already implemented the reform were themselves more likely to implement the reform; numerous interviews revealed that this was likely due to a particularly effective county administrator. As a result, the research supports a complex adoption process without finding support for a single important factor to explain the implementation process. To examine this issue ourselves, Figures 2, 3 and 4 in the Appendix depict the implementation of the reform by the average income, parental education and size of the municipalities. These figures suggest that there is little relationship between these factors and the timing of the implementation of the reform.

[46] Similar school reforms were undertaken in many other European countries in the same period, notably Sweden, the United Kingdom and, to some extent, France and Germany (Leschinsky and Mayer, 1990).

As a more rigorous test, in Table 1 in the Appendix, we regress the year of implementation on different background variables based on municipality averages, including parental income, the level of education, average age and the size of the municipality, as well as county dummies (there are twenty counties in Norway). Consistent with the existing literature, there appears to be no systematic relationship between the timing of implementation and parental average earnings, educational level, average age, urban/rural status, industry or labour force composition, municipality unemployment rates in 1960 and the share of individuals who were members of the Labour party (the most pro-reform and dominant political party).

2.4 Identification strategy

We study three fertility outcomes: the timing of children; the number of children; and childlessness. With one exception, the fertility outcomes Y_i studied are binary. Thus, the main specification used is a latent variable model:

(1) $$Y_i^* = \alpha_0 + \alpha_1 R_i + \alpha_2 C_i + \alpha_3 M_i + e_i ,$$

where $Y_i = 1$ if $Y_i^* > 0$

$Y_i = 0$ otherwise.

The explanatory variables included are a reform indicator R_i, the set of municipalities *M* and cohorts *C*, which for individual *i* will take the value 1 for the municipality of residence and the person's cohort. The error term e_i is assumed to be i.i.d. and normally distributed $e_i \sim N(0, \sigma)$. A probit model is selected to estimate childlessness and the timing of births, while the number of children is estimated using ordinary least squares.[47]

There are a few points to note about eq. (1). To start with, it contains fixed cohort and municipality effects. The cohort effects are necessary to allow for secular changes in educational attainment over time that may be completely unrelated to compulsory schooling laws. The municipality effects allow for the fact that variation in the timing of the law changes across municipality may not have been exogenous to educational choice. Even if the reform was implemented first in areas with certain unobserved characteristics, consistent

[47] Regarding childlessness and the timing of births, OLS estimation results are reported for comparison purposes.

estimation is still achieved so long as: (a) these characteristics are fixed over time; (b) implementation of law changes are uncorrelated with changes in these characteristics; or (c) these characteristics are unrelated to the probability of the timing, the number of children, or childlessness.[48]

2.5 Data

2.5.1 Data sources

Based on different administrative registers and census data from Statistics Norway, a comprehensive data set has been compiled of the entire population in Norway, including information on family background, age, marital status, country of birth, educational history, neighbourhood information and employment information.[49] The initial database is linked administrative data that covers the entire population of Norwegians aged 0 to 90 years. This administrative data provides information about educational attainment, labour market status and a set of demographic variables (age, gender). To this, we match extracts from the censuses in 1960, 1970 and 1980.

To determine whether women were affected by the changed compulsory schooling legislation, we need to link each woman to the municipality where she grew up. We do this by matching the administrative data to the 1960 census. From the 1960 census, we know the municipality where the woman's mother lived in 1960.[50] The women we are using in the estimation are aged between two and 13 years in 1960. The indicator will be equal to one for a woman if, by her seventh year of schooling, the new system had been implemented in her municipality of residence, which is defined to be where her mother lived in 1960. One concern is that there may be a selective migration into or out of municipalities that implemented the reform early.[51] However, because the reform implementation did not occur before 1960, reform-induced mobility should not be a problem. A related concern is that random mobility at any point after we assign location may imply that an individual is not actually impacted by the reform, although we classify them as being so. This creates a

[48] Local variation in preference changes or shocks, e.g., regional differences in economic activity, is conceivable. As a robustness check, we estimated a model with municipality-specific trends.
[49] See Møen, Salvanes and Sørensen (2003) for a description of the data set.
[50] As very few children live with their father in cases where the parents are not living together, we should only have minimal misclassification through applying this rule.
[51] Evidence from Meghir and Palme (2005) for Sweden and Telhaug (1969) for Norway suggests that reform-induced migration is not a significant consideration.

measurement error problem that will tend to bias our estimates of the effects of the reform towards zero.

The measure of educational attainment is taken from a separate data source maintained by Statistics Norway. Educational attainment is reported by the educational establishment directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. This register provides detailed information on educational attainment. The educational register started in 1970; for women who completed their education before then, we use information from the 1970 Census. Thus, the register data are used for all but the earliest cohorts of women who did not have any education after 1970. Census data are self-reported (four-digit codes of types of education were reported) and the information is considered to be very accurate; there are no spikes or changes in the education data from the early to the later cohorts.

Our primary data source on the timing of the reform in individual municipalities is from Ness (1971). To verify the dates provided by Ness, we examined the data to determine whether there appears to be a clear break in the fraction of students with less than nine years of education. In the rare instances when the data did not seem consistent with the timing stated in Ness, we checked individual municipalities by contacting local sources.[52] We are able to successfully calculate reform indicators for 672 of the 732 municipalities in existence in 1960. If the reform took more than one year to implement in a particular municipality, or we were unable to verify the information given in Ness (1971), we could not assign a reform indicator to that municipality. However, we have reform information for a large majority of individuals in the relevant cohorts.

We include those cohorts of women born between 1947 and 1958 in our sample. For these women, we observe their children in 2002. Thus, the youngest individuals will be 44 years of age and so all but a tiny minority will have completed their fertility. From the year and month of birth of the children and the year and month of birth of the mother, we can determine the age of the mother at birth to the nearest month. We exclude from our sample the small number of women who have a birth before they are aged 15 years and define a teenage birth as one occurring when the mother has not yet reached her 20th birthday. See table 2 in the Appendix.

[52] Between 1960 and 1970, a number of municipalities merged. In our analysis, we use the 1960 municipality as the unit of observation (Juvkam, 1999). In cases where the data were available at the 1970 municipality level, individual municipalities were contacted to determine the appropriate coding.

2.5.2 Descriptive statistics

Table 1 presents the key explanatory variables, the sample split by whether the individual were subject to the reform or not. The estimation sample consists of 290,604 women, 53% (154,818 individuals) of whom were affected by the reform. The subsample that lived in municipalities for which the reform was implemented had, on average, more education (a difference of 0.5 years). While the reform mandated nine years of schooling, the mean length of education for those *not* affected by the reform was 11.25 years; so many women had more than nine years of education even without the reform. The reform cohorts were born later (on average 4.5 years) as the reform was implemented gradually. The long-run trend to greater education explains at least part of the 0.5-year difference in average schooling.

The mean values in the lower part of the table show the distribution of cohorts within the reform and non-reform group, respectively. This reflects the gradual implementation of the reform; only 2.8% of those who were subject to the reform belonged to the three oldest cohorts (born between 1947 and 1949), while for the youngest cohorts the reform had been implemented for almost everybody.

Table 2 presents the outcome variables, split by whether the individuals were or were not subject to the reform. The differences in means are small, but the following patterns can be seen. Within the reform group, there are more women who are childless and the average number of children is slightly lower. The probability of teenage motherhood (first birth before age 20) is very similar for the two groups, but the group subject to the reforms were less likely to give birth in the first half of their twenties, and had a higher propensity to give birth after the age of 30 years.

These data are the result of cohort change as well as reform status. To separate cohort and reform status, Figures 1 to 7 present the outcomes by cohort, splitting the data into those subject to the reforms and those who were not. Figure 1 shows the time trend in the mean probability of a teen birth. This is higher in the non-reform group for almost all cohorts as well as showing a rise (for both groups) for those born from the early 1950s onwards. Figure 2 shows a slight decline in the probability of having a first child when aged between 20 and 25 as the cohorts get younger, which is matched by a slight increase for these same cohorts in having a first child between 25 and 30. However, the differences in these outcomes by reform status are small. Figures 4 and 5 show the trend towards first births being delayed until women are in their thirties. Later-born women were more likely to have first births later, and there is a greater tendency towards this in the reform group. For the youngest cohort (those

born in 1958), the probability of not having a first child until 35 years or older is 0.032 for those subject to the reform; the corresponding figure for those who were not subject to the reform is 0.025, a percentage difference of about 30%. Figure 6 shows the evolution of the average number of children over the cohorts. Total fertility is quite stable over time and there is little difference between groups.[53] Figure 7 shows the trend in the probability of being childless. This increases for both reform and non-reform groups over time, but increases more for the reform group than their untreated counterparts.

Overall, the raw data suggest that those subject to the reforms were more likely to delay first birth, resulting in a drop in teenage motherhood and an increase in first births for those women in their thirties. There is also possibly some indication that the reforms resulted in a higher probability of being childless, at least for the younger cohorts included in the treatment group.

2.6 Results

2.6.1 The effects of the reform on fertility

Table 3 presents our key results. Each coefficient is derived from a separate regression, each of which controls for municipality and year of birth. Row 1 examines the correlation between education and fertility. The estimates show the expected strong statistical relationship between the length of education and fertility. Women who have more years of schooling have a higher tendency to remain childless; they also have fewer children and the probability of a first childbirth among the age groups less than 25 years decreases with education. The correlations estimated are of a rather large magnitude. For example, the probability of remaining childless increases by half a percentage point for each additional year of education.

Our interest lies in the causal relationships from education, and so in the coefficients from models where the educational reform is used as an instrument for education. The second and third rows of the table present the probit and ordinary least squares (OLS) estimates respectively. These are, in fact, very similar. The reform mandating more years of schooling reduces the probability of a teen birth, and increases the chance of not having a first birth until after age 35. There is also a relatively large, though not statistically significant, increase in first births between 20 and 25 years of age. There is no statistical effect on completed family size. The final row repeats the analysis using two stage least squares (2SLS) results. Again, we find the impact of education is to delay first births into the twenties and late thirties, but it

[53] The 1958 cohort is an outlier. The number of non-reform women in that cohort is small (238 observations).

has no effect on completed family size. Therefore, the effect of the reform is essentially to delay child bearing. In contrast with the raw association with education, when controlling for possible endogeneity in the education variable there is no significant relationship between education and the number of children born to a woman or the probability of never having children.

Although the magnitude of the estimated effects on timing is small in absolute terms, some are considerable in relative terms. The decrease in the probability of teenage motherhood is 5% relative to the frequency of teenage motherhood in the whole sample, which is 0.166 ((0.008/0.166)*100% = 5%). At the population level, the estimated effect is that about 260 fewer women would become teenage mothers each year if all individuals were mandated nine years of schooling as opposed to seven years.[54] Likewise, the increase in probability of giving first birth aged 35 to 40 is nearly 8% ((0.002/0.026)*100% = 7.9%), which is equivalent to an increase in 70 more women having children at this age each year.

2.6.2 Robustness checks

It is possible that our results are not picking up the impact of the reform but of unobserved differences between municipalities over time. To test for this, the top four lines of Table 4 present the results allowing for municipality, year-of-birth indicators and the corresponding interaction terms to allow for separate municipality–year effects. Standard errors are adjusted for clustering at the municipality–year level. The results are robust to these additional controls. The OLS and probit estimates using the reform dummy are very similar to those in Table 3; the 2SLS estimates show a larger effect of education on number of children, but again this is not statistically significant.

The group most likely to be affected by the reforms are those who are aged 13 years. To further test that our results are driven by the impact of education and not some omitted time-varying change in tastes, we used a regression discontinuity approach and re-estimated eq. (1) on a sample restricted to girls who were aged thirteen within three (alternatively five) years before or after the year of reform implementation. These results are presented in the second and third block of Table 4 respectively. In these cases, the correlation between education and the various fertility outcomes in the OLS model are very similar to those in Table 3. The causal results in Table 3 are also supported. The signs of the estimated effects

[54] The proportion of first birth by age in the sample is as follows: 0.17 in age group 15–20 years, 0.39 in age group 20–25, 0.23 in age group 25–30, 0.08 in age group 30–35 and 0.03 in age group 35–40, while the remaining 11% of the sample are childless. The mean size of a cohort of women in the whole population is about 32,000 individuals for the years 1947–1958.

are as before,[55] although the standard errors are larger because of the smaller sample size. The reduction of the probability of teenage motherhood is statistically significant in the model with a +/– five-year span, but the increased likelihood of first birth at age 35 to 40 is not.

2.6.3 Discussion

We can think of two mechanisms through which education can affect fertility. First, schooling is an activity that may reduce the possibility of behaviour that may lead to pregnancy. This is often referred to as the "incarceration effect". Second, education is an investment in human capital and may affect both the timing of births and the number of children. The incarceration effect is, by nature, temporary. If opportunity costs influence fertility in a lifetime perspective, it must be through the human capital effect.

Black *et al.* (2006) argue that if there is an incarceration effect, the data should show an increase in first births at ages 16 and 17 years after the dropout age was raised from 14 to 16 years. They find the opposite. We use essentially the same data, and also find that there is no catch-up in first births in the 15 to 20 age groups. This result alone tells us that the reform lead to more than merely an incarceration effect.

Our main finding is that the reform resulted in a postponement of births away from very early births and towards first birth at a later age. Due to the unfavourable consequences of teenage births, the results from increasing education that we find should be regarded as positive. Furthermore, the data does not show any statistically significant effects of the reform on total fertility. The allegation that education inevitably leads to fewer children being born is not supported by our data. As a caveat, if more schooling makes women tend to postpone their first birth until the end of their fertile period, this may have unfavourable consequences in terms of the increased risk of problems with fecundity and the corresponding costs to individuals and, in a publicly funded system, to the health care system.

The effects measured are "local average treatment effects"; the reform only affects those who change their behaviour because of the reform, i.e., those who would have chosen seven or eight years of education if compulsory schooling had not been extended to nine years. It may seem far-fetched that these women should postpone their first birth until the age of 35 to 40 years; this is a statistically significant result. It is likely that there is a great deal of heterogeneity between women regarding how they respond to the reform in compulsory schooling. Our data indicate that, in most cases, where first births were postponed due to the

[55] An exception is the estimated impact of education on the number of children, where the effect is either positive or zero in Table 4 while it is estimated to be negative, but very small, in Table 3. However, in none of the cases is the effect found to be statistically significant.

reform, the first birth took place at age 20 to 25 or 25 to 30 instead, although the difference between the reform and the non-reform group is not statistically significant for these age groups.

Postponement of births may also be given a human capital explanation.[56] For women who place a priority on establishing themselves in the labour market as a full-time worker, it is less costly to postpone childbirth, provided the age-earnings profile is not too steep. It seems plausible that the reform could lead some women into a different "track" in life: having had more compulsory schooling, the women impacted by the reform may have invested more than they otherwise would in secondary education or on-the-job training. Their preferences regarding when to have children may have changed so that they want to postpone birth as long as possible for career reasons, but eventually, the biological clock sets a fecundity limit.[57]

2.7 Conclusion

Using an educational reform as an instrument for education, we are able to investigate the causal effect of education on fertility. The data indicates that increasing education at the lower tail of the education distribution leads young women to postpone first births, most remarkably away from teenage motherhood towards having the first birth in their twenties and, for a small but statistically significant group, until the age of 35 to 40 years. This result cannot be explained as a mere "incarceration effect", and we interpret it as mainly the result of increased human capital accumulation because of the reform. While the length of education and various fertility outcomes are found to be highly *correlated*, the data do not support any strong *causal* relationship other than the postponement of first birth. In particular, we find no evidence that more education results in more women being childless or leads to women having fewer children.

[56] Gustafsson (2001) summarizes the theory on timing of births and points to the main explanations as being career planning and consumption smoothing. She finds that the main parameters which have an impact on career costs are the amount of prematernity human capital, the rate of depreciation of human capital due to non-use, the rate of return to human capital investments, the profile of human capital investments and the length of time spent out of the labour force.

[57] In principle, we may think of institutional changes as shocks that may alter both timing and total fertility, for instance make childless women change their mind about having a child or not. Perhaps the 1993 extension of paid marental leave spurred fertility in the reform group among cohorts that were fertile, but yet childless then – and in our sample that would be the 1953-1958 cohorts, who would be aged precisely 35-40 years. To assess the effect of the 1993 reform, we would need total fertility data on younger cohorts that are not included in our sample.

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Figures and tables

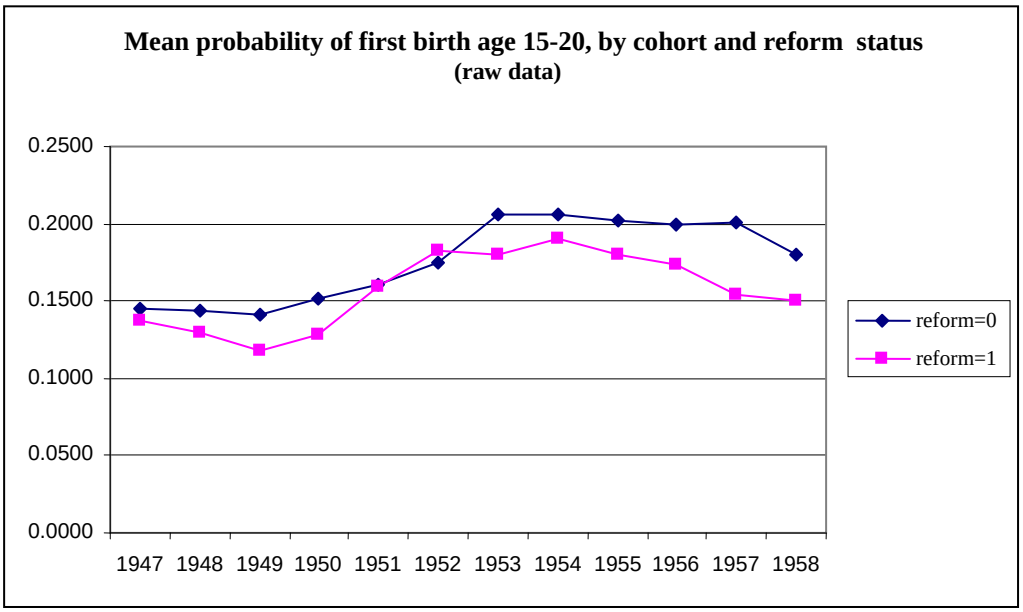


Figure 1

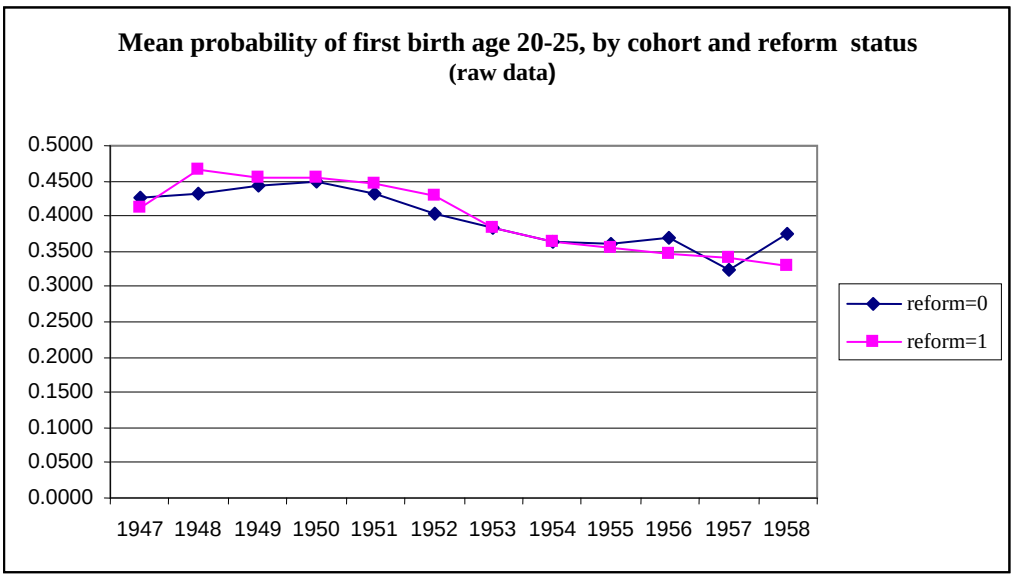


Figure 2

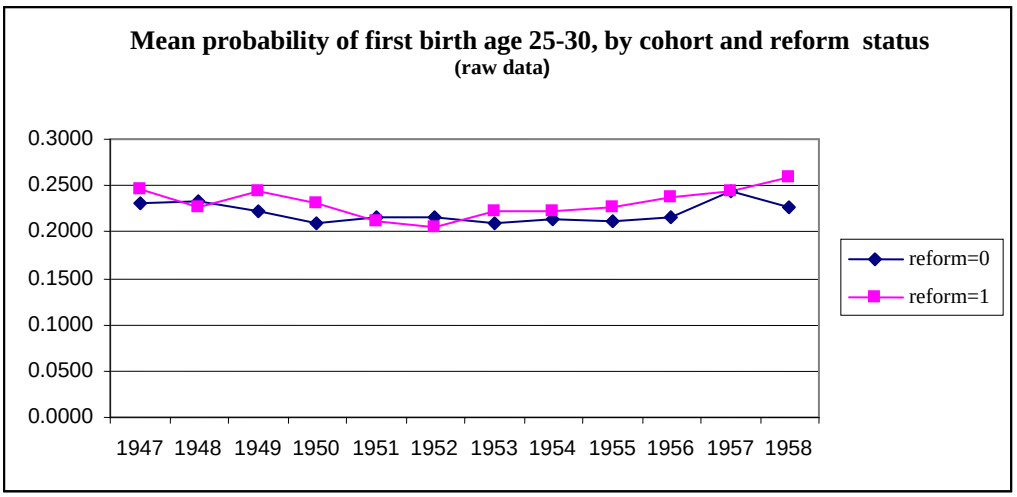


Figure 3

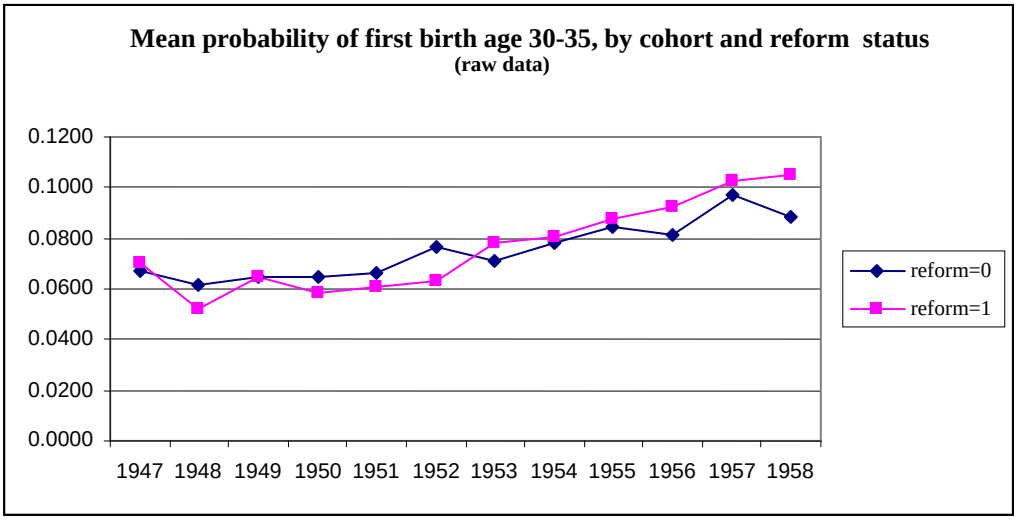


Figure 4

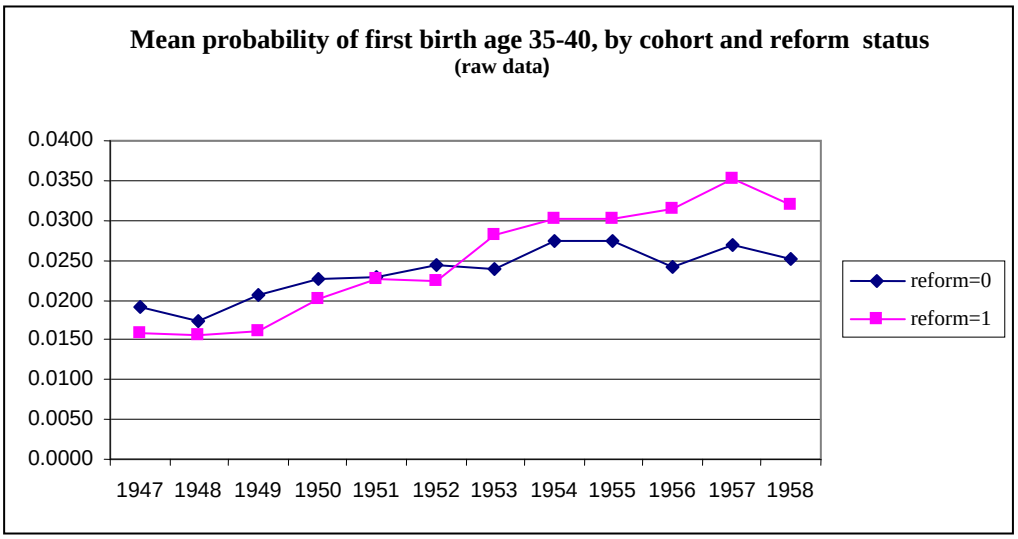


Figure 5

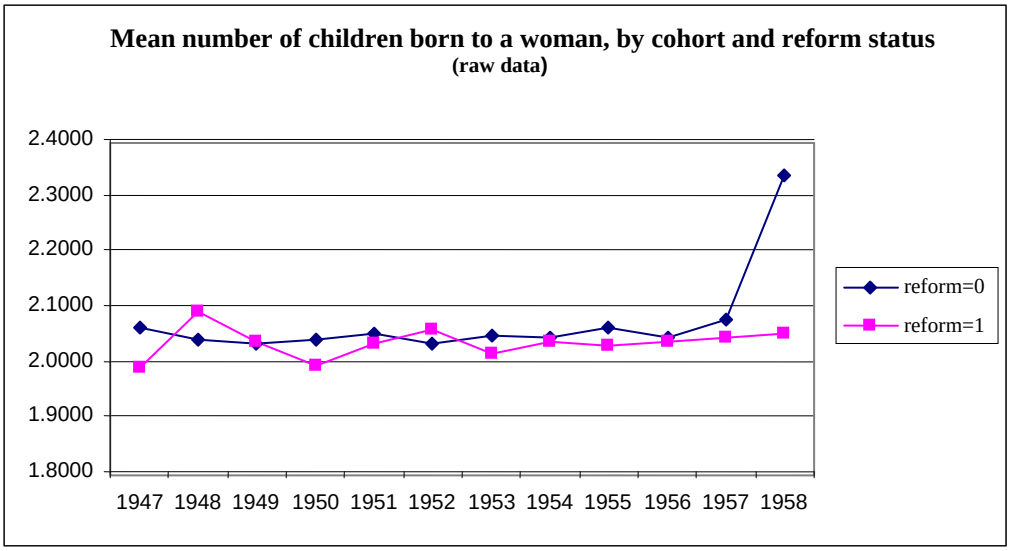


Figure 6

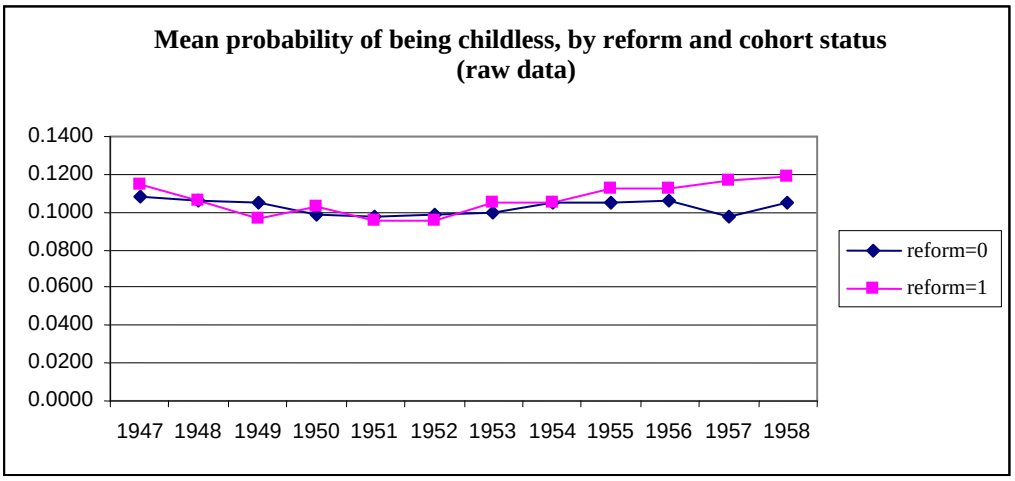


Figure 7

Table 1. Descriptive statistics, explanatory variables

	Reform=0					Reform=1				
	Obs.	Mean	Std. Dev.	Min	Max	Obs.	Mean	Std. Dev	. Min	Max
Years of education	135786	11.3	2.7	7	21	154818	11.7	2.5	7	21
Municipality	135786	1015.6	613.2	101	2030	154818	997.5	581.2	101	2030
Year of birth	135786	1950.6	2.6	1947	1958	154818	1955.1	2.4	1947	1958
Reform	135786	0.0	0.0	0	0	154818	1.0	0.0	1	1
Cohorts:										
1 if born 1958	135786	0.002	0.042	0	1	154818	0.176	0.381	0	1
1 if born 1957	135786	0.010	0.101	0	1	154818	0.169	0.374	0	1
1 if born 1956	135786	0.025	0.156	0	1	154818	0.159	0.365	0	1
1 if born 1955	135786	0.046	0.209	0	1	154818	0.137	0.344	0	1
1 if born 1954	135786	0.075	0.263	0	1	154818	0.110	0.312	0	1
1 if born 1953	135786	0.087	0.282	0	1	154818	0.099	0.298	0	1
1 if born 1952	135786	0.114	0.318	0	1	154818	0.064	0.245	0	1
1 if born 1951	135786	0.124	0.330	0	1	154818	0.039	0.193	0	1
1 if born 1950	135786	0.135	0.342	0	1	154818	0.022	0.146	0	1
1 if born 1949	135786	0.133	0.339	0	1	154818	0.012	0.107	0	1
1 if born 1948	135786	0.127	0.334	0	1	154818	0.009	0.092	0	1
1 if born 1947	135786	0.122	0.327	0	1	154818	0.007	0.081	0	1

Table 2. Descriptive statistics, outcome variables

	Reform=0					Reform=1				
	Obs.	Mean	Std. Dev.	Min	Max	Obs.	Mean	Std. Dev	Min	Max
Outcome variables:										
1 if childless	135786	0.102	0.303	0	1	154818	0.110	0.313	0	1
Number of children	135786	2.044	1.095	0	14	154818	2.035	1.120	0	16
1 if first birth at age 15-20	135786	0.165	0.371	0	1	154818	0.167	0.373	0	1
1 if first birth at age 20-25	135786	0.417	0.493	0	1	154818	0.363	0.481	0	1
1 if first birth at age 25-30	135786	0.220	0.414	0	1	154818	0.235	0.424	0	1
1 if first birth at age 30-35	135786	0.069	0.254	0	1	154818	0.088	0.284	0	1
1 if first birth at age 35-40	135786	0.022	0.148	0	1	154818	0.030	0.170	0	1

Table 3. Results: Marginal effects of education

Expl. var.	Childless	Number of children	First birth age 15–20	First birth age 20–25	First birth age 25–30	First birth age 30–35	First birth age 35–40
Schooling,	0.006 ***	–0.013 ***	–0.032 ***	–0.024 ***	0.030 ***	0.015 ***	0.005 ***
OLS	(0.001)	(0.004)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Reform,	0.001	–0.001	–0.009 **	0.005	0.001	–0.001	0.002 **
OLS	(0.002)	(0.010)	(0.005)	(0.004)	(0.003)	(0.002)	(0.001)
Reform,	0.001	Irrelevant	–0.008 **	0.005	0.001	–0.001	0.002 **
Probit	(0.002)		(0.004)	(0.004)	(0.003)	(0.002)	(0.001)
Schooling,	0.011	–0.009	–0.080 **	0.044	0.012	–0.008	0.021 **
2SLS	(0.018)	(0.087)	(0.039)	(0.032)	(0.028)	(0.018)	(0.009)
N	*290596*	*290604*	*290604*	*290604*	*290604*	*290591*	*289057*

Single, double and triple asterisks indicate significant coefficients at the 10%, 5% and 1% levels, respectively. Number of observations is reported for probit estimations, except for number of children, where n refers to an OLS model with reform as an explanatory variable.

The table shows the estimated coefficients from OLS estimations and marginal effects from probit estimations. Each column denotes separate regressions. Also included in the specifications are municipality and year-of-birth indicators. Standard errors are adjusted for clustering at the municipality level, confer eq. (1).

Table 4. Robustness checks

Expl.var.	Childless	Number of children	First birth age 15-20	First birth age 20-25	First birth age 25-30	First birth age 30-35	First birth age 35-40
(i)							
Schooling, OLS	0.006 ***	–0.013 ***	–0.032 ***	–0.024 ***	0.030 ***	0.015 ***	0.005 ***
Reform, OLS	–0.001	0.006	–0.008 ***	0.007 **	0.001	–0.002	0.002 **
Reform, Probit	–0.001	irrelevant	–0.007 **	0.006 *	0.001	–0.003	0.002 **
Schooling, 2SLS	–0.004	0.046	–0.070 ***	0.062 *	0.011	–0.021	0.022 **
(ii)							
Schooling, OLS	0.005 ***	–0.011 ***	–0.033 ***	–0.023 ***	0.030 ***	0.015 ***	0.005 ***
Reform, OLS	0.001	0.004	–0.006	0.006	0.002	–0.005 *	0.002
Reform, Probit	0.014	irrelevant	–0.005	0.006	0.002	–0.005 *	0.002
Schooling, 2SLS	0.001	0.041	–0.067	0.060	0.018	–0.051	0.022
(iii)							
Schooling, OLS	0.005 ***	–0.012 **	–0.033 ***	–0.024 ***	0.030 ***	0.015 ***	0.005 ***
Reform, OLS	0.001	0.000	–0.009 **	0.005	0.002	–0.002	0.002
Reform, Probit	0.001	irrelevant	–0.008 **	0.005	0.002	–0.002	0.002
Schooling, 2SLS	0.013	–0.004	–0.077 **	0.049	0.016	–0.016	0.016
n, (i)	*290596*	*290604*	*290604*	*290604*	*290604*	*290591*	*289057*
n, (ii)	*160044*	*160122*	*160075*	*160122*	*160122*	*159772*	*156875*
n, (iii)	*227188*	*227217*	*227217*	*227217*	*227217*	*226977*	*224970*

For general comments, see Table 3.

The table shows the results: (i) allowing for municipality-specific trends, (ii) Regression Discontinuity Approach with 3 year time span, (iii) with 5 year time span. The table shows the estimated coefficients from OLS estimations and marginal effects from probit estimations. Each column denotes separate regressions. Also included in the specifications are municipality and year-of-birth indicators. Standard errors are adjusted for clustering at the municipality-year level in estimation (i) and at the municipality level in estimations (ii) and (iii).

Appendix

App. Figure 1
The Number of Municipalities Implementing the Education Reform, by Year

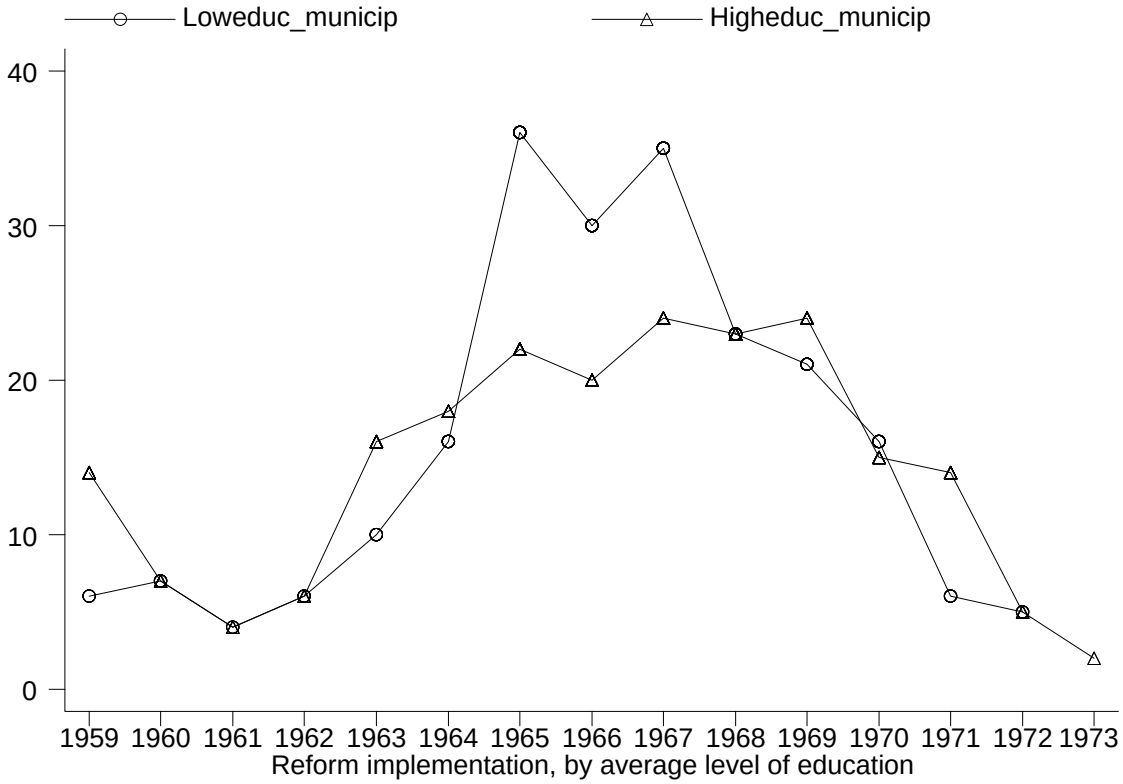
App. Figure 2

Reform implementation in Poor vs Rich Municipalities Based on Average Family Income.

Poor (rich) municipality is calculated as below (above) median parent's income by municipality. Parent's average income is calculated for each municipality in 1970.

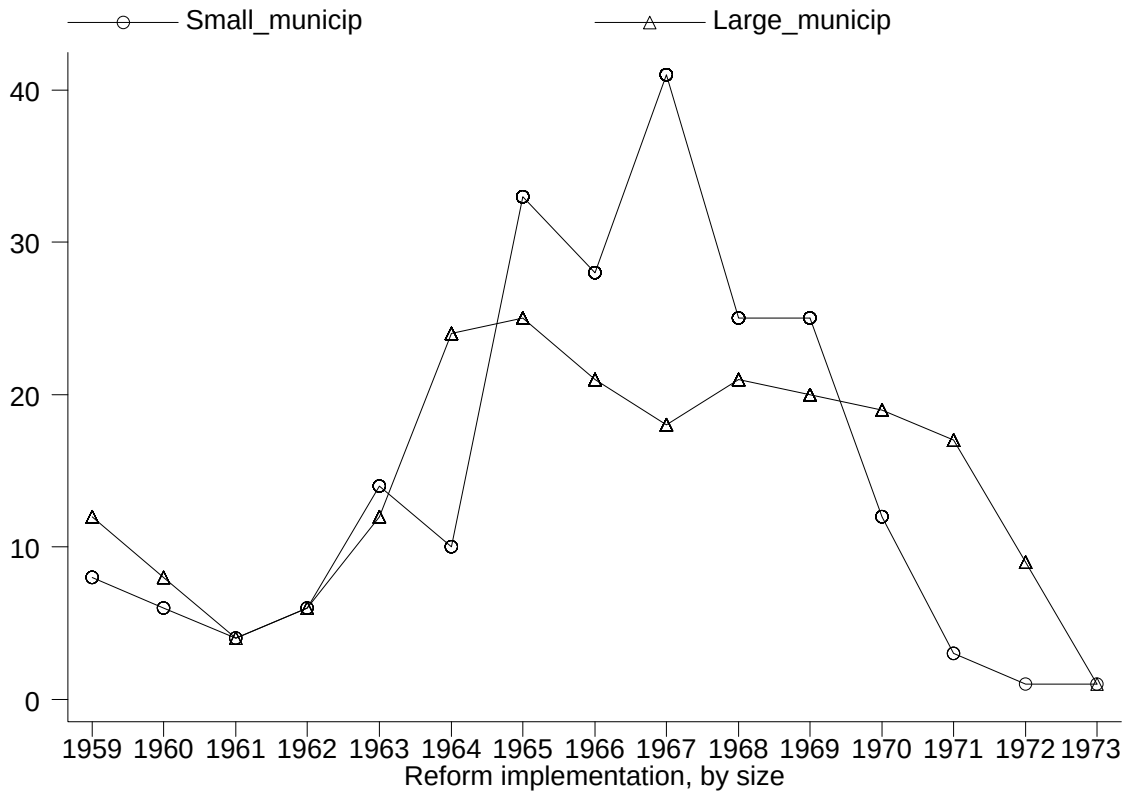
App. Figure 3

Reform Implementation in High vs Low Education Municipalities Based on Average Years of Father's Education in the Municipality



Low (high) education municipality is calculated as below (above) median education by municipality. Father's average years of education is calculated for each municipality in 1960.

App. Figure 4
Reform Implementation in Small vs Large Municipalities



Small (large) municipality is defined as below (above) median municipality as measured by population size in 1960.

Appendix Table 1
Timing of the Implementation of the Reform

Dependent Variable: Year of Reform	Coefficient	Standard error
County 2	–1.95	.65
County 3	5.02	5.23
County 4	–.64	.70
County 5	–.88	.67
County 6	–.90	.62
County 7	–1.21	.63
County 8	–1.90	.64
County 9	–1.21	.64
County 10	–2.20	.71
County 11	–.54	.63
County 12	–1.4	.60
County 13	–.45	.70
County 14	1.23	.59
County 15	–1.54	.58
County 16	.04	.60
County 17	–1.21	.57
County 18	–.26	.65
County 19	–2.77	.71
Share of Fathers with Some College	.92	3.88
Share of Mothers with Some College	12.30	8.31
Father's Income (mean)	–.007	.004
Mother's Income (mean)	–.01	.01
Father's Age (mean)	.11	.16
Mother's Age (mean)	–.12	.19
Size of Municipality/100	–.03	.03
Unemployment Rate 1960	–6.22	11.63
Share Workers in Manufacturing 1960	1.15	3.05
Share Workers in Private Services 1960	5.95	6.23
Share Labour Vote 1961	2.34	2.19
Constant term	1969.14	6.95

All variables are municipality level variables. Standard errors are adjusted for clustering at the municipality level.

Appendix Table 2
Data selection process

	Number of observations
Women born 1947–1958, in total	384385
Excluded because of motherhood before age 15	101
Excluded because woman's education <7 years	783
Missing on municipality	78952
Missing on reform indicator	11841
Missing on woman's length of education	2104
Sample size	290604