Chapter 3

Education and Fertility: Testing for Family Background and Spillover Effects*

by

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Abstract

This paper analyses the effect of family background and social interaction on fertility choices over a woman's fertile period. The outcomes studied are the timing of first birth and whether women become mothers at all. I exploit a natural experiment—in the form of an educational reform—to correct for selection into education. The analysis benefits from a rich data set with information on parental education, age and income and the municipality of residence. In addition to examining parents' influence, I also investigate the impact of elder siblings of the same gender. Interest lies in how various aspects of family background interact with education, resulting in differences in fertility behaviour. Judging by the reaction to an increase in compulsory schooling, I find that the most important channel for the impact of family background on fertility is through family income and whether the young woman lives in a city. However, the potential spillover effect of the reform from elder to younger sisters is not found to be significant. The group that seems to have responded to the reform most strongly in terms of delaying first birth consists of women from low-income families, living in cities. The heterogeneity in responses is especially strong regarding the likelihood of first birth as a teenager. Thus, family background proves to be an important causal determinant for the effect of educational reform on fertility.

3.1 Introduction

In studies of fertility, it is a common finding that women's choice of education is an important explanatory factor (Kravdal, 1994; Hotz, Klerman and Willis, 1997). Studying the causal relationship between fertility and education, Monstad, Propper and Salvanes (2007) find that more education leads women to postpone first births, but that it does not result in lower total fertility or the greater incidence of childlessness. The causality is based on a natural experiment, i.e., an educational reform that increased compulsory schooling in Norway by two years. The effect estimated is by definition a "local average treatment effect" (Angrist, 2004); this of course raises the question about the generality of the results. Policy measures are often intended to benefit certain segments of the population, which is another reason to study heterogeneity in policy response. Indeed, one of the main aims of the educational reform in question, as stated explicitly in government documents, was to enhance the equality of opportunity along both socio-economic and geographic dimensions (Black, Devereux and Salvanes, 2005a). Furthermore, if education has a causal impact on fertility, particularly the timing of births, this is a potential channel through which education can have distributional consequences across generations.

Investment in education can be evaluated by the private rate of return. If externalities arise, the social and private rates of return will differ (Lucas, 1988). Even if educational reforms are hardly ever implemented because of their effect on fertility, one should bear in mind that such policy measures have fertility consequences and that fertility behaviour implies externalities. For instance, at the macro level, the number of children born and the age structure of the population have implications for economic growth. Research also suggests that teenage pregnancy shapes the life conditions for the child to be born in an adverse manner (for references, see Black *et al.,* 2006). Moreover, motherhood at a late age can have unfavourable medical consequences for the child: "…more stillbirths, more infant deaths, more premature births, more chromosomatic problems and more learning problems" (Gustafsson, 2001, p. 244).

One way that externalities may arise is that an individual's behaviour and norms may shape another person's preferences and behaviour. Such spillover effects are a special concern in the "new social economics literature" (Durlauf and Young, 2001). This literature examines such diverse phenomena as residential segregation (Schelling, 1971), neighbourhood effects on teenage childbearing (Crane, 1991) and how the presence of other smokers in a household affects the decision to quit smoking (Jones, 1994). Fertility is influenced by many factors,

e.g., economic and cultural factors. It then appears reasonable that the family is an institution that shapes young girls' values and attitudes towards important decisions, including the choice of education and family formation. In several studies, the characteristics of the family have proven to have a great impact on young people's choice of education, labour market outcome, etc. (see e.g., Aakvik, Salvanes and Vaage, 2005; Black *et al*., 2005a and 2005b; Raaum, Salvanes and Sørensen, 2006). In this paper, I examine whether community and family background play an important role in decisions on fertility, and whether a spillover effect can be traced in the data. Elder relatives (grandparents, uncles and aunts) have been proven to have an impact on educational outcomes for same-gender adolescents (Loury, 2006). I will estimate the impact on fertility of elder sisters' education, while also controlling for the mother's and father's education.

When estimating social interaction effects, one of the challenges is to distinguish group influences (in this instance, sister influences) from any unobserved individual effects. I consider the possibility that growing up with a more educated sister reduces the propensity to become a teenage mother, conditional on other background characteristics, e.g., parental characteristics. The problem is that the sister's level of education is at least partially determined by parental characteristics, some of which are also unobservable. A natural experiment offers an approach to overcome this difficulty (Durlauf and Young, 2001).

The purpose of this paper is twofold. First, to examine the extent of heterogeneity in response to educational reform, and thereby identify the groups of women whose fertility behaviour changed the most owing to the reform. Second, to examine whether education triggers a spillover effect within the family, so that an elder sister's having more compulsory education has an impact on the younger sister's fertility outcomes, in particular the probability of teenage motherhood. Moffitt (2001) points to several methodological problems in identifying the effect of social interactions. This analysis benefits from a natural experiment; this helps solve the problem of unobservable heterogeneity. Unlike many other studies, the impact of family background is studied within the context where the link between education and fertility is causal.

The paper unfolds as follows. Section 3.2 gives a brief overview of the institutional setting and the compulsory schooling laws, as well as references to the relevant literature. The identification strategies chosen are presented in section 3.3 and the data sets used are described in section 3.4. The results are presented and discussed in section 3.5. Section 3.6 concludes.

3.2 Background information

In the literature on fertility choices, a woman chooses between two alternative uses of her time: participating in the labour market or taking care of children (Hotz *et al.*, 1997). Thus, studies on heterogeneity in the returns to education are relevant. Oreopoulos (2006) has addressed the question of heterogeneity from a broad perspective. Often it is claimed that educational reforms only affect the behaviour of a small part of the population, and that the results from studies using these reforms as instruments diverge from the average effect for the whole population. However, when Oreopoulos compares the effects of reforms of compulsory schooling across several countries, he finds that the estimated returns to education are very similar, whether they are estimated using reforms that affected almost half the population or only a small portion.[58] Using Norwegian data, Aakvik *et al.* (2005) have specifically studied the relationship between educational attainment and family background. The sample used is males and females born within the period from 1967 to 1972. The authors have data on family income at different periods of a child's life, which makes it possible to separate the long and short-term effects of income. They find that "…permanent income matters to a certain degree and that family income when the child is 0 to 6 years old is an important explanatory variable for educational attainment later in a child's life". The overall result is that "…long-term factors, such as permanent family income and parental education, are much more important for educational attainment than are short term credit constraints".

While Aakvik *et al.* (2005) study the impact of family background on education by means of a number of control variables, educational choice is still subject to selection because of unobserved factors. For instance, parental education can be positively correlated with parental ability and the ability of the offspring. I am able to examine the interaction between education and family background when there is an exogenous source of variation in education. Work by Oreopoulos indicates that the average and the local average treatment effects of education reforms are quite similar. Regardless, the mean effects may disguise substantial heterogeneity. To my knowledge, the observed heterogeneity in how women

[58] Oreopoulos (2006) focuses on the cross-country comparison of the mean effects. A number of socio-economic variables are used as control variables, but the differences in effects between socio-economic groups are not emphasized. The returns to schooling are estimated to be lower for males in most specifications. This finding holds across the countries studied, including the United States, Canada, the United Kingdom and Britain. Race is included in the model only for the US sample, and its impact depends on the specification employed.

respond to educational reform with respect to the timing of first births and childlessness has not been studied.[59] In this paper, this is analysed over a woman's entire fertile period.

The current analysis makes use of 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 (i.e., nine years) and was implemented by Norwegian municipalities at different times during the period from 1960 to 1972. For details on the reform itself and the implementation process, see Aakvik *et al.* (2003).

3.3 Identification strategies

The heterogeneity in the effect of the reform and the spillover effect are both identified by means of a difference-in-difference approach. Due to the structure of the data, the spillover effect is estimated using a subsample.

3.3.1 Identification strategy regarding heterogeneity analysis

Because interest lies in fertility outcomes Y_i that are binary, a probit model is used.[60] The main specification used is a latent variable model:

(1) $$Y_i^* = \beta_0 + X_i'\beta_1 + Z_i'\beta_2 + R_i Z_i'\beta_3 + e_i$$

where $Y_i = 1$ if $Y_i^* > 0$ and $Y_i = 0$ otherwise, and where I define

$X_i' \equiv (R_i, C_i, M_i)$.

β_1 is a vector of coefficients for the set of individual characteristics X_i. The arguments of X_i are a reform indicator R_i, the set of municipalities C_i and cohorts M_i, which for individual *i* will take the value 1 for the municipality of residence and the reform person's cohort. Variation in the year of implementation among the municipalities makes it possible to control for both cohort and municipality when analysing the effects of the reform. β_2 is a vector of coefficients for the individual's background characteristics Z, where Z = (family income, mother's birth cohort, father's birth cohort, mother's level of education,

[59] McCrary and Royer (2006) include a control for maternal endowments in their analysis of the education effects on infant health. They comment: "…one could instead use an approximation that included interaction terms between schooling and endowments. Richer estimation equations such as these are, however, rare in the literature." Fort (2006) points to the problem of heterogeneity, but likely due to lack of data, does not examine how the effect of educational reform varies according to socio-economic characteristics.

[60] In the benchmark model, OLS estimation results are reported for the purpose of comparison.

father's level of education, urbanity). β_3 measures heterogeneity in the response to the reform, by means of the interaction terms $R_i Z_i$. The error term e_i is assumed to be i.i.d. and normally distributed, $e_i \sim N(0,\sigma)$.

In the benchmark model, all arguments in Z are set equal to zero, so the specification is

(2) $$\tilde{Y}_i^* = \alpha_0 + \alpha_1 R_i + \alpha_2 C_i + \alpha_3 M_i + \tilde{e}_i .$$

In this paper, I control for many aspects of observable heterogeneity. It should be noted that the reason to include background variables in eq. (1) lies in an interest in heterogeneity itself, and not to enable the better identification of the effects of R_i, as is sometimes attempted if there are concerns with endogeneity. The Norwegian mandatory schooling reform that I employ as an instrument for education in this paper, has been applied in other contexts by Aakvik *et al.* (2003), Black *et al.* (2005a, 2005b and 2006) and Monstad *et al.* (2007).[61]

3.3.2 Identification strategy regarding spillover analysis

The fertility outcome Y_i studied here is teenage motherhood of the younger sister in a group of sisters born within the reform cohorts. Thus, the main specification used is a latent variable model which is an extension of eq. (1):

(3) $$Y_i^* = \gamma_0 + X_i'\gamma_1 + Z_i'\gamma_2 + R_i Z_i'\gamma_3 + \varepsilon_i$$

where Y_i =1 if Y_i*>0 and Y_i =0 otherwise,

$X_i' \equiv (R_i, R_i R_i^S, C_i, M_i, D_i)$ and

$\gamma_1' \equiv (\delta_1, \delta_2, ...)$.

Equation (3) introduces the reform indicator R_i^S, which is related to the elder sister closest in age to the unit of observation *i*. R_i^S takes the value of 1 if the elder sister was

[61] In a natural experiment, the identification of the causal effect relies on the assumed source of exogenous variation being uncorrelated with any omitted variables that are correlated with the endogenous variable. The basic justification for the increase in compulsory education to be a natural experiment is the set up of the reform implementation. To demonstrate their point further, Black *et al.* (2006) regressed the year of reform implementation on a number of observable municipality characteristics and found no statistical significant relationships apart from the year dummies.

impacted by the reform, i.e., the mandated nine years of education. Thus, the variable of interest is $R_i R_i^S$. The model also includes as explanatory variables the set of municipalities M and cohorts C_i, and in most estimations the age difference between the sisters, D_i. The error term ε_i is assumed to be i.i.d. and normally distributed. Accordingly, a probit model is chosen for the estimation.

In principle, there are the following possible combinations of reform status for any pair of sisters:

- Case A: both the younger sister and the elder sister are impacted by the reform.
- Case B: the younger sister is impacted by the reform; the elder sister is not.
- Case C: neither the younger sister nor the elder sister is impacted by the reform.

The identification of δ_1 in eq. (3) utilizes variation in the younger sister's reform status, i.e., groups A and B compared to C. δ_2 is identified by means of variation between group A compared to groups B and C.

In the large majority of cases, there is only one sister for each individual in the sample, see Table 4 in the Appendix. When there is more than one possible pair of sisters, eq. (3) is estimated for the pair that is closest in age. The age difference D_i is defined accordingly.

3.4 Data

The analysis makes use of register data with information on all Norwegian women born from 1947 to 1958. To be included in the analysis, the woman's municipality of residence in 1960 and the reform status of the municipality must be known. The data set is very rich and includes background variables such as each parent's education, age and income. The income variable chosen is family income, defined as the sum of the mother's and father's income. For more information on the data set, see Monstad *et al.* (2007).

3.4.1 Data for heterogeneity analysis

After dropping observations because of missing information on background variables, the remaining data set consists of 274,581 observations. The data selection process is described in Table 1.

Within the restricted sample, 53% were affected by the reform. The descriptive statistics shown in Table 2a justify the argument that the effect of the reform must be considered a local average treatment effect. That is, while the reform mandated nine years of schooling, the mean length of education for those *not* affected by the reform was 11.26 years, so many women received more than nine years of education, even without the reform.

Regarding fertility outcomes, the non-reform group were subject to a pile-up of first births in the age group 20 to 25, while the age at first birth is more dispersed in the reform group.

The data show much variation in background variables, as can be expected given that a large part of the population is included. Differences in the year of birth and the years of education should be related to the fact that it took time to implement the reform: girls who were impacted by the reform are of a younger cohort than the non-reform group, and their parents are, on average, five years younger and better educated with 0.3 more years of schooling. The measure of parental education from the 1960 Census has been mapped onto the years of education following Raaum *et al.* (2006). Subsequently, parents are classified into three educational categories according to the length of schooling. There are many more men than women in the highest category defined, i.e., those with at least 12 years of schooling. In the reform group, a higher proportion lives in one of the ten major cities. Mean family income is considerably higher, which could be related to the higher level of education, wages being generally higher in cities and the presence of fewer old age pensioners among parents in the reform group. It should be kept in mind that within the parent generation, the level of education is generally quite low. More particularly, 55% of fathers and 65% of mothers in the sample have no more than compulsory schooling: that is, seven years of schooling. Only 9% of fathers and 2% of mothers received more than 12 years of education. There is a very strong correlation between the father's income and family income (the correlation coefficient is 0.94), though the correlation between the father's education and family income is much weaker (the correlation coefficient 0.41, see Table 1 in the Appendix).

Data on family income are taken from the 1970 Census. This is the data source closest in timing to the reform implementation. The impact of family income may change over a

person's childhood and adolescence. Aakvik *et al.* (2005) have found that with regards to educational attainment, it is especially income in early childhood that matters. The income data in this study originate in one particular year, 1970, when the women in the sample were from 12 to 23 years old, with the mean individual aged 17 years. However, family income is strongly correlated over the life cycle, so I will use these income data as a proxy for income earlier in life.[62]

3.4.2 Subsample for spillover effect analysis

The data set consists of 48,574 observations of women who have at least one elder sister within the 1947 to 58 cohorts. Descriptive statistics are shown in Table 2b.

The population of sisters compares well with the larger population, see Table 2 in the Appendix. The most interesting aspect of the data set is the comparison between the three groups of women labelled A, B and C above. For each observation, the analysis uses two potential "treatments": first, being exposed to the reform yourself; and second, having an elder sister being exposed. The control group for the first treatment, group C, consists of women who were not impacted by the reform themselves, nor were their elder sisters. On average, these women are three years older and have less education, as expected. It took time to implement the reform, so the probability that two sisters have both been exposed to the reform is greater if they both belong to a younger cohort. For both to be in the 1947 to 1958 sample, with the younger sister belonging to group A, the age difference between them cannot be too large. Group B is defined in such a way that it includes many of the elder sisters from the older cohorts. Thus, the age difference between sisters within a family is, on average, 4.5 years in group B as compared to 2.8 years in group A. As a consequence, the sisters of group B members are, on average, 2.5 years older than group A's sisters.

It is noteworthy that the elder sisters of group A, on average born in 1953, had a much higher likelihood of teenage motherhood than the others (0.18 compared to 0.15 and 0.14). The data show a shifting trend in teenage motherhood. The frequency started to rise with the cohorts born in 1951 and 1952 and then fell from the 1955 to 1956 cohorts onwards: see Table 3 in the Appendix.

Equation (3) controls for both the younger sister's birth cohort and the elder sister's, through the age difference dummy.

[62] In principle, I could examine whether the impact of family income depends on the woman's age when income is measured. However, such a specification would introduce many more interaction terms and could become excessively complex.

3.5 Results and discussion

3.5.1 Results from heterogeneity analysis

As a benchmark, I estimated the effect of the reform without any interaction terms, and the results are reported in Table 3. The reform makes it less likely to have a first birth as a teenager and more likely to postpone birth until aged 20 years or above, with a statistically significant increase in the 35 to 40 years age group. The effect on childlessness is positive but statistically insignificant. These results are essentially the same as found when using a sample that is not restricted on background variables (Monstad *et al.*, 2007).

The results of including background variables are given in Tables 4 to 6. All three tables report the results from estimations of eq. (1), but family income is expressed by a whole set of quartile dummies in Table 4, and by a dummy for whether the family belongs to the bottom income quartile or not in Tables 5 and 6. Municipality dummies are also included, implying that fixed characteristics at the municipality level are controlled for, e.g., norms, average income level and local labour market conditions. The partial effects for these dummies are not reported. The additional background variables included are family income, the parents' year of birth and level of education and a dummy for whether the family lived in one of the ten major cities in 1960.[63] The base category is defined as follows: girls not impacted by the reform; those who come from low-income families where the parents are old[64] and belong to the lowest educational category; and who do not live in one of the major cities. Some of the background variables have strong *direct* effects on fertility, as can be read from the upper part of Table 4. However, the analysis will focus on the effect that goes via education, see the lower part of the table.

The overall picture when studying the response to the reform is that family income matters. Table 4 shows that the impact of family income is particularly strong for teenage motherhood. When compared to the bottom income quartile, the interaction terms for higher-level family income have positive signs, meaning that girls living in low-income families had

[63] Ideally, information on the parents' age at first birth would be useful. Unfortunately, such information is not available. As an alternative to using the parents' birth cohort, separate estimations for the mother's and the father's age when the child was born were undertaken. As this effect had the same sign but was of smaller magnitude, I chose to include the parents' birth cohort in the estimation.

[64] Fathers who belong to the oldest age quartile are born in 1914 or before and mothers in 1918 or before. The father's and mother's age when the child is born is on average 42.2 years and 37.6 years, respectively.

the highest reduction in probability.[65] Having a family income above the 1st quartile reduces the tendency of the reform to cause women to postpone first birth past the age range of 15 to 25 years, and it significantly weakens the response for childlessness. The variables representing father's education are dropped because of collinearity. This draws attention to the strong correlation between father's education, family income and mother's education shown in Table 1 in the Appendix. As shown, the family income categories most likely partially capture the effects of the father's education.

The mother's educational level proves to be an independent source of variation. Due to the reform, children of more educated and younger mothers and fathers tended to postpone first birth, not only past the teenage years, but also beyond ages 20 to 25. This impact is particularly strong if the mother has more than 12 years of education. Likewise, living in one of the ten major cities strengthens the effect of the reform in the direction of a decreased likelihood of giving birth as a teenager. Controlling for other variables, the reform also caused a small, but statistically significant, increase in the likelihood of being childless among urban women.

Family income and urbanity prove to be the most important background variables concerning the response to the reform, so I shall focus on these in the following discussion. Estimation with a full set of dummies for family income quartiles has shown that the effect for the bottom quartile is profoundly different from the other three quartiles. Therefore, I simplify the specification so that family income is expressed through a dummy indicating whether the family belonged to the bottom income quartile. Furthermore, the discussion will focus on the heterogeneity related to teenage motherhood. Teenage motherhood is the outcome variable on which the reform has proven to have the strongest estimated impact (see Table 3), and it is also the outcome where the heterogeneity in response to the reform is the greatest (see Table 4).

Table 5 shows the heterogeneity associated with income and urbanity over the whole fertile period, whereas Table 6 focuses on teenage motherhood and reports the heterogeneity with respect to income for urban and non-urban individuals separately. Table 5 further illustrates the finding that the reform had a greater impact on urban girls' tendency to give

[65] The magnitude of the positive partial effects for income quartiles 2, 3 and 4 may appear a puzzle because they are greater in size than the negative partial effect of the reform itself. Accordingly, it appears as if the net effect of the reform is positive for income quartiles above the lowest quartile. However, the magnitude of these partial effects is not comparable because they are computed at different values for the other variables (Wooldridge, 2003, p. 561). For instance, in computing the partial effect of the reform itself (–0.052), each income quartile is assumed to constitute approximately 25% of the population. In computing the partial effect of the interaction term with the second income quartile (0.146), it is assumed that income changes from the 1st income quartile as the base category to the second income quartile.

birth as a teenager. For the remaining outcome variables, the difference-in-effects between urban and non-urban girls are small. From Table 6 we can see that it is the poorer families within the urban community that respond most to the reform.

In most respects, the reform had an equalizing effect on the timing of births: the sign of the interaction term is the opposite of the sign of the background variable. This finding is generally true for family income and parents' age. Along the urban/non-urban dimension, the picture that Tables 4 to 6 provides is more mixed, because urbanity is linked with income.[66] Using a specification that focuses on the poorest income quartile, I find that the gap between urban and non-urban women is diminished because of the reform. On the other hand, the reform reinforced differences in fertility patterns according to the mother's level of education.

The finding that daughters of the most educated women respond so strongly to the reform is somewhat surprising, because one would think that girls from such families would be strongly encouraged to have an education at any rate, and that they would be less credit constrained than other groups. I interpret this result as an indication that the more educated mothers are, the more receptive they are to the general message of the reform: namely, that education is important for everybody. Through their own education or later career, these mothers may have become more oriented towards modern ideas. The reform is exogenous to marital ability, so if the daughters of well-educated women respond differently to the reform, it must be because of environmental factors, e.g., values and norms in their upbringing that correspond particularly well with the signal that the reform brings. Well-educated women are likely to advocate education for their daughters in general, and the educational reform seems to have helped stimulate their daughters further into postponing childbirth.

A clear result is that the reform had the greatest impact on women from low-income families. These individuals could be credit constrained or lack other resources at home, including stimulation, norms and role models that encouraged them to have an education beyond compulsory schooling or kept them from activities connected with a high risk of teenage motherhood. The estimated difference in the effect of the reform is quite dramatic:

[66] According to Tables 5 and 6, the difference between urban and non-urban women diminishes with the reform, whereas Table 4 provides the opposite picture. The result in Table 4 may be explained as follows: as poor women benefited most from the reform, but urban women are underrepresented within the lowest income quartile, the overall effect of the reform, as measured across all income quartiles, is to widen the gap between urban and non-urban women. Given that the main distinction in terms of fertility is between the lowest and the other income quartiles, the results in Tables 5 and 6 are far more interesting than those in Table 4.
In the estimation that Tables 5 and 6 are based upon, the effect of the urban variable itself is positive, whereas in Table 4 it is negative. This difference in signs stems from different ways of specifying the family income variable. It suggests that there may be different effects of being in the lowest income quartile (defined on a national basis) in a city than in a non-urban community. To avoid making the analysis too complex, I have not included interaction terms between urbanity and income.

while the probability of teenage motherhood is unchanged or slightly increased in the three upper income quartiles, it falls by 12 percentage points in the bottom income quartile, see Table 5. The change among the poorest is particularly strong in the larger cities (20 percentage points, as compared to 11 percentage points in rural municipalities or small towns, see Table 6). One possible explanation is that urban families who are poor compared to the national standard are relatively poorer than non-urban families, because the overall income level is higher in the major cities. Thus, poor urban families are negatively selected, and the reform has a stronger impact on young women's behaviour.

The reform lead both urban and non-urban women to postpone childbirth past the age of 25 years. This tendency cannot be interpreted as an "incarceration effect". According to human capital theory, it may be explained by the greater investment in women's education and the higher opportunity cost of her time (Monstad *et al.*, 2007). One possible reason why poor urban women react strongest to the reform could be that two additional years of compulsory schooling yields a higher return in a city because of the better labour market for women. Secondary and higher education is also generally more easily available in the cities. If the reform spurred some women into desiring further education, the lower cost of education in the cities could play a greater role after the reform than before.

3.5.2 Results of the spillover effect analysis

The direct effect of the reform, on the person exposed to it, is to decrease the likelihood of teenage motherhood, confer Table 3. The spillover effect measured by the interaction term δ_2 in eq. (3) must be interpreted as an additional effect of the reform, which may reinforce or weaken the negative effect.

The descriptive data indicate that age difference may be important in the analysis of spillover effects. One obvious reason is that the strength of a potential spillover effect could fade with the growing age difference; the closer in age sisters are, the more likely they are to share experiences, interests, friends, etc. Another reason is created by the natural experiment at hand, as the reform was implemented gradually. Trends in fertility behaviour also affect the elder sisters, and may have an impact on how they behave as role models. There are two similar ways of correcting for these trends: through an age difference variable as in eq. (3) or through indicators for the elder sister's cohort. In Table 7, four different models have been estimated.[67]

[67] All four estimations confirm the results previously displayed in Tables 3 and 4 that the reform reduces the probability of teenage motherhood for the mean individual.

For comparison, I have estimated eq. (3) without background variables (see the specifications labelled (I) and (II) in Table 7). The results for the variable of interest, the spillover effect, demonstrate that it can be important to control for age difference. In the model without an age difference variable, the spillover effect is positive and even statistically significant. The sign of the spillover effect turns negative once we control for age difference, which is what we should expect. That is, having an elder sister who has been mandated more education should set up a role model that makes younger sisters less inclined to become teenage mothers. However, the magnitude of the estimated effect is small, and the spillover effect is not statistically significant. In the specifications labelled (III) and (IV), I control for background variables as well. The main result is the same; the spillover effect is negative but statistically insignificant. A more complete picture of the estimation of eq. (3) is presented in Table 5 in the Appendix.[68]

3.6 Conclusion

In an earlier study, Monstad *et al.* (2007) found that a reform that enhanced mandatory education in Norway lead to the postponement of first births. In this paper, I examine to what extent it applies for different socio-economic groups, examining fertility over the whole of the women's fertile period. I also investigate whether an elder sister's reform status has any spillover effect on the younger sister's propensity to become a teenage mother.

Family background proves to be an important causal determinant for fertility behaviour in general, but also for the effect of educational reform on fertility. The analysis shows much heterogeneity in response to educational policy. In particular, the effect depends on family income and whether the young woman lives in a city. The heterogeneity in response is especially strong regarding the likelihood of first birth as a teenager. The group that responded to the reform most strongly in terms of delaying first birth consists of women from low-income families living in cities. These women also show an increase in the tendency to remain childless. However, the effect of family background does not seem to incorporate spillover effects of the reform from elder to younger sisters within the same family. The

[68] The table shows the partial effects of the background variables from eq. (3). These vary somewhat from the estimation without spillover effects, i.e., eq. (1). The decrease in teenage motherhood due to the reform is still greatest for women from low income families and those with young fathers. The interaction terms with urbanity and mother's education are no longer statistically significant. It should be kept in mind that the estimation is undertaken with a much smaller subsample, and that a relatively small proportion is classified as "urban" or have mothers with the highest level of education (13.8% and 2.4%, respectively).

spillover effect of the reform is estimated to have the expected sign (to reduce teenage motherhood), but it is small and statistically insignificant.

One of the main goals of the reform was to enhance the equality of opportunity along socio-economic and geographic dimensions. There was no objective stated with respect to differences in fertility patterns between socio-economic groups. Still, it is worth noting that as a consequence of the reform, the timing of first births and especially the frequency of teenage motherhood became more similar among the different income groups. Along the urban/non-urban dimension, the picture is more mixed. Using a specification that focuses on the poorest income quartile, I find that the gap between urban and non-urban women is diminished because of the reform.

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Tables

Table 1. Data selection process

	Number of observations
Women born 1947–1958, in total	384385
Missing on cohort member's characteristics, or excluded:	
Excluded because motherhood before age 15 years	101
Excluded because woman's education is less than 7 years	783
Missing on municipality	78952
Missing on reform indicator	11841
Missing on woman's length of education	2104
	290604
Missing on background variables:	
Missing on father's education	7251
Missing on mother's education	239
Missing on mother's age	4029
Missing on father's age	2348
Missing on family income	2156
Sample size heterogeneity sample	274581
Subsample used in spillover effect analysis:	
Missing on mother's identification code	46433
The woman has no sister in the sample	136459
Dropped because is part of a group of triples	12
Sample of sisters	91677
The woman is the elder sister in the family, within the sample	43100
Dropped because less than 9 months interval between sisters' births	3
Sample of younger sisters used in spillover effect analysis	48574

Table 2a. Summary statistics, by reform indicator

	reform=0				reform=1			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Years of education	11.26	2.66	7	21	11.74	2.47	7	21
Municipality	1018.9	611.61	101	2030	997.7	580.23	101	2030
Reform	0	0	0	0	1	0	1	1
Year of birth	1950.7	2.57	1947	1958	1955.1	2.42	1947	1958
Background variables:								
1 if lived in one of the 10 major cities	0.15	0.35	0	1	0.22	0.42	0	1
Mother's education, years	7.94	1.64	7	18	8.19	1.78	7	18
1 if mother's education is 7 years	0.69	0.46	0	1	0.61	0.49	0	1
1 if 7<mother's education<= 12 years	0.29	0.45	0	1	0.36	0.48	0	1
1 if mother's education >12 years	0.02	0.14	0	1	0.03	0.16	0	1
Mother's age when daughter born	29.63	6.13	7	81	29.12	6.24	12	77
Mother's age in 1960	38.97	6.74	19	89	34.03	6.76	18	83
Father's education, years	8.65	2.53	7	18	8.97	2.65	7	18
1 if father's education is 7 years	0.58	0.49	0	1	0.51	0.50	0	1
1 if 7<father's education <= 12 years	0.33	0.47	0	1	0.38	0.49	0	1
1 if father's education >12 years	0.08	0.28	0	1	0.10	0.30	0	1
Father's age when daughter was born	33.22	7.03	0	86	32.63	7.02	1	87
Father's age in 1960	42.57	7.53	7	90	37.54	7.46	12	90
Family income in 1970, 100 NOK	260.43	286.58	0	14439	382.83	253.22	0	14058
Outcome variables:								
1 if childless	0.10	0.30	0	1	0.11	0.31	0	1
1 if first birth at age 15–20	0.16	0.37	0	1	0.17	0.37	0	1
1 if first birth at age 20–25	0.42	0.49	0	1	0.36	0.48	0	1
1 if first birth at age 25–30	0.22	0.41	0	1	0.24	0.42	0	1
1 if first birth at age 30–35	0.07	0.25	0	1	0.09	0.28	0	1
1 if first birth at age 35–40	0.02	0.15	0	1	0.03	0.17	0	1
N	*127733*				*146848*			

Table 2b. Summary statistics for subsample
Younger sisters used in the estimation of the spillover effect

	Younger sister non-reform elder sister non-reform (Group C)			Younger sister reform elder sister non-reform (Group B)			Younger sister reform elder sister reform (Group A)		
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Expl. variables, younger sister:									
Years of education	12825	11.30	2.63	16721	11.70	2.48	19028	11.63	2.41
Municipality	12825	1118.41	597.51	16721	1100.69	603.88	19028	1048.04	567.58
Reform	12825	0.00	0.00	16721	1.00	0.00	19028	1.00	0.00
Year of birth	12825	1952.84	2.16	16721	1955.49	1.99	19028	1956.20	1.78
No. of sisters in family	12825	2.43	0.66	16721	2.27	0.54	19028	2.34	0.62
Age at first birth	11577	24.25	4.87	14967	24.55	5.03	16998	24.52	5.06
1 if first birth at age 15–20	12825	0.17	0.38	16721	0.17	0.38	19028	0.17	0.38
Information on elder sister:									
Age difference between sisters	12825	2.92	1.54	16721	4.49	2.16	19028	2.79	1.47
Year of birth	12825	1949.97	2.17	16721	1950.98	2.28	19028	1953.46	2.09
Age at first birth	11481	24.25	4.50	15033	24.24	4.61	17125	24.09	4.74
1 if first birth at age 15–20	12825	0.14	0.35	16721	0.15	0.36	19028	0.18	0.38

Table 3. Results benchmark model, without interaction terms

Explanatory variables	First birth age 15–20	First birth age 20–25	First birth age 25–30	First birth age 30–35	First birth age 35–40	Childless
Length of education, OLS	–0.032 ***	–0.024 ***	0.030 ***	0.015 ***	0.005 ***	0.006 ***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)
Reform, OLS	–0.009 **	0.005	0.002	–0.001	0.002 **	0.001
	(0.005)	(0.004)	(0.003)	(0.002)	(0.001)	(0.002)
Reform, probit	–0.008 **	0.005	0.002	–0.002	0.002 **	0.001
	(0.004)	(0.004)	(0.003)	(0.002)	(0.001)	(0.002)
n in probit model	*274581*	*274581*	*274581*	*274570*	*272838*	*274574*

Single, double, and triple asterisks indicate significant coefficients at the 10%, 5% and 1% levels, respectively. The table shows estimated coefficients from OLS estimations and marginal effects from probit estimations, confer eq. (2). Each column denotes separate regressions. Also included in the specifications are municipality and year-of-birth indicators. Standard errors are adjusted for clustering at the municipality level.

Table 4. Effects of reform, controlling for observed heterogeneity

	First birth aged 15-20		First birth aged 20-25		First birth aged 25-30		First birth aged 30-35		First birth aged 35-40		Being childless	
	Partial effects		Partial effects		Partial effects		Partial effects		Partial effects		Partial effects	
Reform	–0.052	***	–0.040	***	0.040	***	0.013	***	0.007	***	0.033	***
	(0.008)		(0.010)		(0.008		(0.004)		(0.002)		(0.005)	
Background variables:												
Urban	–0.024	***	–0.011	***	0.024	***	0.018	***	–0.003	***	–0.013	***
	(0.003)		(0.003)		(0.002)		(0.001)		(0.001)		(0.002)	
Family income, 2nd quartile	–0.169	***	–0.044	***	0.169	***	0.059	***	0.017	***	0.086	***
	(0.003)		(0.009)		(0.006)		(0.004)		(0.002)		(0.004)	
Family income, 3rd quartile	–0.177	***	–0.042	***	0.195	***	0.057	***	0.019	***	0.072	***
	(0.004)		(0.010)		(0.006)		(0.004)		(0.002)		(0.006)	
Family income, 4th quartile	–0.188	***	–0.073	***	0.206	***	0.068	***	0.022	***	0.073	***
	(0.006)		(0.013)		(0.006)		(0.004)		(0.002)		(0.004)	
Mother's age, 1st quartile	0.065	***	0.042	***	–0.048	***	–0.023	***	–0.009	***	–0.030	***
	(0.005)		(0.006)		(0.005)		(0.003)		(0.002)		(0.004)	
Mother's age, 2nd quartile	0.033	***	0.023	***	–0.022	***	–0.009	***	–0.005	***	–0.017	***
	(0.004)		(0.004)		(0.004)		(0.003)		(0.001)		(0.003)	
Mother's age, 3rd quartile	0.017	***	0.016	***	–0.011	***	–0.003		–0.003	**	–0.014	***
	(0.003)		(0.004)		(0.003)		(0.002)		(0.001)		(0.002)	
Father's age, 1st quartile	0.068	***	0.031	***	–0.037	***	–0.019	***	–0.006	***	–0.031	***
	(0.006)		(0.007)		(0.005)		(0.003)		(0.002)		(0.004)	
Father's age, 2nd quartile	0.035	***	0.030	***	–0.022	***	–0.013	***	–0.004	***	–0.020	***
	(0.004)		(0.006)		(0.004)		(0.002)		(0.001)		(0.003)	
Father's age, 3rd quartile	0.025	***	0.014	***	–0.015	***	–0.008	***	–0.003	**	–0.012	***
	(0.003)		(0.004)		(0.003)		(0.002)		(0.001)		(0.002)	
Father's education,8-12 years	–0.046	***	–0.025	***	0.041	***	0.016	***	0.006	***	0.005	**
	(0.002)		(0.003)		(0.003)		(0.002)		(0.001)		(0.002)	
Mother's education> 12 years	–0.067	***	–0.099	***	0.066	***	0.032	***	0.008	***	0.015	**
	(0.008)		(0.010)		(0.012)		(0.005)		(0.004)		(0.006)	
Father's education, 8-12 years	–0.041	***	–0.019	***	0.042	***	0.016	***	0.006	***	0.005	***
	(0.002)		(0.002)		(0.002)		(0.001)		(0.001)		(0.002)	
Father's education > 12 years	–0.082	***	–0.091	***	0.075	***	0.042	***	0.014	***	0.021	***
	(0.002)		(0.005)		(0.005)		(0.003)		(0.001)		(0.004)	

(The table continues on the next page)

Table 4. Effects of reform, controlling for observed heterogeneity, cont.

	First birth aged 15-20 Partial effects		**First birth aged 20-25** Partial effects		**First birth aged 25-30** Partial effects		**First birth aged 30-35** Partial effects		**First birth aged 35-40** Partial effects		**Being childless** Partial effects	
Interaction terms:												
Urban	–0.033	***	–0.003		0.013	***	0.006	*	0.000		0.014	***
	(0.009)		(0.009)		(0.005)		(0.004)		(0.001)		(0.004)	
Family income, 2nd quartile	0.146	***	0.077	***	–0.076	***	–0.029	***	–0.008	***	–0.052	***
	(0.016)		(0.013)		(0.008)		(0.003)		(0.002)		(0.004)	
Family income, 3rd quartile	0.139	***	0.075	***	–0.080	***	–0.023	***	–0.008	***	–0.046	***
	(0.018)		(0.013)		(0.009)		(0.003)		(0.002)		(0.006)	
Family income, 4th income	0.124	***	0.093	***	–0.067	***	–0.022	***	–0.007	***	–0.048	***
	(0.022)		(0.016)		(0.010)		(0.003)		(0.002)		(0.005)	
Mother's age, 1st quartile	–0.019	***	–0.008		0.033	***	0.003		0.000		0.005	
	(0.006)		(0.008)		(0.008)		(0.005)		(0.003)		(0.005)	
Mother's age, 2nd quartile	–0.019	***	–0.013	**	0.023	***	0.001		0.001		0.004	
	(0.005)		(0.007)		(0.006)		(0.004)		(0.002)		(0.005)	
Mother's age, 3rd quartile	–0.011	***	–0.005		0.010	*	0.000		–0.001		0.005	
	(0.004)		(0.006)		(0.006)		(0.003)		(0.002)		(0.004)	
Father's age, 1st quartile	–0.023	***	–0.008		0.014	**	0.009	*	0.004		0.012	**
	(0.006)		(0.009)		(0.007)		(0.005)		(0.003)		(0.005)	
Father's age, 2nd quartile	–0.017	***	–0.019	**	0.015	**	0.009	**	0.002		0.008	*
	(0.006)		(0.008)		(0.006)		(0.004)		(0.002)		(0.005)	
Father's age, 3rd quartile	–0.017	***	–0.009		0.013	**	0.003		0.003	*	0.006	*
	(0.005)		(0.006)		(0.006)		(0.003)		(0.002)		(0.004)	
Mother's education,8–12 years	–0.006	**	–0.004		–0.001		0.003		0.001		0.004	*
	(0.003)		(0.004)		(0.004)		(0.002)		(0.001)		(0.002)	
Mother's education > 12 years	–0.037	***	–0.028	**	0.006		0.007		0.005		0.007	
	(0.011)		(0.013)		(0.011)		(0.009)		(0.004)		(0.007)	
Father's education, 8-12 years	#		#		#		#		#		#	
Father's education > 12 years	#		#		#		#		#		#	
N	*274581*		*274581*		*274581*		*274570*		*272838*		*274574*	
Pseudo_R2	*0.11*		*0.02*		*0.04*		*0.04*		*0.03*		*0.02*	

= dropped due to collinearity.

Single, double, and triple asterisks indicate significant coefficients at the 10%, 5% and 1% levels, respectively. The table shows marginal effects from probit estimations, confer eq. (1). Each column denotes separate regressions. Also included in the specifications are municipality and year-of-birth indicators. Standard errors are adjusted for clustering at the municipality level, and are available from the author.

Table 5. Heterogeneity in the response to the reform

Change in probabilities due to the educational reform. Timing of first birth and childlessness

	First birth at age 15-20			First birth at age 20-25			First birth at age 25-30			First birth at age 30-35			First birth at age 35-40			Being childless		
	Reform	Non-reform	Effect of reform	Reform	Non-reform	Effect of reform	Reform	Non-reform	Effect of reform	Reform	Non-reform	Effect of reform	Reform	Non-reform	Effect of reform	Reform	Non-reform	Effect of reform
Family income:																		
Bottom quartile	0.26	0.39	-0.12	0.37	0.42	-0.05	0.15	0.11	0.04	0.05	0.04	0.01	0.02	0.01	0.01	0.09	0.06	0.03
Above bottom quartile	0.11	0.08	0.03	0.40	0.37	0.03	0.24	0.26	-0.02	0.08	0.09	-0.01	0.03	0.03	0.00	0.10	0.12	-0.02
			-0.16			-0.08			0.07			0.02			0.01			0.05
Living in a major city:																		
Urban	0.12	0.18	-0.06	0.31	0.33	-0.01	0.25	0.22	0.04	0.10	0.09	0.02	0.02	0.02	0.00	0.12	0.09	0.03
Non-urban	0.11	0.12	-0.01	0.38	0.39	-0.01	0.23	0.22	0.02	0.07	0.07	0.00	0.03	0.02	0.00	0.12	0.10	0.02
			-0.05			0.00			0.02			0.01			0.00			0.01

Probabilities are computed after probit estimations, confer eq. (1). Family income is expressed through a dummy for whether or not the individual belonged to the lowest income quartile. When computing the probabilities, all variables except those specified in the table above (income, urbanity, reform) are kept at mean values. Also included in the specifications are each parent's age and level of education, as well as indicators for the woman's cohort and municipality. Standard errors are adjusted for clustering at the municipality level.

Table 6. Heterogeneity: income and urbanity combined
Change in probabilities due to reform. First birth at age 15–20

	Urban			Non-urban		
	Reform	Non-reform	Effect of reform	Reform	Non-reform	Effect of reform
Family income:						
Bottom quartile	0.27	0.47	–0.20	0.26	0.37	–0.11
Above bottom quartile	0.12	0.12	0.00	0.11	0.07	0.03
			–0.20			–0.15

Probabilities are computed after probit estimations, confer eq. (1). Family income is expressed through a dummy for whether or not the individual belonged to the lowest income quartile. When computing the probabilities, all variables except the specified (income, urbanity, reform) are kept at mean values. Also included in the specifications are each parent's age and level of education, as well as indicators for the woman's cohort and municipality. Standard errors are adjusted for clustering at the municipality level.

Table 7. Results, spillover effects among sisters
on probability of teenage motherhood

	Without background variables		With background variables	
	(I)	(II)	(III)	(IV)
First birth at age 15–20	partial effect	partial effect	partial effect	partial effect
Reform	–0.013 *	–0.014 **	–0.054 ***	–0.054 ***
	(0.007)	(0.007)	(0.016)	(0.016)
1 if sister impacted by reform	0.012 *	–0.004	–0.006	–0.005
	(0.007)	(0.008)	(0.006)	(0.007)
Age difference between sisters		–0.006 ***		0.000
		(0.001)		(0.001)
N	*48358*	*48358*	*48358*	*48358*
Observed P	*0.174*	*0.174*	*0.174*	*0.174*
Predicted P	*0.163*	*0.162*	*0.146*	*0.146*

Single, double, and triple asterisks indicate significant coefficients at the 10%, 5% and 1% levels, respectively. The estimates show partial effects from probit models. Four different specifications have been used, which all relate to eq. (3): in (I) and (II), all arguments in the *Z* vector are set equal to zero, while the background variables *Z* are included in (III) and (IV). In specifications (I) and (III), the age difference variable *D* is omitted. Also included in each specification are municipality and year-of-birth indicators related to the younger sisters. Standard errors are adjusted for clustering at the municipality level.

Appendix

App. Table 1. Correlations

	Years of education	Reform	Year of birth	Urban	Mother's education, years	Mother's age	Father's education, years	Father's age	Family income	Mother's income	Father's income
Years of education	1.00										
Reform	0.09	1.00									
Year of birth	0.10	0.66	1.00								
Urban	0.08	0.09	–0.03	1.00							
Mother's education, years	0.37	0.07	0.08	0.15	1.00						
Mother's age	–0.01	–0.34	–0.51	0.03	–0.04	1.00					
Father's education, years	0.40	0.06	0.05	0.19	0.56	–0.01	1.00				
Father's age	–0.02	–0.32	–0.46	–0.01	–0.06	0.82	–0.03	1.00			
Family income	0.30	0.22	0.29	0.18	0.38	–0.20	0.41	–0.22	1.00		
Mother's income	0.13	0.07	0.07	0.14	0.26	–0.05	0.12	–0.05	0.41	1.00	
Father's income	0.28	0.22	0.29	0.14	0.32	–0.20	0.41	–0.23	0.94	0.09	1.00

In the table, "years of education" and "year of birth" refer to the 1947 to 1958 cohort member (n = 274,581).

App. Table 2. Summary statistics for the sister population

	Population of sisters			The whole sample		
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Explanatory variables:						
Years of education	91677	11.58	2.59	274581	11.52	2.57
Municipality	91677	1074.91	589.02	274581	1007.61	595.13
Reform	91677	0.57	0.50	274581	0.53	0.50
Year of birth	91677	1953.41	3.08	274581	1953.03	3.33
1 if born in 1958	91677	0.10	0.30	274581	0.10	0.29
1 if born in 1957	91677	0.10	0.29	274581	0.10	0.29
1 if born in 1956	91677	0.11	0.31	274581	0.10	0.30
1 if born in 1955	91677	0.11	0.31	274581	0.10	0.29
1 if born in 1954	91677	0.11	0.31	274581	0.09	0.29
1 if born in 1953	91677	0.11	0.31	274581	0.09	0.29
1 if born in 1952	91677	0.10	0.30	274581	0.09	0.28
1 if born in 1951	91677	0.08	0.27	274581	0.08	0.27
1 if born in 1950	91677	0.07	0.25	274581	0.07	0.26
1 if born in 1949	91677	0.05	0.23	274581	0.07	0.25
1 if born in 1948	91677	0.04	0.20	274581	0.06	0.24
1 if born in 1947	91677	0.04	0.18	274581	0.06	0.24
1 if lived in a major city	91677	0.14	0.35	274581	0.19	0.39
Outcome variables:						
1 if childless	91677	0.10	0.30	274581	0.11	0.31
1 if first birth at age 15–20	91677	0.16	0.37	274581	0.16	0.37
1 if first birth at age 20–25	91677	0.39	0.49	274581	0.39	0.49
1 if first birth at age 25–30	91677	0.23	0.42	274581	0.23	0.42
1 if first birth at age 30–35	91677	0.08	0.27	274581	0.08	0.27
1 if first birth at age 35–40	91677	0.03	0.16	274581	0.03	0.16

App. Table 3. Teenage motherhood by cohort

	Sister population (n = 91,677) Unconditioned			**The whole sample** (n = 274,581) Unconditioned		
Cohort	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
1947	3209	0.11	0.31	16156	0.14	0.35
1948	3954	0.12	0.33	17338	0.14	0.35
1949	4907	0.11	0.32	18499	0.14	0.34
1950	6310	0.11	0.31	20389	0.15	0.35
1951	7311	0.12	0.32	21552	0.16	0.37
1952	8860	0.16	0.37	24125	0.18	0.38
1953	9921	0.19	0.39	25667	0.19	0.39
1954	9940	0.20	0.40	25754	0.19	0.40
1955	9889	0.20	0.40	26091	0.18	0.39
1956	9721	0.18	0.39	26597	0.17	0.38
1957	8806	0.17	0.37	26179	0.15	0.36
1958	8849	0.16	0.37	26234	0.15	0.36

App. Table 4. Rank (reversed birth order)

Sister population (n = 91,677)

Rank within sisters in the family (1 = youngest)	Freq.	Per cent
1	43419	47.4
2	41197	44.9
3	6159	6.7
4	801	0.9
5	92	0.1
6	8	0.0
7	1	0.0

App. Table 5. Results, spillover effects among sisters

Teenage motherhood

	Without age difference:		With age difference:	
First birth at age 15–20	Partial effect	P>|z|	Partial effect	P>|z|
Reform	–0.054	0.00	–0.054	0.00
Age difference between sisters			0.000	0.72
Background variables:				
Urban	–0.008	0.56	–0.008	0.54
Family income, 2nd quartile	–0.139	0.00	–0.139	0.00
Family income, 3rd quartile	–0.144	0.00	–0.144	0.00
Family income, 4th quartile	–0.147	0.00	–0.147	0.00
Mother's age, 1st quartile	0.089	0.00	0.090	0.00
Mother's age, 2nd quartile	0.043	0.00	0.043	0.00
Mother's age, 3rd quartile	0.031	0.00	0.031	0.00
Father's age, 1st quartile	0.071	0.00	0.071	0.00
Father's age, 2nd quartile	0.045	0.00	0.046	0.00
Father's age, 3rd quartile	0.031	0.00	0.031	0.00
Mother's education, 8–12 years	–0.058	0.00	–0.058	0.00
Mother's education > 12 years	–0.082	0.00	–0.082	0.00
Father's education, 8–12 years	–0.044	0.00	–0.044	0.00
Father's education > 12 years	–0.095	0.00	–0.095	0.00
Interaction terms:				
1 if sister impacted by reform	–0.006	0.33	–0.005	0.48
Urban	0.003	0.87	0.003	0.88
Family income, 2nd quartile	0.114	0.00	0.114	0.00
Family income, 3rd quartile	0.115	0.00	0.115	0.00
Family income, 4th quartile	0.066	0.00	0.066	0.00
Mother's age, 1st quartile	–0.016	0.31	–0.016	0.31
Mother's age, 2nd quartile	–0.013	0.33	–0.013	0.33
Mother's age, 3rd quartile	–0.020	0.11	–0.020	0.11
Father's age, 1st quartile	–0.028	0.06	–0.028	0.06
Father's age, 2nd quartile	–0.023	0.05	–0.023	0.05
Father's age, 3rd quartile	–0.022	0.05	–0.022	0.05
Mother's education, 8–12 years	0.002	0.87	0.002	0.87
Mother's education > 12 years	–0.028	0.41	–0.029	0.41
N	*48358*		*48358*	
Observed P	*0.174*		*0.174*	
Predicted P	*0.146*		*0.146*	

The estimates show partial effects from probit models. The table reports results from two different specifications, which both relate to eq. (3), but in the second column the age difference variable *D* is left out. Also included in each specification are municipality and year-of-birth indicators related to the younger sisters. Standard errors are adjusted for clustering at the municipality level.