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KARIN MONSTAD, CAROL PROPPER AND  
KJELL G. SALVANES

EDUCATION AND FERTILITY:  
EVIDENCE FROM A NATURAL  
EXPERIMENT



Department of Economics  

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UNIVERSITY OF BERGEN

# **Education and Fertility: Evidence from a Natural Experiment\***

Karin Monstad  
Department of Economics  
Health Economics Bergen  
University of Bergen  
karin.monstad@econ.uib.no

Carol Propper  
Department of Economics  
Bristol University, Imperial College and CEPR  
Carol.Propper@bristol.ac.uk

Kjell G. Salvanes  
Department of Economics  
Norwegian School of Economics, Statistics Norway, Center for the Economics of Education  
(CEP) and IZA  
kjell.salvanes@nhh.no

## **Abstract**

In many developed countries a decline in fertility has occurred. This development has been attributed to greater education of women. However, establishing a causal link is difficult as both fertility and education have changed secularly. The contribution of this paper is to study the connection between fertility and education over a woman's fertile period focusing on whether the relationship is causal. We study fertility in Norway and use an educational reform as an instrument to correct for selection into education. Our results indicate that increasing education leads to postponement of first births away from teenage motherhood towards having the first birth in their twenties and, for a smaller group, up to the age of 35-40. We do not find, however, evidence that total fertility falls as a result of greater education.

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## 1. Introduction

The demographic transition in developed countries is characterised by low fertility and mortality rates. Low fertility has come to be an issue of public concern as low population growth and higher dependency ratios due to the aging of the population have been argued to strangle economic growth (Turner *et al.*, 1998). Many policy observers and researchers have pointed to the timing of the secular decline in fertility in the Western countries and the increased education level for women and suggested that increased education for women leads to lower completed fertility (Schultz, 1997; Cochrane, 1979; Skirbekk, Kohler and Prskawetz, 2004). However, while numerous studies have found support for a correlation between female education and fertility decisions, there are few examples that establish the causal effect of female education on fertility decisions. Many factors are likely to influence fertility, education and employment decisions including unobservable factors that are difficult to control for. Thus, causation is difficult to establish.

Compared to most other developed countries, Nordic countries have high levels of completed fertility. The number of children per woman in the different countries has converged over time and for those cohorts who have now completed their fertility, the number of children is close to the replacement level at 2.1 (Sleebos, 2003; Björklund, 2006). The major policy 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). In this paper we exploit an educational reform which extended the mandatory years of schooling from 7 to 9 years to identify the causal effect of education on both the timing of, and completed, fertility of women born between 1947 and 1958 in Norway.<sup>1</sup> During the 1960s in Norway, there was a large change in the compulsory schooling laws affecting primary and

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<sup>1</sup> This extends Black, Devereux and Salvanes (2008) who studied the effect of the same reform on teenage pregnancy.

middle schools. Pre-reform, the Norwegian education system required children to attend school through the seventh grade; after the reform, this was extended to the ninth grade, adding two years of required schooling. Evidence in the literature suggests that these reforms had a large and significant impact on educational attainment. Central to our identification strategy, implementation of the reform occurred in different municipalities at different times, starting in 1960 and continuing through 1972, leading to variation at municipal level as well as over time in the minimum years of schooling.

We examine the relationship between the education of women and three fertility outcomes: the timing of children; childlessness; and the number of children. The data shows the expected correlation between fertility outcomes and education: women with more education are more often childless; they have fewer children and postpone births. However, despite these statistically significant correlations, we do not find evidence of a causal relationship between the length of education and completed fertility or childlessness 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 rest of the paper is as follows. Section 2 begins with a brief overview of the literature on the relationship between fertility and education before presenting data on fertility in Norway and the institutional setting, outlining the major elements of the support system for parents in Norway. Section 3 describes the change in compulsory schooling that is used as an instrument in this study. The identification strategy is formalised in section 4. Section 5 presents the data. The results are presented in section 6 and section 7 concludes.

## 2. The institutional setting

### *Education and fertility*

Economists have modelled fertility in terms of the costs and benefits of children. In this setting, education may affect fertility decisions through several channels. For example, education may provide better knowledge of contraceptives or educational activity may be incompatible with time spent engaged in childcare. Of special focus has been the impact of changes in labour market returns as a result of greater education.<sup>2</sup> 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 (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 to put more resources into each child's upbringing.

Models of the timing of births (as distinct from the quantity) present a trade-off between the greater pleasure of early births and the lower costs of later births with a focus on the latter.<sup>3</sup> 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 capital in later periods due to depreciation.<sup>4</sup>

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<sup>2</sup> Hotz *et al.* (1997) provide a review of static models on completed fertility.

<sup>3</sup> Happel *et al.* (1984) assume that there is no pure time preference associated with the household's "effective" number of children.

<sup>4</sup> Gustafsson (2001) presents a list of parameters that will have a positive effect on birth postponement: 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.

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) utilized an Italian mandatory school reform introduced in 1963 that prescribed junior high school attendance, so that compulsory schooling increased from five to eight years. The implementation period turned out to be unintentionally long and compliance was poor, especially in Southern Italy. The estimated effects are restricted to those who had at most eight years of schooling. Her findings suggest that the reform led these women to postpone their first childbirth, but they caught up in terms of fertility by age 26.<sup>5</sup> However, Fort is not able to control for region, and economic conditions, traditions regarding fertility and labour market aspirations differed profoundly between regions.<sup>6</sup>

For Norway, Lappegård and Rønsen (2005) present data for 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

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<sup>5</sup> 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.

<sup>6</sup> 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.

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.

Closest to our paper is Black, Devereux and Salvanes (2008) who study the relationship between education and the probability of teenage motherhood.<sup>7</sup> The authors use changes in compulsory schooling laws in the USA and Norway as instruments for education in the two countries to identify the effect under two very different institutional environments. They find evidence that increasing mandatory education reduces the incidence of teenage motherhood and that the size of the effect is quite similar in the USA and Norway. Their results indicate that the effect of compulsory schooling laws goes beyond a pure “incarceration” effect.<sup>8</sup>

### *Institutional setting*

Norway has seen changes over time in the support systems for the costs of bringing up children such as direct financial support and the provision of public day care, as well as the availability and acceptance of contraception and in norms towards single mothers.<sup>9</sup> Thus an understanding of the setting is important to establish that the educational reform is not correlated with other policies that affect the costs of bringing up children.

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

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<sup>7</sup> To our knowledge, this is the only existing paper that aims to estimate a causal relationship between fertility and education using Norwegian data.

<sup>8</sup> Skirbekk, Kohler and Prskawetz (2004) analysed the causal effect on the timing of fertility (and marriage) using Swedish data and exploiting the administrative rule for school start age in Sweden. They found quite strong effects on the timing of birth on graduation age for women.

<sup>9</sup> 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.

child are the statutory universal rights parents have in connection with birth and the supply of subsidized childcare (Rønsen, 2004). A universal right to 12 weeks paid maternity leave was introduced in 1956, but the income compensation was relatively low. A major extension came in 1978 when maternity benefits were raised to cover 100% of the pre-birth income for 18 weeks.<sup>10</sup> This entitlement was extended to 20 weeks in 1987 and 22 weeks in 1988. Since 1993, mothers can choose between 42 weeks maternity leave at full pay and 52 weeks at 80% compensation. Since 1977, parents have been entitled to unpaid leave with job security until the child is one year old.<sup>11</sup> 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. 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).

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. All parents receive a tax-free child allowance in Norway and single parents get about 1000 NOK extra per month in 2007.

Public day care, which is subsidized in Norway, is subject to excess demand. Single parents pay a reduced rate for day care. Enrolment rates have risen sharply, from 5% in 1973

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<sup>10</sup> 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.

<sup>11</sup> 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.



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.

Attitudes towards teenage mothers became more accepting in Norway during the 1970s (see Furre, 1996). Knowledge about sexual behaviour was made part of the compulsory school curriculum and contraceptives became more widely available. The pill was introduced in the late 1960s and was widely used. It is estimated 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; by 1988, an even higher proportion used contraceptives (Noack and Østby, 1991). Abortion was legalized in Norway in 1979.<sup>12</sup>

We study in this paper women born between 1947 and 1958. Thus these various policy changes aimed at reducing the costs of fertility will have affected this group. For example, these women were in their peak fertility ages (20 to 31 years old) when the major extension of maternity rights was implemented in 1978, though 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. Changes in availability of contraception and norms towards single parents also started within our period of analyses, though in this case will apply to the younger, but not the older, cohorts in our data. Our approach therefore has to control for these changes brought about by national policies and attitudinal change.

### **3. Changes to compulsory schooling laws**

This paper exploits the differential introduction of an increase in compulsory schooling across municipalities in Norway to identify the impact of greater education on fertility. We begin by describing the changes in the schooling laws and then examine whether the date of

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<sup>12</sup> 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).

implementation in each municipality is likely to be a function of fertility or other observed factors that may be associated with fertility decisions.

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 goal of standardizing the curriculum was to improve the average level of quality of the schools; the increase in mandatory education was therefore likely accompanied by an improvement in school quality. As a result, our estimates will incorporate both the increase in years of education along with an improvement in quality. Given the positive correlation between the two, we will likely *overestimate* the effect of extra years of education on children's educational attainment.

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.<sup>13</sup> This suggests that for more than a decade Norwegian schools were divided into two separate systems. The system experienced by children would depend on year of birth and municipality of residence. The first cohort that could have potentially been subject to the reform was that born in 1947. These individuals started school 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.

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<sup>13</sup> 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.

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).<sup>14</sup> Figure 1 in the Appendix depicts the spread of the reform, focusing on the number of municipalities implementing the reform each year.

For our identification strategy to work, we need, *inter alia*, the data of implementation to be uncorrelated with fertility in the municipality. We include municipality fixed effects and municipality-specific trends in our specifications, and hence it is not necessary for our estimation strategy that fertility outcomes are uncorrelated with the timing of the implementation of the reform across municipalities. If other fertility enhancing changes took place differently across Norwegian municipalities but in a linear fashion, they will be picked up by the municipality-specific trends. However, it is useful to look in more detail as to whether the implementation of the reform is uncorrelated with fertility behaviour. In what follows, we discuss some of the main technological or policy changes that may have affected fertility and relate them to the reform implementation process.

In a comparative study of Norwegian family policy, the programs with the greatest implications for the cost of a child are regarded to be the availability of subsidized child care and maternity leave (Rønsen, 2004). Maternity leave is decided at a national level so the effect of these policies will be picked up by cohort fixed effects, as all municipalities would have been subject to the policy change at the same pace and with equal strength. Day care is state-subsidized in Norway and has been subject to excess demand. At a national level,

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<sup>14</sup> 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).

enrolment rates have risen sharply<sup>15</sup>. But the availability of day care, and the fees charged to parents, have varied profoundly among Norwegian municipalities. For instance, enrolment for children under the age of 6 ranged from 11 to 92 percent in 1991 (Kravdal, 1996). Two studies on the association between day care supply and parity-specific fertility reach somewhat different conclusions regarding first births<sup>16</sup>. Kravdal (1996) studied the association between the local coverage rate for children 1-3 years old and the probability of a first birth for women aged 15-29 from cohorts born between 1945 and 1968. The estimated relationship is negative but became statistically insignificant when controlling for region, degree of urbanization, occupational structure, religious activities, and female employment rate in 1980. Day-care supply appears to be important for timing of fertility only at the very lowest level of coverage (0-4 percent). Interactions between day care coverage and educational level of the women were also not found to be statistically significant. Rønsen (2004) found a statistically significant negative association. But in this study, the data on day care coverage are less accurate, because the 1973 coverage levels have been used also for the years 1960-1973 and in contrast to Kravdal(1996), Rønsen did not control for degree of urbanization. It is likely that the supply of day-care in the initial years was strongly correlated with urbanization, which is itself correlated with postponement of first births.

The Norwegian legislation on induced abortion was liberalized in two steps during the 1970s. From 1976, induced abortion on social indications was legally accepted, and in 1979 women won the right to induced abortion on demand (Skjeldestad, 1986). Before the law implemented in 1979, applications for abortion had to be approved of by a local committee with two medical doctors as members. The great majority of applications were approved<sup>17</sup>.

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<sup>15</sup> The number of child care spaces per 100 children aged 0-6 has risen from 5 in 1973 and 13 in 1977, to 21 by 1980, 36 in 1990, 40 by 1992 and 54 in 2001((Rønsen (2004) and Kravdal(1996)).

<sup>16</sup> For higher parities, Kravdal (1996) and Rønsen (2004) both find that the association is insignificant; negative for second birth and positive for third birth.

<sup>17</sup> The approval rate was 90% in the early 1970s and 98% in 1978 (Blom and Elvbakken, 2001)

However, the liberalization of the law in 1979 was not followed by an increase in abortion rates. On the contrary, the general abortion rate decreased from 1975 onwards (Skjeldestad, 1986).<sup>18</sup> Skjeldestad studied induced abortion in seven counties from all parts of the country in the period 1972-1983<sup>19</sup>. He concludes that the difference in abortion rates among the counties remained the same throughout the period of the study, except for one county (Finnmark). Thus, the law changes did not lead to any drastic changes in abortion rates either at a national level or regional level.

Østby (1983) examined the use of contraceptives in the 1960s and 1970s and found that there was very little difference in the proportion who used contraceptives from one region to another, but that the proportion was somewhat higher in densely populated areas. The use of contraceptives followed a rising trend that was parallel for municipalities of different centrality (Østby, 1983, fig. 3). By 1988, the use of contraceptives is about equally widespread in sparsely and densely populated areas (Noack and Østby, 1991). It seems reasonable to think of the diffusion of contraceptive technology and the change in norms as a gradual process, which can be captured by municipality-specific trends and cohort fixed effects.

As a more formal examination of whether reform was endogenous to fertility, we examine the relationship between the introduction of the reform (by municipality) and the pre reform changes in rates of teenage pregnancy. We estimate a hazard model of time to implementation where the explanatory variable is the lagged change in teenage birth rate

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<sup>18</sup> The decrease from 1975 onwards refers to the whole population of women. But there is one important exception: the age group 15-19. For this group the abortion rate (legally induced abortions per 1000 women in age group) increased from 13.0 in 1972 to 21.8 in 1976 and 1977. This age group is most affected by educational reform and presumably most constrained in their choice before the liberalization of the laws. Our cohorts were 15-19 in 1962-1973. The increase in teenage abortion rates from 1972 onwards are therefore only relevant for the last two cohorts in our sample.

<sup>19</sup> These seven counties represent 23 % of the total population of fertile women in Norway. Taken together, they have the same abortion rate as the whole population for the period where a comparison is possible (from 1979-1983, Skjeldestad, 1986). The author considers them to be “a reasonably good estimate of the development of induced abortion in Norway.”

(birth rate defined as the number of first births in the age group 15-20 years per 100 women of that age group in the municipality). The results, shown in Table A1 in the Appendix, shows there is no relationship between the lagged change (from 2 to 5 years) in teenage fertility and the timing of reform.

Other municipality characteristics may also be associated with fertility outcomes. For example, poorer municipalities might be earlier implementers of the reform given the substantial state subsidies, while wealthier municipalities would move at a much slower pace. However, research which has examined 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. Municipalities that were located geographically near municipalities that had already implemented the reform were themselves more likely to implement the reform (interviews revealed that this was likely due to a particularly effective county administrator). This research supports a complex adoption process without finding support for the implementation process to be explained by one single factor (Lie, 1973, 1974)..

Aakvik, Salvanes and Vaage (2008) and Black and Salvanes (2008) find no support for a relationship between the timing of the educational reform and education level, income level or the size of the municipalities. As a further test, in Table A2, 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).

#### 4. Our identification strategy

Our source of exogenous variation in mothers' education is the education reform in Norway that increased the number of years of compulsory schooling from 7 to 9 years and was implemented over a 12 year period from 1960 to 1972 in different municipalities at different times. We observe the children of this generation in 2002.

Our empirical model is given by the following two equations:

$$Y_i = \beta_0 + \beta_1 ED_i + \beta_2 COHORT_i + \beta_3 MUNICIPALITY_j + \beta_4 MUNTREND_j + \varepsilon_{ij} \quad (1)$$

$$ED_i = \alpha_0 + \alpha_1 REFORM_j + \alpha_2 COHORT_i + \alpha_3 MUNICIPALITY_j + \alpha_4 MUNTREND_j + \nu_{ij} \quad (2)$$

$Y_i$  is the fertility outcome. We study three fertility outcomes: the timing of children; the number of children; and childlessness: the first and third of these are binary variables.  $ED$  is the number of years of education obtained,  $COHORT$  refers to a full set of years of age indicators,  $MUNICIPALITY$  refers to a full set of municipality indicators,  $MUNTREND$  denotes municipality-specific trends, and  $REFORM$  equals 1 if the individual was affected by the education reform and 0 otherwise. We estimate the model using Two Stage Least Squares (2SLS) so that equation (2) is the first stage and  $REFORM$  serves as an instrumental variable for  $ED$ . In our estimation, we test the strength of the first stage relationship.

Our identification strategy rests on the assumptions that we can isolate the exogenous variation in education that results from the reform and that the reform affects fertility only via education. In terms of the first assumption, equation (1) contains fixed cohort and municipality effects. The first of these allow for secular changes in educational attainment over time that may be completely unrelated to compulsory schooling laws; the second allows for variation in the timing of the reform across municipalities that may not have been

exogenous to educational choice. However, our discussion in Section 3 above has indicated that the adoption of reform does not appear to be associated with possible political concerns over fertility (the change in teenage pregnancy rates) or with other observed characteristics of the municipality. We also allow for municipality specific trends to pick up other observables which were trended differentially across municipalities.

In terms of the assumption that reform only affects fertility via education, many of the policy changes that might affect the costs of having children - and so affect fertility - were, as documented above, implemented at national level and introduced at the same date in all municipalities. These will not destroy our identification strategy. Attitudinal changes might be more likely to be variable across municipalities but, as shown above, these tend to be related to urban-ness, which is controlled for by the municipality fixed effects. The possibility remains that the reform timing was associated with unobserved characteristics which may affect fertility. But in our design, even if the reform was implemented first in areas with certain unobserved characteristics, consistent estimation is still achieved so long as (a) these characteristics are fixed over time; (b) the implementation of the law change is uncorrelated with changes in these characteristics; or (c) these characteristics are unrelated to the probability of childlessness, the timing of births and the number of children. As these characteristics are unobservable, we cannot test this directly.

## **5. Data**

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.<sup>20</sup> To this, we match extracts from the censuses in 1960, 1970 and 1980.

Our sample contains all women born between 1947 and 1958. To determine whether women were affected by the changed compulsory schooling legislation, we link each woman to the municipality where she grew up 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.<sup>21</sup> 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 age 13 (the seventh year of schooling), the new system had been implemented in her municipality of residence, which is defined as 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. However, because the reform implementation did not occur before 1960, reform-induced mobility should not be a problem.<sup>22</sup> 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 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

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<sup>20</sup> See Møen, Salvanes and Sørensen (2003) for a description of the data set.

<sup>21</sup> 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.

<sup>22</sup> Evidence from Meghir and Palme (2005) for Sweden and Telhaug (1969) for Norway suggests that reform-induced migration is not a significant consideration.

(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.<sup>23</sup> 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.

We observe the children on our sample in 2002. At that date, the youngest women in our sample were 44 years of age and thus for all but a tiny minority we observe the complete fertility history.<sup>24</sup> 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 20<sup>th</sup> birthday. Table A3 provides details of the data selection process.

### *Descriptive statistics*

Table 1 presents the key explanatory variables as well as background and outcome variables, the sample split by whether the individual was subject to the reform or not. The estimation sample consists of 290,604 women, 53% (154,818 individuals) of whom were affected by the

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<sup>23</sup> 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.

<sup>24</sup> We also do not observe birth dates of children who subsequently died.

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.

As for fertility outcomes, the differences in means are small between the two groups. 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 was 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.<sup>25</sup> 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.

## **6. Results**

Table 2 presents our key results. Each coefficient is derived from a separate regression, each of which controls for municipality and year of birth. Municipality-specific trends were initially included, but were not statistically significant and the results did not change when including them, so they are not included in the results reported here.<sup>26</sup>

In row 1 we present the OLS estimates of the correlation between years of education and the different fertility outcomes (i.e. estimation of equation (1)). 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. These correlations estimated are of a rather large magnitude. For

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<sup>25</sup> The 1958 cohort is an outlier. The number of non-reform women in that cohort is small (238 observations).

<sup>26</sup> The F tests have been executed after OLS estimations for each fertility outcome. F-values range from 0.50 to 0.76 with a critical value of 1.30. Available from authors.

example, the probability of remaining childless increases by half a percentage point for each additional year of education.

Our interest lies in the causal relationship of education and so in the coefficients from models where the educational reform is used as an instrument for education. The second row presents the results from the 2SLS estimation. The results show that the impact of education is to reduce the probability of a teen birth and delay first births into the twenties and late thirties, but it has no effect on completed family size. Therefore, the effect of schooling is essentially to delay child bearing. In contrast with the raw association with education, when controlling for possible selection into education, there is no significant relationship between length of education and the number of children born to a woman or the probability of never having children. The first stage equation is presented in Table A4 and shows a strong impact of the reform on educational attainment.

The magnitude of the estimated effects on timing is considerable. At the margin, each year of additional education reduces the probability of teenage motherhood by 8 percentage points, which is a large impact relative to the frequency of teenage motherhood in the whole sample, which is 16.6 %.<sup>27</sup> Likewise, the increase in probability of giving first birth aged 35 to 40 is 2 percentage points, which is almost as much as the frequency (3%) in the population. The estimated effect when controlling for possible endogeneity is substantially larger than the OLS estimate: a common result when using these kinds of instruments due to heterogeneity in the effect of education (Card, 1999; Aakvik, Vaage and Salvanes, 2008). The effect estimated is a local average treatment effect and at the bottom part of the education distribution the returns are expected to be high from two extra years of education.

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<sup>27</sup> 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.

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 the OLS and the 2SLS results using a sample restricted to those girls who are most likely to be affected by the reform. We define these as those who were aged thirteen within five years before or after the year of reform implementation. The results are presented in Table 3. The correlation between education and the various fertility outcomes estimated by OLS are very similar to those in Table 2. The coefficient estimates for the causal model are also very similar to those in Table 2. The statistical significance of the estimates falls, due to the smaller sample, and the impact of education on first births at age 35-40 is not longer statistically significant, but the effect of education on delaying births beyond the teenage years is still well defined.

### *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, Devereux and Salvanes (2008) 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 shows 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 therefore 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 increased risks of fecundity problems, which will have costs to individuals and, in a publicly funded system, to the health care system.

The effects we find 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. However, it is likely that there is a great deal of heterogeneity between women in how they respond to the reform. Our results show that in most cases where first births were postponed due to the 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. However, it is also 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.<sup>28</sup>

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<sup>28</sup> In principle, institutional changes may alter both timing and total fertility, for instance by making childless women change their mind. Perhaps the 1993 extension of paid leave spurred fertility in the reform group among cohorts that were fertile but still childless. In our sample these would be the 1953-1958 cohorts who would be aged 35-40 years. To assess the effect of the 1993 reform, we would need total fertility data on younger cohorts that is not yet available.

## 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 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.



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Figure 1. Mean unconditional probability of first birth age 15-20 by cohort and reform status

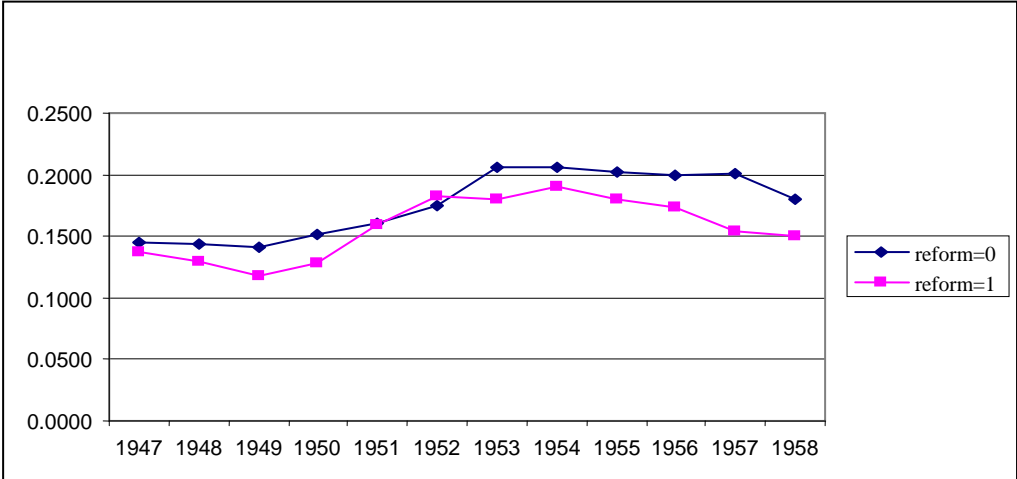


Figure 2. Mean unconditional probability of first birth age 20-25 by cohort and reform status.

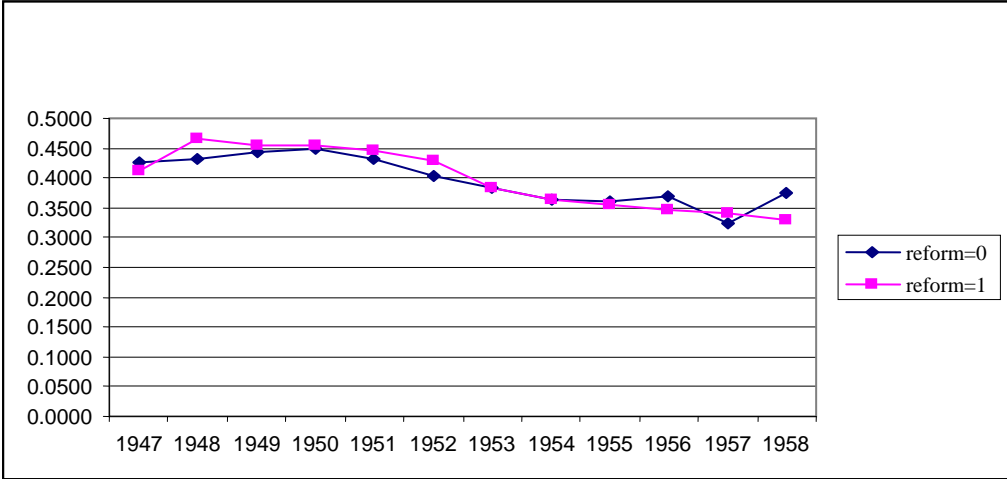


Figure 3. Mean unconditional probability of first birth age 25-30 by cohort and reform status.

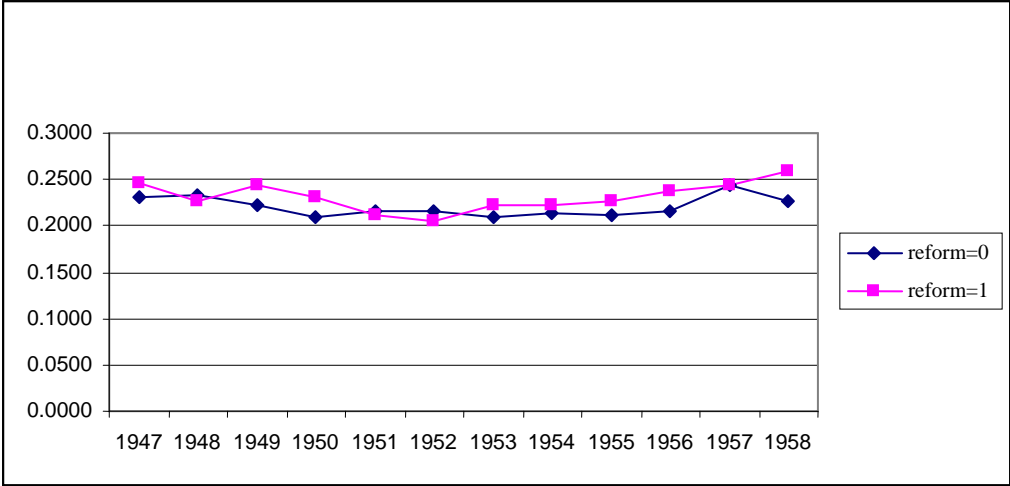


Figure 4. Mean unconditional probability of first birth age 30-35 by cohort and reform status.

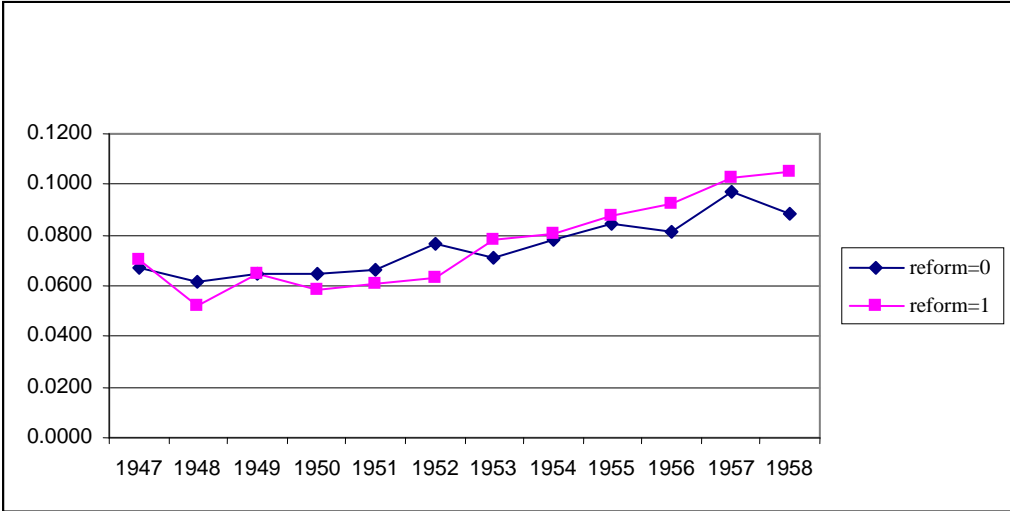


Figure 5. Mean unconditional probability of first birth age 35-40 by cohort and reform status.

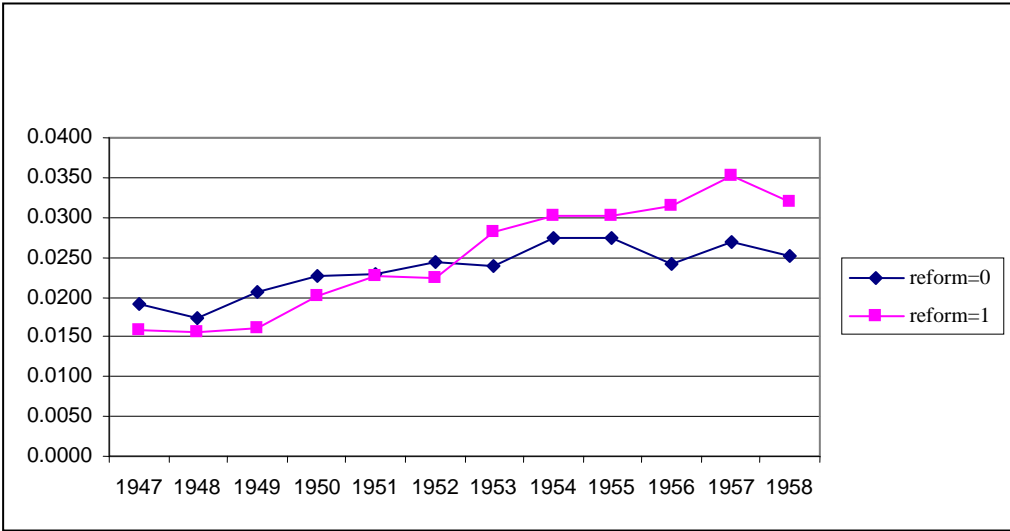


Figure 6. Mean number of children born to a woman by cohort and reform status.

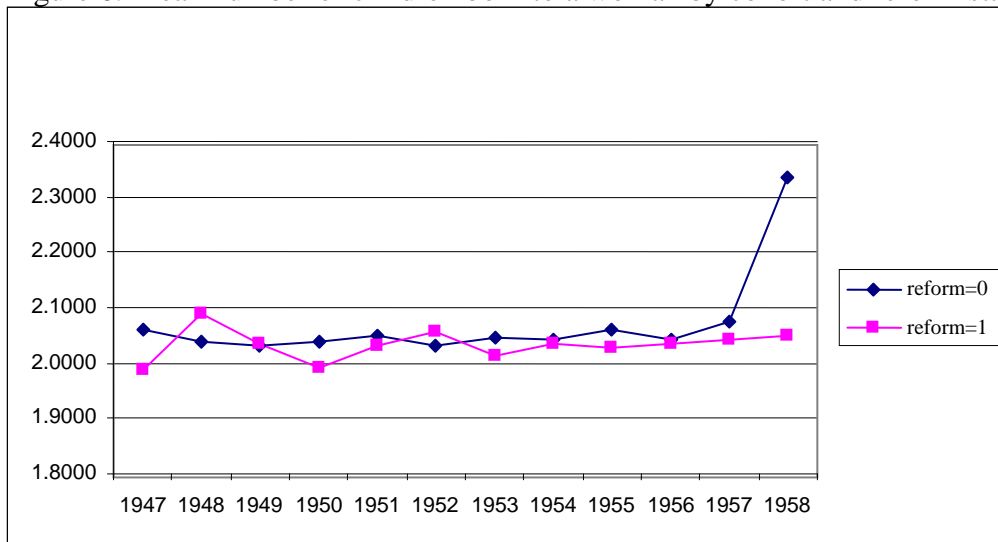


Figure 7. Mean unconditional probability of being childless by cohort and reform status.

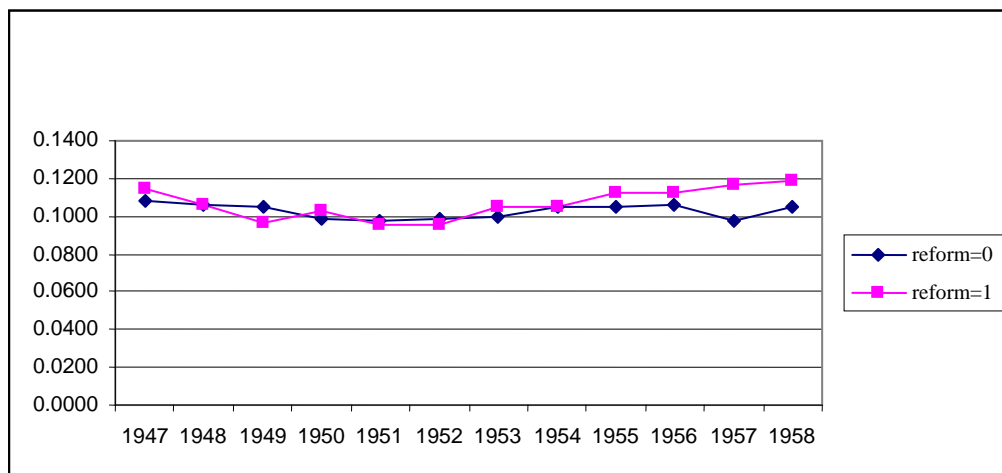


Table 1. Descriptive statistics: data separated by whether women lived in a reform municipality at age 13

	Without reform (n=135786)				With reform (n=154818)			
	Mean	Std.Dev.	Min	Max	Mean	Std.Dev.	Min	Max
Years of education	11.25	2.66	7	21	11.72	2.47	7	21
Year of birth	1950.62	2.56	1947	1958	1955.08	2.43	1947	1958
<i>Background variables*:</i>								
1 if lived in one of the ten major cities in 1960	0.153	0.360	0	1	0.226	0.418	0	1
1 if lived in one of 49 other cities in 1960	0.123	0.329	0	1	0.110	0.313	0	1
Father's years of schooling	8.65	2.54	7	18	8.97	2.66	7	18
Father's age in 1960	42.58	7.55	6	90	37.55	7.49	2	90
Mother's years of schooling	7.94	1.65	7	18	8.19	1.78	7	18
Mother's age in 1960	39.05	6.80	7	89	34.04	6.85	4	86
Parents' income in 1970, 100 NOK	256.28	285.05	0	14439	378.61	254.14	0	14058
<i>Outcome variables:</i>								
1 if first birth at age 15-20	0.165	0.371	0	1	0.167	0.373	0	1
1 if first birth at age 20-25	0.417	0.493	0	1	0.363	0.481	0	1
1 if first birth at age 25-30	0.220	0.414	0	1	0.235	0.424	0	1
1 if first birth at age 30-35	0.069	0.254	0	1	0.088	0.284	0	1
1 if first birth at age 35-40	0.022	0.148	0	1	0.030	0.170	0	1
1 if childless	0.102	0.303	0	1	0.110	0.313	0	1
Number of children	2.04	1.09	0	14	2.04	1.12	0	16

Notes:

\*A few observations lack information on parental background, particularly father's age and schooling (5% missing in both subsamples).

Table 2. The effect of education on fertility

Outcome	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
OLS	0.006*** (0.001)	–0.013*** (0.004)	–0.032*** (0.001)	–0.024*** (0.001)	0.030*** (0.000)	0.015*** (0.000)	0.005*** (0.000)
2SLS	0.011 (0.018)	–0.009 (0.087)	–0.080** (0.039)	0.044 (0.032)	0.012 (0.028)	–0.008 (0.018)	0.021** (0.009)
N	290596	290604	290604	290604	290604	290591	289057

Notes:

Each column is a separate regression. Included in the specifications are municipality and year-of-birth indicators. Municipality-specific trends are excluded due to non-significance. Standard errors are adjusted for clustering at the municipality level. Single, double and triple asterisks indicate significant coefficients at the 10%, 5% and 1% levels, respectively.

Table 3. Regression Discontinuity Approach: sample restricted to those aged 13 within a 5 span before or after reform implementation in municipality of residence.

Outcome	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
OLS	0.005*** (0.001)	–0.012** (0.005)	–0.033*** (0.001)	–0.024*** (0.001)	0.030*** (0.001)	0.015*** (0.000)	0.005*** (0.000)
2SLS	0.013 (0.022)	–0.004 (0.083)	–0.077** (0.032)	0.049 (0.042)	0.016 (0.032)	–0.016 (0.021)	0.016 (0.012)
N	227188	227217	227217	227217	227217	226977	22497

Notes:

Each column is a separate regression. Included in the specifications are municipality and year-of-birth indicators. Municipality-specific trends are excluded due to non-significance. Standard errors are adjusted for clustering at the municipality level. Single, double and triple asterisks indicate significant coefficients at the 10%, 5% and 1% levels, respectively.

# Appendix

Figure A1. The Number of Municipalities Implementing the Education Reform by Year

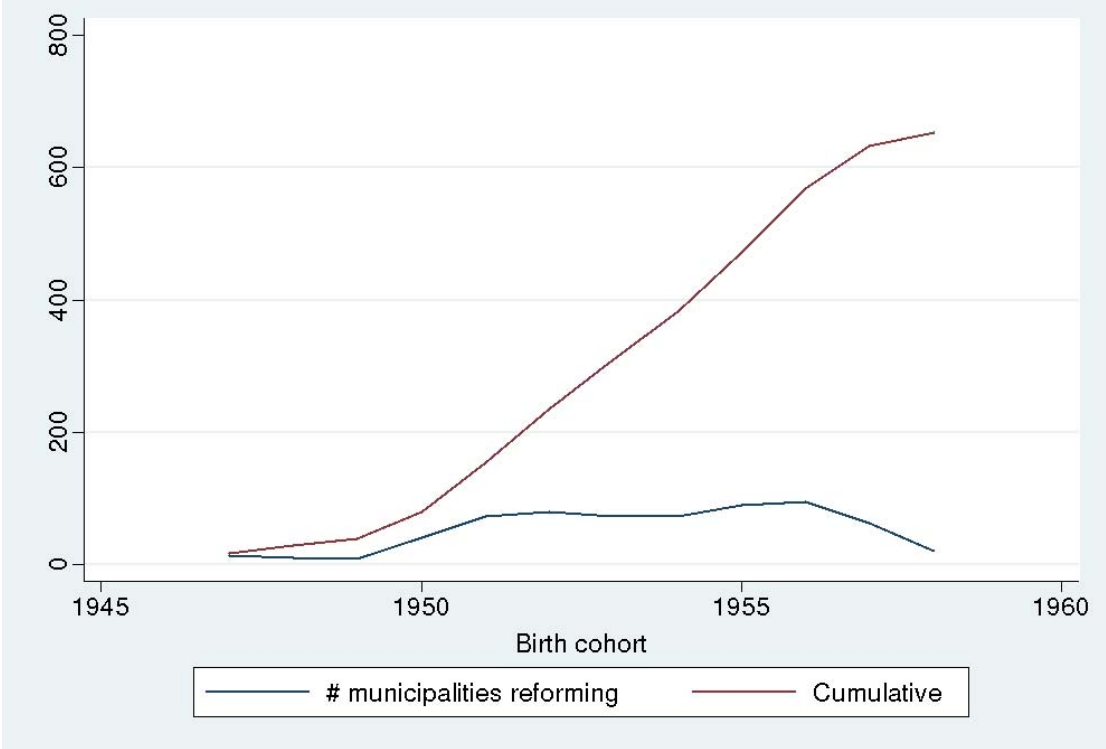


Table A1. Estimates of the association between the date of reform implementation and lagged changes in teenage birth rate pre reform.

Lag in change in teen birth rate (years)	Coef.	Std. Err.	P> z	Pseudo-R2	N
5	0.002	0.005	0.661	0.0001	3650
4	0.004	0.005	0.437	0.0002	3142
3	0.012	0.008	0.115	0.0008	2562
2	0.010	0.013	0.454	0.0002	1940

Notes:  
 Hazard estimated using logit model. Each row is a separate estimation. Data used is combinations of municipalities and potential years of reform implementation, i.e. 1960-1972, up to the actual year of implementation. The dependent variable equals 1 for the year when the reform was implemented, and 0 for pre-reform years.



Table A2. Test for exogeneity of the school reform at the municipality level

	Year of introduction of reform
	Coeff (se)
Mean income	.000 (.000)
% Tertiary education	-13.174 (16.18)
% Secondary education	8.609 (9.47)
% Primary education	-1.497 (2.21)
% working in Services	-.006 (4.35)
% working in Manufacturing	-2.797 (3.56)
% out of labour force	-5.618 (8.87)
% Married at least once	-4.649 (6.40)
Log Municipal population	-.051 (0.19)
% voting for the Labour party	-.335 (0.84)
% in ages 0-17	-7.891 (7.32)
% in ages 18-34	7.241 (10.03)
% in ages 35-64	17.677 (10.96)
County dummies	Yes
Sample size	650
R-squared	0.180

Notes:

All explanatory variables are aggregated from the 1960 census, except for the electoral results and turnout, which are from 1959 elections, and income data, which is from 1967. Dependent variable is birth year of first cohort subject to reform.

Table A3. Data selection

	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

Table A4. First stage estimates: Years of education as a function of reform

	Coef.	Std. Err.	P> t
Reform	0.116	0.017	0.000
<i>Cohort</i>			
Born 1957	-0.094	0.022	0.000
Born 1956	-0.151	0.021	0.000
Born 1955	-0.178	0.022	0.000
Born 1954	-0.238	0.022	0.000
Born 1953	-0.257	0.023	0.000
Born 1952	-0.329	0.024	0.000
Born 1951	-0.379	0.026	0.000
Born 1950	-0.456	0.027	0.000
Born 1949	-0.549	0.028	0.000
Born 1948	-0.706	0.028	0.000
Born 1947	-0.797	0.029	0.000
Constant	11.644	0.103	0.000
N	290604		
Adjusted R-squared	0.0415		

Notes:

The model also includes cohort fixed effects and municipality-fixed effects. Municipality-specific trends are excluded due to non-significance in an OLS regression of fertility on years of schooling. The 1958 birth cohort is the reference group. Standard errors are adjusted for clustering at the municipality level.

Department of Economics  
University of Bergen  
Fosswinckels gate 6  
N-5007 Bergen, Norway  
Phone: +47 55 58 92 00  
Telefax: +47 55 58 92 10  
<http://www.svf.uib.no/econ>