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FAST TIMES AT RIDGEMONT HIGH? THE EFFECT OF COMPULSORY SCHOOLING LAWS ON TEENAGE BIRTHS

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ABSTRACT

Research suggests that teenage childbearing adversely affects both the outcomes of the mothers as well as those of their children. We know that low-educated women are more likely to have a teenage birth, but does this imply that policies that increase educational attainment reduce early fertility? This paper investigates whether increasing mandatory educational attainment through compulsory schooling legislation encourages women to delay childbearing. We use variation induced by changes in compulsory schooling laws in both the United States and Norway to estimate the effect in two very different institutional environments. We find evidence that increased compulsory schooling does in fact reduce the incidence of teenage childbearing in both the United States and Norway, and these results are quite robust to various specification checks. Somewhat surprisingly, we also find that the magnitude of these effects is quite similar in the two countries. These results suggest that legislation aimed at improving educational outcomes may have spillover effects onto the fertility decisions of teenagers.

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1. Introduction

Research suggests that teenage childbearing adversely affects women's economic outcomes such as the level of completed schooling, labor market participation, and wages.[1] Given these deleterious consequences, it is important to determine what factors contribute to this decision. We know that low-educated women are more likely to have a teenage birth, but does this imply that policies that increase educational attainment reduce early fertility? In particular, would increasing mandatory educational attainment (through compulsory schooling legislation) encourage women to delay childbearing? If compulsory schooling reduces harmful or risky behaviors, then these factors should be considered when evaluating the benefits of this type of legislation.

This paper proposes to provide evidence on the causal effects of changes in compulsory schooling laws on teenage childbearing using data from the United States and Norway. Having data from these two countries provides an interesting contrast: one country is very supportive of teenagers who have children, with extensive financial support (Norway), while the other is much more punitive in its treatment (the United States). Understanding the differences in responses to compulsory schooling laws can provide useful information, not only on the direct effect of schooling laws on teenage fertility, but also the relative difference across different institutional environments.

In the United States, there has been extensive variation in compulsory schooling laws across states and over time. Changes in these laws have been used as instruments for education in other contexts by Acemoglu and Angrist (2001), Lochner and Moretti (2004), and Lleras-Muney (2002). There were many changes in minimum schooling requirements between the 1920s and the 1970s; we utilize changes over this entire time period by using data from the Census from 1940 through 1980 to analyze cohorts born between 1910 and 1960.

During the 1960s in Norway, there was a drastic change in the compulsory schooling laws affecting primary and 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. Implementation of the reform occurred in different municipalities at different times, starting in 1960 and continuing through 1972, allowing for regional as well as time series variation. Evidence in the literature suggests that these reforms had a large and significant impact on educational attainment.[2] We study cohorts impacted by the reform -- women born between 1947 and 1958.

Our results suggest that increased compulsory schooling does seem to reduce the incidence of teenage childbearing in both the United States and Norway. These findings suggest that policy interventions to increase female education at the lower tail of the educational distribution may be an effective means of reducing rates of teenage childbearing.

Once this relationship is established, it is then useful to attempt to understand the mechanisms through which this relationship works. We examine two possible mechanisms. The first is the "incarceration effect"; to the extent that compulsory schooling reduces the time available to engage in risky behavior, the incidence of teenage

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1 See Klepinger, Lundberg, and Plotnick (1999), Angrist and Evans (1996), and Levine and Painter (2003).
2 See Black, Devereux and Salavanes (2003). Results on the impact of similar reforms on educational attendance also exists for Sweden, see Meghir and Palme (2003) and for England and Ireland, see Harmon and Walker (1995) and Oreopulos (2003).

pregnancy might go down. The second is the "human capital effect"; additional education increases both current and expected future human capital and this higher level of human capital could change fertility decisions. We describe these mechanisms in more detail and discuss possible tests to distinguish between them. Our estimates suggest that the effect of the laws on fertility is not just an "incarceration" effect, resulting also from the effects of the laws on human capital accumulation.

The paper unfolds as follows. In Sections 2 and 3, we provide a short overview of the literature and brief descriptions of the support systems for single mothers in Norway and the U.S., as well as a description of the compulsory schooling law changes used for identification. Sections 4 and 5 present our estimation strategy and the data sets used. Section 6 presents the estimation results and robustness checks. In Section 7, attempts are made to disentangle some possible explanations for a causal relationship between compulsory schooling laws and fertility choice. Section 8 concludes.

2. Background Information

2.1 Previous literature

Teenage motherhood has been associated with many long-term economic and health disadvantages such as lower education, less work experience and lower wages, welfare dependence, lower birth weights, higher rates of infant mortality, and higher rates of participation in crime (Ellwood, 1988; Jencks, 1989; Hoffman et al., 1993; Kiernan, 1997). There is an ongoing debate as to the extent that these adverse effects of teen childbearing are truly caused by having a teen birth rather than reflecting unobserved family background differences. (See Hotz et al. 2002 for an example). However, the balance of evidence suggests that at least part of the negative consequences of teen births on mothers is causal (Klepinger, Lundberg, and Plotnick, 1999; Angrist and Evans, 1996; Levine and Paintner, 2003). Thus, as a policy matter, efforts to reduce the rate of teen childbearing are often considered as a strategy to improve the life chances of young women.

In addition to the effects of teen childbearing on mothers, there is also a literature concerned with the negative effects on children. In recent work, Francesconi (2004) takes a family fixed effects approach to show that children born to teenagers have poorer outcomes as adults than children born to women when older. In addition, Hunt (2003) provides evidence that children of teen mothers are more likely to engage in crime. Thus, the policy interest in this topic also arises from presumed negative effects on children.

Despite the policy relevance, there has been little work studying the role of education policy in reducing teen fertility. A recent paper by McCrary and Royer (2003) focuses on two states (California and Texas) and examines the effect of education on teenage childbearing and child health by applying a regression discontinuity approach using school starting-age rules. They find little evidence that the induced educational changes affect children's health or woman's fertility choices. However, while their data set is very well-suited for studying children's health (administrative data on all births in California and Texas from 1989 to 2001), it is less appropriate for focusing on teenage fertility decisions (as it contains a sample of only those women who did in fact have children). We attempt to enhance our understanding of the link between teenage fertility and education by using data that is better suited for this particular question, examining a broader region (the entire United States and Norway), and using a different source of variation by focusing on changes in dropout ages rather than school entry ages.

2.2 Institutional setting

In addition to the fact the our identification strategy allows us to use large and representative data sets, another advantage of our study is that we can compare the effect of changes in compulsory schooling laws on teenage fertility choice across two countries. Norway and the United States are similar in that both have very high GDP per capita and education levels, but differ in terms of institutional environment; of particular importance are the welfare support systems for teenage mothers.

The U.S. system of support for teenage mothers is considered to be relatively unsupportive when compared to many other industrialized countries. Established under Title IV of the Social Security Act, Aid to Dependent Children was operated largely under state and local control (Baicker 2004). Targeted primarily at the children (and not the parents), eligibility was often limited by "suitable home" requirements (stating that benefits could only be given when eligible children resided in a suitable home), seasonal employment policies, and illegitimacy exclusions. Although there is significant variation across states, there is a common belief that it is not a generous system relative to more socialized countries such as Norway.

In contrast, since the early 1960s, the relevant time period for the compulsory schooling legislation change in Norway, the Norwegian welfare system has been very generous (Rønsen and Strøm, 1991). To enable single parents to take care of their children without working, the government provides income support via the social security system until the child is ten years of age (so long as the woman is not living with the child's father).[3] The government also helps to enforce child support payments from the father. In addition, the government pays all education expenses for the mother (reimbursement is only partial if the woman is working) and provides subsidized housing and child care[4] Finally, single parents get double child allowances from the government.[5] In summary, the Norwegian system of support for single mothers is very generous.

Access to contraception and abortion has changed in both countries in the past 40 years. Although the birth control pill was approved in 1960 by the Food and Drug Administration and spread rapidly among married women, it wasn't until the late 1960s that it diffused among single women (with a series of changes in state legislation reducing the age of majority and extending mature minor decisions. See Goldin and Katz (2002) for more details.) Thus, the pill influenced behavior during the teenage years of only the youngest cohorts in our U.S. sample. In Norway, the birth control pill was introduced in the late 1960s and spread quite quickly and so was available during the teenage years of some of the later cohorts in the sample we study (Noack and Ostby, 1981).[6]

Abortion was not legalized in Norway until 1979. In the U.S., abortion was legalized in 1973 through the landmark Roe vs. Wade Supreme Court decision.[7] As a result, almost none of the women in either our Norwegian or U.S. sample had access to legal abortion during their teenage years.

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3 This system was introduced in 1964 and became a part of the social security system in 1971.
4 In 1990 the income support system was made less generous in order to provide incentives for work; however, this is not relevant to the cohorts we study.
5 All parents get a child allowance in Norway (about 1000 NOK per year).
6 Interestingly, we have examined the effects of the compulsory schooling legislation on earlier cohorts only and find similar effects, suggesting that this changing environment has no significant impact on our results.
7 Abortion was legal in New York, California, Washington, Hawaii, and Arkansas beginning in 1970. See Levine (2004a, 2004b) for more details.

3. The Compulsory Schooling Laws

(a) Changes in U.S. Compulsory Schooling Laws

Since the history of compulsory schooling laws in the U.S. is by now well documented (see, in particular, Lleras-Muney 2001, and Goldin and Katz 2003), we will not describe them in great detail here. Essentially, there were five possible restrictions on educational attendance: 1. maximum age by which a child must be enrolled, 2. minimum age at which a child may drop out, 3. minimum years of schooling before dropping out, 4. minimum age for a work permit, and 5. minimum schooling required for a work permit. In the years relevant to our sample, 1924 to 1974, states changed compulsory attendance laws many times, usually upwards but sometimes downwards. Appendix Table 1 shows the minimum dropout age by states over time. Although there is variation, there is also substantial persistence, highlighting the importance of adjusting standard errors for clustering at the state level. Papers on the topic have used a variety of combinations of these restrictions as their measures of compulsory schooling. To be consistent with the source of variation in our Norwegian data, our baseline specification will examine the effect of the minimum dropout age on teenage pregnancy. As a specification check, we will also examine the sensitivity of our results to the use of required years of schooling, defined as the difference between the minimum dropout age and the maximum enrollment age following Lleras-Muney and Goldin and Katz, as well as to the inclusion of the minimum age for a work permit. We follow Acemoglu and Angrist (2001) and Lochner and Moretti (2004) in assigning compulsory attendance laws to women on the basis of state of birth and the year when the individual was 14 years old (with the exception that the enrollment age is assigned based on the laws in place when the individual was 7 years old).

Lleras-Muney (2001) thoroughly investigates the relationship between changes in compulsory schooling laws and other state-level variables. She finds no evidence that the relationship between the laws and education is related to manufacturing wages, manufacturing employment, expenditures on education, or demographic characteristics of the population. In the robustness checks in Section 6, we carry out several checks that suggest that the relationship between compulsory schooling laws and early fertility is not spurious.

(b) The Norwegian Primary School Reform

In 1959, the Norwegian Parliament legislated a mandatory school reform that increased the minimum level of education in society by extending the number of compulsory years of education from 7 to 9 years (thereby increasing the minimum dropout age from 14 to 16, as students started at age 7). Prior to the reform, children started school at the age of seven and finished compulsory education after seven years, i.e. at the age of fourteen. In the new system, the starting age was still seven years old, but the time spent in compulsory education was now nine years. In addition, the reform standardized the curriculum and increased access to schools, since 9 years of mandatory school was eventually made available in all municipalities.

The parliament mandated that all municipalities (the lowest level of local administration) must have implemented the reform by 1973; as a result, although it was started in 1960, implementation was not completed until 1972.[8] This suggests that, for more than a decade, Norwegian schools were divided into two separate systems; which system you were in depended on the year you were born and the municipality in which you lived. The first cohort that could have been involved in the reform was the one born in 1947. They started school in 1954, and either (i) finished the pre-reform compulsory school in 1961, or (ii) went to primary school from 1954 to 1960, followed by the 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 school in 1972.[9] Early work by Lie (1973, 1974) suggests that the timing of the implementation was unrelated to municipality characteristics such as industrial structure and size. We present a more rigorous discussion of the education reform and the determinants of the timing of implementation in the Appendix.

4. Empirical Methodology: Probability of Having First Birth as a Teenager

In both the U.S. and Norway, there is time-series as well as cross-sectional variation in the number of years of compulsory schooling required of individuals during the periods studied.

U.S. Model:

The empirical model for the United States is as follows:

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[8] 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.

[9] 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).

$$TEENBIRTH = \alpha_0 + \alpha_1 COMPULSORY + \alpha_2 COHORT + \alpha_3 STATE + \alpha_4 WHITE + \upsilon \quad (1)$$

where COHORT refers to a full set of year of birth indicators, STATE refers to a full set of state indicators, and WHITE is a dummy indicator for whether the woman is white. For the U.S., COMPULSORY is a vector of three dummy variables describing the minimum dropout age in a state, with a minimum dropout age of less than 16 as the omitted category. Because *TEENBIRTH* is a binary indicator for whether the woman had her first birth as a teenager, we estimate the model using maximum likelihood probit.

It is important to note that we are including both cohort and state effects. The cohort effects are necessary to allow for secular changes in educational attainment over time that may be completely unrelated to compulsory schooling laws. The state effects allow for the fact that variation in the timing of the law changes across states may not have been exogenous to fertility decisions. 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 or (b) implementation of law changes is not correlated with changes in these characteristics or (c) these characteristics are not related to the probability of having a teen birth.

Norway Model:

We use a similar specification for the Norwegian data, replacing the state dummies with municipality dummies. The specification is as follows:

$$TEENBIRTH = \alpha_0 + \alpha_1 COMPULSORY + \alpha_2 COHORT + \alpha_3 MUNICIPALITY + \upsilon \quad (2)$$

For Norway, COMPULSORY equals 1 if the individual was affected by the education reform (minimum dropout age of 16), and 0 otherwise (minimum dropout age of 14).[10] Again, cohort effects are quite important since the income support system for single mothers changed somewhat over time and we want to compare the effect of the compulsory schooling laws on teenage fertility within cohorts.

5. Data

(a) United States

We use the IPUMS extracts from the decennial Census from 1940 to 1980. The particular samples we use are the 1% 1940 sample, the 1% 1950 sample, the 1% 1960 sample, the two 1% 1970 state samples, and the 5% 1980 samples. Analysis using the Census is complicated by the fact that children are only observed if they are living in the household with their mother. It is possible to link mothers to their children in these data and use the age of the eldest own child in the household to determine the age at which the mother first gave birth. We omit women from the sample if their first birth occurred before age 15. Since children tend to start leaving home about age 16, this implies that we can only get an accurate count on teenage births for the sample of women aged no more than about 31 (15+16). Thus, we restrict our Census sample to women aged between 20 and 30.

For most of our sample (1960-1980), the data include quarter of birth and we use this variable to determine the woman's age at first birth to within 3 months. For the 1940 and 1950 samples, we do not observe quarter of birth and so age at first birth is known to within a year. As is standard in the literature, we assign the compulsory schooling law indicators on the basis of state of birth rather than state of residence. We do so because

[10] Note that we do not include race dummies for Norway as there is very little variation in race in Norway

mobility across states may be influenced by educational attainment and hence by the compulsory schooling laws. Random mobility at any point after birth may imply that an individual is not actually impacted by the laws that we think they are; this creates a measurement error problem that will tend to bias our estimates of the effects of the laws towards zero.

(b) Norway

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, educational history, neighborhood information, and employment information.[11] Note that, unlike with the U.S. data, we are able to observe all children and not just those living in the household.

The initial database is linked administrative data that covers the entire population of Norwegians aged 16-74. These administrative data provide information about educational attainment, labor market status and a set of demographic variables (age, gender).

To determine whether women were affected by the compulsory schooling legislation, we need to link each woman to the municipality in which she grew up. We do this by matching the administrative data to the 1960 census. From the 1960 census, we know the municipality in which the woman's mother lived in 1960.[12] In 1960, the women

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during this period.

[11] See Møen, Salvanes and Sørensen (2003) for a description of the data set.

[12] Since very few children live with their father in the cases where parents are not living together, we should only have minimal misclassification by applying this rule.

we are using in the estimation are aged between 2 and 13.[13] As in the U.S. case, random mobility at any point after we assign location may imply that an individual is not actually impacted by the reform although we think they are. This creates a measurement error problem that will tend to bias our estimates of the effects of the reform towards zero.

Our primary data source on the timing of the reform in individual municipalities is the volume by Ness (1971). To verify the dates provided by Ness, we examined the data to determine whether or not there appears to be a clear break in the fraction of students with less than 9 years of education. In the rare instance when the data did not seem consistent with the timing stated in Ness, we checked these individual municipalities by contacting local sources. We are able to successfully calculate reform indicators for 545 out of the 728 municipalities in existence in 1960. If the reform took more than one year to implement in a particular municipality or we were not able to verify the information given in Ness (1971), we could not assign a reform indicator to that municipality. However, we have reform information for a large majority of individuals in the relevant cohorts.

We include cohorts of women born between 1947 and 1958 in our sample. For these women, we observe their children in 2000. 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 her first birth to the nearest month. We exclude from our sample the small number of women who have a first birth before age 15 and we define a teenage birth as one occurring when the mother has not yet reached her 20th birthday at the birth of her

[13] One concern is that there may be selective migration into or out of municipalities that implement the reform early. However, since the reform implementation did not occur before 1960, reform-induced mobility should not be a problem for us. Evidence from Meghir and Palme (2003) on Sweden and Telhaug (1969) on Norway suggest that reform-induced migration was not a significant consideration.

first child.

Descriptive Statistics

Table 1 provides summary statistics for the women in our sample. First consider the U.S. data. About 4 percent of women in the sample faced a dropout age of less than 16, 75 percent had a minimum dropout age of 16, 12 percent had a dropout age of 17 and 9 percent had a minimum dropout age of 18. Also, we see that 17 percent of women have their first birth as a teenager.

In the Norwegian data, we see that 52 percent of women are affected by the reform. Similar to the U.S., 17 percent of women have their first birth as a teenager.

Effect of Laws on Educational Attainment

To provide some background, we assess the impact of the compulsory schooling laws on educational attainment by regressing completed education on the laws and on the cohort dummies and state dummies (for the U.S.) and municipality dummies (for Norway).[14] The results are presented in Appendix Table 2.

Consistent with our earlier work on Norway (See Black, Devereux, and Salvanes 2003), we estimate a coefficient on the reform of 0.122 (0.022) indicating that, on average, education increased by 0.12 of a year as a result of the law change. In the U.S., the coefficient on Dropout Age=16 is 0.404, suggesting that the educational impact of a similar change in the U.S. is bigger than that in Norway (however, the U.S. estimate is quite imprecise and is statistically insignificant).

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14 We restrict our U.S. sample to women aged between 22 and 30 in order to reduce the problem of

Readers may find it surprising that we find no statistically significant relationship between U.S. compulsory schooling laws and educational attainment. Many previous studies have used these laws as instruments for education and reported very strong first stage relationships (for example, Lochner and Moretti, 2004; Lleras-Muney, 2002). This discrepancy appears to arise because we cluster our standard errors at the state level while these other papers cluster at the state-year level.[15] In the third column of the table, we report estimates when we cluster at the state-year level and the standard errors are about four times lower. This probably reflects the long time-series component to our state year panel and the presence of serial correlation (see Bertrand, Duflo, and Mullainathan, 2004).

6. Results for the Probit Models

The probit marginal effects of compulsory schooling on teenage childbearing (equation 1) are presented in Table 2.[16] The marginal effects for the U.S. are in the top panel. These numbers reflect the effect of the minimum dropout age specified by the compulsory schooling law on the probability of having the first birth before each age. To assess the magnitude of the coefficients, it is important to know the probabilities of births during these years: The percentage of women have their first birth before 17 is 2%, before 18 is 6%, before 19 is 11%, before 20 is 17%, and before 21 is 24% (See Table 1). We find no evidence that the small probability of having a first birth before age 17 is influenced by the laws. However, the results in the other 4 columns suggest that the laws

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censored educational attainment.

15 Goldin and Katz (2003) also cluster at the state level and they find marginally significant effects of the laws on educational attainment during the early part of the 20th century.

16 In all the probit models, we report robust standard errors that allow for clustering at the state level in the

have a significantly negative effect on the probability of having a first child before ages 18 to 21. The magnitude of the effects is also quite large. The coefficient of -0.008 on Dropout Age=16 in the fourth column implies that the effect of compelling women to stay in school until 16 is to reduce the probability of a teen birth by 4.7% ((0.008*100)/0.17). The effect of imposing a law mandating women to stay in school until 17 is to reduce the probability of a teen birth by 8.8% ((0.015*100)/0.17). In contrast, we do not find any significant effect of having a minimum dropout age of 18. However, the standard errors are very high for this variable.

The proportion of first births by age in our Norwegian sample is as follows: 1% before age 17, 4% before age 18, 9% before age 19, 17% before age 20, and 25% before age 21 (See Table 1). The probit marginal effects for Norway are in the second panel of Table 2. Note that the effect of the reform was to increase the minimum dropout age from 14 to 16 in Norway. Once again, there is evidence that the compulsory schooling law reduced the likelihood of births during the teenage years. The marginal effects imply that the implementation of the reform reduced the probability of a first birth as a teenage by about 3.5% ((0.006*100)/0.17). This is similar but somewhat smaller than the 4.7% effect of Dropout Age=16 in the United States. Generally, the effects of the reform in Norway are very similar to those of Dropout Age=16 in the United States.

To better understand our results, we stratify our sample based on the urban/rural status of the individuals. One might expect enforcement to be easier in urban areas and hence the laws may have a larger impact. While we can get a precise breakdown of the urban/rural status in our Norwegian data using the metropolitan status of the individuals from the 1960 census, it is more difficult for the U.S. data. As a proxy for the urban/rural

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United States and the municipality level in Norway.

status in the United States, we are forced to use the status at the time of the Census, when women are aged between 20 and 30. Given that there is significant mobility, we view this as a rough proxy for the actual urban/rural status of the individual when she was in school.[17]

As one might have predicted, the results, presented in Table 3, appear to be stronger for the urban sample. This is particularly true for Norway, where the effects are much larger for the urban sample. While the rural results are roughly consistent, they are never statistically significant. We also tried stratifying our U.S. sample based on the race of the individual; when we do this, it becomes clear that the compulsory schooling laws had a more significant effect on teenage childbearing among whites. The results for blacks were never statistically significant. This is consistent with the work of Goldin and Katz (2003) who find smaller effects of compulsory schooling laws on educational attainment for blacks than whites.

Robustness/Specification Checks

We have carried out numerous specification checks to verify our findings. They are as follows.

<u>Inclusion of State-Year Trends</u>

Because we are identifying off of variation in compulsory schooling across states over time, it is not possible to include state by time dummies. However, we can allow for state-specific trends (in the Norwegian case, municipality-specific trends). When we include these trends (Table 4), we get estimates that are quite similar to those in Table

17 In the U.S., we define urban as being resident in a metropolitan area. In Norway, individuals are classified as urban if their mother lived in one of the main cities and towns in 1960.

2.[18]

Alternative Weighting Schemes

When using the U.S. Census data, we are able to get 1% samples from 1940, 1950, and 1960, a 2% sample from 1970, and a 5% sample from the 1980 data. As a result, we are giving more weight to the most recent cohorts. If there is no difference in the effect of compulsory schooling laws on teenage childbearing over time, the results should be the same whether we weight each cohort equally or not. This, however, is testable. In Table 5, we present the results when we weight each cohort equally (thereby weighting each observation by the inverse of the number of individuals in that cohort in our sample). While the results are consistent with those in Table 2, it does seem that, when more weight is placed on the earlier periods, the more stringent compulsory schooling laws are more effective.

Effect of Future Laws on Current Fertility

Future law changes should have no impact on current fertility behavior. If they do, it would suggest that something other than changes in compulsory schooling may be driving our results. To check this, we have calculated the minimum dropout ages that exist 10 years into the future, and have added these to the specification. The estimates are in Table 6.[19] Although we see that there are three instances where the future laws have a statistically significant effect on fertility behavior (one negative effect, and two positive effects), the addition of the future laws does not change the effects of the actual laws. If anything, the results are strengthened by the addition of the future laws, suggesting that

[18] We have also tried adding state of residence fixed effects and Census year fixed effects in the U.S. sample and these have had little effect on the estimates.
[19] The sample size is smaller in Table 5 because the compulsory schooling law file finishes in 1978 and so we cannot calculate the dropout ages ten years into the future for cohorts born after 1964.

the change in fertility we observe is in fact caused by the change in compulsory schooling laws.

Alternative Measures of Compulsory Schooling

While we have used the minimum dropout age as our indicator for compulsory schooling in the U.S., other work has used a variety of measures, and we test the sensitivity of our results to this choice. To do so, we apply the same estimation strategy but use the required number of years of schooling (defined as the minimum dropout age minus the maximum enrollment age 7 years prior) as our measure of compulsory education instead. We split the years of compulsory schooling into 4 categories – less than 9, 9, 10, and 11 or more. We exclude the less than 9 category from the specification. Table 7 presents these results. Consistent with our earlier findings, the estimates show that the probability of early childbearing is negatively affected by the number of years of compulsory education. As we would expect, there is a general pattern that the more strict the law, the greater the impact on teen childbearing. The results for years of compulsory schooling are robust to all the specification checks discussed above.

In Table 8, we report estimates where we use the minimum age at which an individual could get a work permit as our measure of compulsory schooling. We include two dummy variables – one for whether the dropout age was 15, and the other for whether the age was greater than 15. The omitted category is a dropout age of less than 15. We find that the presence of a dropout age of 15 has a statistically significant negative effect on early fertility. Overall, we find that changes in compulsory schooling laws impact fertility, irrespective of how they are defined.[20]

[20] Recent work by Goldin and Katz (2003) suggests that child labor laws may be more significant than the compulsory schooling laws in terms of affecting school attendance. However, in our sample, this is not the

7. Why Do Compulsory Schooling Laws affect Timing of Births?

Given that we find an effect of compulsory schooling laws on teenage fertility, the next step it to try to uncover mechanisms through which this relationship is working.

Consider a static model of schooling and fertility decisions. At the beginning of their teenage years, young women choose their schooling level and their fertility behavior. In the absence of institutional constraints, these decisions are made optimally and depend on the utility function of each individual as well as her ability. If, however, a compulsory schooling law is in place, this constrains the educational choice of some women and leads them to choose greater education than would have been chosen otherwise. In turn, this new optimal educational level may be associated with a new set of fertility choices.[21]

In particular, because it is likely to be quite costly to be in school as a young mother, an exogenous increase in education of the individual may be associated with a postponement of fertility. We call this the "incarceration effect"; while women are in school, they do not have the desire/time/opportunity to have a child.[22]

In addition, since more education increases human capital, this is also a mechanism through which increases in education may lead to postponed fertility.[23] We

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case; among women in our sample, the compulsory schooling laws were as effective as the child labor laws in influencing fertility.

[21] It is important to note that this "rational choice" approach assumes women make optimal decisions on timing of births taking into account all the costs and benefits involved. This is often discussed in conjunction with an alternative approach that sees many teenage pregnancies as "mistakes" resulting from thoughtless behavior, lack of knowledge about the long run consequences, or lack of knowledge about birth control. It is this view that fertility behavior may not be optimal that underscores much of the policy interest in this topic.

[22] Jacob and Lefgren (2003) discuss the "incarceration effect" in the context of the effects of education on teenage criminal behavior.

[23] Happel et al. (1984) model the timing of children and argue that, if capital markets are perfect, the timing

call the fact that the additional schooling may make you “smarter” and hence decide to postpone childbearing the “current human capital effect”, and we call the fact that expectations about the future acquisition of human capital are changed by compulsory schooling laws and this may change fertility decisions the “future human capital effect.”

If the effects of the compulsory schooling laws occur solely due to the "incarceration" effect, then the laws should have no impact on behavior at ages above which the laws induce schooling. To examine this, we estimate probit models of the probability of having a first birth at age x, conditional on having no birth prior to that. We estimate this model for x equal to 16, 17, 18, 19, and 20. If the laws impact the probability of first birth at ages above which they bind, then this is strong evidence that the "incarceration" effect is not the only effect.

Table 9 presents the probit estimates of the probability of a birth at a particular age conditional on no prior birth. Given that women are pregnant for about 9 months, the "incarceration" model implies that the Dropout Age=16 should impact births at age 16 and 17, but should have little impact on births at higher ages.[24] However we find that Dropout Age=16 has a small and statistically insignificant effect on the probability of first birth at age 16, but has a statistically significant negative effect on births at ages 17 and 18. Likewise, although Dropout Age=17 does have a negative effect at age 17, it has, if anything, larger negative effects at ages 18 and 19. In the Norwegian data, the reform raised the minimum school leaving age from 14 to 16 for most people. Despite this, there

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of first child depends on the rate at which earnings depreciate due to absence from the labor market, and the initial level of earnings at the start of the woman's life cycle. If women start with very low earning power, and skills depreciate with absence from the market, then it is optimal to have children early. On the other hand, if initial earnings are high, postponing childbirth is optimal. Thus, in this framework, women with more human capital are more likely to postpone childbirth.

24 If people do actually drop out of school on their 16th birthday, this compulsory schooling law should only impact births at age 16 through the “incarceration” effect.

is a negative effect of the reform on the probability of giving birth at age 18 (although this is statistically insignificant), which is too old for the "incarceration" effect to be relevant. Thus, the evidence for the "incarceration" effect of compulsory schooling laws is very weak in this probit analysis and suggests that the human capital effects are likely playing a role. We can be certain that whether or not there is an "incarceration" effect, there are also other mechanisms that influence fertility behavior.[25] Unfortunately, the nature of our data prohibits us from distinguishing much beyond this point.

8. Conclusions

Many studies find that early fertility adversely affects women's economic outcomes such as the level of completed schooling, labor market participation, and wages. However, there is limited information about policy relevant factors that might be important determinants of early fertility decisions. This paper has attempted to increase our knowledge by studying the role of compulsory schooling laws.

We find that minimum school requirements have a significantly negative effect on the probability of having a child as a teenager both in the United States and in Norway. Our results are robust to a number of specification checks. It is noteworthy that our estimates are fairly similar in two countries – the U.S. and Norway – that are so different institutionally. These findings suggests that policy interventions to increase female education at the lower tail of the educational distribution may be an effective means of

[25] In Appendix Table 4, we present the equivalent estimates for the U.S. where each cohort is weighted equally. Once again, there is evidence against a pure incarceration effect as Dropout Age=17 affects fertility behavior at 19 and 20, and Dropout Age=18 affects behavior at age 20. There is also a shred of evidence for the "future human capital effect" in that Dropout Age=18 has a statistically significant negative effect on the probability of a birth at age 17. That is, women who know that they will have to stay in school until age 18 are less likely to have a child at age 17 than other women with the same amount of schooling who do not face this future compulsory schooling.

reducing rates of teenage childbearing, regardless of the welfare structure in place.

In addition to studying the effects of compulsory schooling laws on teenage fertility choice, we also examine different mechanisms through which the compulsory schooling legislation may be affecting fertility behavior. The first mechanism we consider is an "incarceration" effect or the fact that educational attendance reduces time available to engage in risky behavior. Alternative mechanisms are related to human capital theory where both current and expected human capital may impact teenage fertility choice. Our results suggest that the effect of compulsory schooling laws goes beyond a pure "incarceration" effect.

Appendix: The Norwegian Education Reform

To receive funds from the government to implement the reform, municipalities had 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 did want to ensure that implementation was representative of the country, conditional on having an acceptable plan. (Telhaug, 1969, Mediås, 2000). Appendix Figure 1 presents the spread of the reform over time, focusing on the number of municipalities implementing the reform per year.

While it is not necessary for our estimation strategy, it would be useful if the timing of the implementation of the reform across municipalities were uncorrelated with general educational levels. One might worry that poorer municipalities would be among the first to implement the reform, given the substantial state subsidies, while wealthier municipalities would move much slower. However, work examining the determinants of the timing of implementation finds no relationship between municipality characteristics such as average earnings, taxable income, and educational levels, and the timing of implementation. (See Lie 1973, 1974.) Municipalities that are located geographically near municipalities that already implemented the reform were themselves more likely to implement the reform; numerous interviews revealed that this was likely due to a particularly effective county administrator. As a result, the research supports a complex adoption process without finding support for a single important factor to explain the implementation process. To examine this ourselves, Appendix Figures 2, 3, and 4 examine the implementation of the reform by the average income, parental education, and size of the municipalities; these figures suggest that there is little relationship between these factors and the timing of the implementation of the reform.

As a more rigorous test, in Appendix Table 3 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 20 counties in Norway). Consistent with the existing literature, there appears to be no systematic relationship between the timing of implementation and parent average earnings, education levels, average age, urban/rural status, industry or labor force composition, municipality unemployment rates in 1960, and the share of individuals who were members of the Labor party (the most pro-reform and dominant political party).

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Table 1: Descriptive Statistics

United States

Variable	Mean	Std. Dev.	Min	Max
Birth Cohort	1948.14	12.34	1910.00	1960.00
Age at Census	24.80	3.16	20.00	30.00
Education	12.30	2.52	0.00	17.00
White	0.86	0.34	0.00	1.00
Child before 17	0.02	0.15	0.00	1.00
Child before 18	0.06	0.23	0.00	1.00
Child before 19	0.11	0.31	0.00	1.00
Child as teenager	0.17	0.38	0.00	1.00
Child before 21	0.24	0.43	0.00	1.00
Dropout Age is <16	0.04	0.19	0.00	1.00
Dropout Age is 16	0.75	0.43	0.00	1.00
Dropout Age is 17	0.12	0.32	0.00	1.00
Dropout age is 18	0.09	0.28	0.00	1.00
Enrolment Age is 6	0.14	0.35	0.00	1.00
Enrolment Age is 7	0.66	0.47	0.00	1.00
Enrolment Age is 8+	0.20	0.40	0.00	1.00
N=1,584,094				

Norway

Variable	Mean	Std. Dev.	Min	Max
Birth Cohort	1953	3.35	1947	1958
Age in 2000	47.01	3.35	42.00	53.00
Education	11.50	2.58	5.00	21.00
Child before 17	0.01	0.09	0.00	1.00
Child before 18	0.04	0.19	0.00	1.00
Child before 19	0.09	0.29	0.00	1.00
Child as teenager	0.17	0.38	0.00	1.00
Child before 21	0.25	0.43	0.00	1.00
Reform Implemented	0.52	0.50	0.00	1.00
N=260,641				

Table 2: Effect of Compulsory Schooling Laws on the Probability of First Birth by A Certain Age: Probit Marginal Effects

United States

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
Dropout Age=16	-.00002	-.0025	-.0058*	-.0077*	-.0095*
	(.0009)	(.0017)	(.0018)	(.0024)	(.0047)
Dropout Age=17	-.0008	-.0053*	-.0106*	-.0147*	-.0186*
	(.0011)	(.0022)	(.0027)	(.0031)	(.0050)
Dropout Age=18	.0012	-.0004	-.0004	-.0023	-.0085
	(.0014)	(.0060)	(.0128)	(.0147)	(.0153)
White	-.0421*	-.0749*	-.1035*	-.1222*	-.1275*
	(.0029)	(.0053)	(.0077)	(.0094)	(.0106)
N=1,584,094					

Norway

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
Reform	-0.0006	-0.0020	-0.0047*	-0.0063	-0.0087*
	(0.0006)	(0.0015)	(0.0024)	(0.0037)	(0.0043)
N=260,641					

Estimates are marginal effects from probit maximum likelihood. Each column denotes a separate regression. Also included in the specifications are year-of-birth indicators. The U.S. specifications also include state dummies; the Norway specifications include municipality indicators. Standard errors are all adjusted for clustering at the state/municipality level.
* denotes statistically significant at the 5% level.

Table 3: Effect of Compulsory Schooling Laws on the Probability of Birth: Urban/Rural Distinction

United States

	Birth by Age 16	Birth By Age 17	Birth By Age 18	Birth by Age 19	Birth by Age 20
Urban					
N=1,063,181					
Dropout Age=16	.0007	-.0016	-.0019	-.0031	-.0030
	(.0009)	(.0017)	(.0030)	(.0055)	(.0089)
Dropout Age=17	-.0003	-.0057*	-.0100*	-.0146*	-.0163
	(.0012)	(.0018)	(.0029)	(.0052)	(.0087)
Dropout Age=18	.0028	.0025	.0068	.0012	-.0033
	(.0024)	(.0084)	(.0173)	(.0071)	(.0182)
White	-.0448*	-.0818*	-.1166*	-.1425*	-.1556*
	(.0034)	(.0064)	(.0093)	(.0114)	(.0127)
Rural					
N=520,913					
Dropout Age=16	.0008	-.0002	-.0039	-.0029	-.0031
	(.0016)	(.0033)	(.0034)	(.0037)	(.0064)
Dropout Age=17	-.0007	-.0033	-.0066	-.0003	-.0097
	(.0020)	(.0038)	(.0047)	(.0053)	(.0084)
Dropout Age=18	.0007	-.0004	-.0020	.0089	.0061
	(.0032)	(.0040)	(.0053)	(.0095)	(.0115)
White	-.0376	-.0643	-.0830	-.0924	-.0862
	(.0017)	(.0028)	(.0041)	(.0051)	(.0054)

Norway

	Birth by Age 16	Birth By Age 17	Birth By Age 18	Birth by Age 19	Birth by Age 20
Urban					
N=87,752					
Reform	-.0010	-.0053*	-.0094*	-.0212*	-.0275*
	(.0010)	(.0026)	(.0038)	(.0067)	(.0089)
Rural					
N=172,889					
Reform	-.0003	-.0005	-.0027	.0008	.0003
	(.0008)	(.0016)	(.0028)	(.0035)	(.0041)

Estimates are marginal effects from probit maximum likelihood. Each column denotes a separate regression. Also included in the specifications are year-of-birth indicators. The U.S. specifications also include state dummies; the Norway specifications include municipality indicators. Standard errors are all adjusted for clustering at the state/municipality level.
* denotes statistically significant at the 5% level.

Table 4: Effect of Compulsory Schooling Laws on the Probability of Birth Including State-Year Trends

United States

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
Dropout Age=16	.0016*	-.0003	-.0036	-.0086*	-.0151*
	(.0006)	(.0016)	(.0031)	(.0040)	(.0060)
Dropout Age=17	.0008	-.0032	-.0072	-.0131*	-.0233*
	(.0011)	(.0019)	(.0037)	(.0046)	(.0072)
Dropout Age=18	.0042*	.0020	.0003	-.0087	-.0175
	(.0018)	(.0079)	(.0037)	(.0177)	(.0187)
White	-.0417*	-.0744*	-.1031*	-.1219*	-.1274*
	(.0028)	(.0052)	(.0076)	(.0093)	(.0106)
N=1,584,094					

Norway

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
Reform	-0.0002	-0.0015	-0.0046*	-0.0049	-0.0063
	(0.0002)	(0.0015)	(0.0021)	(0.0028)	(0.0034)
N=260,637					

Estimates are marginal effects from probit maximum likelihood. Each column denotes a separate regression. Also included in the specifications are year-of-birth indicators. The U.S. specifications also include state dummies; the Norway specifications include municipality indicators. Standard errors are all adjusted for clustering at the state/municipality level.
* denotes statistically significant at the 5% level.

Table 5: Effect of Compulsory Schooling Laws on the Probability of Birth: Equal Weighting of Cohorts: Probit Marginal Effects, United States

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
Dropout Age=16	.0001	-.0008	-.0023	-.0051	-.0079*
	(.0009)	(.0023)	(.0033)	(.0039)	(.0039)
Dropout Age=17	-.0017	-.0051	-.0104*	-.0185*	-.0248*
	(.0012)	(.0025)	(.0038)	(.0060)	(.0052)
Dropout Age=18	-.0004	-.0099*	-.0202*	-.0294	-.0365*
	(.0012)	(.0039)	(.0088)	(.0149)	(.0115)
White	-.0354*	-.0606*	-.0803*	-.0853*	-.0765*
	(.0023)	(.0046)	(.0072)	(.0088)	(.0103)
N=1,584,094					

Estimates are marginal effects from probit maximum likelihood. Each column denotes a separate regression. Also included in the specifications are year-of-birth indicators and state dummies. Standard errors are all adjusted for clustering at the state/municipality level.

Table 6: Effect of Compulsory Schooling Laws on the Probability of Birth Including Future Legislation: Probit Marginal Effects, United States

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
Dropout Age=16	0.0006	-0.0024	-0.0051*	-0.0087*	-0.0113*
	(0.0008)	(0.0018)	(0.0020)	(0.0025)	(0.0051)
Dropout Age=17	-0.0002	-0.0050*	-0.0101*	-0.0175*	-0.0236*
	(0.0008)	(0.0016)	(0.0019)	(0.0027)	(0.0051)
Dropout Age=18	0.0012	-0.0076*	-0.0174*	-0.0215	-0.0315*
	(0.0015)	(0.0032)	(0.0073)	(0.0148)	(0.0133)
Future Laws					
Dropout Age=16	-0.0002	0.0027	0.0047	0.0105*	0.0127*
	(0.0007)	(0.0020)	(0.0022)	(0.0039)	(0.0060)
Dropout Age=17	-0.0010	0.0008	-0.0010	0.0013	0.0042
	(0.0014)	(0.0028)	(0.0030)	(0.0050)	(0.0072)
Dropout Age=18	-0.0041*	0.0006	-0.0004	0.0052	0.0073
	(0.0015)	(0.0030)	(0.0035)	(0.0054)	(0.0072)
White	-.0403*	-.0728*	-.1013*	-.1170*	-.1181*
	(.0030)	(.0057)	(.0085)	(.0102)	(.0113)
N=1,001,121					

Estimates are marginal effects from probit maximum likelihood. Each column denotes a separate regression. Also included in the specifications are year-of-birth indicators and state dummies. Standard errors are all adjusted for clustering at the state/municipality level. * denotes statistically significant at the 5% level.

Table 7: Effect of Compulsory Schooling Laws on the Probability of Birth: Required Years of Schooling (Minimum Dropout Age-Maximum Enrollment Age), United States Data

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
9 Years of Schooling	-.0006 (.0010)	-.0044* (.0016)	-.0085* (.0019)	-.0122* (.0025)	-.0140* (.0032)
10 Years of Schooling	-.0006 (.0011)	-.0047* (.0023)	-.0089* (.0035)	-.0114* (.0047)	-.0167* (.0046)
11+ Years of Schooling	-.0020 (.0024)	-.0054 (.0054)	-.0073 (.0103)	-.0148 (.0123)	-.0174 (.0137)
White	-.0421* (.0029)	-.0748* (.0053)	-.1034* (.0077)	-.1222* (.0094)	-.1275* (.0106)
N=1,584,094					

Estimates are marginal effects from probit maximum likelihood. Each column denotes a separate regression. Also included in the specifications are year-of-birth indicators and state dummies. Standard errors are all adjusted for clustering at the state/municipality level.

Table 8: Effect of Child Labor Laws on the Probability of Birth, United States Data

Dependent Variable:	Birth by Age 16	Birth by Age 17	Birth by Age 18	Birth by Age 19	Birth by Age 20
Dropout Age=15	-.0007 (.0006)	-.0044* (.0010)	-.0100* (.0012)	-.0136* (.0022)	-.0096 (.0057)
Dropout Age>15	-.0006 (.0013)	-.0016 (.0026)	-.0015 (.0039)	.0011 (.0052)	-.0016 (.0054)
White	-.0420* (.0029)	-.0749* (.0053)	-.0103 (.0077)	-.1222* (.0094)	-.1276* (.0106)
N=1,584,094					

Estimates are marginal effects from probit maximum likelihood. Each column denotes a separate regression. Also included in the specifications are year-of-birth indicators and state dummies. Standard errors are all adjusted for clustering at the state level.
* denotes statistically significant at the 5% level.

Table 9: Effect of Compulsory Schooling Laws on the Probability of Birth Conditional on Not Already Having a Child

United States

Dependent Variable	Birth at 16/No prior birth	Birth at 17/No prior birth	Birth at 18/No prior birth	Birth at 19/No prior birth	Birth at 20/No prior birth
Dropout Age=16	-.0001	-.0025*	-.0035*	-.0021	-.0024
	(.0006)	(.0009)	(.0013)	(.0019)	(.0036)
Dropout Age=17	-.0009	-.0045*	-.0057*	-.0047*	-.0056
	(.0008)	(.0012)	(.0013)	(.0018)	(.0040)
Dropout Age=18	.0002	-.0015	.0005	-.0007	-.0061
	(.0010)	(.0049)	(.0078)	(.0054)	(.0050)
White	-.0271*	-.0349*	-.0344*	-.0292*	-.0182*
	(.0020)	(.0029)	(.0034)	(.0033)	(.0033)
N	1,572,513	1,545,369	1,493,288	1,414,844	1,311,693

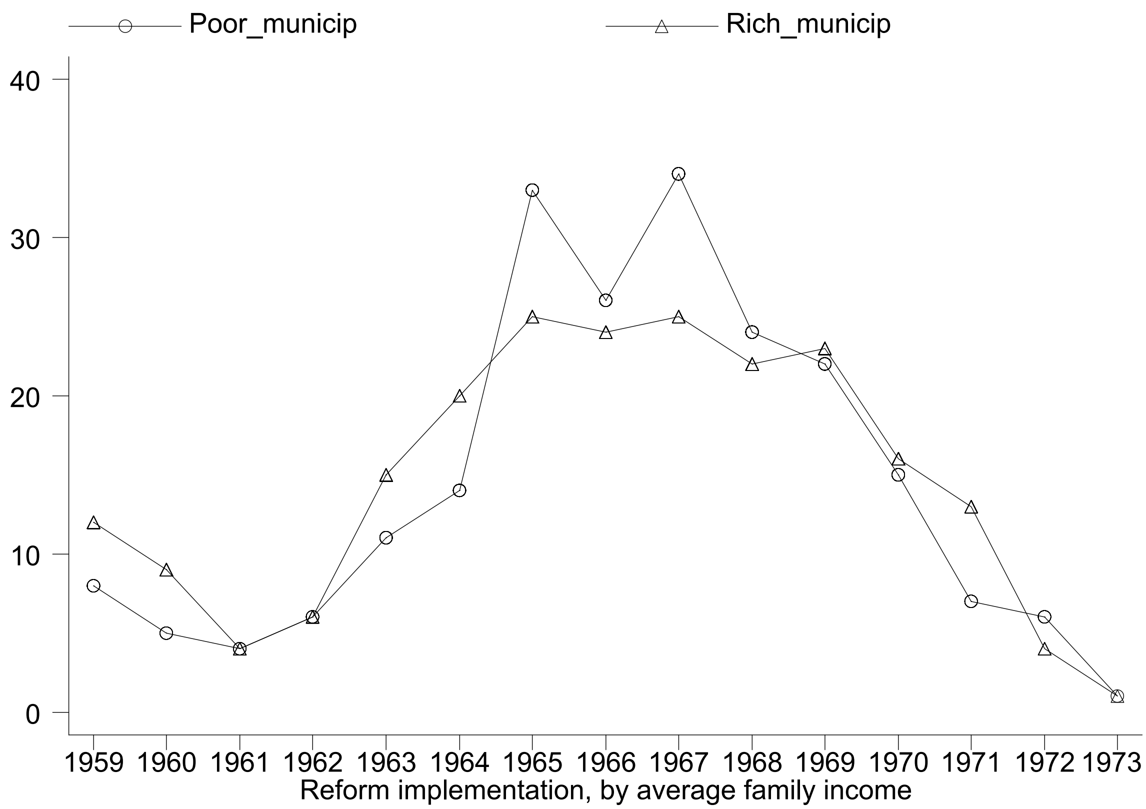
Norway

Dependent Variable:	Birth at 16/No prior birth	Birth at 17/No prior birth	Birth at 18/No prior birth	Birth at 19/No prior birth	Birth at 20/No prior birth
Reform	-0.0006	-0.0014	-0.0029	-0.0022	-0.0032
	(0.0006)	(0.0012)	(0.0017)	(0.0025)	(0.0025)
N	260,637	256,869	251,249	236,876	217,128

Estimates are marginal effects from ordered probit estimation. Each column denotes a separate regression. The sample includes women between 20 and 30 years of age. Also included in the specifications are year-of-birth indicators. The U.S. specifications also include state dummies; the Norway specifications include municipality indicators. Standard errors are adjusted for clustering at the state/municipality level.
* denotes statistically significant at the 5% level.

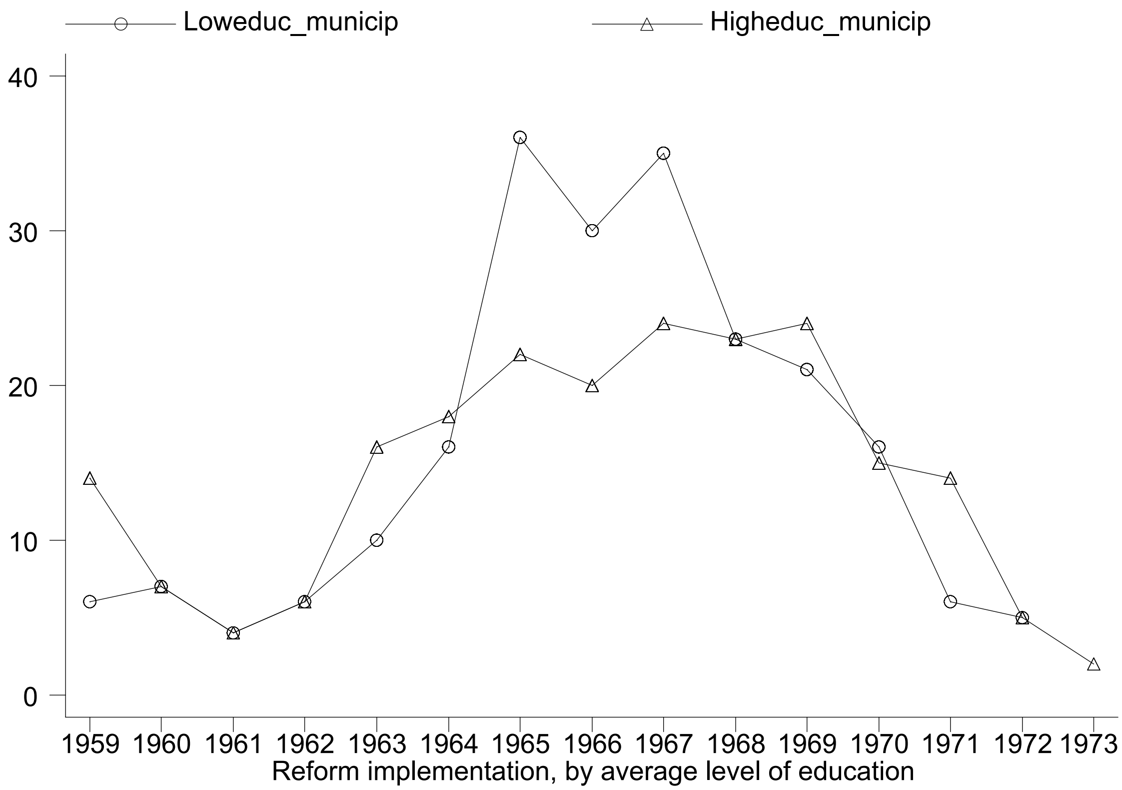
Appendix Figure 1
The Number of Municipalities Implementing the Education Reform, by Year
Norway

Appendix Figure 2
Reform implementation in Poor vs Rich Municipalities
Based on Average Family Income, Norway.



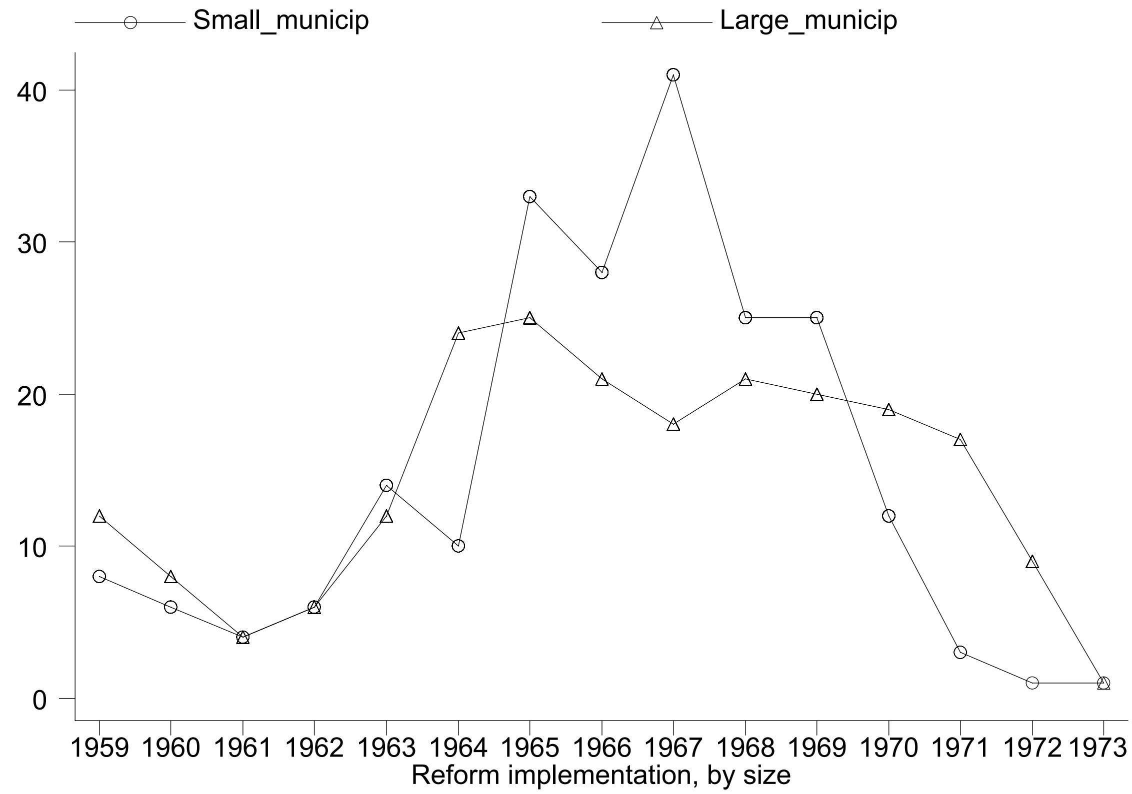
Poor (rich) municipality is calculated as below (above) median parent's income by municipality Parent's average income is calculated for each municipality in 1970.

Appendix Figure 3
Reform Implementation in High vs. Low Education Municipalities
Based on Average Years Father's of Education in the Municipality, Norway



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

Appendix Figure 4
Reform Implementation in Small vs. Large Municipalities, Norway



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

Appendix Table 1: Minimum Dropout Ages for the United States by State

	Dropout Age: 1924	Dropout Age: 1934	Dropout Age: 1944	Dropout Age: 1954	Dropout Age: 1964	Dropout Age: 1974
Alabama	16	16	16	16	16	16
Arizona	16	16	16	16	16	16
Arkansas	15	16	16	16	16	16
California	16	16	16	16	16	16
Colorado	16	16	16	16	16	16
Connecticut	16	16	16	16	16	16
Delaware	16	16	16	16	16	16
District of Columbia	14	16	16	16	16	16
Florida	16	16	16	16	16	16
Georgia	14	14	14	16	16	16
Idaho	18	18	16	16	16	16
Illinois	16	16	16	16	16	16
Indiana	16	16	16	16	16	16
Iowa	16	16	16	16	16	16
Kansas	16	16	16	16	16	16
Kentucky	16	16	16	16	16	16
Louisiana	14	14	14	16	16	16
Maine	17	17	14	16	16	16
Maryland	16	16	16	16	16	16
Massachusetts	16	16	16	16	16	16
Michigan	16	16	16	16	16	16
Minnesota	16	16	16	16	16	16
Mississippi	14	17	16	16	0	0
Missouri	16	16	14	16	16	16
Montana	16	16	16	16	16	16
Nebraska	16	16	16	16	16	16
Nevada	18	18	18	18	17	17
New Hampshire	16	16	16	16	16	16
New Jersey	16	16	16	16	16	16
New Mexico	16	16	16	17	17	17
New York	16	16	16	16	16	16
North Carolina	14	14	14	16	16	16
North Dakota	17	17	17	17	16	16
Ohio	18	18	18	18	18	18
Oklahoma	18	18	18	18	18	18
Oregon	16	18	16	18	18	18
Pennsylvania	16	16	18	17	17	17
Rhode Island	16	16	16	16	16	16
South Carolina	14	14	16	16	0	16
South Dakota	17	17	17	17	16	16
Tennessee	16	16	16	16	16	17
Texas	14	14	16	16	16	17
Utah	18	18	18	18	18	18
Vermont	16	16	16	16	16	16
Virginia	14	15	15	16	16	17
Washington	16	16	16	16	16	16
West Virginia	16	16	16	16	16	16
Wisconsin	16	16	16	16	16	18
Wyoming	16	17	17	16	17	17

Appendix Table 2: Effect of Compulsory Schooling Laws on Educational Attainment

Dependent Variable:	Norway: Education Clustering at Municipality Level	U.S.: Education Clustering at State Level	U.S.: Education Clustering at State-Year Level
	(1)	(2)	(3)
Dropout Age=16	.1218	.4041	.4041*
	(.0217)	(.2520)	(.0590)
Dropout Age=17		.4709	.4709*
		(.2865)	(.0696)
Dropout Age=18		.4245	.4245*
		(.3045)	(.0959)
White		.7298*	.7298*
		(.0715)	(.0228)
	N=260,641	N=1,270,753	N=1,270,753

Each column denotes a separate regression. The sample includes women between 22 and 30 years of age. Also included in the specifications are year-of-birth indicators. The U.S. specifications also include state dummies; the Norway specifications include municipality indicators. Standard errors are adjusted for clustering at the state/municipality level in columns (1) and (2). Standard errors are adjusted for clustering at the state-year level in column (3).
* denotes statistically significant at the 5% level.

Appendix Table 3:
Timing of the Implementation of the Reform in Norway

Dependent Variable: Year of Reform

	Coefficient	Standard error
County2	-1.95	.65
County3	5.02	5.23
County4	-.64	.70
County5	-.88	.67
County6	-.90	.62
County7	-1.21	.63
County8	-1.90	.64
County9	-1.21	.64
County10	-2.20	.71
County11	-.54	.63
County12	-1.4	.60
County13	-.45	.70
County14	1.23	.59
County15	-1.54	.58
County16	.04	.60
County17	-1.21	.57
County18	-.26	.65
County19	-2.77	.71
Share of Fathers with Some College	.92	3.88
Share of Mothers with Some College	12.30	8.31
Father's Income (mean)	-.007	.004
Mother's Income (mean)	-.01	.01
Father's Age (mean)	.11	.16
Mother's Age (mean)	-.12	.19
Size of Municipality/100	-.03	.03
Unemployment Rate 1960	-6.22	11.63
Share Workers in Manufacturing 1960	1.15	3.05
Share Workers in Private Services 1960	5.95	6.23
Share Labour Vote 1961	2.34	2.19
Constant term	1969.14	6.95

Robust standard errors. All variables are municipality level variables.

Appendix Table 4: Effect of Compulsory Schooling Laws on the Probability of Birth Conditional on Not Already Having a Child, Equal Weighting of Cohorts, United States Data

Dependent Variable	Birth at 16/No prior birth	Birth at 17/No prior birth	Birth at 18/No prior birth	Birth at 19/No prior birth	Birth at 20/No prior birth
Dropout Age=16	.0000	-.0008	-.0014	-.0030	-.0034
	(.0007)	(.0015)	(.0015)	(.0021)	(.0025)
Dropout Age=17	-.0013	-.0033	-.0056*	-.0091*	-.0083*
	(.0010)	(.0019)	(.0022)	(.0038)	(.0030)
Dropout Age=18	.0011	-.0093*	-.0109	-.0120	-.0106*
	(.0013)	(.0031)	(.0055)	(.0081)	(.0039)
White	-.0218*	-.0260*	-.0226*	-.0100*	.0040
	(.0017)	(.0027)	(.0036)	(.0031)	(.0035)
N	1,572,513	1,545,369	1,493,288	1,414,844	1,311,693

Estimates are marginal effects from ordered probit estimation. Each column denotes a separate regression. The sample includes women between 20 and 30 years of age. Also included in the specifications are year-of-birth indicators and state dummies. Standard errors are adjusted for clustering at the state level.
* denotes statistically significant at the 5% level.