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ESTIMATING LONG-TERM CONSEQUENCES OF TEENAGE CHILDBEARING – AN EXAMINATION OF THE SIBLINGS APPROACH

by

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Estimating Long-Term Consequences of Teenage Childbearing – An Examination of the Siblings Approach[*]

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Abstract

One of the remedies to selection bias in estimates of the labour market consequences of teenage motherhood has been to estimate within-family effects. A major critique, however, is that heterogeneity within the family might still bias the estimates. Using a large Swedish data set on biological sisters, I revisit the question of the consequences of teenage motherhood. My contribution is that I am able to control for heterogeneity within the family; I use grade-point-averages at age 16, a pre-motherhood characteristic that differs across sisters within the same family. My findings confirm the presumption that within-family heterogeneity can result in biased within-family estimates. Moreover, my results show that when controlling for school performance, the siblings approach and a traditional cross section yield similar coefficients.

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1. Introduction

Teenage childbearing is widely recognized as a cause of poor labour market outcomes for mothers, adverse outcomes for children, and as a cost to society at large. Teenage mothers have on average lower income and lower educational levels than non-teen mothers, and children born to teen mothers are found to have worse short- and long-term outcomes than children to women who postpone motherhood (Angrist and Lavy 1996, Bronars and Groggers 1993, 1994, Card and Wise 1978, Geronimus and Korenman 1992, Hoffman et al. 1993, Otterblad Olausson et al. 2001).

The view that teenage childbearing is costly to individuals and to society is (probably) most frequently advocated in countries where fertility among teenagers is high, as in the U.S. and the U.K. In both these countries, teenage fertility declined in the 1970s but started to rise again in the late 1980s. In 1995, the level of teenage fertility in the U.S. was back at around 60 births per 1,000 women - the same level as in the early 1970s. In contrast, teenage fertility continued to decline throughout the 1980s in European countries apart from the U.K. Most striking is the pattern for Sweden, Denmark and Norway - countries that all started off at a level of teenage fertility similar to that of the U.K. in the mid 1960s (around 50 births per 1,000 women) - but where the decline continued also throughout the 1980s. In 1995, the level of teenage fertility had declined to a moderate 10 births per 1,000 women, compared to 30 births per 1,000 women in the U.K. (Santow and Bracher 1999).

Sweden's sharp decline in teenage childbearing has also been accompanied by a trend towards higher age at first birth. In 2002, age at first birth was on average 28.5, compared to 23.5 in 1970 (Statistics Sweden, 2003). This is further evidence that young motherhood has increasingly become a rare phenomenon in Sweden, which could possibly affect both the social stigma attached to it, and also its labour market consequences. Moreover, if there is a causal link between teen childbearing and poor outcomes, the Swedish decline in teenage motherhood should have been beneficial to mothers and children in terms of quantifiable outcomes, but the question of causality still remains an intriguing research topic.

Previous research on outcomes of teen mothers has recognized the difficulty in isolating the true causal effect, one obstacle being that researchers have to rely on non-experimental methods that are prone to selection bias. Teenage mothers typically have disadvantaged backgrounds and other pre-motherhood characteristics that would predict poor outcomes also in the absence of a child at early age. Even when including parental education and income, traditional cross-sectional estimates fail to fully control for family background. To be able to assess the question of causality, the researcher would need information on, for example, family values, upbringing, and the parents' ambitions for their children. Clearly, this information is not available and requires adoption of an alternative identification strategy.

Sibling-differences control for unobserved family characteristics shared by sisters (Bennett et al. 1995, Geronimus and Korenman 1992, Hoffman et al. 1993). In general, these studies have the drawbacks of very small and unrepresentative samples. However, they find that the sister-differences approach reduces the estimated effects of teenage childbearing. Bronars and Groggers (1993, 1994) make use of twin births as a natural experiment, comparing outcomes of teenage or unwed mothers who had a twin birth with those who had a single birth. Their results show that women who had a twin birth come out worse on several indicators of economic status, but that this effect dissipates over time for white women. Klepinger et al. (1999) apply instrumental variables to remedy the endogeneity of fertility with respect to human capital accumulation, using state and county level indicators of abortion and family planning policies as instruments. Their findings support the notion that teenage childbearing substantially reduces future labour market outcomes for mothers. Hotz et al. (2002) use miscarriage as an instrument to study how delaying age at first birth affects socioeconomic status. Short-run effects of early motherhood are negative, but surprisingly, Hotz et al. (2002) find small negative and even positive long-run effects for some outcomes. More recently, Chevalier and Viitanen (2003) use propensity score matching, and find that teenage childbearing decreases the probability of post-compulsory education. Levine and Painter (2003) adopt within-school propensity score matching; that is, they match teen mothers to similar individuals attending the same school. They find negative effects on education (measured at age 20) of teenage out-of-wedlock fertility.

Ribar (1999) reconciles previous evidence on the effects of teenage motherhood, presenting both OLS, within-family and instrumental variables estimates. He concludes that under plausible assumptions on the correlation structure of the unobservable determinants of fertility and the outcome variable, the siblings approach is informative since it restricts the range of possible estimates by providing a lower bound.

Further references on the topic of teenage childbearing include for example Ermisch and Pevalin (2003), Olsen and Farkas (1989), Ribar (1994) and Rosenzweig and Wolpin (1995).[1]

Since previous studies report mixed evidence when it comes to the causal effect of teen motherhood on labour market outcomes, it is highly interesting to further investigate the question of causality vs. selection bias. In this paper I study the consequences of teenage motherhood using a large Swedish data set consisting of a 20 per cent random sample of each cohort born in Sweden 1974-1977. In addition, siblings of the individuals in the random sample have been identified, which allows me to adopt a siblings approach to control for unobserved family background by comparing sisters. To be specific, I compare teenage mothers with their sisters who had their first child after their teens or have no child at all, using within-family estimates. The underlying assumption when using this method is that births are random to sisters within the same family, i.e., that there is no unobserved heterogeneity within the family. Thus, differences across siblings that are related to both outcomes and the propensity to become a teenage mother could bias the estimates. Unlike previous studies using the siblings approach, I am able to control for one major source of heterogeneity between sisters. I use grade-point-averages from primary school graduation – a variable that is likely to be correlated both with future outcomes and the propensity to become a teenage mother.[2] Therefore, this study is able to assess the question of selection bias in within-family estimates, and to evaluate the reliability of such estimates. I will also contrast the within-family estimates to traditional cross-sectional estimates.

My findings show that heterogeneity within the family indeed is a concern in sibling models. When I control for grade-point-averages and a few other between-sister variables, the point estimate of the effect of teenage motherhood on years of education is reduced from -0.93 to -0.59; this reduction being both statistically and economically significant. Moreover, when controlling for grade-point-averages, the siblings approach and a traditional cross section yield similar coefficients.

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[1] It is noteworthy that the American literature does not always distinguish between teenage motherhood and teenage out-of-wedlock births. In Sweden, out-of-wedlock births are not as strongly associated with poverty and welfare dependency as in the U.S.; many births occur out-of-wedlock but within consensual unions. Therefore, when studying adolescent fertility in Sweden, it is teenage motherhood rather than teenage out-of-wedlock births that should be the main focus.

[2] The above mentioned studies that adopt propensity score matching (Chevalier and Viitanen 2003, Levine and Painter 2003) both match on pre-motherhood test scores. To the best of my knowledge, no one has used a measure of school performance when adopting the siblings approach.

The remainder of this paper is organized as follows: Section 2 presents the econometric model, Section 3 describes the data set, Section 4 reports summary statistics and findings, in Section 5 I test the robustness of the results to alternative specifications and Section 6 concludes.

2. Econometric model

The following model embodies the ideas of the siblings approach:

$$y_{ij} = \alpha + \beta T_{ij} + \mathbf{X_{ij}}'\boldsymbol{\gamma} + f_j + u_{ij}$$

where y_{ij} is the outcome of individual *i* in family *j*, T_{ij} is an indicator variable which takes the value one if the individual is a teenage mother and zero otherwise, $\mathbf{X_{ij}}$ is a set of control variables, f_j is an unobserved family component, common to all siblings within the same family and u_{ij} is the error term. By differencing/demeaning over siblings, the unobserved family effect f_j is removed from the equation, as is any bias caused by unobservables common to all siblings. This specification, known as family fixed effects or the within-family estimator, requires that strict exogeneity holds within each family. In the context of teenage motherhood, the strict exogeneity assumption requires that teen births are random to sisters, conditional on $\mathbf{X}$.

Compared to the cross-sectional estimator, the family fixed-effect estimator has its merit from reducing selection bias from unobservable family-specific variables. However, this does not mean that the family fixed-effects estimator is unbiased or even less biased than the cross-sectional estimator.

First, consider the assumption of strict exogeneity within the family, which implies that teenage births should be random to sisters, conditional on $\mathbf{X}$. Clearly, sisters differ in many respects, and these differences may be correlated with both the propensity to become a teenage mother and the labour market outcome variable. For example, consider a pre-motherhood characteristic such as school performance. It is reasonable to assume that the correlation between school performance and labour market outcome is positive, while the correlation between school performance and teenage motherhood is negative. Failing to control for school performance (or any other variable with a similar correlation structure) will cause a downward bias of the effect of teenage motherhood on labour market outcomes, i.e., the estimated coefficient will be more negative than the true coefficient.

Previous studies have not been able to control for heterogeneity within the family to any larger extent. For example, Geronimus and Korenman (1992) control for age and include a dummy variable indicating rural/urban residence. In this study I test the assumption of strict exogeneity within the family by adding several controls that have not previously been used in within-family estimates of the effect of teenage motherhood on educational outcomes. Most importantly, I control for pre-motherhood school performance.

Second, as is shown by Griliches (1979) and Bound and Solon (1999), it is not always the case that the bias in the within-family estimator is smaller than the bias in the cross-sectional estimator. The bias in the within-family estimator is smaller than the bias in the cross-sectional estimator under the following condition: the common family component should account for a larger fraction of the variance in those unobservables correlated with both fertility and the outcome (e.g. school performance), than in other unobservables affecting the outcome only indirectly through the fertility variable. Otherwise stated, the within-family estimator is less biased if f_i has a stronger correlation across siblings than has other unobserved determinants of adolescent fertility (Ribar 1999).

Finally, the within-family estimator exacerbates measurement errors, which is likely to bias the estimates towards zero. Thus, the smaller (in absolute value) within-family coefficient (compared to the cross-sectional coefficient) will be attributed to unobserved family background, while it is really caused by a bias towards zero (Griliches 1979). The siblings approach also has a drawback in that we must rely on a restrictive sample. For example, in this study only individuals with sisters enter the analysis, and only those sisters who differ in their timing on their first child will identify the coefficient of interest.

Previously it has been argued that if measurement error is less of a problem, both the within-family and the cross-sectional estimators are downward biased, since the unobserved heterogeneity is of the same type in the cross section as between siblings. Given that within-family coefficients often are less negative than the cross-sectional coefficients, and that both estimators are downward biased, the within-family coefficient provides a lower bound of the estimated coefficient of interest (Bound and Solon 1999, Ribar 1999).

3. Data

The data set is based on a 20 per cent random sample of each cohort born in Sweden 1974-1977.[3] Using a population register from Statistics Sweden, the biological parents and siblings of the individuals in the random sample have been identified. The children of the individuals in the random sample and their siblings have also been merged to the data set. Moreover, population censuses from 1975, 1980, and 1985 are used to retain information on resident parents and siblings, as well as family background information on parent's attained levels of education and father's earnings. Statistics Sweden's education register from 2002 has been used to obtain educational levels and years of schooling for the sampled individuals. Finally, register information on grades at graduation from compulsory school (at age 16) is merged to the individuals in the random sample and their siblings.[4]

For the purpose of this study, two different samples have been constructed; a random sample and a sister sample. The random sample used in this study is based on the 20 per cent random sample of cohorts born in Sweden 1974-1977 described above, but is restricted to include only women for whom we have sufficient background information. This leaves 33,626 individuals in the sample.[5] Of these 33,626 individuals, 927 (2.8 per cent) had a child as a teenager.

The sister sample used in the within-family estimates will consist of women in the random sample who have one or more full biological sisters born in 1972-1977, and their respective sisters. The sisters are allowed to be born no earlier than 1972, since this is the first cohort for which the grade register exists.[6] In addition, sisters are required to have lived

[3] The data is a sub-sample of a larger data set used for several research purposes. Examples of other studies using the same data are Björklund et al. (2003) and Hsieh and Lindahl (2003).

[4] The grade register is unique in the sense that it covers the whole population, starting with the cohort born in 1972. This enables us to study also rare phenomena such as teenage childbearing; if the grade information was available only for a sub-sample of the population we would possibly end up with too few observations identifying the parameter of interest.

[5] About 4,000 observations are lost due to missing observations (leaving 33,600 in the sample). A major concern is that these lost observations could introduce a bias due to non-random deletion. However, comparing the characteristics of the dropped observations with those of the remaining observations reveals small differences. Among the deleted observations, there are 5.7 per cent teenage mothers, compared to 2.8 per cent in the remaining sample. Average years of education is 12.98 in the remaining sample and 12.28 among the lost observations. The mean GPA is 3.34 in the random sample compared to 3.18 among the deleted observations. Finally, parental education seems to be somewhat higher among the lost observations than among the remaining ones, but father's annual earnings are higher in the remaining sample.

[6] Unfortunately, the random sample used in this study does not cover cohorts born before 1974, which explains why identified sisters are born 1972-1977, while individuals in the random sample are born 1974-1977.

in the same household in the population census of 1985, when they are aged 8 to 13. This is to ensure that the siblings actually share as much family background as possible. These restrictions limit the sister sample to 12,105 individuals. Of these 12,105 individuals, only pairs (or groups) of sisters where there is any within-family variation in the teenage motherhood variable, will identify the parameter of interest. In total, the 12,105 individuals form 5,948 different family-groups, and of these, 322 family-groups (consisting of 333 teen mothers and 340 sisters) identify the teen motherhood coefficient.[7] Note also that even if both sisters in a sister-pair occur in the random sample, they enter the sister sample only once.

The outcome variable for the sampled individuals and their sisters is years of education in 2002.[8] This means that years of education is measured when the individuals are 25 to 30 years old. Ideally, one would like to measure the outcome variable later than at age 25 to 30, but since the grade register exists only for the cohorts born in 1972 and onwards, it is not possible to use older cohorts. Appendix A reports how the education register of Statistics Sweden has been transformed to measure years of education.

In the baseline specifications, the independent variable of interest is a dummy variable indicating whether the woman had a child before the month she turned 20. In a sensitivity analysis I allow for different cut-off points when defining the teen mother variable.

In the cross-section analysis, parental background variables are mother's and father's years of schooling (reported in the census of 1990), and the logarithm of father's annual earnings from work in 1975 and 1985. Other control variables are whether the individual lived with both biological parents in the 1985 census (intact family), or with either only the mother or the father. Moreover, I control for year of birth, number of full siblings, number of half siblings and include dummy variables indicating whether the individual is the oldest sibling, a single child and whether she herself was born to a teenage mother. I also control for having children, which means that the effect of teenage motherhood will be evaluated comparing teenage mothers with women who have had a child after her teens.

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[7] The number of sisters exceeds the number of teen mothers since a few sister-groups appear in the data, meaning that a teen mother has more than one sister entering the analysis.

[8] Previous studies have used several different outcome variables. For example, Geronimus and Korenman (1992) look at family income and at several binary outcomes, e.g., whether on welfare or not, high school completion, whether employed and whether married. Ribar (1999) studies family income and also completed years of education.

In the within-family estimates, variables common to biological sisters will difference out of the model (for example parental education, father's earnings and number of siblings). For both the cross-section and the within-family estimations, I add a set of region-specific dummy variables indicating in which county the individual lived as a teenager; this is to control for regional variation in educational attainment.

The novel part of the within-family analysis is that I am able to control for heterogeneity within the family by adding the grade-point-average at graduation from compulsory school (9th grade) to the specifications. The grades are set according to a nation-wide grading system, and for some subjects partly reflect pupil performance on national tests. The grade-point-average at graduation from compulsory school is a determinant for entry into upper-secondary education.

I have constructed the grade-point-average by using the grade register, which provides information on grades in specific subjects, the grade scale ranging from 1 to 5. To form a meaningful measure of the grade-point-average, I have excluded grades in English and Math, since these subjects are offered at different levels and therefore do not allow a straightforward comparison. The grade-point-average is the mean of the grades in Swedish, Social Science, Science, Music, Handicraft, Art and Physical Training. Moreover, when the grade is missing due to low school attendance or insufficient information, I have assigned the value zero to these grades. In a sensitivity analysis I also allow for non-linearity in the grade-point-average variable, and I also enter the grades in different subjects separately.

Age at graduation is normally 16, which raises concerns about whether the grade variable is endogenously determined. If a teenage mother has her child as early as age 15 or 16, the grades are likely to have been affected by the fact that she had or expected a child. However, only 5 per cent of all the teenage mothers in the sister sample had their child before age 17, and these 16 individuals have been excluded from the analysis. [9]

Apart from adding controls for school performance in the within-family analysis, a few other explanatory variables that might capture some within-family heterogeneity are included. I add controls for whether the individuals themselves were born to teenage mothers, whether they are oldest sibling, oldest among their sisters that enter the analysis,

[9] However, including or excluding the observations where the grades are potentially endogenously determined, gives almost identical coefficients in the empirical analysis.

year of birth and finally, I also control for having children (a dummy variable indicating if the woman had had at least one child in 2002).[10]

Previous studies adopting a siblings approach to the topic of teenage childbearing have used survey data with limited sample sizes. The data set used in this study is register based, which should reduce the risk of measurement errors. This is of particular importance since the fixed effect estimator tends to exacerbate measurement errors, leading to coefficients biased towards zero. Therefore, in comparison to previous studies, it is less likely in this study that the drop (in absolute value) in the coefficient typically found when using the within-family estimator is attributable to measurement error rather than to controlling for selection.

This study also has the advantage of a significantly larger sample size. For example, Geronimus and Korenman (1992) have between 50 and 125 sister-pairs identifying the coefficient of interest, compared to 322 sister-pairs (or sister-groups) in my data.

4. Empirical Findings

4.1 Summary Statistics

Table 1 summarizes the background characteristics for the random sample and the sister sample. In the random sample of women born 1974 to 1977 (column 1), around 3 per cent had a child before age 20 (teen mother). The average length of education is 12.98 years, whereas the average length of mother's and father's education lies somewhere between 10.5 and 11 years. 6 per cent of the women in the sample had themselves a teenage mother. The mean grade-point-average is 3.34. 81 per cent of the sample lived with both biological parents in 1985. Turning to column 2 in Table 1, striking differences between the average and teenage mothers emerge. Teenage mothers have significantly fewer years of education; 10.65 years compared to 12.98 years for the full sample. Teenage mothers also come out worse than the average in terms of family background characteristics; they have less educated parents and their father's annual earnings are lower. Moreover, teenage mothers are more frequently exposed to family dissolution (a lower share with intact family), and they have a

[10] Geronimus and Korenman (1992) restricted their sister samples to include only mothers, whereas Ribar (1999) did not impose this restriction. In this paper, I allow for both mothers and non-mothers to enter the sibling analysis, but I enter a control variable indicating if the woman has had a child (in 2002).

higher probability of themselves being born to teenage mothers. Their mean grade-point-average is also significantly lower; 2.53 compared to 3.34 in the full sample.

Column 3 in Table 1 reports characteristics for the sister sample, consisting of women born 1972-1974. When comparing the random sample (column 1) with the sister sample (column 3), some expected differences in terms of family structure emerge. The women in the sister sample have more full biological siblings, fewer half biological siblings, and have less frequently experienced parental separation.

The attempt to try to control for selection bias by comparing sisters motivates a closer look at the sisters identifying the teen mother coefficient in the within-family analysis. The characteristics of the identifying sample (a sub sample of the sister sample) are shown in columns 4 and 5 in Table 1. Column 4 shows that teenage mothers have fewer years of education than the average, and their parental characteristics indicate disadvantaged backgrounds, just like in the random sample. Comparing the teen mothers with their sisters (columns 4 and 5), we see that teenage mothers are less educated than their sisters. However, the difference in years of education between teen mothers and the average is larger than the same difference between teen mothers and their sisters. Indeed, this suggests that there is a selection process at stake, and that the siblings approach might contribute to reducing selection bias in the case where we have a limited set of controls for family and individual characteristics. In terms of parental education and father's annual earnings, there are no significant differences between teen mothers and their sisters, as expected.

Since I introduce the grade-point-average as a means to control for heterogeneity within the family, it is of interest whether it differs across teen mothers and sisters of teen mothers. In the identifying sample, the mean grade-point-average is 2.63 for teen mothers and 2.79 for their sisters. This difference is significant, and suggests that there is variation in school performance within the family that can be captured with this variable.

In the sister sample, I have also introduced a dummy variable indicating whether the individual is the oldest among the sisters from her family that enter the sister sample. Teenage mothers are more frequently the oldest among her sisters; 59 per cent of the teen mothers are oldest sister compared to 40 per cent of the sisters.

4.2 Regression results

I first estimate cross-sectional regressions of the effect of teen motherhood on educational outcomes. Then, I estimate the coefficient using within-family variation, and compare these estimates with the ones obtained in the cross section. To assess the importance of heterogeneity within the family, I use specifications that include and exclude the grade-point-average variable and the other control variables. The estimation results are summarized in Table 2 but full estimation results are available in Appendix B.

Panel A of Table 2 presents the coefficients of the effect of teenage motherhood on years of education, using the random sample cross-section data set. First, I estimate the coefficient excluding any control variables. On average, teenage mothers have 2.4 fewer years of education than non-teenage mothers. Controlling for the grade-point-average reduces the coefficient to a one year difference in education between teen mothers and non-teen mothers (column 2), and controlling for family background characteristics (column 3) yields a difference of 1.21 years. Finally, including both the grade-point-average and family background variables reduces the coefficient further, to -0.59 (column 4). Comparing this coefficient to the traditional cross-section result when only controlling for family background, reveals that information on pre-motherhood individual characteristics is crucial. The absolute size of the coefficient is reduced by one half, from -1.21 to -0.59.

Panel B of Table 2 reports the corresponding cross-sectional results for the identifying sample (i.e., the teenage mothers and their sisters that will identify the teen mother coefficient in the within-family analysis). Here, teen mothers have on average 0.94 fewer years of schooling than non-teen mothers. When introducing controls for family background and for individual grade-point-averages, the coefficient is reduced to -0.7.

Turning to the within-family estimates in panel C of Table 2, the raw education gap shows that on average, teenage mothers have 0.93 fewer years of education than their sisters, compared to a gap of 2.4 years in the cross-sectional estimates on the random sample. Interestingly, the corresponding within-family coefficient in Ribar (1999) is -0.89.

Previous studies that have addressed the topic of teenage childbearing with the siblings approach have been able to control only for age and residential area in the within-family analysis (Geronimus and Korenman 1992). However, a major concern is that these coefficients are also downward biased due to selection within the family. Still, an argument in favour of the siblings approach has been that the within-family coefficients are typically smaller (in absolute value) than the corresponding cross-sectional coefficients, and therefore constitute a lower bound on the effect of teenage motherhood.

In this study, I introduce new controls for pre-motherhood characteristics that differ across sisters, whereby I am able to test whether selection within the family has a major impact on the size of the coefficient. Thus, I test the assumption of strict exogeneity within the family. First, in column 2 (panel C of Table 2) I add grade-point-average to the specification. This reduces (in absolute terms) the coefficient of the effect of teenage motherhood on years of education from -0.93 to -0.73, the fall in the coefficient being significantly different from zero.[11] In column 3 I control for year of birth, for having children (a dummy variable) and a few family characteristics (see Appendix B, Table 3). Adding these controls produces a coefficient of -0.76. Most interestingly, controlling both for grade-point-average, year of birth, having children and family characteristics (column 4) reduces the coefficient further, to -0.60. The fall in the coefficient from -0.93 to -0.60 (no controls vs. all controls), as well as the fall from -0.76 to -0.60 (basic controls excluding grade-point-average vs. all controls) are statistically significant at significance levels lower than 5 per cent. Finally, the inclusion of regional dummies does not have any major impact on the estimated coefficient of interest, it produces a point estimate of -0.59 (column 5).

By introducing variables that control for within-family heterogeneity, the estimated negative coefficient of the effect of teenage motherhood on years of education is reduced from -0.93 to -0.59. This indicates that unobserved heterogeneity within the family indeed is a source of bias when estimating family fixed effects. We cannot assume that teenage births are random to sisters within the same family (i.e., that strict exogeneity holds in the family), and this calls for caution in interpreting results when we can not control for sibling's individual characteristics. Moreover, looking back to panel A in Table 2, we see that when including grade-point-average as a control, the coefficient of teenage motherhood is the

[11] To test whether the coefficient of teen motherhood on years of education differs significantly across the different specifications I perform the following Wald test (r and u denoting restricted and unrestricted models respectively):
The null hypothesis is that $\beta_r - \beta_u = 0$, and to test this I need
$Var(\beta_r - \beta_u) = Var(\beta_r) + Var(\beta_u) - 2Cov(\beta_r, \beta_u)$. To find the covariance I estimate the restricted and the unrestricted models simultaneously in the same estimation; this is done by stacking the data twice (hence the number of observations is doubled). The first replica of the data is assigned a dummy for the restricted model and the other is assigned a dummy for the unrestricted model. A cluster adjustment of the standard errors is used since each individual appears twice in the data. For references, see Stata base reference manual (2003) and for a general discussion, see Clogg et al. (1995) and Allison (1995).

same both for the cross-sectional estimates on the random sample and for the within-family specification. That is, when controlling for pre-motherhood school performance, the siblings approach is not more informative than a traditional cross-section.

It is noteworthy however, that even though the inclusion of new controls works to reduce the coefficient of teen motherhood on educational outcomes, there is still a negative and significant effect of adolescent childbearing. I discuss this further when concluding in Section 6.

5. Sensitivity Analysis

Above, the within-family analysis shows that teenage motherhood significantly reduces years of education. However, this result can be discussed further from several points of view. First, the siblings approach assumes that siblings' decisions and choices are independent of each other's. A natural objection is that if a teenage girl has a child, this will also affect her sister's behaviour. Second, how should the negative effect of teenage motherhood on educational outcome be interpreted? What we observe could be a delay of human-capital investment, meaning that teenage mothers are catching up later in life. It could also be the case that teenage mothers have permanently lower educational levels. Below, I discuss these points further, and test them empirically. I also consider how varying the cut-off age in the definition of a teen mother affects the results, as well as comment on the use of the grade-point-average instead of entering each grade individually in the equation.

5.1 Do sisters influence each other?

This paper contributes to the literature by controlling for differences between sisters within the same family. However, one concern in this type of study is that sisters interact with each other and influence one another's decisions. Given all the characteristics we can control for, teenage births might still not be random to sisters if one sister becomes a teenage mother and by that influences her sister's fertility and human-capital investment decisions.

To the extent that the teenage birth of a woman influences her sister, it is likely that this influence is stronger from older sister to younger than the other way around. The reason for this is that when the younger sister has her child (in her teens), the older sister will be in her late teens or past her teens, and by that time she has already taken some decisions regarding her human-capital formation and her childbearing is already postponed past her teens. In contrast, if the older sister has a child as a teenager, this is more likely to affect her younger sister's fertility and human-capital decisions.[12]

Returning to Table 1 reveals that of the teenage mothers in the identifying sample, 59 per cent were the oldest of the sisters in this particular sample, compared to 40 per cent among their sisters.[13] This raises concerns about the degree of interdependence between sisters, and the extent to which older sisters influence their younger sisters. Since we have already concluded that the influences are less likely to run from younger sister to older, Table 3 presents the regression results on the sister sample when excluding the teenage mothers who are the oldest sister. Now, the estimated effects of teenage motherhood are identified only using sister-pairs (or groups) where the younger sister had a teenage birth, and the bias due to interdependence should therefore be reduced. We should expect that if the influences from older sister to younger stimulate the younger sister to follow her sister's behaviour and have a child early in life and/or put less effort in to human-capital formation, the coefficient of the effect of teenage motherhood should be biased upward. On the other hand, if the influence from older to younger stimulates the younger sister not to have a child early, and to focus more on human-capital investments, it will lead to a downward bias of the coefficient. Looking at the possible change of the coefficient when excluding the older sisters might therefore reveal something about the direction of the bias, and which type of influence that is at stake here.

The estimated within-family coefficients in panel B of Table 3 are (not surprisingly) measured with less precision than the previous coefficients, and are somewhat less negative than the respective coefficients in Table 2 (-0.42 compared to -0.59). This is consistent with the hypothesis that the coefficients in Table 2, panel C, are downward biased due to sisters of teen mothers choosing a different track in life than their sisters. Whether or not previous estimates are biased due to interaction between sisters, reducing the possible source of bias still leaves a significant negative effect of teenage motherhood on years of education. Hence,

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12 If a younger sister is inspired to also have a child in her teens, the particular sister-pair will not be included in the identifying sample. So the interdependence we might be worried about in this empirical analysis is effects on human capital investments and on early childbearing that takes place just after the teenage period.

13 This does not necessarily mean that they are the oldest sister among all full siblings in the same family, only that they are the oldest of their sisters in the sample used in this study.

even though interaction between sisters might play an important role, it is not driving the results of this study.

5.2 A delay or a level effect?

In the previous literature, there is some evidence that the negative consequences of early motherhood are short-lived, indicating that teen mothers delay their human-capital investments and catch up later in life. For example, Hotz et al. (2002) find that teenage mothers are not less educated, when evaluating their outcomes at age 28.

To be able to assess the question of a delay vs. a level effect of teenage motherhood on educational outcomes, the first thing to note is that the cohorts in this study are still relatively young and human-capital investments may not be completed for neither teenage mothers nor non-teenage mothers.

If the coefficient of teenage motherhood reflects that teenage mothers postpone their human-capital investments, we should expect to see a pattern where the older cohorts are more educated than the younger cohorts. To study this, a descriptive approach is to tabulate years of education or attained educational levels by cohort, to see if there are any visible trends in the data. Unless there is no trend towards increased education among the younger cohorts in the data, we would expect younger cohorts to be less educated than the older cohorts, if human-capital investments are not completed. That is, the older women in the sample have had more time to accumulate years of schooling than the younger, so if the younger women are lagging behind purely because of their age, we should expect to see a decrease in education from older to younger cohorts.

To overcome the problem that human-capital investments might not be completed even for non-teenage mothers, I choose to take a closer look at completion of upper secondary schooling, since human-capital investments at this level of education are more likely to have been completed than post-secondary schooling investments, in particular for non-teenage mothers.

Table 4 tabulates the proportion with at least two years of upper secondary schooling for each cohort in the random sample and the sister sample. Column 1 shows that for the full samples, the cohort born in 1977 is somewhat less educated, but apart from that there is stability over birth cohorts. Turning to column 2 in Table 4 reveals that for teenage mothers, there is a slight tendency that the younger cohorts are less educated than the older cohorts, which would be consistent with teenage mothers delaying their human-capital investments. This finding is, however, not very robust. Thus, descriptive statistics give weak support of the idea that the effect of teenage motherhood is a pure delay of human-capital investment.

A final test of the delay vs. level hypothesis is to re-run the regression analysis above, excluding the youngest cohorts. If a delay effect is present, we should expect the estimated coefficient to go down (in absolute value) when excluding the youngest individuals, since those are the ones presumably lagging behind most. Table 5 reports the regression results when the cohorts born in 1976 and 1977 have been excluded. The estimated coefficients of the effect of teenage motherhood on years of education are strikingly similar to those of Table 2, and even somewhat larger than those found in Table 2 (comparing columns 4 and 5 in Table 2 and Table 5). This does not support the notion that the negative effect of teenage motherhood is driven by teenage mothers delaying their human-capital investment.

5.3 The definition of teenage mother

The choice of cut-off age in the definition of teen motherhood is not straightforward. Using a binary indicator variable for teenage motherhood does not necessarily mean that there is a discrete jump, attached to a certain age, in the consequences of early motherhood. It could be the case that the consequences are linearly related to age at first birth rather than to teen/non-teen mother. It is also plausible, however, that there is a discrete shift in the consequences of teen motherhood and that this shift is related to completion of upper secondary schooling.

In the above analysis the teen mother variable is an indicator variable that takes on the value one if the woman had a child before the month she turned 20. Setting the cut-off age to 19 and 18 yields within-family coefficients of -0.63 and -0.90, respectively.[14] This is to be compared with -0.59, the corresponding coefficient in the baseline specification (Table 2). This is not a surprising result, since it is most likely that the consequences of teenage motherhood are aggravated the younger is the mother.

[14] I do not report these estimation results in further detail, but they can be obtained from the author upon request.

5.4 The grade variable

So far, I have entered information on school performance linearly, in the form of a grade-point-average. One possibility is that entering grades non-linearly and/or individually by subject would capture more of the heterogeneity between sisters, and therefore give different coefficients of the effect of teen motherhood on educational outcomes than the baseline coefficient reported in Table 2.

Introducing the grade-point-average non-linearly, allowing for a squared term, yields a point estimate of -0.60 in the within-family analysis, compared to -0.59 in Table 2. When entering all grades individually and linearly, the estimated effect of teen motherhood on educational outcome is -0.66, and when allowing for non-linearity's (entering squared terms) the corresponding coefficient is -0.63.[15] The main insight from this exercise is that irrespective of how the information on school performance is entered in the specifications, it contributes by controlling for differences between sisters.

6. Conclusions

To sum up, the results of this paper confirm the presumption that previous within-family estimates of the labour market consequences of teenage motherhood are biased towards too negative effects. The source of this bias is a selection process within the family, since previous studies have been unable to control for differences in individual-specific factors across sisters. In this study I contrast traditional cross-sectional estimates with within-family estimates, and in the within-family analysis I control for heterogeneity within the family by including a set of variables that differ between sisters, pre-motherhood grade-point-averages being particularly important. When including controls for between-sister heterogeneity, within-family estimates are significantly reduced, from -0.93 (no controls) to -0.59 (all controls). This fall of the coefficient (in absolute terms) represents 0.34 years of schooling, which is economically significant considering that these results are concerned with women at the lower end of the schooling distribution. Thus, previous studies have somewhat overstated the negative consequences of teenage motherhood.

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[15] I do not report these estimation results in further detail, but they can be obtained from the author upon request.

Still, even though the estimated effects are shown to have been previously biased, my results indicate that there indeed is a penalty to teenage motherhood in terms of years of education. That is, teen mothers come out worse also in a welfare state like Sweden, where single mothers have priority to publicly subsidized day care and adult education is readily available. The estimated negative effect of -0.59 years of schooling is substantial, considering, as mentioned above, that this concerns women at the lower tail of the distribution.

It is also noteworthy that the inclusion of the grade-point-average yields similar coefficients of the effect of teen motherhood on educational outcomes in both the cross-section analysis on the random sample and the within-family analysis. Therefore, with detailed individual-specific information, the siblings approach is not more informative than a traditional cross section.

The results of this study contribute not only to the knowledge of the effects of teenage motherhood. Within-family heterogeneity is likely to play a role also in other research areas where the siblings approach has been adopted. Moreover, the results shed some light on the discussion of ability bias in twin estimates of returns to schooling (Behrman and Rosenzweig 1999, Bound and Solon 1999, Neumark 1999). If unobservable differences between biological siblings bias within-family estimates, one might expect that unobservable between-twins differences in ability have a similar effect on estimates of returns to schooling.

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TABLE 1

SUMMARY STATISTICS

	THE RANDOM SAMPLE (WOMEN BORN 1974-1977)		THE SISTER SAMPLE (WOMEN BORN 1972-1977)		
				Identifying Sample	
	1) ALL	2) TEEN MOTHERS	3) ALL	4) TEEN MOTHERS	5) SISTERS OF TEEN MOTHERS
VARIABLE	MEAN (STD. DEV.)	MEAN (STD. DEV.)	MEAN (STD.DEV.)	MEAN (STD. DEV.)	MEAN (STD. DEV.)
Teen Mother	0.03	1.00	0.03	1.00	0.00
	(0.16)	(0.00)	(0.18)	(0.00)	(0.00)
Year of Birth	1975.4	1975.3	1974.74	1974.3	1974.9
	(1.12)	(1.11)	(1.67)	(1.63)	(1.67)
Years of Education (2002)	12.98	10.65	13.03	10.73	11.67
	(1.87)	(1.52)	(1.91)	(1.53)	(1.72)
Years of Education, Mother	10.97	9.82	11.08	9.74	9.67
	(2.54)	(1.92)	(2.59)	(1.98)	(1.99)
Years of Education, Father	10.70	9.62	10.82	9.54	9.49
	(2.90)	(2.32)	(2.97)	(2.29)	(2.27)
Log Earnings 1975, Father	10.61	10.49	10.65	10.55	10.53
	(0.56)	(0.54)	(0.54)	(0.43)	(0.43)
Log Earnings 1985, Father	11.60	11.37	11.62	11.41	11.39
	(0.63)	(0.68)	(0.64)	(0.69)	(0.69)
Having children (2002)	0.29	1.00	0.35	1.00	0.56
	(0.46)	(0.00)	(0.48)	(0.00)	(0.50)
Born to Teen Mother	0.06	0.19	0.06	0.16	0.13
	(0.24)	(0.39)	(0.23)	(0.37)	(0.34)
Grade-Point-Average	3.34	2.53	3.39	2.63	2.79
	(0.68)	(0.64)	(0.68)	(0.66)	(0.71)
Intact Family(1985)	0.81	0.58	0.87	0.71	0.72
	(0.39)	(0.49)	(0.33)	(0.45)	(0.45)
Living with Mother Only (1985)	0.16	0.35	0.11	0.26	0.26
	(0.36)	(0.48)	(0.31)	(0.44)	(0.44)
Living with Father Only (1985)	0.02	0.05	0.02	0.02	0.02
	(0.15)	(0.22)	(0.12)	(0.15)	(0.13)
Single Child	0.05	0.03	0.00	0.00	0.00
	(0.23)	(0.17)	(0.00)	(0.00)	(0.00)
Oldest Sister	-	-	0.52	0.59	0.40
			(0.50)	(0.49)	(0.49)
Oldest Sibling	0.40	0.35	0.37	0.34	0.21
	(0.49)	(0.48)	(0.48)	(0.47)	(0.40)
Nr of Full Siblings	1.35	1.49	1.83	2.30	2.35
	(1.00)	(1.33)	(1.07)	(1.57)	(1.63)
Nr of Half Siblings, Mother	0.24	0.70	0.14	0.38	0.40
	(0.67)	(1.10)	(0.49)	(0.82)	(0.86)
Nr of Half Siblings, Father	0.31	0.71	0.22	0.59	0.56
	(0.81)	(1.21)	(0.67)	(1.05)	(1.02)
N	33 626	927	12 105	333	340

TABLE 2

ESTIMATES OF THE EFFECT OF TEENAGE MOTHERHOOD ON YEARS OF EDUCATION

A. OLS ON THE RANDOM SAMPLE	(1)	(2)	(3)	(4)	(5)
Explanatory Variable					
Teen Mother	-2.40***	-0.99***	-1.21***	-0.59***	-0.59***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Grade-Point-Average		1.71***		1.44***	1.43***
		(0.01)		(0.01)	(0.01)
Other Controls (1)			Yes	Yes	Yes
County dummies					Yes
R^2	0.04	0.42	0.25	0.46	0.47
N	33 626	33 626	33 626	33 626	33 626
B. OLS ON THE IDENTIFYING SAMPLE					
Explanatory Variable					
Teen Mother	-0.94***	-0.76***	-0.82***	-0.70***	-0.67***
	(0.13)	(0.11)	(0.14)	(0.13)	(0.12)
Grade-Point-Average		1.14***		1.00***	1.05***
		(0.09)		(0.10)	(0.09)
Other Controls (2)			Yes	Yes	Yes
County dummies					Yes
R^2	0.08	0.29	0.19	0.34	0.39
N	673	673	673	673	673
C. WITHIN-FAMILY ESTIMATES ON THE SISTER SAMPLE					
Explanatory Variable					
Teen Mother	-0.93***	-0.73***	-0.76***	-0.60***	-0.59***
	(0.16)	(0.15)	(0.16)	(0.15)	(0.15)
Grade-Point-Average		1.29***		1.25***	1.26***
		(0.05)		(0.05)	(0.05)
Other Controls (3)			Yes	Yes	Yes
County dummies					Yes
R^2-within	0.01	0.19	0.04	0.21	0.21
Nr of groups identifying (4)	322	322	322	322	322
N identifying (4)	673	673	673	673	673
Nr of groups	5 948	5 948	5 948	5 948	5 948
N	12 105	12 105	12 105	12 105	12 105

Notes: Heteroskedastic robust standard errors in parenthesis. In panel C the standard errors are adjusted for clustering on sisters.
*** denotes statistical significance at the 1 per cent level.
1)/2) /3) Full regression results are reported in Tables 1, 2 and 3, Appendix B.
4) Indicates the nr of groups/individuals identifying the teen mother coefficient.

TABLE 3

ESTIMATES OF THE EFFECT OF TEENAGE MOTHERHOOD ON YEARS OF EDUCATION

RESTRICTION: OLDEST SISTER TEEN MOTHERS ARE EXCLUDED

A. OLS ON THE IDENTIFYING SAMPLE	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Explanatory Variable					
Teen Mother	-1.05***	-0.68***	-0.81*	-0.48	-0.35
	(0.19)	(0.17)	(0.42)	(0.36)	(0.36)
Grade-Point-Average		1.28***		1.08***	1.07***
		(0.13)		(0.14)	(0.14)
Other Controls (1)			Yes	Yes	Yes
County dummies					Yes
R^2	0.09	0.35	0.27	0.42	0.49
N	293	293	293	293	293
B. WITHIN-FAMILY ESTIMATES ON THE SISTER SAMPLE					
Explanatory Variable					
Teen Mother	-0.98***	-0.61***	-0.64***	-0.44*	-0.42*
	(0.24)	(0.23)	(0.24)	(0.23)	(0.23)
Grade-Point-Average		1.32***		1.28***	1.29***
		(0.05)		(0.05)	(0.05)
Other Controls (2)			Yes	Yes	Yes
County dummies					Yes
R^2-within	0.01	0.19	0.04	0.21	0.21
Nr of groups identifying (3)	140	140	140	140	140
N identifying (3)	293	293	293	293	293
Nr of groups	5 741	5 741	5 741	5 741	5 741
N	11 673	11 673	11 673	11 673	11 673

Notes: Heteroskedastic robust standard errors in parenthesis. In panel C the standard errors are adjusted for clustering on sisters.
***, **, * denote statistical significance at the 1, 5 and 10 per cent level respectively.
1)/2) Control variables are the same as the ones presented in Tables 1, 2 and 3, Appendix B. Full estimation results for this sample can be obtained from the author upon request.
3) Indicates the nr of groups/individuals identifying the teen mother coefficient.

TABLE 4

TWO YEARS OF UPPER SECONDARY SCHOOLING OR MORE AVERAGES BY YEAR OF BIRTH

THE RANDOM SAMPLE

YEAR OF BIRTH	1) ALL	2) TEEN MOTHERS
1974	0.947	0.651
	(0.002)	(0.028)
	n=9 196	*n=298*
1975	0.956	0.696
	(0.002)	(0.028)
	n=8 520	*n=263*
1976	0.949	0.641
	(0.002)	(0.035)
	n=8 067	*n=184*
1977	0.929	0.560
	(0.003)	(0.037)
	n=7 843	*n=182*

THE SISTER SAMPLE

		Identifying Sample	
YEAR OF BIRTH	1) ALL	2) TEEN MOTHERS	3) SISTERS OF TEEN MOTHERS
1972	0.941	0.672	0.800
	(0.006)	(0.062)	(0.065)
	n=1 610	*n=58*	*n=45*
1973	0.956	0.830	0.862
	(0.006)	(0.052)	(0.065)
	n=1 375	*n=53*	*n=29*
1974	0.951	0.732	0.806
	(0.004)	(0.053)	(0.049)
	n=2 532	*n=71*	*n=67*
1975	0.958	0.729	0.823
	(0.004)	(0.058)	(0.049)
	n=2 083	*n=59*	*n=62*
1976	0.945	0.569	0.887
	(0.005)	(0.070)	(0.041)
	n=2 009	*n=51*	*n=62*
1977	0.934	0.585	0.800
	(0.005)	(0.078)	(0.046)
	n=2 496	*n=41*	*n=75*

Note: Standard errors in parenthesis.

TABLE 5

ESTIMATES OF THE EFFECT OF TEENAGE MOTHERHOOD ON YEARS OF EDUCATION

RESTRICTION: EXCLUDING WOMEN BORN 1976 AND 1977

A. OLS ON THE RANDOM SAMPLE	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Explanatory Variable					
Teen Mother	-2.44***	-0.99***	-1.29***	-0.61***	-0.62***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Grade-Point-Average		1.74***		1.44***	1.44***
		(0.02)		(0.02)	(0.02)
Other Controls (1)			Yes	Yes	Yes
County dummies					Yes
R^2	0.05	0.41	0.26	0.46	0.47
N	17 716	17 716	17 716	17 716	17 716
B. OLS ON THE IDENTIFYING SAMPLE					
Explanatory Variable					
Teen Mother	-0.93***	-0.75***	-0.81***	-0.62***	-0.66***
	(0.20)	(0.17)	(0.21)	(0.19)	(0.19)
Grade-Point-Average		1.06***		0.97***	0.99***
		(0.13)		(0.15)	(0.16)
Other Controls (2)			Yes	Yes	Yes
County dummies					Yes
R^2	0.08	0.28	0.21	0.36	0.43
N	267	267	267	267	267
C. WITHIN-FAMILY ESTIMATES ON THE SISTER SAMPLE					
Explanatory Variable					
Teen Mother	-0.92***	-0.75***	-0.80***	-0.63***	-0.68***
	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)
Grade-Point-Average		1.22***		1.23***	1.23***
		(0.10)		(0.10)	(0.10)
Other Controls (3)			Yes	Yes	Yes
County dummies					Yes
R^2-within	0.01	0.17	0.03	0.18	0.19
Nr of groups identifying (4)	131	131	131	131	131
N identifying (4)	267	267	267	267	267
Nr of groups	1 887	1 887	1 887	1 887	1 887
N	3 800	3 800	3 800	3 800	3 800

Notes: Heteroskedastic robust standard errors in parenthesis. In panel C the standard errors are adjusted for clustering on sisters.
*** denotes statistical significance at the 1 per cent level.
1)/2) / 3) Control variables are the same as the ones presented in Tables 1, 2 and 3, Appendix B. Full estimation results for this sample can be obtained from the author upon request.
4) Indicates the nr of groups/individuals identifying the teen mother coefficient.

APPENDIX A

ASSIGNING YEARS OF SCHOOLING

The Swedish Education Register (SUN2000), administered by Statistics Sweden, contains information of both level and field of education. The information on level of education is a three-digit code, where the first digit indicates the following levels (1-6), and the number of years of education assigned to each of these levels is added in parethesis:

1) comprehensive school (seven years for incomplete grundskola (years 1-6) or folkskola[18])
2) new comprehensive school or realskola (nine years of education)
3) upper secondary school (ten to twelve years of education)
4) post secondary schooling of less than two years (thirteen years of education)
5) post secondary schooling of two years or more (fourteen to seventeen years of education)
6) upper graduate level education (seventeen to nineteen years of education)

The second digit gives a more precise measure of the level and length of education. In particular, it reveals the length of upper secondary schooling (less than two years, two or three years), the length of post secondary schooling (two, three, four or five years) and upper graduate education (two or four years). This means that the variable years of schooling can take on the following values: 7, 9, 10, 11, 12, 13, 14, 15, 16, 17 and 19. The third digit is not informative concerning length of education.

When assigning years of education to a specific level, it is automatically assumed that lower levels of education have been completed. For example, for upper secondary schooling, it is assumed that the individual has completed comprehensive school (nine years) and to these nine years, the number of years at upper secondary level is added. The same principle holds for post secondary schooling; it is assumed that an individual with any post secondary schooling has twelve years of education before entering post secondary education. And finally, it is assumed that an individual at upper graduate level has completed a three year undergraduate degree (fifteen years of education) before reaching upper graduate level.

[18] It is not obvious how to assign years of schooling to individuals with incomplete compulsory school. However, in the data used in the current paper, there are no individuals belonging to this category.

Moreover, assigning years of schooling in this manner assumes that the actual years of schooling for an individual at a certain level is the same as the formal length of education. However, it is likely that for some individuals the actual time to reach a specific level is longer (or even shorter) than the time the curriculum has assigned to it.

APPENDIX B TABLE 1

ESTIMATES OF THE EFFECT OF TEENAGE MOTHERHOOD ON YEARS OF EDUCATION

COMPLETE REGRESSION RESULTS, THE RANDOM SAMPLE

	COEFFICIENT (STD.ERR.)	COEFFICIENT (STD.ERR.)	COEFFICIENT (STD.ERR.)
Teen Mother	-1.207	-0.591	-0.586
	(0.053)	(0.048)	(0.048)
Grade-Point-Average		1.436	1.431
		(0.013)	(0.013)
Born 1974	0.297	0.216	0.218
	(0.025)	(0.021)	(0.021)
Born 1975	0.260	0.198	0.199
	(0.025)	(0.021)	(0.021)
Born 1976	0.190	0.151	0.149
	(0.025)	(0.021)	(0.021)
Years of Education, Mother	0.137	0.065	0.069
	(0.004)	(0.003)	(0.003)
Years of Education, Father	0.088	0.038	0.044
	(0.004)	(0.003)	(0.003)
Log Earnings 1975, Father	0.130	0.076	0.080
	(0.017)	(0.014)	(0.014)
Log Earnings 1985, Father	0.117	0.048	0.066
	(0.016)	(0.013)	(0.013)
Having children	-0.871	-0.496	-0.506
	(0.021)	(0.018)	(0.018)
Born to Teen Mother	-0.400	-0.190	-0.197
	(0.039)	(0.034)	(0.034)
Intact Family (1985)	0.669	0.282	0.244
	(0.098)	(0.083)	(0.083)
Living with Mother Only (1985)	0.189	0.074	0.064
	(0.099)	(0.085)	(0.084)
Living with Father Only (1985)	0.236	0.077	0.077
	(0.113)	(0.097)	(0.097)
Single Child	-0.049	0.021	0.034
	(0.044)	(0.038)	(0.037)
Oldest Sibling	0.259	0.131	0.123
	(0.020)	(0.017)	(0.017)
Nr of Full Siblings	-0.043	-0.031	-0.038
	(0.011)	(0.009)	(0.009)
Nr of Half Siblings, Mother	-0.134	-0.065	-0.067
	(0.016)	(0.014)	(0.014)
Nr of Half Siblings, Father	-0.071	-0.035	-0.030
	(0.013)	(0.011)	(0.011)
Constant	7.361	5.516	4.919
	(0.235)	(0.198)	(0.201)
County dummies			Yes
R^2	0.25	0.46	0.47
N	33 626	33 626	33 626

Notes: Heteroskedastic robust standard errors in parenthesis.
Reference category for cohort dummies is individuals born in 1977.

APPENDIX B TABLE 2

ESTIMATES OF THE EFFECT OF TEENAGE MOTHERHOOD ON YEARS OF EDUCATION

COMPLETE REGRESSION RESULTS, THE IDENTIFYING SAMPLE

	COEFFICIENT (STD.ERR.)	COEFFICIENT (STD.ERR.)	COEFFICIENT (STD.ERR.)
Teen Mother	-0.823	-0.698	-0.668
	(0.140)	(0.126)	(0.124)
Grade-Point-Average		1.001	1.053
		(0.098)	(0.094)
Born 1973	0.471	0.455	0.451
	(0.243)	(0.218)	(0.209)
Born 1974	0.163	0.229	0.222
	(0.215)	(0.190)	(0.187)
Born 1975	0.337	0.383	0.331
	(0.247)	(0.217)	(0.219)
Born 1976	0.461	0.483	0.398
	(0.262)	(0.232)	(0.234)
Born 1977	0.386	0.329	0.287
	(0.273)	(0.251)	(0.250)
Years of Education, Mother	0.117	0.056	0.067
	(0.034)	(0.032)	(0.034)
Years of Education, Father	0.096	0.066	0.059
	(0.028)	(0.026)	(0.027)
Log Earnings 1975, Father	0.353	0.245	0.269
	(0.149)	(0.147)	(0.151)
Log Earnings 1985, Father	-0.078	-0.011	0.002
	(0.094)	(0.092)	(0.100)
Having children	-0.411	-0.260	-0.286
	(0.184)	(0.164)	(0.166)
Born to Teen Mother	-0.351	-0.290	-0.338
	(0.186)	(0.167)	(0.168)
Intact Family (1985)	1.294	0.977	0.327
	(0.643)	(0.783)	(0.828)
Living with Mother Only (1985)	1.057	0.966	0.411
	(0.657)	(0.792)	(0.834)
Living with Father Only (1985)	0.013	0.040	-0.602
	(0.745)	(0.844)	(0.890)
Oldest Sister	0.258	0.132	0.072
	(0.198)	(0.182)	(0.182)
Oldest Sibling	0.196	0.168	0.192
	(0.196)	(0.174)	(0.172)
Nr of Full Siblings	-0.020	0.002	0.005
	(0.049)	(0.041)	(0.040)
Nr of Half Siblings, Mother	-0.177	-0.076	-0.058
	(0.070)	(0.065)	(0.070)
Nr of Half Siblings, Father	-0.075	-0.066	-0.084
	(0.064)	(0.054)	(0.059)
Constant	5.586	4.143	3.596
	(1.962)	(1.993)	(2.045)
County dummies			Yes
R^2	0.19	0.34	0.39
N	673	673	673

Notes: Heteroskedastic robust standard errors in parenthesis.
Reference category for cohort dummies is individuals born in 1972.

APPENDIX B TABLE 3

ESTIMATES OF THE EFFECT OF TEENAGE MOTHERHOOD ON YEARS OF EDUCATION

COMPLETE REGRESSION RESULTS, WITHIN-FAMILY ESTIMATES

	COEFFICIENT (STD.ERR.)	COEFFICIENT (STD.ERR.)	COEFFICIENT (STD.ERR.)
Teen Mother	-0.762	-0.600	-0.589
	(0.157)	(0.152)	(0.151)
Grade-Point-Average		1.255	1.260
		(0.053)	(0.053)
Born 1973	0.038	0.075	0.071
	(0.104)	(0.095)	(0.095)
Born 1974	0.197	0.221	0.217
	(0.100)	(0.091)	(0.091)
Born 1975	0.166	0.165	0.160
	(0.124)	(0.112)	(0.112)
Born 1976	0.089	0.101	0.100
	(0.157)	(0.143)	(0.143)
Born 1977	-0.025	-0.025	-0.028
	(0.177)	(0.161)	(0.161)
Having children	-0.480	-0.353	-0.354
	(0.064)	(0.058)	(0.058)
Born to Teen Mother	-0.250	-0.299	-0.299
	(0.130)	(0.117)	(0.117)
Oldest Sister	0.195	0.078	0.078
	(0.116)	(0.107)	(0.107)
Oldest Sibling	0.073	0.066	0.068
	(0.084)	(0.076)	(0.076)
Constant	13.032	8.788	8.526
	(0.151)	(0.224)	(0.585)
County dummies			Yes
R^2-within	0.04	0.21	0.21
Nr of groups identifying (1)	322	322	322
N identifying (1)	673	673	673
Nr of groups	5 948	5 948	5 948
N	12 105	12 105	12 105

Notes: Standard errors are adjusted for clustering on sisters.
Reference category for cohort dummies is individuals born in 1972.
1) Indicates the nr of groups/individuals identifying the teen mother coefficient.