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Educated mothers, healthy infants: the impact of a school reform on  
the birth weight of Norwegian infants 1967-2005

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This is the accepted and refereed manuscript to the article published in

*Social Science and Medicine*, 105(2014):84-92

Publisher's version available at <http://dx.doi.org/10.1016/j.socscimed.2014.01.008>

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## EDUCATED MOTHERS, HEALTHY INFANTS

The impact of a school reform on the birth weight of Norwegian infants 1967-2005

### **Abstract**

Birth weight is an important predictor of health and success in later life. Little is known about the effect of mothers' education on birth weight. A few causal analyses have been done, but they show conflicting results. We estimated the effect of mothers' education on birth weight by using data on a school reform in Norway. During the period 1960-1972, all municipalities in Norway were required to increase the number of compulsory years of schooling from seven to nine years. We used this education reform to create exogenous variation in the education variable. The education data were combined with large sets of data from the Medical Birth Registry and Statistics Norway. Since municipalities implemented the reform at different times, we have cross-sectional as well as time-series variation in the reform instrument. In the analyses, we controlled for municipality fixed effects, municipality-specific time-trends and mothers' and infants' year of birth. Using this procedure we found a fairly large effect of mothers' education on birth weight.

Increasing mothers' education reduces the likelihood of low birth weight, even in a publically financed health care system. In interpreting these results it is important to keep in mind that we have examined only one channel, which is through birth weight, through which education may explain differences in health. There are other potential channels that should be explored by future research. In particular, it would be of interest to examine whether education has causal effects on the broader determinants of health, such as psychopathology, social capital and networks, and family stress and dysfunction.

**Keywords:** infant health, education, inequality, birth weight, public health

## **Introduction**

Infant health is of great importance for health and success in later life. One important indicator of infant health is birth weight. Infants with a low birth weight are at a disadvantage in later life with respect to cognitive, mental and physical development. They are also more at risk of experiencing poor health as adults (Currie & Hyson, 1999; Shenkin, Starr, & Deary, 2004; Case & Paxson, 2010; Currie & Moretti, 2007). More education may improve mothers' appreciation of a healthy lifestyle during pregnancy, thereby increasing the probability of having healthy babies. On this background, we address the causal effect of mother's education on birth weight.

Several empirical studies document a positive correlation between mother's level of education and infant health (for example see: Kramer, Séguin, Lydon, & Goulet, 2000; Gortmaker & Wise, 1997; MacDorman, 2011). Since causal effects can be smaller or larger than these correlations, it remains an open question whether increasing mothers' education is beneficial for infant health. We have identified only four studies, two from the United States, one from the United Kingdom and one from Taiwan, where causal effects of education on birth weight have been estimated (Currie & Moretti, 2003; McCrary & Royer, 2011; Lindeboom, Llena-Nozal, & Van der Klaauw, 2009; Chou, Liu, Grossman, & Joyce, 2007). They show conflicting results.

We have attempted to estimate causal effects of education on birth weight in a country with a long tradition for publically financed health services. Since the early 1960s, Norway has had a comprehensive and universal prenatal care programme for all pregnant women. One might expect that such a prenatal programme would reduce or eliminate the influence of mothers' own resources as a cause for having an infant with low birth weight.

We were able to estimate the effects by using data on a school reform in Norway. During the period 1960-1972, all municipalities in Norway were required to increase the number of compulsory years of schooling from seven to nine years. The education data were combined with large sets of data from the Medical Birth Registry and Statistics

Norway. Since municipalities implemented the reform at different times, we have cross-sectional as well as time-series variation in the reform instrument. Thus we were able to estimate the effect of mothers' education on birth weight by controlling for municipality fixed effects and trend variables. We found that a higher level of maternal education substantially improved infant health, measured as a reduction in the likelihood of low birth weight in Norway.

### **Theory and background**

An important determinant of low birth weight is inadequate nutrition in the third trimester of pregnancy. It has also been shown that birth weight is influenced by other environmental factors, in particular smoking and alcohol consumption (Brooke, Anderson, Bland, Peacock, & Stewart, 1989; Shu, Hatch, Mills, Clemens, & Susser, 1995). One possible way to reduce differences in birth weight is to increase mothers' level of education. Mothers are the core producers of health capital for their infants – they can influence the birth weight of the infant through their behaviour during pregnancy (Grossman, 1972). Education improves mothers' stock of knowledge. According to Grossman, this stock of knowledge makes educated mothers more efficient producers of health than less educated mothers (Grossman, 2006). This takes two forms. First, educated mothers absorb and process information so that they can make healthy choices during pregnancy (allocative efficiency). For example, more highly educated mothers will have more knowledge about the harmful effects of smoking and alcohol or what constitutes an appropriate diet than less educated mothers. Second, educated mothers “obtain a larger output from a given level of health inputs than the less educated (productive efficiency)” (Grossman & Kaestnar, 1997).

An increase in mothers' educational level will only reduce the likelihood of low birth weight if the education effect is causal. Ordinary least squares estimation (OLS) is

likely to lead to biased results, mainly because the estimation does not take account of unobserved variables that are correlated with both education and infant health. The unobservable variables most frequently cited in the literature are ability, morbidity and time preferences. Mothers with a high level of ability will most likely have a high level of education, and at the same time they are also aware of the importance of favourable health behaviour during pregnancy. Hence, to obtain an unbiased estimate, ability has to be controlled for. Poor health of the mother might be transmitted to her offspring – for example hereditary disease. In addition, there may be a positive correlation between the health of the mother and her level of education. In that case OLS estimation will overestimate the effect of mothers' education on infant health. Numerous studies have examined the impact of time preferences for investments in education and health. A finding in several studies is that the positive correlation between education and health is significantly reduced after time preferences have been controlled for (Farrell & Fuchs, 1982; Van der Pol, 2011; Ippolito, 2003; Chapman & Coups, 1999; Cutler & Lleras-Muney, 2010).

Currie & Moretti (2003) used availability to college education as an instrument for maternal education. On the basis of a large sample from the United States, their IV-estimate suggests that education improves birth weight. One additional year of education reduces the probability of low birth weight by 0.010 with a standard error of 0.004. This implies that one year of extra schooling will decrease the prevalence of low birth weight by 10%. Chou, Liu, Grossman, & Joyce (2007) studied the effect of mothers' education on birth weight in Taiwan. In 1968, compulsory school education was extended from six to nine years. This was followed by an increase of 254 new junior high schools in different regions during the period 1968-1973. Chou, Liu, Grossman, & Joyce (2007) exploited the variation across regions in the opening of new schools as an instrument for maternal education. They found that one extra year of schooling led to a decrease in the prevalence of babies with a low birth weight of 5.5%. McCrary & Royer (2011) used school entry dates to isolate exogenous variations in women's educational level. Based on large data

sets from Texas and California, they found that one additional year of education increased the probability of low birth weight by 0.014. The direction of this effect is the opposite of what we would expect. Finally, Lindeboom, Llena-Nozal, & Van der Klaauw (2009) addressed the United Kingdom case. Compared to analyses carried out in the United States, they relied on a relatively small dataset. They estimated causal effects on infant health by using data on an educational reform in 1947, when the minimum school leaving age was increased from 14 to 15 years of age. The best results were obtained when the sample was restricted to mothers who finished school at the age of 14 to 15. The estimated causal effect was imprecise. For example, one year of extra schooling for mothers decreased the probability of low birth weight by 0.033 with a standard error of 0.050 (see Lindeboom, Llena-Nozal, & Van der Klaauw (2009), Table 6b).

## **Identification and Data**

### *Identification using a compulsory school reform in Norway*

From 1960 and onwards a school reform was implemented in Norway (comprehensive descriptions of the reform are given by Aakvik, Salvanes, & Vaage, 2010; Lie, 1973; Telhaug, 1969). The time of implementation was decided by the individual municipalities, but all had to implement the reform by the end of 1972. The reform increased the minimum number of years of schooling from seven to nine years. The school year was from mid August until mid June both before and after the reform. School entry is not spread out over the whole year, but occurs once a year, which is around the middle of August. Before the reform, children started school in the year they became seven, i.e. they were between six and a half and seven and a half years of age when they started school. They finished their compulsory schooling at age 14. Under the new system children still start school in the year they become seven, but finish their compulsory schooling the year they become 16.

For more than a decade Norway had two separate school systems. Which system the child was in depended on which municipality he/she lived in, and in which year he/she was born. The first birth cohort that could start nine years compulsory schooling was born in 1947, and the last cohort to finish in the old system was born in 1958.

This school reform has been used as an instrument variable in several papers by Salvanes and co-workers to study causal effects of education on the following outcomes: intergenerational transmission of education, family size, teenage births, mobility in the labour market, IQ and earnings (Aakvik, Salvanes, & Vaage, 2010; Black, Devereux, & Salvanes, 2005, 2007, 2008, 2010; Machin, Salvanes, & Pelkonen, 2012). The current dataset has been designed in a similar way to the design of Salvanes and co-workers (for details see Aakvik, Salvanes, & Vaage, 2010; Black, Devereux, & Salvanes, 2005). Thus our main analysis was performed on a sample that was restricted to mothers with 9 years or less of education.

The timing of the reform was identified for 706 of the 735 municipalities that existed in 1960. The municipalities implemented the reform at different times as shown in Fig. 1. Similar to Salvanes and co-workers, we used the 1960 census to identify the municipality in which each of the mothers grew up. The mothers would then have been between 2 and 13 years of age, i.e. at an age where they would have been affected by the reform during the following years. Only a small percentage of mothers belonging to the early birth cohorts were affected by the reform. This percentage increased markedly for the mothers belonging to the 1950-1954 birth cohorts. This increase occurred at a time when the major cities implemented the reform. From Fig. 2 we also see that there was an increase in educational attainment for mothers who were affected by the reform compared to mothers who were not affected.

(Figures 1 and 2 here)

The key advantage of this design is the time series variation across municipalities. We were able to compare mothers in the same municipality who were subjected to 9 years

of compulsory schooling with those who were not. Since municipalities implemented the reform at different points in time, we were able to include municipality fixed effects in the regression model. Therefore, all unobserved cross-sectional variation between municipalities that could be correlated with mothers' level of education and birth weight was cancelled out.

Let subscript  $ktj$  denote a mother  $j$  who has grown up in municipality  $k$ , and was born in year  $t$ .  $R_{ktj}$  is a dummy variable that equals 1 if the mother was 14 years or younger when the reform was introduced in the municipality, (i.e. she was affected by the reform), and 0 otherwise.  $T_{kt}$  is a centred linear trend variable, i.e. defined as zero the first year of the reform. Since municipalities implemented the reform at different points in time, the trend variable is defined by the timing of the reform. The after-reform trend was flat when we restricted the sample to women with a maximum of nine years of education. We therefore allowed the model to have different time trends before and after the reform. This was captured by the interaction term between reform ( $R_{kt}$ ) and time-trend ( $T_{kt}$ ). The first stage regression for the mother's years of education ( $E_{ktj}$ ) can be written as:

$$\begin{aligned}
 E_{ktj} = & \gamma_0 R_{kt} + \gamma_1 T_{kt} R_{kt} + \text{Fixed effect for mother's year of birth} \\
 & + \text{Fixed effect for infant's birth year} \\
 & + \text{Municipality fixed effects} + e_{ij}
 \end{aligned} \tag{1}$$

The infant's year of birth was included to capture changes in the technology of child delivery which affects infant health outcomes (Grytten, Monkerud, & Sørensen, 2012). In supplementary analyses we also interacted the trend variable ( $T_{kt}$ ) with municipality fixed effects (altogether 706 municipalities) to capture municipality-specific trends. These specific trends were estimated with both a linear ( $T_{kt}$ ) and a quadratic specification ( $T_{kt}^2$ ). Let  $\hat{E}_{ktj}$  be the predicted value of the mother's years of education from the first stage regression, and birth weight ( $BW_{ktj}$ ) be the dependent variable. The second stage regression is then:

$$\begin{aligned}
BW_{ktj} = & \beta_0 \hat{E}_{ktj} + \beta_1 T_{kt} + \text{Fixed effect for mother's year of birth} \\
& + \text{Fixed effect for infant's year of birth} \\
& + \text{Municipality fixed effects} + v_{ij}
\end{aligned} \tag{2}$$

Low birth weight (=1) is defined as less than 2500g (Paneth, 1995; World Health Organization, 2010). The proportion of babies with a low birth weight was 0.0585.

### *Data*

The analyses were carried out on data from the Medical Birth Registry of Norway (MBRN) for the period 1967-2005. All maternity units have a duty to report all births to MBRN (Irgens, 2000). On the registration form, the personal identification numbers of the child and the parents are recorded. This made it possible to merge the data from MBRN with two data registers in Statistics Norway. The first register, the Norwegian Standard Classification of Education (Statistics Norway, 2000), contains information about the highest education of the parents when the baby was born for all Norwegians from 1967. The second register, the Population and Housing Census, contains information about place of residence (municipality) for the mothers in 1960 (Statistics Norway, 1987).

MBRN includes the whole population of mothers who give birth in Norway. Our sample was restricted to mothers who had 9 years or less of education. We further restricted the sample to mothers who gave birth for the first time. This is because there are well known differences in infant health outcomes by birth order (Dowding, 1981; Elliott, 1992). MBRN has information about parity for all mothers. Mothers who were represented in the data with second or higher births only were excluded (i.e. those mothers with their first birth from before 1967). Immigrants to Norway have not been exposed to the school

reform (Grytten, Skau, & Sørensen, 2013). Therefore they were not included in our analyses. We also excluded all multiple births from the analyses.

## **Results**

### *Descriptive statistics*

In Table 1 we present descriptive statistics for some of our key variables by reform status. We present mean values based on samples with eight time periods: 1) one year before the reform, 2) one year after the reform, 3) one to two years before the reform 4) one to two years after the reform, 5) one to five years before the reform, 6) one to five years after the reform, 7) one to eight years before the reform, 8) one to eight years after the reform.

Before the reform, the proportion of babies with low birth weight was within the range 0.061 to 0.064. This proportion fell for babies who were born after the reform, then ranging from 0.051 to 0.056. Before the reform, the mother's number of years of education was within the range 8.16 to 8.42 years.

For the other variables, the mean values were very similar before and after the reform. For example, for the time period one year before the reform, i.e. for mothers who were not affected by the reform, the mean value for mother's age was 21.39 years. The corresponding value for the period one year after the reform was 21.41. These mothers were affected by the reform. The proportion of mothers who were married was 0.652 for the time period one year before the reform. The corresponding value for the period one year after the reform was 0.651.

### *Naive OLS estimate*

Table 2 gives the OLS estimates for an additional year of education on the probability of having a low birth weight (<2500g). The sample was limited to the birth cohort 1939-1966. The only control variables that were included are dummies for the municipality the mother lived in at the age of 14. The regression coefficient is -0.0050. This implies that the reduction in the probability of having a low birth weight is 0.50 percentage points per additional year of schooling.

### *First stage estimates*

In Table 3 alternative estimates are based on samples with four time periods: 1) one year before to one year after the reform, 2) two years before to two years after the reform, 3) five years before to five years after the reform, and 4) eight years before to eight years after the reform.

We observe that the reform caused 0.43-0.57 years of additional education. The standard errors for the reform variable indicate a high degree of estimate precision, i.e. the regression coefficients were highly significant with t-values ranging from 18 (one year before to one year after the reform) to 71 (eight years before to eight years after the reform). This means that we have high F-values for the instrument, meeting all the criteria for a strong instrument as proposed in the literature (Stock, Wright, & Yogo, 2002).

The positive estimate for the trend variable indicates that educational levels increased over time prior to the reform. Since the trend variable has been centred at the reform year, the sum for the trend coefficient and the Reform x Trend coefficient yields the post-reform trend. The sum is close to zero in all specifications. This is due to the fact that we have capped educational levels at nine years.

Our coefficients for the effects of the reform on years of education are well within the range that Salvanes and co-workers report from their studies. Their estimates vary from 0.12 to 0.65 (Black, Devereux, & Salvanes, 2008; Machin, Salvanes, & Pelkonen, 2012).

### *The second stage estimates*

The overall interpretation of the second stage estimates is that one year of additional education causes a decrease in the probability of low birth weight of about 0.01 or one percentage point (Table 3). With the exception of the estimate based on samples with one year before to one year after the reform, all the estimates are statistically significant at conventional levels. The estimates are fairly similar for the samples that include five years before to five years after the reform, and eight years before to eight years after the reform. The estimates are slightly larger in absolute values for the sample that includes two years before to two years after the reform.

The proportion of infants with low birth weight in the population is about 0.05. One additional year of education reduced the prevalence of infants with low birth weight by 20%. The school reform increased mothers' educational level by about half a year, which means that the reform caused a 10% reduction in the prevalence of low birth weight.

### *Additional analyses*

We carried out several additional analyses. First, we examined whether a change in teenage births could be one channel through which education influences birth weight. Increased compulsory schooling in Norway has reduced the incidence of teenage births (Black, Devereux, & Salvanes, 2008). Further, it is well known that infants born of teenage mothers have lower birth weight than infants with non-teenage mothers (Strobino, Ensminger, Kim, & Nanda, 1995; Swamy, Edwards, Gelfand, James, & Miranda, 2012). In our sample, the proportion of babies with a low birth weight where the mother was 18 years or younger was 0.069 (cohort 1939-1966). The corresponding figure for infants with mothers who were older than 18 years was 0.056. If the negative effect that education has on birth weight is driven by a reduction in teenage births, we expect this effect to be significantly reduced or to disappear when the analyses are done on a sample of non-teenage mothers. This was not the case. In Table 3 (columns to the right) we present

results for a sample of mothers older than 18 years. The coefficients were similar or slightly larger in absolute values compared to the coefficients for the whole sample (columns to the left). Our interpretation of this finding is that our main results are not driven by the lower birth weight of teenage mothers.

Second, we estimated Equations (1) and (2) with municipality-specific trends. The second stage estimates are shown in Table 4. The overall interpretation is that the main results, which are presented in Table 3, are insensitive to the inclusion of these extra variables. This is the case independent of whether the trends are specified as linear or quadratic. Salvanes and co-workers reported that the inclusion of municipality-specific trends did not influence their results in any significant way (Black, Devereux, & Salvanes, 2008; Machin, Salvanes, & Pelkonen, 2012). Our results correspond with this.

Third, we did three placebo tests (Table 5). This is an experiment in which we pretend that the reform was introduced in the municipalities from two to four years earlier than it actually was introduced. In this experiment, we do not expect the reform to have any effect on birth weight. This is also supported by our analyses. None of our estimates were statistically significant at conventional levels. Also, for two of the three coefficients the sign is incorrect (positive) in relation to what we would have expected if the reform had had an effect.

Fourth, we tested whether family characteristics could be sources of confounding. This was done by identifying siblings who turned 14 years at either side of the reform date. Equation (1) was then estimated by including siblings as fixed. If the negative effect that education has on birth weight is caused by family characteristics, we expect this effect to be significantly reduced or to disappear when sibling fixed effects are included in the estimation. This was not the case. In fact, the coefficients were slightly larger in absolute values. In the sample with a time period from five years before to five years after the reform, the regression coefficient was -0.095 (standard error 0.061;  $n=3118$ ). The corresponding value of the regression coefficient for the sample with a time period from

eight years before to eight years after the reform was -0.092 (standard error 0.044; n=4123).

Fifth, we carried out additional analyses using a sample that included mothers with more than 9 years of education. This sample was also restricted to non-immigrant mothers, singleton births and first birth mothers. The precision of the first stage estimates was lower than in the sample that was restricted to mothers with 9 years or less of education. Therefore, most of the second stage coefficients were not significant at conventional levels ( $p < 0.10$ ). However, the size and sign of the regression coefficients (not reported) were similar to those that were estimated on the sample that was restricted to mothers with 9 years or less of education.

## **Discussion**

We found an effect of mother's education on birth weight. For most of our specifications, the effect is slightly stronger than the effect found by Currie & Moretti (2003). Comparisons of our findings with the findings of Lindeboom, Llena-Nozal, & Van der Klaauw (2009) and McCrary & Royer (2011) must be made with caution. The coefficients are not significant at conventional levels in these two studies. However, it is worth noting that the regression coefficients of Lindeboom, Llena-Nozal, & Van der Klaauw (2009) are about twice as high as ours.

Our school reform variable gives a local average treatment effect at the bottom tail of the educational distribution. Therefore, we should be cautious in generalizing the findings to mothers with more than nine years schooling, that is to the middle and upper ranges of the educational distribution (Imbens & Angrist, 1994). These are basically a selected group of mothers who would be motivated to acquire knowledge, independently of the number of years they attended school. Therefore, for mothers with more than nine years schooling, the education effect could be weaker. Currie & Moretti (2003) use the

availability of colleges in the mother's county when she was 17 years old as the instrument variable. This variable yields treatment effects at a higher level of education than in our study. This may explain why their estimates are lower than ours.

Similar to Currie & Moretti (2003), we found that the second stage estimates were larger than the OLS estimates. This is also a consistent finding in most studies where school reforms have been used as instruments for the estimation of causal effects on adult health outcomes, and on labour market outcomes (for example see Oreopoulos, 2006; Van Kippersluis, O'Donnell, & Van Doorslaer, 2011; Card, 2001). Card (2001) suggests different reasons why this may be the case. The most plausible is that the reform variables only identify local average treatment effects. This is also an explanation that Currie & Moretti (2003) support. They argue that "the marginal benefit of schooling for individuals whose education has been affected by college openings may be larger than the average benefit for the population" (Currie & Moretti, 2003, page 1508). In that case, and according to Card (2001), our average treatment effect can be closer to the OLS estimate, and even lower than the OLS estimate if the OLS estimate is strongly upward biased.

An instrument variable should satisfy the criteria of relevance and exogeneity (Woolridge, 2013). Instrument relevance implies that the implementation of the reform must explain a substantial variation in the mother's years of education. If that is not the case, the random error term will tend to mask the effect of the treatment variable (= mother's years of education). As a consequence, the results from the estimation with instrument variables and ordinary least squares will be fairly similar (Newhouse & McClellan, 1998). We believe that our instrument is relevant because it is highly positively correlated with our endogenous variable. This is a reasonable finding, as we expected that the implementation of the school reform would lead to an increase in years of education. Further, the instrument fulfills the formal test statistics for being a strong instrument (Stock, Wright, & Yogo, 2002).

Instrument exogeneity requires that the implementation of the reform influences birth weight only through mothers' educational level. There are several reasons why this is likely to be the case. First, when it was decided to implement the reform in a municipality, the reform encompassed all children or adolescents in the municipality, i.e. the reform affected all mothers independent of their abilities, time preferences, health status and health behaviour. Second, there is no evidence of selective migration from or to municipalities in which the reform was implemented early in the 1960s. For example, resourceful families could have moved to "early implementers" so that their children could get the advantage of 9 years schooling rather than 7 years. Conversely, less resourceful mothers could have been more reserved to move. This issue has been examined by Lie (1973) and Telhaug (1969). They found no evidence that inhabitants moved to other municipalities in relation to when the reform was implemented. Third, Salvanes and co-workers have shown that there is no relationship between the timing of the implementation of the reform and municipal characteristics such as the size of the municipality, the unemployment rate or the proportion of employed people who work in manufacturing industries (Aakvik, Salvanes, & Vaage, 2010; Black, Devereux, & Salvanes, 2008). Further, they found no relationship between the timing of implementation and inhabitants' level of income or their age. In Table 1 we extended the analyses of Salvanes and co-workers to show that the mean values on some key variables were the same before and after the reform. Thus it is unlikely that our results are driven by, for example, changes in the mothers' age distribution or changes in the proportion of mothers who are married. Therefore, in sum, there are reasons to believe that the use of the school reform as an instrument variable fulfills the criteria of relevance and exogeneity.

One limitation of our study is that we lack data to examine fully the different channels through which education influences birth weight. We have examined one channel, that is, the impact of the reduced incidence of teenage births. There are other, probably more important ones. For example, differences in tobacco smoking and use of

alcohol according to education group, may be important. During the period from the beginning of the 1970s and until the late 1990s, the percentage of women in Norway who smoked was about 30% (Norwegian Ministry of Health and Care Services, 2007). In MBRN, figures about smoking were available from 1999 only. The proportion of pregnant women with university/college education who smoked at the beginning of their pregnancy in 1999 was 0.10 (Fig. 3). The corresponding figure for those with compulsory school education was 0.51. Even though fewer pregnant women smoked in 2005 compared to in 1999, the difference according to education group still persisted.

(Figure 3 here)

These figures, even if they are only descriptive, give an indication that tobacco smoking may be one channel through which education influences birth weight. This is also supported by several studies that show a strong relationship between maternal education, maternal smoking and birth weight (Ventura, Hamilton, Mathews, & Chandra, 2003; Kleinman & Madans, 1985). Such a relationship has also been found for use of alcohol during pregnancy (Kramer, 1987; Mills, Graubard, Harley, Rhoads, & Berendes, 1984). We were not able to examine this relationship any further, as there are no data on use of alcohol in MBRN.

The present study is performed within the framework of the fetal origin hypothesis (Barker, 1995, 2001; Ellison, 2005; Almond, 2006). According to this hypothesis, negative environmental factors at a critical phase in the development of the fetus can influence the fetus's genes causing a permanent change in the structure and function of the organs (Waterland & Jirtle, 2003, 2004). This change is then, the cause of poor health in later life. The fetal origin hypothesis is controversial (Almond & Currie, 2011). In particular, social scientists have suggested other mechanisms through which poor health may arise. During the last two decades, an extensive amount of research has been carried out on the role of psychopathology, social capital and networks, and family stress and dysfunction as important determinants of poor health (for example see Kawachi (1999),

Coyne & Downey (1991), McCubbin, Joy, Cauble, Comeau, Patterson, & Needle (1980) for a review). With our data, we were not able to estimate the effect of education on these potential health determinants. However, education is likely to play an important role. For example, people with low education may be excluded from access to social capital: they may be discriminated against on the labour market, they are often segregated according to place of residence and they may feel isolated due to lack of support and help through networks (Kawachi & Berkman, 2000; Helliwell & Putnam, 1999). There is evidence that shows that socially isolated individuals have an increased risk of premature mortality, reduced survival after major illness and poor mental health (Kawachi, 1999; Berkman & Glass, 2000). Thus, differences in access to social capital according to education group can be one mechanism through which inequalities in health arise. Further, individuals with high education cope better with family stress and dysfunction than individuals with low education – this manifests itself in differences in health between these education groups (Ross, Mirowsky, & Goldstein, 1990). Our study focuses on one mechanism through which education may explain differences in health. That mechanism, which is through birth weight, may not be the most important one, and is most likely not the only one. The fact that we lack data to examine the effects of education on the broader determinants of health, such as psychopathology, social capital and networks, and family stress and dysfunction, is another limitation of our study.

The main strength of this study is the design, which made it possible to study the impact of a policy reform on health outcome. Our dataset is large, and covers the whole population of infants born in Norway during a 39-year period. During that time period the mothers' of these infants were exposed to a school reform that we could use to create exogenous variation in our education variable. In Norway, as in most other western countries, the authorities have given priority to increasing the level of education in the population, for example, by introducing school reforms to increase the minimum number of years of compulsory schooling. Our results indicate that this policy has been

successful, not only in raising the level of education within the population, but also in improving infant health.

Our study was carried out in a country with publically financed health services. One would expect the relationship between mother's education and infant health to be weaker than in a country with market-based system, such as the USA. One factor that points in this direction is that both access to prenatal care and the level of prenatal care services have been better in Norway than in the USA (Backe, 2001; Buekens, Kotelchuck, Blondel, Kristensen, Chen, & Masuy-Stroobant, 1993; Kogan, Martin, Alexander, Kotelchuck, Ventura & Frigoletto, 1998). Several studies have shown that good access to prenatal care is important for both infant health and the health of the mother during and after the pregnancy (Arima, Guthrie, Rhew, & De Roos, 2009; Currie & Gruber, 1996; Wehby, Murray, Castilla, Lopez-Camelo, & Ohlsfeldt, 2009a, 2009b). This is also highlighted in WHO's guidelines and recommendations for maternal care (Carroli et al., 2001; Villar et al., 1998). Since the 1970s, all pregnant women in Norway have been offered from 8 to 10 free prenatal check-ups, and most women have taken advantage of this offer (Norwegian Directorate of Health and Social Affairs, 2005; Eskild & Grytten, 2013). For example, in the middle of the 1990s, only 0.1% of pregnant women in Norway had not attended prenatal check-ups compared to 12% in the USA (Backe, 2001; Kogan, Martin, Alexander, Kotelchuck, Ventura & Frigoletto, 1998). In Norway, the prenatal programmes include routine clinical examinations of the mother and the foetus, in addition to health education messages. These programmes may have been effective in improving the birth weight for the population of infants as a whole. However, the programmes have not been successful in closing the gap in birth weight between infants with mothers who have few years of education as opposed to mothers with more years of education.

In conclusion, our results indicate that education is important for infant health, even in a country with a strong public involvement in maternal care. Through education,

mothers gain knowledge and skills that make them more able to make healthy choices (Grossman, 2006). This is first and foremost an advantage for the foetus, but is also important for the infant's health and success in later life. In interpreting these results, it is important to keep in mind that we have examined only one channel through which education may explain differences in health. That channel, which is through birth weight, may not be the most important one, and is most likely not the only one. Future research should look into the causal effects that education might have on the broader determinants of health, such as psychopathology, social capital and networks, and family stress and dysfunction.

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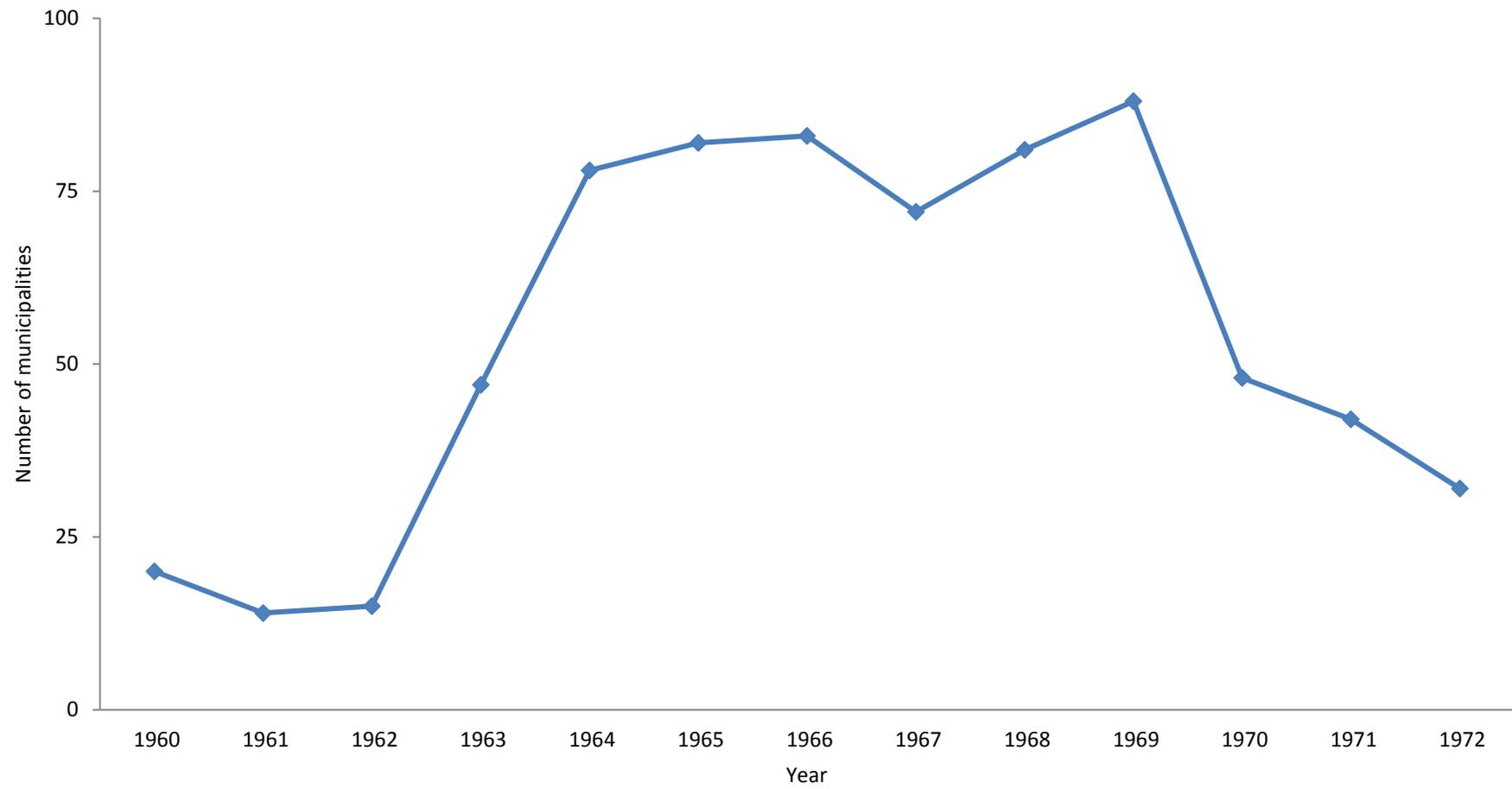
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**Fig. 1.** The number of municipalities by year of implementation of the reform.