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Is teenage motherhood contagious? Evidence from a Natural Experiment

BY

Karin Monstad, Carol Propper, AND **Kjell G. Salvanes**

Norges Handelshøyskole

NORWEGIAN SCHOOL OF ECONOMICS AND BUSINESS ADMINISTRATION

Is teenage motherhood contagious? Evidence from a Natural Experiment

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

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

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

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Address for correspondence
Carol Propper
Department of Economics
University of Bristol
Bristol BS8 ITN, UK
Carol.Propper@bristol.ac.uk

Abstract

There is relatively little research on peer effects in teenage motherhood despite the fact that peer effects, and in particular social interaction within the family, are likely to be important. We estimate the impact of an elder sister's teenage fertility on the teenage childbearing of their younger sister. To identify the peer effect we utilize an educational reform that impacted on the elder sister's teenage fertility. Our main result is that within families, teen births tend to be contagious and the effect is larger where siblings are close in age and for women from low resource households.

Introduction

Several recent studies have demonstrated that teenage fertility has long-term adverse social and economic impacts on both teen mothers and their children (for example, Angrist and Evans, 1996; Chevalier and Viitanen, 2003; Fletcher and Wolfe, 2008; Levine and Painter, 2003). Given this negative impact of early parenthood, it is important to understand the factors that shape the fertility decisions of teenagers. Previous research has shown that family background and own education affects teenage fertility. In addition, teen pregnancy is the type of decision where it might be expected that peer effects, within and outside families, are important.

Several papers have sought to provide robust evidence on the impact of social influences on fertility. Research has shown an effect in different contexts: for example, within ethnic or religious groups in developing countries (Manski and Mayshar, 2003), within geographic areas or neighborhoods (Bloom, Canning, Gunther, and Linnemayr, 2008), within workplaces (Hensvik and Nilsson, 2010; Ciliberto, Miller, Nielsen and Simonsen, 2010) and within families (Kuziemko, 2006). However, despite the importance of family in fertility decisions, there is little robust evidence on whether teenage motherhood is contagious within families.

Identification of a peer effect within families is difficult: it is well established that when estimating social interaction effects, one of the challenges is to distinguish group influences (in this instance, sibling influences) from any unobserved individual effects, endogenous peer group selection and correlated unobservables of peers (Manski, 1993, Moffitt, 2001, Sacerdote, 2010). In the case of sibling effects on teen motherhood, the problem is that each sibling's fertility behaviour is at least partially determined by parental characteristics, some of which are also unobservable.

In this paper we seek to overcome this problem. We examine the impact of sisters on teenage motherhood, exploiting a reform in education to get identification of the peer effect. We use data on teenage motherhood for many complete cohorts of sisters in Norway to estimate the impact of an elder sister's teenage motherhood on the teenage childbearing of their younger sister. We focus on sister-to-sister relations for a number of reasons. First, sisters generally spend more time together than schoolmates or friends and so sisters are likely to be influenced by the behaviour of their siblings (for example, the time siblings spend together has been shown in the psychological literature to be important, Azmitia and Hesser, 1993; Pine, 1995). Second, there is evidence that older sister sexual behaviour impacts on their younger siblings; for example, Rogers, Rowe, and Harris (1992) show younger siblings are sexually active earlier than their elder siblings. Third, the costs of a teen pregnancy may be lower if a sibling has already had a child. For example, Case and Katz (1991) argue the stigma costs of being a teenage mother may fall if a peer such as an elder sister already had a child as a teenager while Kuziemko (2006) argues that basis for peer effects in fertility may be shared monetary costs and time use as well as shared experiences and information.

To identify the effect of a sister's fertility behaviour on her sibling, we exploit an educational natural experiment in the 1960s in Norway which raised the minimum school leaving age (Black, Devereux and Salvanes, 2005; Aakvik, Salvanes and Vaage, 2010). Previous research has shown that education reduces teenage motherhood (Black, Devereux and Salvanes, 2008) in Norway. We therefore exploit this exogenous policy (discussed in section 2 below) to estimate the effect of a sister being a teen mother on their younger female sibling's chance of becoming a teen mother, instrumenting the elder sibling's teen motherhood by the education reform. We find that the peer effect is positive and significant for the mean individual: an older sister who has a teenage pregnancy considerably increases the chances that her younger sister will also have a teen birth. The effect is larger for women

from more resource constrained households. It also diminishes with a rise in the age gap between siblings, which fits with the explanation of a smaller influence of a peer as the age difference between sisters increases.[1]

In addition to contagion in teen pregnancy, education may have a multiplier effect on teenage pregnancy over and above any direct effect on an individual who has more education herself. A series of recent papers have exploited quasi-experimental changes or actual experiments in education to examine the direct impact of education on teen pregnancy. In the main these show that raising the education of girls decreases the chances of teenage pregnancy. Breirova and Duflo (2002) find a negative impact on fertility on early age fertility in Nigeria; Black, Devereux and Salvanes (2008) find the same for Norway; Monstad, Propper and Salvanes (2008) find no effect of education on total fertility in Norway but that education alters the timing of births so that children are more likely at later ages. Lavy and Zablotsky (2011) use changes in the access to schools in Israel for Arabic women as an instrument for increased education and find that education results in a large decrease in completed fertility. For Kenya, Duflo, Kremer and Dupas (2010) find access to education reduces fertility for adolescent girls who were otherwise likely to drop out of school. In contrast to these findings McCrary and Royer (2011), using the variation in schooling from exact dates for school entry, do not find an impact on fertility.

Our data also allow us to examine whether there is a multiplier effect over and above the direct individual returns from reducing teenage motherhood. We estimate the reduced form equation of the impact of being exposed to more education of a sibling, controlling for own educational exposure, on teen pregnancy. This allows us to assess the multiplier effect of education reforms through peer behaviour. In this first study of the impact of education

[1] Our paper has a similar focus to Kuziemko (2006) who uses family fixed effects for identification to examine peer effects in fertility among siblings in the USA in 1968-2001. Her dependent variable is giving birth in a given month at any age. Using family fixed effects, she cannot control explicitly for family size, which is likely to be highly correlated with age difference between siblings and nor does she look at teenage fertility. Arguably the policy reform we exploit gives a stronger identification strategy.

reforms working through peer effects, we find no multiplier effect of education at the mean. However, there is some indication of one within households with lower resources.

Section 2 gives a brief overview of the compulsory schooling law that we use as an instrument for education. The identification strategy is presented in Section 3 and the data is described in Section 4. The results are presented and discussed in Section 5. Section 6 concludes.

2. The schooling reform

The exogenous source of variation in fertility that we use derives from a compulsory schooling reform that the Norwegian Parliament legislated in 1959. This reform mandated that all Norwegians pupils attend two additional years of primary schooling, making the minimum years of schooling nine years. The curriculum was also standardized in order to improve school quality.[2] All municipalities (the lowest level of local administration) were mandated to have implemented the reform by 1973. As a result, although it was started in 1960, implementation was not completed until 1972. This meant that for more than a decade Norwegian schools were divided into two separate systems and the system experienced by children would depend on year of birth and municipality of residence. The first cohort that could have potentially been subject to the reform was that born in 1947. These individuals started primary school in 1954, and either finished pre-reform compulsory schooling in 1961, or finished post-reform compulsory schooling in 1963. The last cohort that could have gone through the old system was born in 1958. This cohort started school in 1965 and finished compulsory schooling in 1972.

The extra costs of the reform were provided by the national government and in order to receive these funds municipalities needed to present a plan to a committee under the

[2] 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. See Leschinsky and Mayer (1990).

Ministry of Education. While the criteria determining selection by the committee are somewhat unclear, the committee wanted to ensure that implementation was representative across the country, conditional on an acceptable plan; see Telhaug (1969) and Mediås (2000). In Figure A1 in the Appendix we depict the development of the reform, focusing on the number of municipalities implementing the reform each year. This shows after a slow start, the pace of implementation accelerated in 1949 and the number of municipalities implementing the reform each year was roughly constant between 1951 and 1956, tailing off thereafter.

For our identification strategy to work we need the data of implementation to be uncorrelated with fertility in the municipality. Monstad et al (2008) discuss some of the main technological or policy changes that may have affected fertility and relate them to the reform implementation process. These changes include the availability of subsidized child care and maternity leave, legislation on induced abortion, and the use of contraceptives in the 1960s and 1970s. Monstad et al (2008) show there is no relationship between the lagged change (from 2 to 5 years) in teenage fertility and the timing of reform. Other municipality characteristics could also be associated with fertility outcomes. For example, poorer municipalities might be earlier implementers of the reform given the substantial state subsidies, while wealthier municipalities would move at a much slower pace. However, Black, Devereux and Salvanes (2005, 2008) and Aakvik, Salvanes and Vaage (2010) find no support for a relationship between the timing of the educational reform and education level, income level or the size of the municipalities.

3. Identification strategy

The fertility outcome studied, denoted *TEENFERT*, is teenage motherhood of a woman *i* who is potentially impacted by the teenage motherhood of her older sister closest in age, labelled

sis. Our source of exogenous variation in sister's fertility is the education reform that increased the number of years of compulsory schooling from 7 to 9 years, labelled *EDUREF*. Both siblings we study are born within the reform implementation period, but both siblings may not be affected by the reform. Three combinations of exposure to reform are possible. A younger sister may be affected by the reform and her elder sister not, both sisters may be affected by reform, or neither sister. We exploit this variation by using the education reform as an instrument for the elder sister's fertility and estimating the following empirical model where equation 2 represents the first stage:

$$(1) TEENFERT_i = \alpha_0 + \alpha_1 TEENFERT^{sis} + \alpha_2 EDUREF_i + \alpha_3 CO_i + \alpha_4 MUN_i + \alpha_5 X_i + e_i$$

$$(2) TEENFERT^{sis} = \beta_0 + \beta_1 EDUREF^{sis} + \beta_2 CO^{sis} + \beta_3 MUN^{sis} + \beta_4 X^{sis} + \varepsilon^{sis}$$

where *CO* is a full sets of years-of-age and MUN is a full set of municipality indicators. Other control variables are represented by the vectors, X_i and X^{sis}, for the individual and the sister respectively. Both vectors contain the (common) variables for mother and father having completed any education above compulsory schooling, parents' age, family income, municipality size and family size.

Because both sisters are born within the reform implementation period they are potentially both individually affected by the reform. Thus the educational reform may impact on the younger sister's fertility in two ways. First, it has been shown to have a *direct* impact on the probability of teenage motherhood for the person exposed to the reform (Black, Devereux and Salvanes, 2008; Monstad, Propper and Salvanes, 2008). Second, it has a potential *indirect* impact via the elder sister's fertility, in which case it triggers a spillover effect, which is the main interest of this paper. In order to exploit $EDUREF^{sis}$ as an instrument, we need to control explicitly for the direct effect of the reform on the fertility of the woman of interest, $EDUREF_i$.

We also allow for heterogeneity in the response to the impact of a sibling. We are particularly interested in resources of the family and the age gap between sisters. We split the sample by income and education of parents and age gap of the sisters.[3]

To examine the more general multiplier effect of greater education within a family on teen fertility behaviour we estimate the reduced form specification:

$$(3)\text{TEENFERT}_i = \gamma_0 + \gamma_1 \text{EDUREF}^{sis} + \gamma_2 \text{EDUREF}_i + \gamma_3 \text{CO}_i + \gamma_4 \text{MUN}_i + \gamma_5 \text{X}_i + \gamma_6 \text{EDUREF}^{sis} \times \text{X}_i + \text{e}_i$$

where the coefficient of interest is γ_1 – the additional impact of (sister's) education over and above that of individual *i*. We can estimate (3) because there is variation in whether either only the younger sister is affected by the reform, or both the sister and the girl of interest or neither. Given the size of our data set, we estimate all equations as linear probability models.[4]

4 Data

From different administrative registers and census data from Statistics Norway we have access to a data set encompassing the entire population in Norway, including information on family relationships, family background, age, marital status, country of birth, educational history, neighbourhood information and employment information.[5] We also match data from the censuses in 1960 and 1970.

The focus of the analysis is how interactions between sisters affect female fertility, and we define sisters as individuals having the same mother and identify them using the mother's unique personal identification code. Our sample contains all individuals born between 1947 and 1958 and contains 384385 women. To be included in our estimation

[3] The precise definition of the subsamples is given in the results in Table 2.
[4] As a robustness check we also estimated probit models. The results were very similar (available from authors).
[5] See Møen, Salvanes and Sørensen (2003) for a description of the data set.

sample, the woman must have an older sister born in the same period.[6] Other sample restrictions arise from information on municipality, educational reform and family background variables.

The analysis requires that we identify whether an individual is impacted by the changed compulsory schooling law. To determine whether the women were affected by the reform, we link each woman to the municipality where she grew up by matching the administrative data to the 1960 census. From the 1960 census, we know the municipality where the woman's mother lived in 1960 and use this to define the municipality of the siblings.[7] The women we use in the estimation are aged between two and 13 years in 1960. The reform indicator will be equal to one for a woman if by age 13 (the seventh year of schooling), the new system had been implemented in her municipality of residence.[8] The same procedure is used for her older female sibling. We exclude from our sample the small number of women who have a birth before they are aged 15 years and define a teenage birth as one occurring from age 15 to anytime before the individual reaches her 20th birthday. Appendix Table A1 provides details of the data selection process.

Descriptive statistics

The data set consists of 42606 observations of women who have at least one elder sister where both sisters are born within the 1947-58 cohorts. Descriptive statistics are shown in Appendix Table A2 where the sample is divided according to elder sister's teenage motherhood. We see that teenage motherhood is much more frequent among women whose elder sister became a teenage mother (category A) than if she did not (category B); the

[6] If the closest elder sister belongs to a pair of twins, there are two observations (relations) in the sample. In the case where the individual at risk is herself a twin, only one of the twin sisters at risk will be included, by random choice.

[7] As very few children lived with their father in cases where the parents were not living together, we should only have minimal misclassification through applying this rule.

[8] From the year and month of birth of any children and the year and month of birth of the woman, we can determine the age of birth to the nearest month.

frequency is 31% compared to 14%. Women in category A, as well as their elder sisters, are born later than their counterparts in category B. The difference in means is 0.5 and 0.8 years, respectively. Consequently, as the educational reform was implemented gradually, more women and elder sisters in category A were impacted by the reform than in category B. Nevertheless, the average level of schooling is 1.2 years higher among category B women. Note that background characteristics differ between the two groups as teenage motherhood is associated with low family income, low parental education, living in a rural area, living in a big family and having relatively young parents.

5. Results

The effect of a teen pregnancy of an elder sister

Table 1 reports the estimates of the spillover effect of a teen pregnancy of an older sibling on her younger sister's teen fertility. The first column reports the OLS results and shows that teenage pregnancy of an elder sister has a positive effect on the fertility of the younger sister, with a coefficient of 0.115, i.e. around an 11 percentage point increase. On the other hand, being exposed to the education reform decreases the probability that the younger sister will have a teen birth but the estimated effect is considerably smaller (around 1.5 percentage points).

The rest of the columns report the IV results. The coefficient from the first stage is reported in the first row and shows, in line with previous research, that the reform had a negative effect on teenage motherhood for the person impacted by it (in this case the elder sister). The effect is statistically significant for the full sample. It is also very similar in size to that of the education reform for the younger sibling. For example, for the whole sample in column (2), the estimate of a sister being exposed to the reform is to decrease her chance of

teen pregnancy by 1.5 percentage points, while the effect of the individual being exposed to the reform is to decrease the chance by 1.4 percentage points.

The remaining rows of column (2) present equation (1) for the full sample. This shows that having a sister who has a teen birth increases the probability of own teenage motherhood. As is common in the literature when using educational reforms as instruments, the IV results have a bigger effect than the OLS results. The standard interpretation of this is heterogeneous effects in the returns to education – in this case on fertility – and that we should interpret the results as LATE effect for the group complying to the reform (Card, 1999). In this case this group is primarily those at the lower end of the education distribution.

The estimated effect is fairly large – the coefficient represents an effect of 24 percentage points which is an increase of about 60 percent of a standard deviation of teenage fertility (see Table A2). Moreover, the positive sibling effect dwarfs the negative effect of own education, which has a coefficient of 0.014. The results are also robust to exclusion of own reform status. For the sample in column (2), after dropping own reform status, the coefficient on teen motherhood of the sibling remains unchanged at 0.241 with a standard error of 0.059.

Columns (3) - (7) provides estimates for different subgroups based on family income and other characteristics. Column (3) examines only low income families.[9] The impact of an elder sister's teen motherhood rises compared to the full sample by a few percentage points to 28 percentage points. Column (4) and (5) report the results for girls from families where the mother and father respectively had more than compulsory education. Estimates of the spillover effect for both these groups are smaller than for the low income group or the full sample and are not, in fact, significantly different from zero. Column (6) shows the effect for families where there are only 2 siblings born in the study period. The effect is similar to that

[9] Low income is defined as in the bottom quartile of the full income distribution (i.e. before sample selection).

for all families reported in column (2). Column (7) reports results for families where the two sisters are less than 4 years different in age. This shows a spillover effect which is larger than for all families.[10]

On the basis of these results, we conclude that the spillover effect is large and there is also indication that it is larger in low income families and where the two siblings are closer in age. Our results also echo those of Kuziemko (2006). She uses a sample of all mothers and an approach that differs from ours, making it difficult to compare the magnitude of the estimates. But she also finds a large sibling effect. For example, she finds that the probability of having a child rises by 11 percentage points in the 24 months after the birth of a niece or a nephew and finds that the peer effect is especially strong for couples having their first child, when the sibling is female, and when the siblings are close in age.

The impact of greater education

Table 2 presents the reduced form equation (equation 3) in which we estimate the effect on teenage fertility of an exogenous increase in education for both a young woman and her older sister. Column (1) shows the effect of a two year mandatory ($EDUREF_i$) increase in own education is to reduce the probability of teenage motherhood, confirming the results from previous research of a direct effect of own education on own teen fertility (Black, Devereux and Salvanes, 2008): our result of a negative marginal effect of 0.013 percent reduction is similar to that in Black, Devereux, and Salvanes (2008) of 0.009. However, on average the

[10] Table 1 estimates are from estimates where equation (1) is not jointly estimated with equation (2). This is because the age controls in the two equations differ – the first stage (equation 2) includes controls for the cohort of the sibling, *sis*, and the second stage (equation 1) has controls for the cohort of *i*. The standard errors in Table 1 are therefore not corrected for the fact that the first stage is estimated. As a test of robustness we bootstrapped the standard errors for equation (1) using the full sample. We estimated the two equations 50 times and used this to estimate the standard errors in model (1). The standard error of the coefficients on teen motherhood of elder sister is 0.066 and on own reform status is 0.007. These are almost identical to those in column (2) of table 1.

effect of an elder sister being exposed as well as the younger sister ($EDUREF^{sis}$) has no statistically significant impact.[11]

Columns (2) –7 allow for heterogeneous responses across different social groups by interacting sister's reform status with family background variables. In column (2) we interact family income in the lowest (approximately the bottom quartile) income group with sister's reform status. This shows that the impact of a two year increase in older sister's education is to reduce the probability that her younger sibling will have a teenage pregnancy and the net effect of an exogenous increase in the education of a sister on own teenage pregnancy is negative for women in this group. In percentage point terms the net effect of an older sister's education is to decrease the probability of having a teenage baby by 3.4 percentage points or a fall of around a fifth given the mean rate of 17%. Column (3) shows no difference in effect between the richest families (approximately the top quartile of income distribution) and the rest of the sample. Columns (4)-(6) allow for heterogeneity in maternal education, paternal education and household size. The effects of sister's education over and above a girl's own are not significantly different from zero for these groups. Column (7) allows for heterogeneity by the age gap between the siblings. This shows that any effect of education of a sibling is reduced the more far apart the siblings are in age. For siblings who are more than three and a half years apart, the net effect of an increase in education of the elder sibling is negative.

In summary, the net effect of an increase in education of a sibling, allowing for the fact that this reform also impacts on the individual, is zero, but for resource constrained families both children being impacted by an education reform has a multiplier effect which reduces the probability of teen pregnancy.

[11] The background characteristics show the same patterns as the descriptive statistics. Teen pregnancy is more common in low income families, in families where the mother or father has the minimal level of education, in non-urban areas, in large families and where the parents are younger.

6. Conclusions

This paper exploits a natural experiment in education to examine within family peer effects in teen fertility. Using the fact that education reduces own teen fertility, we estimate the causal effect of having an older sister who is a teen mother on the probability that her younger sister will also have a teen birth. Our results show that teen motherhood of the older sister has a significant positive impact on the probability that her younger sister will also have a teen birth. This effect is large on average, dwarfing the (negative) impact of an increase in own education. It is larger for siblings who are close in age, supporting our interpretation of a peer effect, as we would expect any such effect to wear off as the age gap between the sisters increases. It is also higher for women from low income households where shared resources between siblings may matter more. We conclude that, within families, teen births tend to be contagious.

We also are able to add to evidence on the general multiplier effect of education on teenage pregnancy. We find that, over and above an own education effect for the individual at risk, there is no additional effect in the whole population of having a better educated sister on the probability of teen motherhood. So within the population as a whole there do not appear to be multiplier effects. However, there is some indication that within more resource constrained households greater education for an older sister reduces the chances that her younger sister will have a teen birth.

Our findings also show that the direct contagion effect of teen pregnancy is larger (and in the opposite direction) than the general effect of more education. To the extent that we can read from changes in Norway in the 1970s and 1980s, this suggests that increasing education for girls will decrease teenage fertility but the strong effect of within household spillovers also suggests policies aimed directly at decreasing teenage pregnancy may also be needed to reduce teen births.

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Table 1: The impact of an older sister's teenage motherhood on teenage motherhood of her younger sister

	OLS	IV					
	Full sample	Full sample	Lowest income	Mother more than compulsory education	Father more than compulsory education	Two child family	Sisters close in age
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First stage							
Elder sister exposed to education reform		-0.015**	-0.043**	-0.020**	-0.022***	-0.016*	-0.020***
		(0.007)	(0.019)	(0.009)	(0.008)	(0.009)	(0.008)
Second stage							
Teen motherhood of sister	0.115***	0.242***	0.282**	0.149	0.143	0.214***	0.262**
	(0.007)	(0.059)	(0.134)	(0.102)	(0.096)	(0.075)	(0.111)
Individual exposed to education reform	-0.015*	-0.014*	-0.019	-0.024**	-0.020**	-0.018*	-0.010
	(0.008)	(0.008)	(0.020)	(0.010)	(0.009)	(0.010)	(0.008)
Background characteristics							
Family in lowest income group	0.093***	0.086***	-	0.088***	0.090***	0.069***	0.099***
	(0.006)	(0.007)		(0.012)	(0.011)	(0.010)	(0.009)
Mother's education compulsory schooling only	0.056***	0.049***	0.038**	-	0.059***	0.045***	0.047***
	(0.004)	(0.006)	(0.016)		(0.009)	(0.007)	(0.009)
Father's education compulsory schooling only	0.058***	0.051***	0.050***	0.064***	-	0.054***	0.049***
	(0.005)	(0.006)	(0.017)	(0.010)		(0.008)	(0.009)
Living in large family (>2 children)	-0.005	0.013***	0.024**	0.009	0.012**	-	-0.069***
	(0.004)	(0.004)	(0.012)	(0.006)	(0.005)		(0.004)
Living in 50 largest municipalities	0.016***	-0.002	-0.357***	-0.059***	0.018**	0.008	0.014***
	(0.004)	(0.004)	(0.028)	(0.006)	(0.007)	(0.010)	(0.005)
Mother's age below top quartile	0.028***	0.023***	0.047***	0.017**	0.023***	0.024***	0.019**
	(0.006)	(0.006)	(0.017)	(0.007)	(0.008)	(0.009)	(0.008)
Father's age below top quartile	0.035***	0.032***	0.045***	0.025***	0.029***	0.029***	0.036***
	(0.005)	(0.006)	(0.014)	(0.009)	(0.007)	(0.008)	(0.008)
N	42606	42606	7615	15588	19552	21496	32041

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in the first row is teenage motherhood of elder sister. Dependent variable in the second stage results is teenage motherhood of *i*. Columns (2) - (7) are two stage estimates for different samples. Standard errors are clustered at the municipality level. A full set of municipality indicators and a constant term are included in all estimates (not shown here). The specifications include age dummies for elder sister *sis* in the first row and for *i* in second stage results. All variables are dummy variables. Low income family (column 3) is defined as the lowest quartile of family income. The division into quartiles was made before sample restrictions. Close in age (column 7) is age difference less than 4 years.

Table 2: The impact of an older sister's exposure to more education on teenage motherhood of her younger sister

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Elder sister exposed to education reform	0.009	0.017***	0.009	0.007	0.011	0.011	0.025***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Individual exposed to education reform	-0.013*	-0.012	-0.014*	-0.013*	-0.013*	-0.013*	-0.017**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Interaction terms with sister's reform status							
Family in lowest income group		-0.051***					
		(0.012)					
Family not in highest income group			-0.000				
			(0.007)				
Mother's education compulsory schooling only				0.004			
				(0.008)			
Father's education compulsory schooling only					-0.002		
					(0.008)		
Living in large family (> 2 children)						-0.004	
						(0.007)	
Age difference between *i* and sister							-0.007***
							(0.002)
Background characteristics							
Family in lowest income group	0.098***	0.116***	0.060***	0.098***	0.098***	0.098***	0.098***
	(0.006)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mother's education compulsory schooling only	0.063***	0.063***	0.060***	0.061***	0.063***	0.063***	0.064***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
Father's education compulsory schooling only	0.065***	0.065***	0.060***	0.065***	0.066***	0.065***	0.065***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Living in 50 largest municipalities	-0.013***	-0.016***	-0.008*	-0.012***	-0.013***	-0.013***	-0.010**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Living in large family (>2 children)	0.018***	0.018***	0.020***	0.017***	0.018***	0.019***	0.017***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Mother's age below top quartile	0.032***	0.033***	0.032***	0.032***	0.032***	0.032***	0.032***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Father's age below top quartile	0.038***	0.038***	0.029***	0.038***	0.038***	0.038***	0.037***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
N	42606	42606	42606	42606	42606	42606	42606

Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. Each column shows the result from a separate estimation. Standard errors are clustered at the municipality level. A full set of municipality and cohort indicators and a constant term are included (not reported here). All variables are dummy variables. The reference categories for background characteristics in columns 1-2 and 4-7 are as follows: family income above lowest income group, mother's (father's) completed education is above compulsory schooling, living in a municipality that is not among the 50 largest, living in a small family (<=2 children), mother's (father's) age is in top quartile. The estimates in column (3) use the income variable defined as 'family not in highest income group',i.e., the reference category is income in top quartile.For precise definition of income variables see table A2.

Appendix

Figure A1. The number of municipalities implementing the reform by year

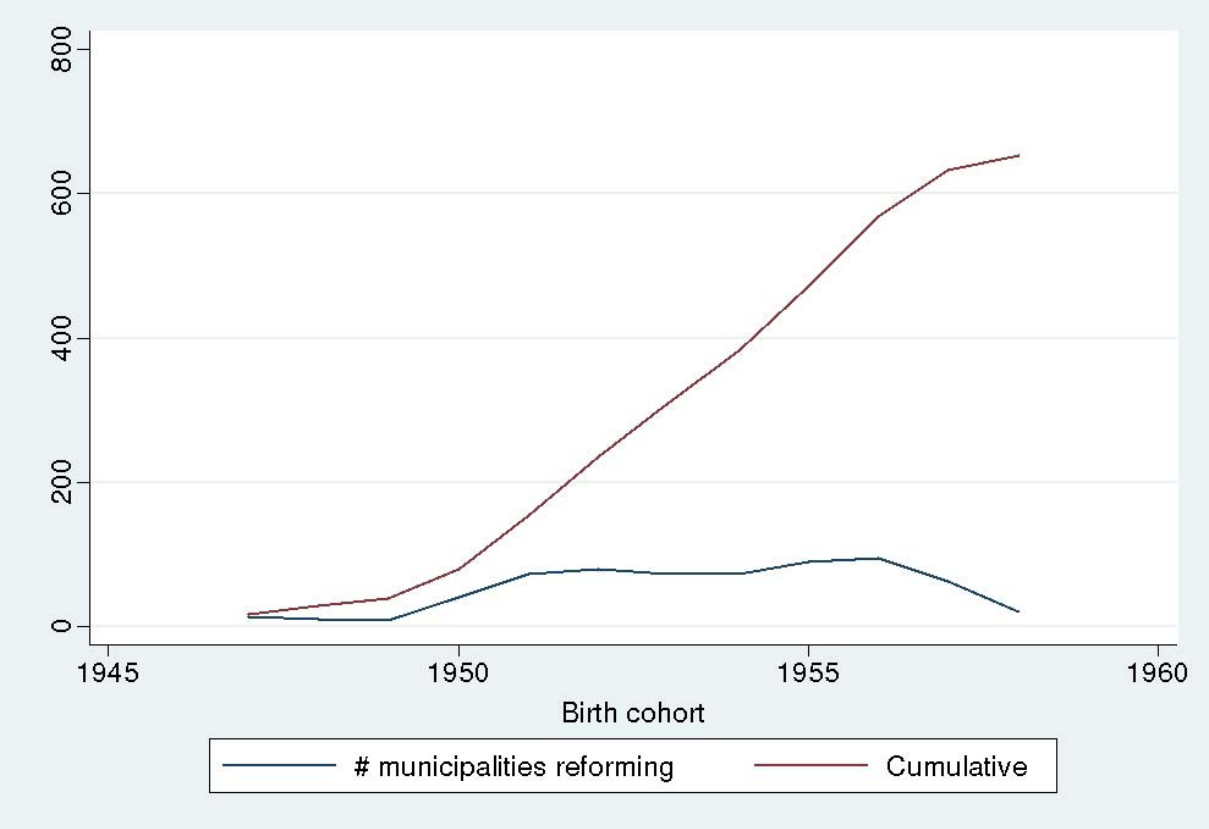

Table A1. Data selection process

	Observations
Women born 1947-1958, in total	384385
Excluded for following reasons	
Motherhood before age 15	101
Woman's education <7years	783
Missing municipality	78952
Missing reform indicator	11841
Missing woman's length of education	2104
Missing father's education	7251
Missing mother's education	239
Missing mother's age	4029
Missing father's age	2348
Missing family income	2156
Missing mother's identification code	46433
Woman is only child within the family	69952
Woman part of a group of triplets	30
Woman is the elder sibling in the family within the sample	63825
Less than 9 months interval between siblings' births	13
Sample of women with an elder sibling after exclusions	94328
Estimation sample: women whose closest elder sibling is a sister	42606

Table A2. Descriptive statistics

	Full estimation sample		Elder sister is a teen mother (Category A)		Elder sister is not a teen mother (Category B)	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Younger sister:						
1 if first birth at age 15-20	0.17	0.38	0.31	0.46	0.14	0.35
Age at first birth	24.5	5.0	22.6	4.6	24.8	5.0
Years of schooling	11.6	2.5	10.6	2.0	11.8	2.6
Impacted by reform	0.73	0.45	0.76	0.43	0.72	0.45
Year of birth	1955.0	2.4	1955.4	2.3	1954.9	2.4
Elder sister:						
Age at first birth	24.2	4.6	18.8	0.9	25.4	4.3
Impacted by reform	0.41	0.49	0.46	0.50	0.40	0.49
Year of birth	1951.9	2.6	1952.5	2.5	1951.7	2.6
Background characteristics:						
Family in lowest income group	0.18	0.38	0.24	0.42	0.17	0.37
Family is not in highest income group	0.72	0.45	0.84	0.37	0.70	0.46
Mother's education is compulsory schooling only	0.63	0.48	0.79	0.41	0.61	0.49
Father's education is compulsory schooling only	0.54	0.50	0.70	0.46	0.51	0.50
Living in one of 50 largest municipalities	0.35	0.48	0.26	0.44	0.36	0.48
Living in a large family (> 2 children)	0.50	0.50	0.54	0.50	0.49	0.50
Mother's age is below 4th quartile	0.86	0.35	0.91	0.29	0.85	0.36
Father's age is below 4th quartile	0.84	0.36	0.88	0.33	0.84	0.37
N	42606		6715		35891	

Lowest income group is defined as the lowest quartile of family income. Not in highest income group is defined as family income below the highest quartile. The division into quartiles was made before the sample restrictions were imposed. The number of observations is the same for all variables except age at first birth, which by definition is missing for childless women.

NHH

Norges Handelshøyskole

Norwegian School of Economics
and Business Administration

NHH
Helleveien 30
NO-5045 Bergen
Norway

Tlf/Tel: +47 55 95 90 00
Faks/Fax: +47 55 95 91 00
nhh.postmottak@nhh.no
www.nhh.no